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Essays in Labor and Public Economics

par
Louis-Philippe Béland

Département de sciences économiques
Faculté des arts et sciences

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présentée par :

Louis-Philippe Béland

évaluée par un jury composé des personnes suivantes :

Andriana Bellou, présidente-rapporteure
Baris Kaymak, directeur de recherche
Daniel Parent, co-directeur de recherche
Joshua Lewis, membre du jury
Fabian Lange, examinateur externe
Michael Huberman, représentant du doyen de la FAS

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To my parents, Sylvain and Suzanne and my fiancée, Sandra

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Résumé

Dans ma thèse, je me sers de modèles de recherche solides pour répondre à des questions importantes de politique publique.

Mon premier chapitre évalue l'impact causal de l'allégeance partisane (républicain ou démocrate) des gouverneurs américains sur le marché du travail. Dans ce chapitre, je combine les élections des gouverneurs avec les données du March CPS pour les années fiscales 1977 à 2008. En utilisant un modèle de régression par discontinuité, je trouve que les gouverneurs démocrates sont associés à de plus faibles revenus individuels moyens. Je mets en évidence que cela est entrainée par un changement dans la composition de la main-d'oeuvre à la suite d'une augmentation de l'emploi des travailleurs à revenus faibles et moyens. Je trouve que les gouverneurs démocrates provoquent une augmentation de l'emploi des noirs et de leurs heures travaillées. Ces résultats conduisent à une réduction de l'écart salarial entre les travailleurs noir et blanc.

Mon deuxième chapitre étudie l'impact causal des fusillades qui se produisent dans les écoles secondaires américaines sur les performances des éléves et les résultats des écoles tels que les effectifs et le nombre d'enseignants recruté, a l'aide d'une stratégie de différence-en-différence. Le chapitre est coécrit avec Dongwoo Kim. Nous constatons que les fusillades dans les écoles réduisent significativement l'effectif des élèves de 9e année, la proportion d'élèves ayant un niveau adéquat en anglais et en mathématiques. Nous examinons aussi l'effet hétérogene des tueries dans les écoles secondaires entre les crimes et les suicides. Nous trouvons que les fusillades de natures criminelles provoquent la diminution du nombre d'inscriptions et de la proportion d'élèves adéquats en anglais et mathématiques. En utilisant des données sur les élèves en Californie, nous confirmons qu'une partie de l'effet sur la performance des élèves provient des étudiants inscrits et ce n'est pas uniquement un effet de composition.

Mon troisième chapitre étudie l'impact des cellulaires sur la performance scolaire des élèves. Le chapitre est coécrit avec Richard Murphy. Dans ce chapitre, nous combinons une base de données unique contenant les politiques de téléphonie mobile des écoles obtenues à partir d'une enquête auprès des écoles dans quatre villes en Angleterre avec des données administratives sur la performance scolaire des éleves. Nous étudions ainsi l'impact de l'introduction d'une interdiction de téléphonie mobile sur le rendement des éleves. Nos résultats indiquent qu'il y a une augmentation du rendement des éleves après l'instauration de l'interdiction des cellulaires à l'école, ce qui suggère que les téléphones mobiles sont sources de distraction pour l'apprentissage et l'introduction d'une interdiction à l'école limite ce problème.

Mots-clés : Partis Politiques, Marché du travail, écart salarial entre blancs et noirs, Tueries, éducation, Performance scolaire, Cellulaires, Technologies, Régression discontinuité, Différence-en-différence.

Abstract

In my thesis, I use compelling research designs to address important public policy issues.

My first chapter estimates the causal impact of the party allegiance (Republican or Democratic) of U.S. governors on labor market outcomes. I match gubernatorial elections with March CPS data for income years 1977 to 2008. Using a regression discontinuity design, I find that Democratic governors are associated with lower average individual earnings. I provide evidence that this is driven by a change in workforce composition following an expansion in employment of workers with low and medium earnings. I also find that Democratic governors cause a reduction in the racial earnings gap between black and white workers through an increase in the annual hours worked by blacks relative to whites.

My second chapter analyze how shootings in high schools affect schools and students using data from shooting databases, school report cards, and the Common Core of Data. The chapter is co-written with Dongwoo Kim. We examine schools' test scores, enrollment, and number of teachers, as well as graduation, attendance, and suspension rates at schools that experienced a shooting, employing a difference-in-differences strategy that uses other high schools in the same district as the comparison group. Our findings suggest that homicidal shootings significantly decrease the enrollment of students in Grade 9, and reduce test scores in math and English. We find no statistically significant effect for suicidal shootings on any outcome variables of interest.

Using student-level data from California, we confirm that some of the effects on student performance occur as a result of students remaining enrolled and not only due to changes in student body composition.

My third chapter investigates the impact of school mobile phone policy on student performance. The chapter is co-written with Richard Murphy. Combining a unique dataset on autonomous mobile phone policies from a survey of schools in four cities in England with administrative data, we investigate the impact of imposing a mobile phone ban on student performance. Our results indicate an improvement in student results after a school bans the use of mobile phones; this suggests that mobile phones distract learning and imposing a ban limits this problem.

Keywords : Political parties, labor market outcomes, black-white wage gap, shootings, education, academic performance, mobile phones, technologies, regression discontinuity, difference-in-differences.

Coauthors contribution: I am the lead author of all chapters in this thesis. Chapter 2 is co-written with Dongwoo Kim and Chapter 3 is co-written with Richard Murphy.

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General Introduction

Each chapter of this dissertation was written as a separate paper capable of standing on its own as a piece of publishable research. The structure of this dissertation thus includes each of my three papers in unique, self-contained chapters, numbered 1, 2, and 3, respectively. I am fascinated by how policies and policymakers can influence citizens, particularly in the labor market and at school. In this thesis, I am answering three policy-relevant questions using solid research design. Chapter 1 is entitled "Political Parties and Labor Market Outcomes. Evidence from U.S. States". Chapter 2 is entitled "The Effect of High School Shootings on Schools and Student Performance" (with Dongwoo Kim). Chapter 3 is entitled "Ill Communication: Mobile Phones and Student Performance"(with Richard Murphy).

Chapter 1 studies the impact of Partisan allegiance of politicians on labor market outcomes. Politicians and political parties play a crucial role in the economy. A common perception is that Democrats favor pro-labor policies, and are more averse to income inequality than Republicans. Chapter 1 evaluates the veracity of such claims at the U.S. state level by estimating the causal impact of the partisan identity of U.S. governors (Republican vs Democratic) on several labor market outcomes. Using a regression discontinuity design, I find that Democratic governors are associated with lower average individual earnings. I provide evidence that this is driven by a change in workforce composition following an expansion in the employment of workers with low and medium earnings. I also find that Democratic governors cause a reduction in the racial earnings gap between black and white workers through an increase in the annual hours worked by blacks relative to whites. I then explore policies that might explain the results. I find that an increase in public sector employment, an increase in employment in the health and education sectors, a higher state Earned Income Tax Credit (EITC), a (slightly) higher minimum wage, a lower incarceration rate and an impact on worker displacement under Democratic governors contribute to the increase in the employment of low- and medium-earning workers and the increase in blacks' employment and hours worked.

Chapter 2 explores the impact of school violence on student performance. In this article with Dongwoo Kim, I analyze how shootings in high schools affect schools and students using data from shooting databases, school report cards, and the Common Core of Data. Our paper aims to improve the understanding of how extreme violence in schools affects enrollment, student performance, the number of teachers in a school, and student behavior, based on a sample of deadly shootings that occurred between 1994 and 2009. We examine schools' test scores, enrollment, and number of teachers, as well as graduation, attendance, and suspension rates at schools that experienced a shooting. We employ a differences-in-differences strategy that uses other high schools in the same district as the comparison group. We address three questions related to the consequences of homicidal and suicidal high school shootings. First, we address whether enrollment patterns change after the shootings, which could be a result of school selection by students and parents or students dropping out of the school system. Second, we examine whether deadly shootings cause longer-term trauma that lowers test scores in the school up to three years after the incident. Third, we look at behavioral variables and study how they are affected. Our findings suggest that homicidal shootings significantly decrease the enrollment of students in grade 9, and test scores in math and English. We find no statistically significant effect for suicidal shootings on all outcome variables of interest. Using student-level data from California, we confirm that part of the effect on student performance operates through students remaining enrolled and not only through a composition effect.

Chapter 3, written with Richard Murphy, investigates if mobile phones have a negative impact on student performance when allowed on school grounds and in classrooms. Mobile phones are a big part of teenagers' social lives. Mobiles phones are very popular in England, 94% of adults owned a mobile phone in 2012 and 90.3% of teenagers. In this chapter, we examine if they might cause distractions at school and lower test scores. Mobile phones might be distracting to a classroom in many ways. Teachers usually need to work very hard to keep students' attention. With mobile phones, students have access to chat applications, text messaging, the Internet and games and can go on Facebook and Twitter when teachers are trying to teach. Texting is also seen as today's version of passing notes in class but is much more frequent. Using a unique data set on mobile phone policies that we obtained by surveying schools in four cities in the England (London, Manchester, Birmingham and Leicester) along with administrative data, we explore the impact on student performance of imposing a mobile phone ban at school. Our results suggest that introducing a ban leads to improvement in student performance and that mobile phones cause distractions from learning.

Chapter 1

Political Parties and Labor Market Outcomes. Evidence from U.S. States.

Author: Louis-Philippe Béland

1.1 Introduction

Politicians and political parties play a crucial role in the U.S. economy. The common perception is that Democrats favor pro-labor policies, and are more averse to income inequality than Republicans. This paper evaluates the veracity of such claims at the U.S. state level by estimating the causal impact of the party affiliation of U.S. governors (Republican vs. Democratic) on several labor market outcomes.

Recent work provides evidence that political allegiance plays a role in determining politicians' policy choices and voting behavior at the state level of government in the U.S. Besley and Case (1995) find that Democratic governors are more likely to raise taxes, while Republican governors are less likely to increase the minimum wage. They also find that the joint election of Democrats in the state upper and lower houses and in the governor's office has a significant impact on total tax revenues, total spending, family assistance and workers' compensation (Besley and Case, 2003). Building on this, Reed (2006) finds that tax burdens are higher when Democrats control the state legislature than when Republicans have control, and that the political party of the governor has little effect on tax burdens, after controlling for partisan influences in the state legislature. Lee, Moretti, and Butler (2004) exploit the random variation associated with close U.S. congressional elections in a regression discontinuity design (RDD) to show that party affiliation explains a very large proportion of the variation in congressional voting behavior. Leigh (2008) studies numerous policies and outcomes under Democratic and Republican governors in U.S. states from 1941 to 2002. He finds that Democratic governors tend to preside over lower after-tax inequality, implement a higher minimum wage and oversee a lower incarceration rate.

This paper adds to the literature by studying the impact of gubernatorial party affiliation on labor market outcomes. It also examines the specific policies through which party affiliation may affect labor market outcomes. I match data from gubernatorial elections with data from the Current Population Survey's (CPS's) March supplements from 1977 to 2008. I use an RDD to estimate causal effects by comparing labor market outcomes when a Democrat barely wins with labor market outcomes when a Democrat barely loses an election.

This paper's contribution to the literature is threefold. First, this paper studies the causal impact of gubernatorial party affiliation on labor market outcomes, namely earnings, hours worked, weeks worked, employment and labor force participation. Second, it sheds light on whether the party affiliation of governors has an impact on different type of workers, especially with regard to white and black workers. There is an important and well-documented earnings gap between black and white workers (eg. Card and Krueger (1993) and Bjerk (2007)) and this paper investigates whether the party affiliation of governors might affect this gap as a large fraction of black workers vote for Democrats. Third, it makes a link between the results and some of the policies that were implemented.

The results show that Democratic governors are associated with lower individual earnings for workers. I provide evidence that this is driven by a change in the workforce composition via an expansion in the employment of workers with low and medium earnings. The results also indicate that blacks are more likely to work and to participate in the labor market under Democratic governors. There is an increase in the annual hours worked of blacks relative to whites and a decrease in the earnings gap between blacks and whites when there is a Democratic governor. I find that an increase in public sector employment, an increase in employment in the health and education sectors, a higher state Earned Income Tax Credit (EITC), a (slightly) higher minimum wage, a lower incarceration rate and an impact on worker displacement under Democratic governors contribute to the increase in the employment of low- and medium-earning workers and the increase in blacks' employment and hours worked.[1]

The rest of the paper is organized as follows: Section II discusses the powers and role of governors, Section III presents the methodology used, Section IV provides a description of data and descriptive statistics, Section V presents the main results, Section VI discusses the validity of the RDD and presents some robustness checks, and Section VII discusses mechanisms and policies that may explain the results.

1.2 Power and Role of Governors

The U.S. political system allows states to exercise a high degree of autonomy. States can levy taxes, establish license fees, spend tax revenues, regulate

[1]Worker displacement is studied using the Displaced Worker Survey (DWS). The displaced workers are defined similarly to Neal (1995) and the empirical strategy is described below.

businesses and manage the health care system and emergency services. The governor heads the executive branch in each state. The governor sets policies, prepares and administers a budget, recommends legislation, signs laws and appoints department heads. In some states, the governor has additional roles such as commander-in-chief of the National Guard and has partial or absolute power to commute or pardon criminal sentences. Governors can veto state bills, which gives them considerable control over policies.[2] In all but seven states, governors have the power to use a line-item veto on appropriation bills. This gives the governor the authority to delete part of a bill passed by the legislature that involves taxing or spending. All U.S. governors now serve for four-year terms, except in New Hampshire and Vermont, which have two-year terms. Gubernatorial elections are held in November and the governor takes office the following January. Election years differ from state to state.

1.3 Methodology

My identification strategy is an RDD to account for the potential endogeneity of election outcomes. It follows the work of Lee (2001, 2008) and Pettersson-Lidbom (2001), and is used in papers such as Lee, Moretti, and Butler (2004), Pettersson-Lidbom (2008) and Ferreira and Gyourko (2009, 2012). Endogeneity concerns surrounding election outcomes come from factors such as labor market conditions, voter characteristics, the quality of candidates, which party is incumbent, the resources available for campaigns, and other unmeasured characteristics of states and candidates that would bias the estimates of the impact of the party allegiance of governors. These factors can influence who wins the election. Lee (2001, 2008) demonstrates that looking at close electoral races provides quasi-random variation in winners and allows for the identification of causal effects.

[2] A governor's veto can be overruled by the legislature by a simple, two-thirds or three-fifths majority, depending on the state.

An RDD also allows for the estimation of the local average treatment effect in a case where randomization is infeasible. It can be done using either parametric or non-parametric estimation. My main specification uses a parametric approach, which allows for straightforward hypothesis testing.[3] The discontinuity is defined where the margin of victory is 0%. Positive values indicate that a Democratic governor was elected while negative values indicate that a Republican won.

Specification 1: Main Regression

$$\begin{aligned} Y_{ist} &= \beta_0 + \beta_1 D_{st} + \beta_2 D_{st} \times Black_{ist} + \beta_3 Black_{ist} \\ &+ \beta_4 X_{ist} + \beta_5 Z_{ist} + F(MV_{st}) \\ &+ F_b(MV_{st}) \times Black_{ist} \end{aligned} \tag{1.1}$$

My coefficients of interest are β_1 and β_2. Y_{ist} represents the labor market outcome of interest for individual i in state s in year t. I consider the following labor market outcomes (conditional on having positive earnings and wages): annual earnings, weekly earnings and hourly wages. I also look at labor force participation and employment, as well as (conditional on working): total hours worked per year, usual hours worked per week and weeks worked per year. All earning and wages variables are in real terms, and I use the logarithm for earnings, hours and weeks worked regressions. $Black_{ist}$ represents a dummy for the worker being black. D_{st} is a dummy variable that takes on a value of one if a Democratic governor is in power in state s during year t. MV_{st} refers to the margin of victory in the last gubernatorial election at year t in state s. For example, the 1978 election results (the political party of the winner and the margin of victory) in California are used in employment regressions for 1979, 1980, 1981 and 1982. The margin of victory is defined as the proportion of votes cast for the winner minus the

[3] My specification is similar to that of Ferreira and Gyourko (2009, 2012), which also uses a parametric approach and third-order polynomial. In the robustness section, I discuss regressions with other polynomial degrees and local-linear regressions.

proportion of votes cast for the candidate who finished second. The value is positive if the Democratic candidate won and negative if he or she lost.[4] The pure party effect, β_1, is estimated controlling for the margin of victory using a third-order polynomial $F(MV_{st})$. X_{ist} refers to individual characteristics and includes variables such as dummies for the education level, marital status, age and gender. Z_{st} includes state fixed effects and year fixed effects. $F_b(MV_{st}) \times Black_{ist}$ allows for a different trend for black workers. Standard errors are clustered at the state-term level.[5] I focus on blacks and whites aged 20 to 55.[6]

1.4 Data and Descriptive Statistics

1.4.1 Data

Data are drawn from various sources.

For gubernatorial elections, two main data sources are used. For elections data prior to 1990, I use the ICPSR 7757 (1995) files called "Candidate and Constituency Statistics of Elections in the United States, 1788-1990." Data post-1990 comes from the Atlas of U.S. Presidential Elections (2011).[7] Only elections where either a Democrat or a Republican won are included.[8] All states are included. Variables of interests taken from these sources are the

[4] I exclude observations where neither a Democrat nor Republican won.

[5] Results are robust to alternative clustering. One potential concern is serial correlation; as such, I present clustering at the state level in the appendix. I also present clustering at the state-year level.

[6] Results are robust to using different age groups (for example 18 to 64). I am focusing on prime working age worker in the paper. The results hold if I include other race in the sample and replace the black dummy with a non-white dummy.

[7] Data were double-checked using official sources (such as state legislature websites and Council of State Governments data) wherever possible.

[8] There are a few cases where there is special appointment within a term and there is a change of governor (for example, if a governor dies). I include observations where the new governor is from the same party. However, if the special appointment within a term changes the party in power, I drop these observations from my regressions because I do not have the relevant margin of victory.

party of the winner and the margin of victory.

The March Current Population Survey (CPS) provides a large sample size of workers and individual characteristics such as age, education, race and marital status. I use data from 1978 to 2009, which represents income years 1977 to 2008.[9] The state identifier available after 1977 in CPS data allows for the matching of gubernatorial election data to the CPS. For robustness, I use the CPS Merged Outgoing Rotation Groups (MORG) data from 1979 to 2008 for the following outcomes variables: being employed and hours worked last week.

Some additional state characteristics are added for some robustness specifications. State senate elections, state house elections are taken from University of Kentucky Center for Poverty Research (UKCPR) (2011) for 1980 to 2010, and data from Leigh (2008) for 1977 to 1980.

1.4.2 Descriptive Statistics

In the 1,566 *year* $\times$ *state* observations in my sample, Republicans governed 730 times, compared to 836 times for Democrats. Democrats were more often in office in earlier years (486 observations for Democratic governors versus 300 for Republicans from 1977 to 1992), while Republicans were more often in office in recent years (430 observations for Republicans versus 350 for Democrats between 1993 and 2008).[10]

Table 1.1 presents descriptive statistics for states in years where the election results are close, that is when the margin of victory is within 5 or 10 percentage points. There are 346 *year* $\times$ *state* observations within a five-percentage-point margin of victory (163 observations for Democrat winners and 183 for Republicans), while there are 678 observations within a

[9] To circumvent the top coding of the income variables in the CPS, I replace the top-coded income variables with consistent mean-cell data estimated by Larimore et al. (2008)

[10] As mentioned above, I exclude from my sample cases when an independent governor won or when the party in office changes during the term.

10-percentage-point margin of victory, with Democratic governors in power 339 times.

Table 1.1 indicates that states close to the threshold are similar along a number of dimensions: the proportion of blacks in the population; the proportion of the population less than 15 years old; the proportion of the population older than 65; the proportion of the population between 20 and 55; the proportion of the population for whom the highest level of education completed is elementary school; the proportion of the population for whom the highest level of education is some high school education, high school diploma or some college; the proportion of the population for whom the highest level of study is having a college degree or more; and the logarithm of the population of the state. This suggests that the key underlying assumption of the RDD estimates, which is that states where a Democratic governor barely won should be similar to states where a Republican barely won, is satisfied. I later use these variables as dependent variables when I examine the robustness of the results.

1.4.3 Graphical Evidence

Figures 1, 2 and 3 explore the discontinuity at 0% when a Democratic governor barely wins over a Republican by margin of victory. Figure 1 presents the proportion of whites and blacks employed and Figure 2 presents the hours worked by white and black workers. Figure 3 shows the earnings gap between whites and blacks. Appendix A presents figures for differential impacts by standard covariates: gender, education and age.

Each dot in the panel corresponds to the average outcome that follows election t, grouped by margin of victory intervals. The solid lines in the figures represent the predicted values from the cubic polynomial fit without covariates. The horizontal axis is the margin of victory in percentage points, and the vertical axis is the outcome of interest. Figures 1 and 2 suggest that there is a higher proportion of blacks who work under Democratic governors and that they work more hours. Figure 3 suggests a decrease in the black-white earnings gap, following the increase in blacks' employment and hours. I estimate these effects precisely in the next section, using controls listed above to isolate the effect of party allegiance of governors on labor market outcomes.

1.5 Main Results

Tables 2, 3, and 4 present coefficients from the estimation of the main specification (1) for the variables Democratic governor, Democratic governor $\times$ black, and black, respectively. Democratic governor and Democratic governor $\times$ black are the variables of interest. Column 1 presents results for all black and white men and women, and columns 2 and 3 present results for men and women separately. Where I note that results are statistically significant, I mean that they are significant at the 5% level, unless otherwise specified.

Table 1.2 presents results for the following dependent variables: total hours worked per year, weeks worked per year and usual hours per week. Table 1.2 shows how much more or less an average individual works when a Democrat is in office, conditional on that individual working. Democratic governors do not have a significant impact on the intensive margin for whites (except for usual hours worked for men, statistically significant at the 10% level). However, there is an increase in blacks' hours worked relative to whites under a Democratic governor. On average, black men increase their hours worked per year (4.47%) and hours worked per week (1.76%) relative to white men under Democratic governors. They also increase their weeks worked (2.71%) (statistically significant at the 10% level). Results for black women are less pronounced. Only their weeks worked per year statistically significantly increase relative to white women under a Democratic governor (2.68%) (statistically significant at the 10% level).

Table 1.3 presents results when the dependent variables measure labor force participation and employment. The coefficients for β_1, β_2 and β_3 of specification (1) are estimated using a linear probability model. Table 1.3 shows that the political party of the governor has an impact on black labor force participation and employment, especially for women. Democratic governors have a statistically significant impact on labor force participation and the likelihood of black women being employed (3.65% and 3.82%, respectively). The corresponding coefficients for men are positive but not significant. There is no statistically significant effect of Democratic governors on employment for whites overall, or for white men or women.

Table 1.4 presents results when the dependent variables are real annual earnings, real weekly earnings and real hourly wages. The results indicate that under a Democratic regime, annual earnings, weekly earnings and hourly earnings are lower on average for whites, and that this decrease is larger for men than for women. The estimates for men, which are -3.00% for annual earnings, -2.49% for weekly earnings and -1.87% for hourly wages, are statistically significant for all measures of earnings and wages. The estimates for females are -1.44% for annual earnings, -1.78% for weekly earnings, and -1.51% for hourly wages (only weekly earnings and hourly wages are statistically significant at the 10% level). Table A.1 in Appendix A presents RD estimates for outcomes using whites and blacks combined. It shows that there is an overall decrease in average annual earnings, especially for men.

Table 1.4 also provides evidence that party affiliation plays a role in the black and white earnings gap. Democratic governors have a positive impact on the relative earnings of blacks. The impact is 5.88% for men and 5.03% for men and women combined. Both of these effects are statistically significant. The coefficient for women is positive, but not statistically significant. The Democratic governor $\times$ black interactions are positive but not significant for weekly earnings and hourly wages.

Tables 1.2, 1.3 and 1.4 present results for blacks relative to whites, which is my main focus. Table A.2 in Appendix A presents the total effect for blacks ($\beta_1 + \beta_2$) for all regressions of Tables 2, 3 and 4. It shows than under Democratic governors, there is a statistically significant increase in total hours, weeks worked and employment for blacks. There is no statistically significant effect on earnings.

1.6 Validity and Robustness Checks

I perform a number of robustness checks to ensure that my results are robust.

1.6.1 Validity of the RD Design

I begin my robustness checks by investigating the key assumption of the RDD approach, which is that states where a Democratic governor barely wins a gubernatorial election are similar to states where a Republican barely wins. I verify and confirm that states close to the discontinuity are similar along a number of dimensions. As in Ferreira and Gyourko (2009), I estimate regression discontinuity specifications using variables for state characteristics as dependent variables. I use aggregate data and an aggregate version of specification (1) without the individual characteristics. I find that the coefficient associated with a Democratic governor is never significant for these outcome variables, which indicates that states are not statistically significantly different near the discontinuity.[11]

To address the issues raised in Caughey and Sekhon (2011) about the

[11] The RDD coefficients (with standard errors in parentheses) for a Democratic governor are: proportion of the population that is black [0.153 (0.286)], proportion of the population for whom the highest level of schooling is elementary education [-0.318 (0.452)], proportion of the population for whom the highest level of schooling is some high school, a high school diploma or some college [0.767 (0.506)], proportion of the population with a college degree or more [-0.450 (0.429)], proportion of the population less than 15 years old [-0.009 (0.321)], proportion of the population over 65 [0.0477 (0.222)], proportion of the population between 20 to 55 [-0.005 (0.351)], and a logarithm of state population [-1.647 (2.833)]. The coefficients and the standard errors are multiplied by 100.

RD design in the case of an election, I also verify that situations where Democrats barely win and situations where Democrats barely lose do not differ significantly in pre-treatment covariates. I create a variable which is equal to 1 if the governor at T-1 was from a different party and 0 otherwise and I check for the balance of this covariate. I find no discontinuity in that variable, which is evidence that close elections are not predictable and can be interpreted as random. I also use data on campaign spending from Jensen and Beyle (2003) to check whether this covariate is balanced.[12] This is indeed the case. As to whether close gubernatorial elections can be regarded as random, it may be that close elections won by Democratic governors are more likely to also come with a Democratic house or senate. I check and confirm that the variable indicating who controls the house and who controls the senate are balanced. I also check whether there is a discontinuity in the density of the forcing variable at the threshold. It is important to verify that the number of Democratic governors and Republican governors is similar around the threshold, which is the case here. These results are reported in Appendix B.

A second identification issue concerns the persistence of the outcome variables. It could be that Democratic governors are more likely to be elected (even in close elections) in state-years with relative lower earnings or employment. Even with fixed effects, labor market trends could be state-specific. To address this concern, I do a "placebo" test. I take the outcome variables of interest in T-1 as covariates, and check for balance between the control and treatment group. I find that there is no discontinuity in T-1. Results are shown in Appendix B. I also present graphs at T-1, T+1, and T+1 and T+2 together for employment rates of white and black workers separately. These results are reported in Appendix C. As can be seen from the figures,

[12]The codebook explaining how Jensen and Beyle (2003) created the variables is available at http://www.unc.edu/∼beyle/guber.html. Since the information available differs from state to state and year to year, I use the share of Democratic spending as the outcome variable.

there is no discontinuity at T-1 in employment for blacks and whites, and the impact of political parties is mostly felt after one year for blacks.

1.6.2 Possible Heterogeneity of Party Allegiance and Possible Confounding Factors

In Appendix D, I explore how results are robust to different specifications for one of the outcome variables: total hours worked last year. Results and conclusions are robust to using a 1st-, 2nd-, or 4th-order polynomial. Results for the local-linear specifications using grouped data by state and year are available in Appendix D for different bandwidths, including the optimal bandwidth procedures of Calonico et al. (2012) and Imbens and Kalyanaraman (2012). I also present weighted and unweighted estimates from the grouped data regression to explore how sensitive the results are to weighting. Overall, the results are very robust across different specifications.

I also estimate main specification (using as the outcome variable the number of total hours worked) for different samples of years and states, and find that while the coefficients vary slightly depending on years and states used, the main effects, their significance and the conclusions remain valid. One interesting subsample is non-southern states.[13] Democrats in the south are arguably more conservative and therefore more similar to Republicans (Alt and Lowry, 2000). Therefore, one might expect that the effects of a Democratic governor relative to a Republican would be more marked in non-southern states. I find that for non-southern states, the positive impact of a Democratic governor on total hours for black workers is more pronounced. As another robustness check, I restricted the sample to states that frequently elect both Democrats and Republicans (as opposed to states that consis-

[13]The Census classified states as either Northeastern, Midwestern, Southern or Western. The southern states are: Alabama, Arkansas, Delaware, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia and West Virginia.

tently elect a governor from a single party).[14] The results and conclusions are robust to focusing on these states only.

I also replicate the results using a new database. Using the CPS Merged Outgoing Rotation Groups, I replicate the results for the employment and hours outcomes in the reference weeks using specification (1). Results for those replications are available in Appendix E and are quite similar to the March CPS results.

Another test is to include more state- and time-varying characteristics to better isolate the impact of the gubernatorial election. The objective is to control for possible confounding factors that might influence the results. The results are robust to the addition of controls for the population, the proportion of the population that is black, the proportion of population with a college diploma, the proportion of the population with a graduate degree, and proportion of the population that did not complete high school. The results are also robust to the inclusion of dummies for the governor being a woman or from a minority ethnic group. The results are robust to the inclusion of a dummy variable for having Democrats control the state senate, a dummy variable for Democrats controlling the state house and a dummy variable for the governor being a Democrat during the last term. The results are also robust to including region $\times$ time dummies for the following regions (as defined by the Census Bureau): Northeast, Midwest, South and West. Finally, the results are qualitatively the same if I exclude the first year that a governor is in power, to remove potential lags in policy.

Overall, results are very robust to alternative specifications and a rich set of time-varying state characteristics. These numerous robustness checks provides confidence that there is a causal role played by party allegiance at the gubernatorial level on labor market outcomes.

[14]This subsample includes the states where Democrats and Republicans were each in office at least 30% of the time during my sample period.

1.7 Workforce Changes, Channels and Policies

This paper has established that, on average, Democratic governors have different effects on the labor market than Republicans. When Democratic governors are in office, the following three effects take place: a decrease in average earnings for whites, an increase in employment and hours worked for blacks, and a decrease in the earnings gap between white and black workers.

In this section, I examine the mechanisms through which governors exercise their influence over the labor market. I provide evidence that under Democratic governors, there is an increase of low- and medium-earnings workers and that this change in workforce composition is a main factor explaining the decrease in earnings. I then review potential policies through which the increase in low- and medium- earnings workers and the increase in the employment of blacks takes place.

1.7.1 Impact on Labor Force Composition

To study the impact of party allegiance on workforce composition, I first divide workers into three categories: low-, medium- and high-earnings. Low-earnings workers are defined as those whose earnings are below the 35th percentile, measured in 1977 real earnings (at the national level). Medium-earnings workers are between the 35th and 65th percentiles, and high-earnings workers are those above the 65th percentile.[15] Each worker in the sample is placed into one of these three categories. I use this approximation to study the impact of Democratic governors on the workforce composition relative to Republican governors. It is a simple but efficient way to parse the data to see if party affiliation alters labor force composition. The objective is to investigate whether political parties affect the probability of being a low-, medium-

[15] Results are robust to alternative definitions of low, medium, and high earnings. Similar results emerge from quantile regressions. However, the interpretation is not straightforward with interaction terms.

or high-earnings worker. I conduct the exercise using annual earnings.[16]

Table 1.5 shows that the probability of being a low-earnings worker increases (and, for men only, the probability of being a medium-earnings worker increases) when Democratic governors are in office, while the probability of being a high-earnings worker decreases. However, one cannot determine from Table 1.5 if this effect is caused by an entry of low-earnings workers, high-earnings workers transitioning to lower earnings, or a combination of these two factors.

I use the log of the number of workers in each of the three earnings categories as dependent variables and specification (1) to investigate which explanation is most likely.[17] The RD results are provided in Table 1.6, which suggests that the effect is caused by an increase in the number of low-earnings workers. Moreover, policies studied below also suggest there would be increased numbers of low- and medium-earnings workers entering the labor force.

Employment and labor force participation can also vary by level of education. In Table 1.7, I study the probability of employment and the probability of being in the labor force (dummy 0-1) of people with a high school diploma or less education, compared with people with more education, using an interaction term.[18] Table 1.7 shows that under Democratic governors, less educated workers work more relative to more educated workers. While it is possible that less educated workers enter the labor market and earn a lot, it is unlikely that a newcomer would earn more than average. Table 1.7 also points to the same conclusion as above. Appendix E presents graphs for total hours by gender, age and education for whites and blacks separately. As in

[16]Results are similar if hourly wage is used instead.

[17]I use an aggregate version of specification (1) without the $F_b(MV_{st}) \times Black_{ist}$ interactions, since the entry of low-earnings workers into the labor market is the likely channel through which the effect on black extensive margins occurs.

[18]I use specification (1) without the $F_b(MV_{st}) \times Black_{ist}$ interactions. I include an interaction between a dummy for having at most a high school diploma and a dummy for having a Democrat as governor.

Table 1.7, it shows that less educated workers work more under Democratic governors (this finding is true for both white and black workers).

1.7.2 Including the Share of Low- and Medium-Earnings Workers in the Earnings Regression

The above section shows that there are changes in the composition of the workforce under Democratic governors such that there are more low- and medium-earnings workers. An important next step is to determine whether these changes are also driving the decrease in earnings.

To investigate whether the changes in the composition of the labor force explain the results, I include as additional controls the proportion of workers that are low-earnings workers, and the proportion of workers that are medium-earnings workers. I estimate models using a modification of specification (1) with annual earnings, weekly earnings, and hourly wages as dependant variables.[19] The results reported in Table 1.8 show that the impact of Democratic governors on earnings almost disappears completely when controlling for labor force composition, such that results are no longer statistically significant. This provides strong evidence that the change in labor force composition is a key factor explaining the above results for earnings.

1.7.3 Policies and Channels

Overall, the evidence points to the party allegiance of governors having an impact on the composition of the labor force by increasing low- and medium-earnings workers. This in turn results in a decrease in average earnings. The results also show an increase in employment of blacks. In this section, I examine what policies may lead to these results. The aforementioned findings are likely the result of a combination of policies. Table 1.9 presents potential

[19] I once again use specification (1) without the $F_b(MV_{st}) \times Black_{ist}$ interactions.

policies and summarizes what policies are influenced by the party allegiance of the governor. Detailed estimates are contained in Appendix G.

I find that Democratic governors have a small but statistically significant impact on the probability that a woman works in the public sector and the probability that any worker works in the public sector. Public-sector jobs tend to be in the low- and medium-earnings categories. I also investigate if political parties affect employment in the health and education sectors. State funding might affect employment and hours worked in these sectors.[20] Tables in Appendix G show that there is an increase in employment and hours worked in the health and education sectors for women. In other words, there is an increase in the proportion of low- and medium- earnings workers under Democratic governors due in part by an increase in employment in the public, health and education sectors and hours worked in the health and education sectors.

State earned income tax credits (EITC) can also help explain the results. The EITC is a refundable tax credit primarily for individuals and couples with children. The indirect effect of the policy is to increase employment, mostly of low- and medium-earnings workers (and particularly women). I find that Democratic governors increase the probability that a state offers an EITC and are also associated with higher levels of EITC.[21] Adding state EITC rates (percentage of the federal EITC) to specification (1) shows that state EITC rates have a positive and statistically significant impact on total hours worked, labor force participation and employment for low-earnings women. Moreover, state EITC rates do not reduce overall employment.

The literature suggests that Democratic governors are associated with

[20] I focus on nurses and support staff in the health sector and teachers, counselors and support staff in the education sector. I exclude the post-secondary sector.

[21] I focus on 1990 to 2008, when several states implemented an EITC. Data about state EITC is taken from UKCPR. More details about state EITC are available at http://www.irs.gov/Individuals/States-and-Local-Governments-with-Earned-Income-Tax-Credit.

(slightly) higher minimum wages (Besley and Case, 1995 and Leigh, 2008) and lower incarceration rates (Leigh, 2008), which I also confirm. Both measures could increase the labor supply of low-earnings workers. I find that, by adding state minimum wages to specification (1), the minimum wage has a small positive and significant impact on total hours worked for low-earnings workers, and has a small positive and significant impact on labor force participation and employment for low-earnings workers. Doing the same exercise with state incarceration rates, I find that a higher incarceration rate has a small negative and significant impact on labor force participation and employment for low-earnings workers. Neither the minimum wage nor the incarceration rate affect overall employment. My results for the minimum wage are consistent with studies such as Card and Krueger (1994, 2000). Moreover, a higher proportion of black workers than white workers earn the minimum wage, which helps explain the decrease in the earnings gap between blacks and whites.

Democratic governors could also have an impact on the business sector. I try to examine this in three ways. First, by looking at layoffs using the Displaced Worker Survey (January CPS) to investigate whether Democratic governors affect the probability of being displaced, I find that black men are less likely to be a displaced worker under Democratic governors.[22] This suggests an increase in the intensive margins of black men relative to whites. Moreover, under Democratic governors, displaced workers are less likely to be low-earnings workers and more likely to be high-earnings workers.[23] These findings contribute to the increase in the share of low- and medium-earnings workers in the labor market. As a second step, I add controls for occupation and industry to my specification (1). Adding such controls reduces

[22]The Displaced Worker Survey has the same control variables as March CPS, and the data is available every two years from 1984 to 2008. The definition of displaced worker used here is similar to Neal (1995).

[23]Leigh (2008) calls this a "policy and economic conditions" variable, which represents intermediate outcomes resulting from policy choices and economic conditions (i.e. a function of both the supply of, and demand for, welfare).

the impact of political parties on annual earnings and total hours worked, suggesting that there is an impact on occupation and industry composition. Finally, I examine whether Democratic governors have an impact on business dynamics. I use the following outcome variables provided by the U.S. Census Bureau's Business Dynamics Statistics: establishment entry rates, establishment exit rates, firms' job creation rates, firms' job destruction rates, and firms' net job creation rates.[24] I do not find that business dynamics are affected by gubernatorial party allegiance.[25]

I also investigate the role of taxation (both corporate and personal), which could affect workforce composition. I examine whether the decrease in earnings reported in the previous tables holds after including income taxes using the NBER TAXSIM simulator. I find similar results after and before tax. Therefore, taxation is not a factor explaining the increase of low- and medium-earnings workers.[26] Using the top corporate tax rate as an outcome variable, I do not find that the party allegiance of the governor has an impact on corporate taxation.

1.8 Conclusion

This paper is a broad study of the causal impact of party allegiance of U.S. governors on labor market outcomes using a regression discontinuity

[24]All variables are available for all of my sample years.

[25]This analysis uses aggregate data, and does not consider the quality of firms created and destroyed. Analyzing this problem using firm-level data, which was not available to me, would be an interesting next step.

[26]I use family earnings because of the joint filing for married couples in the U.S. tax system. After-tax income is obtained using the NBER TAXSIM simulator and before-tax income and certain tax credits are obtained from the CPS. I use code provided by James P. Ziliak to incorporate tax credit variables from the CPS into the NBER TAXSIM simulator. The CPS does not have information on certain inputs for the TAXSIM program, such as annual rental payments, child care expenses, and other itemized deductions. These values are set to zero when calculating tax liability. The other variables of the TAXSIM simulator are found in the CPS. Results in Table G.8 are similar if credits from the CPS are not included in the TAXSIM simulation.

approach to address the issue of the potential endogeneity of election results. Results indicate that the party allegiance of U.S. governors affects earnings and that Democratic governors are associated with lower individual earnings. Moreover, the results presented in this paper provide evidence that Democratic governors reduce the average earnings gap between black and white workers. There is also an increase in hours and employment of blacks relative to whites under Democratic governors. I provide evidence that there is an increase of low- and medium-earnings workers under Democratic governors and that this change in the workforce composition is a main factor explaining the results. The results are robust to alternative specifications and a rich set of time-varying state characteristics. I find that an increase in public sector employment, an increase in employment in the health and education sectors, a higher state Earned Income Tax Credit (EITC), a (slightly) higher minimum wage, a lower incarceration rate and an impact on worker displacement under Democratic governors contribute to the increase in the employment of low- and medium-earning workers and an increase in employment of blacks.

Although this paper improves our understanding of the importance of party allegiance at the state level, more work is needed in this area to understand the full extent of the role of political parties. I have provided evidence of a short-term increase of low- and medium-earnings workers under Democratic governors. Subsequent research should investigate if this increase in participation has long-term benefits for these groups, whether there are effects on related variables such as union wage premiums, and if there are heterogeneous impacts within a state.

FIGURE 1.1: Margin of Democratic victory and the proportion of whites (left) and blacks (right) who work

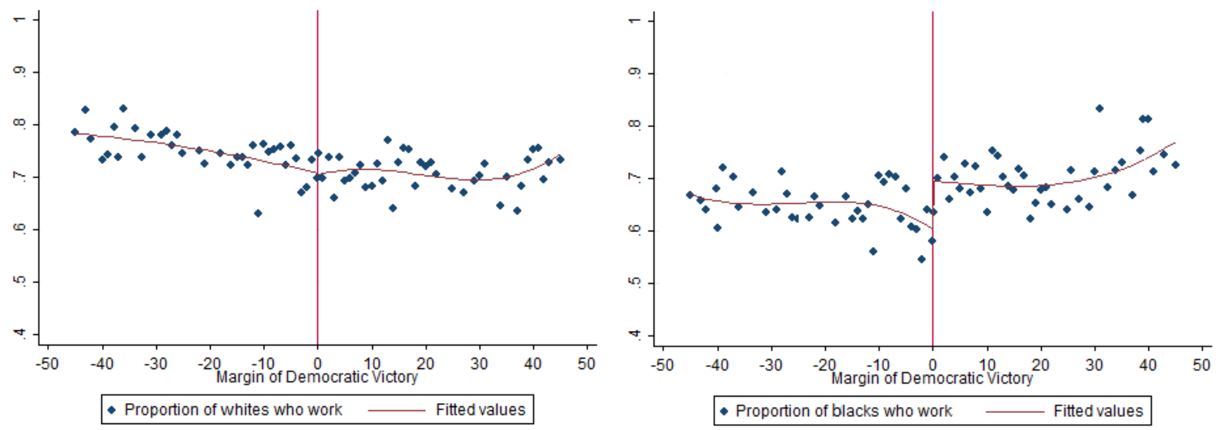

FIGURE 1.2: Margin of Democratic victory and total hours worked per year for whites (left) and blacks (right)

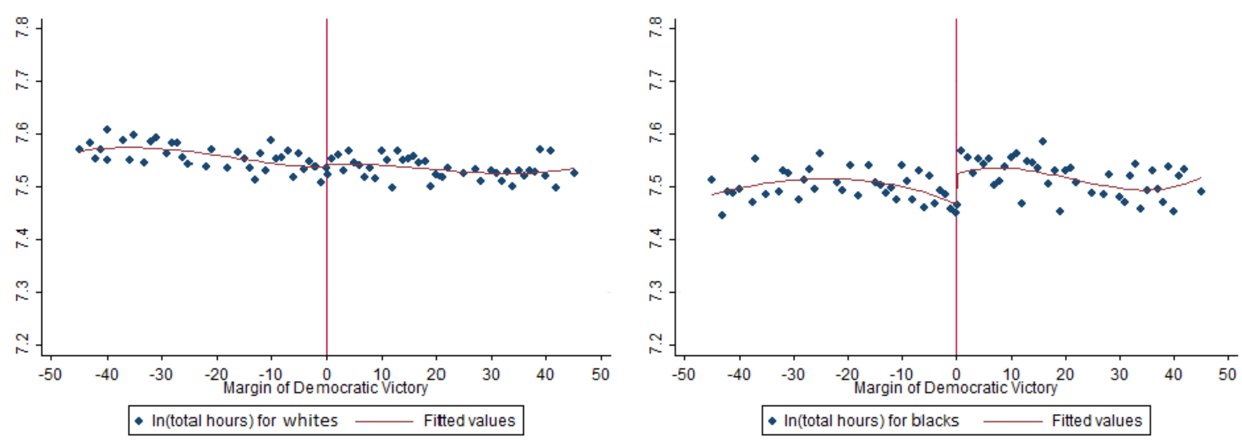

FIGURE 1.3: Margin of Democratic victory and the logarithm of white and black annual earnings gap

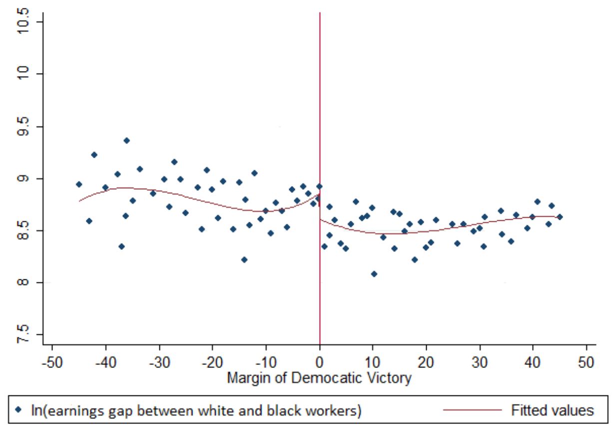

TABLE 1.1 - Descriptive statistics of selected variables for states close to discontinuity

		Black	Age < 15	Age > 65	Age 20 to 55
Margin of victory less than 5 %	Republican	0.0999	0.2342	0.1197	0.5051
		(0.0003)	(0.0020)	(0.0014)	(0.0016)
	Democrat	0.1003	0.2356	0.1170	0.5043
		(0.0004)	(0.0021)	(0.0019)	(0.0020)
Margin of victory less than 10 %	Republican	0.1002	0.2364	0.1204	0.5016
		(0.0002)	(0.0015)	(0.0011)	(0.0012)
	Democrat	0.0998	0.2357	0.1163	0.5037
		(0.0003)	(0.0015)	(0.0012)	(0.0014)
		Elementary	Some HS, HS or Some College	College or more	ln(population)
Margin of victory less than 5 %	Republican	0.2824	0.4946	0.2230	14.9865
		(0.0029)	(0.0027)	(0.0036)	(0.0066)
	Democrat	0.2779	0.5012	0.2208	14.9943
		(0.0031)	(0.0027)	(0.0029)	(0.0079)
Margin of victory less than 10 %	Republican	0.2856	0.4982	0.2162	14.9926
		(0.0021)	(0.0020)	(0.0025)	(0.0049)
	Democrat	0.2813	0.4975	0.2212	14.9842
		(0.0023)	(0.0018)	(0.0022)	(0.0056)

This table reports the proportions of blacks and individuals less than 15 years old, older than 65, and between 20 and 55. It also reports the proportion of the population by the highest level of education completed: elementary school, some high school or a high-school diploma or some college, and a college degree or more, as well as the logarithm of the state's population. Standard errors (of the mean) are in parentheses. Sources: March CPS data, UKCPR data, Leigh data (2008).

TABLE 1.2 - RD for hours worked, weeks worked and usual hours

Intensive	Variables	All	Men	Women
Total hours worked	Democrat	-0.63	-1.13	0.08
		(0.68)	(0.81)	(0.77)
	Democrat x Black	3.79**	4.47**	2.33
		(1.61)	(2.00)	(2.04)
	Black	-6.49***	-14.00***	-0.91
		(1.02)	(1.24)	(1.25)
Weeks worked	Democrat	-0.15	-0.51	0.35
		(0.45)	(0.57)	(0.54)
	Democrat x Black	2.91**	2.71*	2.68*
		(1.18)	(1.58)	(1.42)
	Black	-6.00***	-8.81***	-4.13***
		(0.82)	(1.08)	(1.05)
Usual hours	Democrat	-0.48	-0.62*	-0.27
		(0.32)	(0.32)	(0.45)
	Democrat x Black	0.87	1.76**	-0.35
		(0.69)	(0.73)	(1.27)
	Black	-0.49	-5.19***	3.22***
		(0.41)	(0.49)	(0.84)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level, and * denotes statistically significant results at the 10% level. Control variables include highest level of education, marital status, age, age^2, age^3, age^4, a female dummy, state fixed effects, and year fixed effects. Outcome variables are expressed in log form and coefficients and standard errors (in parentheses in the table) are multiplied by 100. Results are clustered at the state-term level. Source: March CPS data.*

TABLE 1.3 – RD for being in labor force and employed

Extensive	Variables	All	Men	Women
In labor force	Democrat	-0.69*	-0.26	-1.08*
		(0.38)	(0.35)	(0.61)
	Democrat x Black	2.48**	1.37	3.65**
		(1.21)	(1.53)	(1.48)
	Black	-4.71***	-8.75***	-2.61**
		(0.89)	(0.93)	(1.24)
Employed	Democrat	-0.77	-0.40	-1.11
		(0.54)	(0.58)	(0.69)
	Democrat x Black	2.59*	1.50	3.82**
		(1.34)	(1.70)	(1.59)
	Black	-8.65***	-13.15***	-6.23***
		(1.00)	(1.04)	(1.33)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level, and * denotes statistically significant results at the 10% level. The controls are the same as in TABLE2. The in-labor-force variable is 1 if an individual is in the labor force and is 0 otherwise. The employed variable is 1 if an individual is employed, and is 0 if the individual is unemployed or out of the labor force. Estimates are generated using a linear probability model. Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Results are clustered at the state-term level. Source: March CPS data.*

TABLE 1.4 - RD for Earnings

Earnings	Variables	All	Men	Women
Annual	Democrat	-2.42*	-3.00**	-1.44
		(1.23)	(1.47)	(1.29)
	Democrat x Black	5.03**	5.88**	2.56
		(2.57)	(3.00)	(3.32)
	Black	-16.37***	-29.62***	-6.18***
		(1.40)	(1.70)	(1.99)
Weekly	Democrat	-2.24**	-2.49**	-1.78*
		(1.00)	(1.14)	(1.08)
	Democrat x Black	2.11	3.28	-0.19
		(2.07)	(2.28)	(2.84)
	Black	-10.35***	-20.92***	-1.99
		(1.21)	(1.33)	(1.90)
Hourly	Democrat	-1.76**	-1.87**	-1.51*
		(0.82)	(0.95)	(0.88)
	Democrat x Black	1.23	1.52	0.16
		(1.77)	(2.01)	(2.14)
	Black	-9.86***	-15.73***	-5.21***
		(1.11)	(1.24)	(1.46)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. The controls are the same as in TABLE2. Outcome variables are expressed in log form and coefficients and standard errors (in parentheses in the table) are multiplied by 100. Results are clustered at the state-term level. Source: March CPS data.*

TABLE 1.5 - Propensity of being a low-, medium- or high-earnings worker

Outcomes	Variables	All	Men	Women
	Democrat	0.76**	0.38	1.10**
Low-earnings workers		(0.30)	(0.32)	(0.44)
Below 35th percentile	Democrat x Black	-0.17	-0.47	0.50
of 1977 annual earnings		(1.06)	(1.22)	(1.33)
	Black	4.56***	8.84***	1.40
		(0.68)	(0.73)	(0.87)
	Democrat	0.26	1.15***	-0.77*
Medium-earnings workers		(0.30)	(0.38)	(0.44)
35th to 65th percentiles	Democrat x Black	-1.08	-1.66	0.01
of 1977 annual earnings		(0.99)	(1.28)	(1.22)
	Black	2.50***	3.95***	0.91
		(0.65)	(0.77)	(0.84)
	Democrat	-1.03***	-1.54***	-0.35
High-earnings workers		(0.27)	(0.38)	(0.32)
Above 65th percentile	Democrat x Black	1.28*	2.12**	-0.46
of 1977 annual earnings		(0.70)	(1.00)	(0.90)
	Black	-7.06***	-12.77***	-2.33***
		(0.48)	(0.66)	(0.60)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. The controls are the same as in TABLE2. Estimates are generated using a linear probability model. Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Source: March CPS data.*

TABLE 1.6 - Coefficient estimates for number of workers in each category

Outcomes	Democrat
Number of low-earnings workers Below 35^{th} percentile of 1977 annual earnings	2.13** (1.05)
Number of medium-earnings workers 35th to 65^{th} percentiles of 1977 annual earnings	0.64 (1.12)
Number of high-earnings workers Above 65^{th} percentile of 1977 annual earnings	-0.46 (1.14)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. Estimates are generated using an aggregate version of specification (1) without the $F_b(MV_{st}) \times Black_{ist}$ interactions. Outcome variables are expressed in log form and coefficients and standard errors (in parentheses in the table) are multiplied by 100. Source: March CPS data.*

TABLE 1.7 - RD for probability of being in labor force and employed

Extensive	Variables	All	Men	Women
	Democrat	-0.51**	-0.37	-0.76**
In labor		(0.23)	(0.26)	(0.33)
force	Democrat x (<=high school diploma)	0.73***	1.13***	0.61*
		(0.24)	(0.24)	(0.31)
Employed	Democrat	-0.72***	-0.46	-1.10***
		(0.28)	(0.33)	(0.39)
	Democrat x (<=high school diploma)	0.66**	1.10***	0.51
		(0.28)	(0.30)	(0.33)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. Estimates are generated using a regression similar to equation (1) without the $F_b(MV_{st}) \times Black_{ist}$ interactions for annual and weekly earnings and hourly wages and controls for the share of low- and medium-earnings workers. Additional controls are the same as in TABLE2. Outcome variables are expressed in log form and coefficients and standard errors (in parentheses in the table) are multiplied by 100. Source: March CPS data.*

TABLE 1.8 – RD for annual, weekly and hourly earnings including share of workers.

Earnings	Variable	All	Men	Women
Annual	Democrat	-0.39	-0.61	-0.18
		(0.53)	(0.64)	(0.81)
Weekly	Democrat	-0.56	-0.60	-0.94
		(0.41)	(0.53)	(0.58)
Hourly	Democrat	-0.53	-0.38	-0.72
		(0.37)	(0.47)	(0.52)
Includes the share of low- and medium-earnings workers		yes	yes	yes

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. Estimates are generated using a regression of equation (1) without the $F_b(MV_{st}) \times Black_{ist}$ interactions for annual and weekly earnings and hourly wages and controls for the share of low- and medium-earnings workers. Additional controls are the same as in TABLE2. Outcome variables are expressed in log form and coefficients and standard errors (in parentheses in the table) are multiplied by 100. Source: March CPS data.*

Table 1.9 - Summary table of policies

Policy	Partisan Allegiance has significant impact?	Impact of Democrats
State Public Sector	Yes	Higher employment
Health and Education Sector	Yes	Higher employment and hours worked
Minimum Wage	Yes	Slightly higher and increased
Incarceration Rate	Yes	Lower
State EITC	Yes	More frequent and higher
Business sector		
Business Dynamics Statistics	No	
Controling for occupation and industry	Yes	Smaller coefficients
Job displacement via Displaced worker survey	Yes	Black are less displaced Displaced worker are less low earners
Taxation		
Corporate	No	-
Household	No	-

Chapter 2

The Effect of High School Shootings on Schools and Student Performance

Authors: Louis-Philippe Béland and Dongwoo Kim

2.1 Introduction

There have been a total of 157 deadly school shootings in U.S. high schools between 1994 and 2009. Although shootings at schools account for a relatively small number teenage murders, they are not unprecedented in U.S. high schools and the threat of these shootings is ubiquitous. These shooting incidents are often mentioned in national policy discussions because of broad concerns over child safety despite the relatively low number of incidents. For two consecutive years, in 2013 and 2014, the President mentioned in his State of the Union address the negative effects of gun violence in schools. In addition, gun violence affecting teenagers raises the demand for a national debate on gun control.

Deadly school shootings have a potentially large effect on students and schools once they occur. These incidents can affect students' decision about whether to stay at their school, affect their cognitive skills, and influence their behavior during their time at school. The educational consequences of deadly school shootings on enrollment and student performance have not been analyzed.

We address three questions related to the consequences of homicidal and suicidal high school shootings. First, we address whether enrollment patterns change after the shootings, which would result from school selection by students and parents or from students dropping out of the school system. Second, we examine whether deadly shootings lower test scores in schools in subsequent years, which helps to establish longer-term trauma effects. Third, we look at the effects deadly shootings have on a range of behavioral variables such as graduation, attendance, and suspension rates.

This paper provides the first empirical estimates of the effect of extreme violence in schools on enrollment, student performance, and student behavior. Potential negative effects of school shootings on student achievement are important for policy makers, especially when deciding whether or not to allocate resources toward creating a safer school environment. To estimate this, we merge an existing database on fatal shootings with the high-school-level Common Core of Data (CCD) and school report cards to form a panel of schools.

We estimate the causal impact of deadly high school shootings by using a difference-in-difference strategy, comparing schools that had fatal shootings with other high schools in the same district that did not experience such shootings. Because we compare schools within the same district, our comparison group exhibits an environment similar to our group of interest aside from the turmoil generated by the shooting. Our empirical strategy relies on the assumption that these deadly school shooting incidents are exogenous in their timing.

We find that enrollment in grade nine drops following a deadly shooting, though we do not observe enrollment effects on subsequent grades. We also find standardized test scores in math and English are lower in affected schools up to three years after a deadly shooting. However, we do not find statistically significant impacts on behavioral outcomes: graduation, attendance, or suspension rates. We find that suicidal shootings have no significant impact on our variables of interest. This leaves open the question of whether students are affected by shootings or if differences in performance instead reflect a composition effect due to high-achieving students leaving the school following a shooting, which then results in lower average scores. To address this question, we use student-level data from California. This data allows us to identify the average treatment effect of shootings, conditional on students staying at the same high school after a shooting. Using student-level panel data from California high schools, we find that shootings have a negative effect on continuing students' math and English test scores.

2.2 Framework

Student violence in high schools may hinder students from learning efficiently. Extreme violent incidents could be a distraction from learning; fear and an unsafe atmosphere could impede students from being open to new opportunities that are essential to learning and, even more problematically, students may avoid attending school. Also, violent incidents could affect the allocation of teaching time. These factors could influence students' cognitive performance and behavioral outcomes.

Multiple studies show that students are negatively affected by violent crimes on several dimensions. Berman et al. (1996) find that students who have been exposed to violent crimes show a greater number of post-traumatic stress disorder (PTSD) symptoms. Among those who were exposed to a violent crime, 44.3 percent were categorized as having "moderate" PTSD symptoms and 18.6 percent as 'severe' PTSD symptoms.[1] Berman et al. find that victims and witnesses exhibit a similar number of PTSD symptoms.

Likewise, Pynoos et al. (1987) find that elementary school students experienced PTSD after a fatal sniper attack on their school playground.[2] The severity of PTSD was worse for all exposure levels if the students knew the victim well. However, they find no difference in severity by age, sex, or ethnicity.

Building on evidence that violent crime causes PTSD, McEwen and Sapolsky (1995) demonstrate that stress, which is more common in people afflicted with PTSD, increases the frequency of declarative errors, but has no effect on tasks that have fewer declarative and more procedural components. Declarative knowledge involves explicit knowledge of a fact, whereas procedural knowledge is implicit knowledge of how to do something.

Recent papers study the effects of school violence. Poutvaara and Ropponen (2010) analyze the immediate effect of a school shooting in September 2008 at a school in Finland in the middle of a national exam period that lasts 2-3 weeks. They find that the shooting decreased average test scores for boys but not for girls. Since a fatal shooting can be considered exogenous to other determinants of educational outcomes, Poutvaara and Ropponen's estimates can be interpreted as causal.[3]

Other papers that study the effect of neighborhood violence on student performance include Grogger (1997) and Sharkey (2010). Both of these papers show that students are negatively affected by violence in their neighborhood. Grogger (1997) studies how local violence, defined as a combination of

[1] The categorization is based on the Frederick scoring system of the Post-traumatic Stress Disorder Reaction Index (PTSD-RI).

[2] On Feb 24, 1984, a sniper began firing from a second-story window across the street from an elementary school at children on the school playground. Two children were killed and 13 were injured.

[3] Chandler et al. (2011) build a predictive model of shootings, which helps determine which students should be included in a highly targeted and resource-intensive mentorship program in Chicago. Chandler et al. (2010) find that shootings are very hard to predict.

school violence and neighborhood violence, negatively affects educational attainment. Sharkey (2010) identifies the negative effect of exposure to a local homicide on the cognitive performance of children. He finds that a sample of African-American children between five and 17 years old had lower scores on educational assessments when they were exposed to a homicide in the Census block group less than a week before the assessment. Finally, Carrell and Hoekstra (2010) find that children who suffer from domestic violence significantly decrease the reading and math test scores of their peers and increase the amount of misbehavior in the classroom.

Our paper aims to improve the understanding of how extreme violence in schools affects enrollment, student performance, and student behavior, based on a sample of deadly shootings that occurred between 1994 and 2009. We also analyze student performance using nationwide aggregated school-level data up to three years after the shooting, and student-level data from California. Moreover, we hypothesize that homicidal shootings cause more severe effects on students compared to suicidal shootings, and confirm this by analyzing homicidal and suicidal shootings separately. This suggests that the traumatic impact of homicidal shootings plays a key role in explaining our results.

2.3 Data and Descriptive Statistics

Our main data source of shooting incidents is the Report on School Associated Violent Deaths from the National School Safety Center (2010). The report uses newspaper articles to track shootings between 1994 and 2009.[4] Additional school shooting data is from Washington Ceasefire and the National School Safety and Security Services, which we verified with information from newspaper clippings.

[4] We use the year of the fall semester to indicate the school year. For instance, we refer to the 2001-02 school year as 2001.

We use the National School Safety Center's definition of a deadly school shooting, which is any homicidal or suicidal gun-related death in the United States that occurred on the property of a functioning public, private or parochial secondary school; on the way to or from regular sessions at such a school; while a person was attending or was on the way to or from a school-sponsored event; or as an obvious direct result of school incidents, functions or activities, whether on or off a school bus, school vehicle or school property.

As shown in Figure 2.1, we document 157 shootings in high schools between 1994 and 2009 that resulted in one or more deaths. These shooting schools contained approximately 245,391 enrolled students, who may have suffered negative direct or indirect consequences from the event. We do not detect any trend in the annual number of deadly shootings. Among the 157 shootings that occurred in high schools, 104 were categorized as homicidal and 53 were suicidal incidents. Among the 104 homicidal shootings, 27 involved multiple deaths (ranging from 2 to 15 people).[5]

Data on school characteristics is from the Common Core of Data (CCD) from the National Center for Education Statistics (NCES) from 1990 to 2009. The data set provides a complete listing of all public elementary and secondary schools in the U.S. and provides basic information and descriptive statistics on schools, their students, and their teachers. We use CCD data for enrollment per grade (grades 9 to 12) and number of teachers.[6]

We define our comparison group as other high schools in the same district. Schools in the same district have many similar unobservable characteristics. As Figure 2.2 reveals, enrollment in other schools in the same district is not negatively affected by shootings. Thus, it is very unlikely that we double count the movement of students from shooting schools to comparison schools. Our estimates can be viewed as a lower bound of the true effect of

[5]When a person killed someone else and then committed suicide, we categorized the incident as a homicidal shooting. We classify accidental gun-related deaths in the homicidal category.

[6]There is no information on teacher turnover at the school level in the CCD.

school shootings on student outcomes because the comparison schools could be influenced due to their physical proximity, albeit at a different magnitude. Figure 2.2 shows a permanent decrease in entrance grade 9 enrollment after a shooting takes place.

Table 2.1 shows that schools that experience shootings are larger than average, both in terms of the number of total enrolled students and in full-time equivalent teachers (FTEs). This size difference is present in all grades and is noticeably larger in grade 9, the entrance grade for most high schools and before students are permitted to drop out of school.

School performance data is from each state's Department of Education website. A student's ability in math and English is tested at least once during high school using a standardized test. Information is extracted from each school's report card and from data files posted by each state's Department of Education. We focus on data from 2002 to 2010 due to availability. The *No Child Left Behind Act* passed in 2001 requires all schools receiving federal funding to administer a state-wide standardized test; in most states, these results are posted online. Most states only publish the proportion of a school's students who fall into various categories of achievement, such as "minimum," "basic," "proficient" and "advanced" performance, rather than the actual mean scores of the schools. We use the proportion of students achieving a proficient or advanced level on math and English state-wide standardized tests for each school, which we refer to as the "proficiency rate," as the outcome variable.

These tests vary from state to state but are identical within a state for any given year.[7] As Table 2.1 shows, the mean proficiency rate is not statistically

[7]We examine the relationship between 36 high school shootings and the proportion of students achieving a proficient- or advanced-level result on English tests in 14 states. We also examine the relationship between 34 high school shootings and the proportion of students achieving a proficient- or advanced-level result on math tests in 13 states. Not all states have both test results posted on their Department of Education websites, which is the reason why the sample size is different for math and English tests. English test results are from Alabama, California, Florida, Louisiana, Michigan, Minnesota, Nevada, North Carolina, South Carolina, Tennessee, Utah, Washington and Wisconsin. Math test results are from Alabama, California, Florida, Louisiana, Michigan, Nevada, North Carolina,

different between "shooting schools" and comparison schools. Figures 2.3 and 2.4 display the average proficiency rate for the years before and the years after any shooting incidents for shooting schools and comparison schools, which show a decline in the math and English proficiency rates in the years following a homicidal shooting for schools that experienced a shooting.

In addition, we collected school-level graduation rates, average daily attendance rates, and numbers of suspensions per 100 students for all schools in the districts that experienced shootings in all available states.[8]

We use student-level data from California. The data is provided by the California Department of Education (CDE) for 2007 to 2010. During that period, seven deadly high school shootings occurred in seven school districts. The seven affected school districts have 195 high schools within their boundaries and a large number of students. The data contains test results on the California Standards Tests (CST). The CSTs, which are part of the California Standardized Testing and Reporting (STAR) program, are taken by students from grades 2 through 11 in many subjects, but we use only math and English results from grades 9 through 11. We have measures of the proficiency level in math and English for students in the seven districts. The possible levels of math and English proficiency for students in the seven districts are: far below basic (1), below basic (2), basic (3), proficient (4), and advanced (5). We also have information on the sex of the students, which allows us to determine whether shootings affect males and females differently.

South Carolina, Tennessee, Utah, Washington, and Wisconsin.

[8]We have information on graduation rates and attendance rates for shooting-affected school districts for ten shootings in five states (Nevada, North Carolina, South Carolina, Tennessee, and Utah) and information on numbers of suspensions per 100 students for seven shootings in three states (Nevada, North Carolina, and Tennessee).

2.4 Methodology

We use a difference-in-differences (DiD) strategy to analyze the effect of deadly homicidal high school shootings. The comparison group consists of all other high schools in the same district. We estimate

$$Y_{it} = \beta_0 + \beta_1 After_{it} + \beta_2 After_{it} * Shooting_i + \mu_i + \gamma_t + \epsilon_{it} \qquad (2.1)$$

where Y_{it} is one of several different outcome variables for school i in year t; $Shooting_i$ is a dummy variable that takes a value of 1 if there was ever a shooting in school i and 0 otherwise; and $After_{it}$ is an indicator for the period after the shootings.[9] The coefficient of the interaction variable ($After_{it} * Shooting_i$) is of primary interest, as it captures the casual effect of school shootings on various outcomes. The outcomes of interest are: enrollment per grade (9 to 12), number of teachers, proficiency rate (in math and English), and behavioral variables (graduation, suspension and attendance rates). We include school fixed effects, μ_i for school i, to control for any time-invariant school-level factors that may be correlated with shootings and the outcome variables. We also include year fixed effects to control for any policy changes or trends from 1994 to 2009.[10] We use clustered standard errors at the district level. We use a three-year window around the shooting year.[11]

[9] The "after" period is defined differently for the enrollment analysis and the proficiency rate analysis. For the enrollment analysis, the "after" period starts the school year following the shooting, since enrollment data is typically generated very early in the school year (usually in September or October). For proficiency rate analysis, the "after" period starts the same year as the shooting, since the tests are usually administered towards the end of a school year.

[10] We tested different specifications of the model, such as using district and year fixed effects (controlling for enrollment three years prior), which lead to similar results. Results of this specification can be provided upon request.

[11] The nature of difference-in-differences estimation requires us to check whether the schools and districts have multiple shootings over the sample period. Multiple shootings in one school or district could bias our estimates because the "before" and "after" periods of the shootings could overlap with those of another shooting in the same school or district.

We also present matching regression estimates based on four characteristics of the school: state, area (city, suburb, town or rural), size of school and number of teachers. Using these variables, Kernel, Caliper and Nearest Neighbor matching estimators are employed.

To identify whether negative effects of school shootings result from students being directly affected by shootings or from a composition effect (e.g. students with a high level of achievement might not stay or register at a school after a shooting), we use student-level data and condition on having a test result before and after a shooting at the same school.[12] We use a similar empirical strategy for student-level data as for school-level data, so that we can exploit the panel aspect of the data at the student level. We estimate conditional logit models with student-level fixed effects. The primary outcome variables of interest are whether a student is proficient in English and math (whether the student achieves level 4 or 5 in California).

We also investigate the possibility that shootings have heterogeneous effects in two ways. First, we investigate whether shootings affect students in various parts of the test-score distribution differently. To study the most affected part of the distribution, we change the outcome variables in the same regression to the probability of being in level 2 to 5, level 3 to 5, and level 5 to identify which part of the distribution is generating the lower level of test results in schools that experienced a shooting. Second, we study whether boys are affected differently than girls.

High school shootings occur only once in most school districts over the 16 school years; 103 school districts had one shooting, 12 school districts had two shootings, and six school districts had three or more shootings. In our analysis, additional to all initial shootings in a district, we include subsequent shootings in a district if they are six or more years apart. We view shootings six or more years apart as distinct because almost all students who experience a shooting leave their school within three years, which could be interpreted as the school returning to its pre-shooting environment. Another rationale for a three-year window around the year of shootings is that using the entire sample for the difference-in-differences estimator will contain noise in years far from the shooting incidents. This leads us to use a three-year window sample for all analysis.

[12]Similar results are found if we restrict the sample to two observations per student, one before and one after a shooting (balanced panel).

2.5 Results

Table 2.2 reveals that homicidal shooting schools experienced a decline in grade 9 enrollment relative to other schools in the same district.[13] Table 2.2 shows that a shooting reduces enrollment in grade 9 by 28 students on average, which represents a 5.8 percent decline in grade 9 enrollment for the average school experiencing a shooting. This decrease in grade 9 enrollment represents a large change in school selection by students entering high school. One possible explanation for the large decline in grade 9 enrollment is that middle school students and their parents try to avoid the school that had the shooting.[14]

Enrollment in other grades and the number of teachers employed do not show a statistically significant change after a shooting.[15] The fact that enrollment for grades 10, 11, and 12 does not significantly change the following year after a shooting suggests that continuing students in schools experiencing a shooting have established connections that raise the cost of transferring to another school. It is also likely to be administratively difficult for continuing students to transfer.

Table 2.8 presents regression results for enrollment in grades 10 and 11, excluding either the first year or the first two years after a shooting. It shows that a decrease in enrollment for the entrance grade (grade 9) immediately following a shooting is followed by a decrease in the number of student

[13]We use a subset of high schools for the enrollment analysis, which is high schools where the lowest grade is grade 9 and highest grade is grade 12, to ensure a clear interpretation of the coefficient. Among the 157 high school shootings, 136 occurred in high schools that have grades 9 through 12 over the sample period. Results are robust to the inclusion of all high schools.

[14]Smith et al. (2012), for example, find that parents and students change enrollment decisions in response to negative news about schools.

[15]We do not have information on teacher turnover in the data. It is possible that some teachers leave after a shooting and are replaced by younger teachers. An alternative approach would be to use the student to teacher ratio as an outcome variable. The coefficient for the student to teacher ratio is positive but not significant. Results are available upon request.

enrolled in grade 10 (after one year) and grade 11 (after two years).

Columns 1 and 2 of Table 2.3 show that the proficiency rate decreases after homicidal shooting incidents relative to comparison schools. Table 2.3 indicates that the proficiency rate in math is reduced by 4.9 percentage points, which means that the proficiency rate in math decreased by 9.3 percent for the average school experiencing a shooting. For English tests, the effect of shootings is of a slightly smaller magnitude, 3.9 percentage points lower than the comparison schools. This means that in the average school experiencing a shooting, 6.2 percent fewer students achieve a "proficient" or "advanced" level on their English tests.

Columns 3 to 5 of Table 2.3 show the causal effect of deadly shootings on graduation rates, average daily attendance rates, and the number of suspensions per 100 students. We do not find any statistically significant results for all three outcomes.

Table 2.4 presents results for the impact of suicidal shootings on outcome variables: enrollment per grade (9 to 12), number of teachers, and the proficiency rate in math and English.[16] Suicidal shootings can be an important aspect of school shootings as Lang (2013) finds that gun ownership rate is positively associated with firearm suicide rate.

Table 2.4 shows that suicidal shootings have no significant impact on any outcome of interest.[17] This suggests that homicidal shootings may be more traumatic for students than suicidal shootings.

Results from the school-level analysis indicate that a large number of students are likely to change their school selection due to school shootings. This implies that identified school-level effect is a total effect, which is a sum of compositional change and individual effect. Total effect has high policy relevance, however, separating the individual effect will allow us to isolate the true shock on students' educational outcomes from school shootings.

[16]We do not have enough observations to study behavioral outcomes for suicidal shootings.

[17]The sample size is smaller than homicidal shootings and standard errors are bigger.

Individual effect is identified by using student-level data and by conditioning on students staying in the same schools after shooting incidents.

Using California student-level data, Table 2.5 shows 4.2 and 10.2 percentage point decreases in the probability of achieving a proficient-level result (achievement level 4 or 5 in California) in math and English, respectively. These results suggest that the decrease in test scores is not solely due to fewer high-achieving students attending schools where shootings occurred.[18] This suggests that students' academic achievement worsens.

Table 2.6 identifies the effect of shootings on the probability of reaching various achievement levels. It shows that shootings have heterogeneous effects on the math test results. For math tests, the negative effects are concentrated on students who are at the high achievement part of the distribution. The negative effect of shootings on the probability of reaching achievement level 5 in math tests is large, 10.4 percentage points, which is as large as English test results. The magnitude of the effect of shootings decreases as the achievement level of interest goes down, almost disappearing when looking at the probability of achieving level 3 or higher. However, the negative effect is consistent throughout the distribution for English test results. Lastly, when we analyze the effects of shooting by gender, we find that male and female students are both similarly affected by shootings with respect to their English test results (see Table 2.7). Shootings negatively affect math test results for females, but not for males.

Individual effect identified by student-level analysis shows that students' math and English test scores are directly affected by school shootings. These results confirm that effect of school shootings are coming from both compositional change and individual effect. Also, this suggests that academic aspects should be helped in addition to other counselings provided to students in the aftermath of shootings.

[18]The results are similar when we restrict the sample to students who stay in the same school district as well as to those who do not repeat a grade.

2.6 Robustness

We do several tests to ensure that our results are robust and valid.

First, we conduct a randomization of the shooting incidents. We randomize the shootings within the school districts for the year the shooting took place and re-run baseline regressions for our main outcome variables: the proficiency rate in math and English, as well as enrollment in grade 9. The rationale behind this randomization is to provide confidence that our significant results are not caused by a factor other than the shootings. We do 1,000 replications and find that it is unlikely that the results are random. Figure 2.5 and 2.6 present histograms of t values and coefficients by intervals for our main variable of interest. Results from this randomization and these figures give confidence to our results.

Second, our results are robust to alternate specifications, such as using district fixed effects instead of school fixed effects. Results are also robust to alternate error clustering, such as clustering at the state level or using a block bootstrap specification at the state level.

Third, we check that our results are not driven by extreme shooting incidents where multiple people die (the effect on students could arguably be higher in these cases). Restricting the sample to school shootings where only one person dies leads to a similar conclusion for the proficiency rates on math and English tests as well as enrollment in grade 9.

Fourth, in Table 2.9, we also present matching regression estimates based on state, area (city, suburb, town or rural), size of school and number of teachers. We get similar estimates from three types of matching estimates (Kernel, Caliper and Nearest Neighbor) but larger coefficients than our main results. This implies that our preferred estimates could be a lower bound of the true effect of deadly school shootings on educational outcomes.

2.7 Conclusion

We analyze the causal effect of deadly shooting incidents in high schools on these schools and their students. We find that enrollment declines in grade 9 (the high school entrance grade) in schools that experience homicidal shootings. Furthermore, math and English standardized test proficiency rates drop significantly in schools that experience a shooting. However, we do not find a detrimental effect of shootings on suspension, graduation, or average daily attendance rates. We find that there is no significant impact for suicidal shootings. To settle whether students are directly affected by shootings or if it is rather a composition effect, we use student-level data from California. We find that students are directly affected by shootings. There is a decrease in probability of being at proficiency level 4 or 5 (a high achievement level) for math and English tests.

The negative effect of shootings on student achievement on math and English tests could be an important factor in determining wages and employment for these students in the long-run. If students attending schools that experienced a shooting have lower test scores, they might be accepted into less selective colleges, which could lead to lower earnings later in life (Hoekstra, 2009). Moreover, several studies document the links between student performance and labor market outcomes at adulthood. Neal and Johnson (1996) find that scores from tests administered between the ages of 14 and 21 are highly significant predictors of wages at age 26 to 29. Murnane, Willett, and Levy (1995) show that test scores from one's senior year of high school are related to wages at age 24. Currie and Thomas (2001) find that a one standard deviation increase in test scores at age 16 translates into a higher wage rate and higher probability of being employed at age 33. Thus, even though we are looking at the short-run impact, school shootings are likely to have long-run negative effects on students too. Future research should try to answer this question.

Our estimates indicate that schools and students, on average, are highly affected when there is a homicidal shooting. These results indicate that policymakers should consider providing extra support to all students in schools where a shooting occurs. It also suggests that more effort should be invested in preventive measures such as gun control (Duggan, 2001; Marvell, 2001; Lott and Whitley, 2001) and more resources should be made available to students (Carrell and Hoekstra, 2011), especially in the aftermath of shootings. More research should be done regarding the negative effect of high school shootings, such as on the long-term effects of shootings on students since there are direct and indirect burden of crime for students and the entire nation (Anderson, 1999).

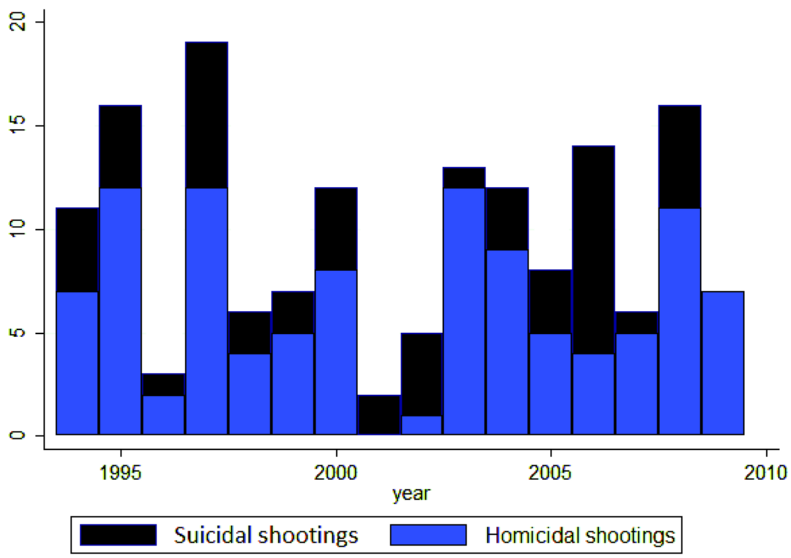

Figure 2.1: Number of Shootings By Type of Shooting
Source: Report on School Associated Violent Deaths from the National School Safety Center (2010), Washington Ceasefire, and the National School Safety and Security Services.

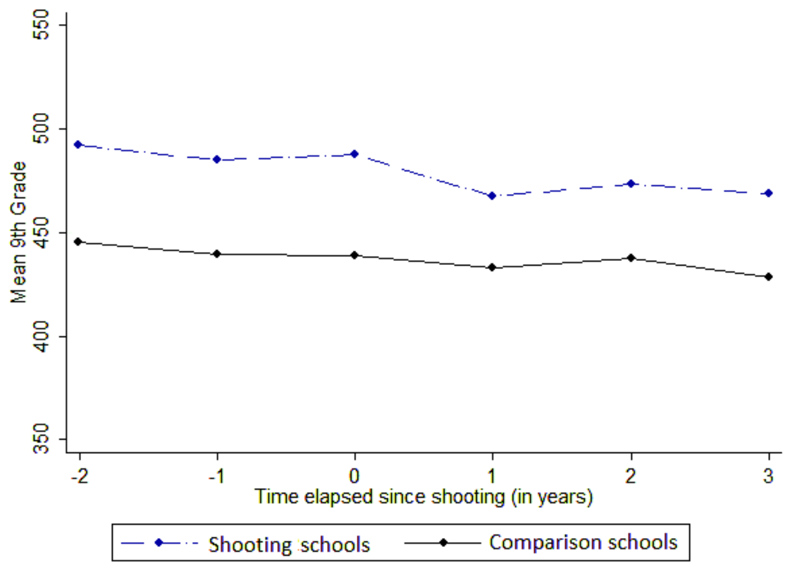

Figure 2.2: The Effect of Shootings on Grade 9 Enrollment (Entrance Grade)
Source: Common Core of Data (CCD) from the National Center for Education Statistics (NCES).

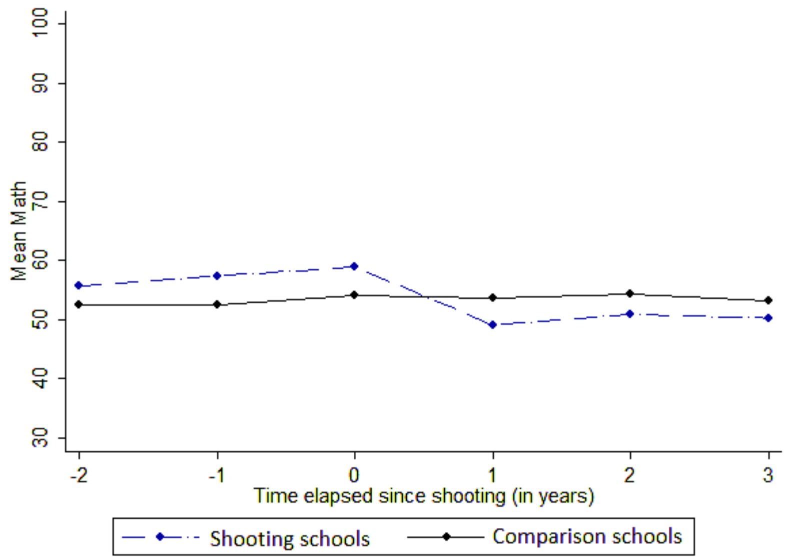

Figure 2.3: The Effect of Shootings on math Proficiency Rate
Source: Information was extracted from each school's report card and from data files posted by each state's Department of Education.

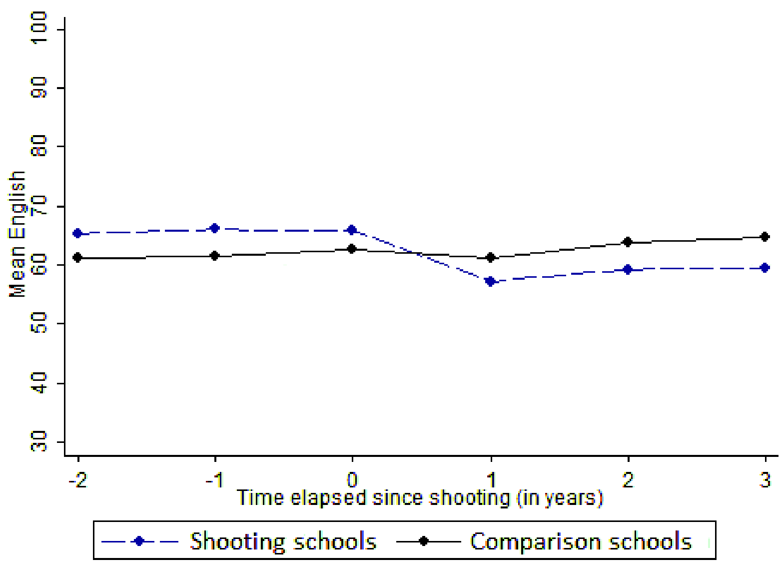

Figure 2.4: The Effect of Shootings on English Proficiency Rate
Source: Information was extracted from each school's report card and from data files posted by each state's Department of Education.

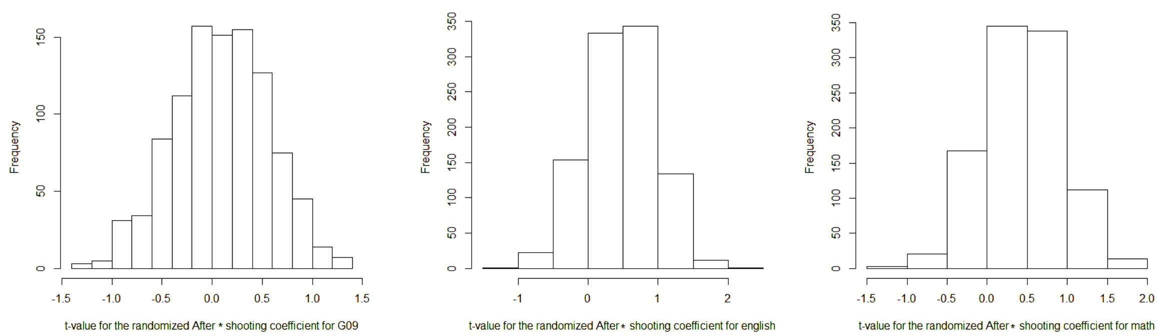

Figure 2.5: Distribution of t-values from Randomization for Enrollment in Grade 9, English, and Math

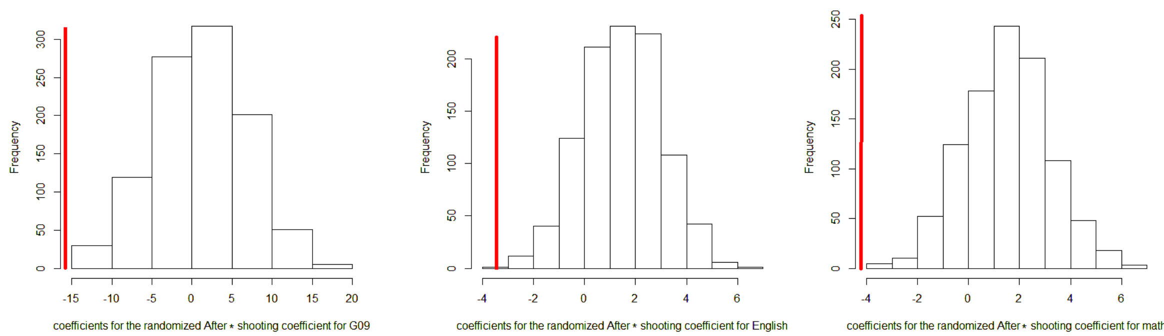

Figure 2.6: Distribution of Coefficients from Randomization for Enrollment in grade 9, English, and math. Verticical line represents estimates from our main specification.

Table 2.1: Summary Statistics - High Schools before a shooting

Variable	All Shooting Schools Mean	All Shooting Schools Std. Dev.	Comparison Schools Mean	Comparison Schools Std. Dev.
Enrollment in				
Grade 9	486	283	436	344
Grade 10	426	238	378	289
Grade 11	352	196	314	240
Grade 12	298	171	262	202
Total Students	1587	835	1408	1044
FTE Teachers	80	37	73	47
English	65.4	23.0	60.5	27.9
Math	57.8	24.3	52.9	28.6
Graduation Rate	71.5	13.6	72.3	17.8
Attendance Rate	92.8	3.5	91.3	3.9
Suspension Rate	19.6	17.7	18.3	18.0

Note: Table 2.1 presents descriptive statistics for key variables for shooting schools and our comparison schools for the three years before the shooting. Enrollment and teacher variables are from the Common Core of Data. Test results and behavioral variables are from school report cards. Only high schools with grades 9 to 12 are included in the enrollment and teacher sample. All high schools are included in the test results and behavioral sample. Math and English variables are the proficiency rate from standardized tests. FTE Teachers are full time equivalent teachers. Suspension rate is number of suspensions per 100 students. The comparison schools are all other schools in the shooting district. Using a t-test or Wilcoxson test, we find that shooting schools are statistically different in terms of students (grade 9 to 12 and total students) and number of teachers but not for proficiency in English and math, as well as graduation, attendance, and suspension rates.

Table 2.2: The Effect of Homicidal Shootings on Enrollment

	Enrollment in Grade					# of
	9	10	11	12	Total	Teachers
After	-3.48	-6.46	-8.08**	0.92	-14.27	0.57
	(7.03)	(4.51)	(4.09)	(2.62)	(12.61)	(1.25)
After*Shooting School	-28.41***	-8.84	6.96	-3.71	-37.79	-1.78
	(10.92)	(8.37)	(9.30)	(6.69)	(23.97)	(1.28)
Observations	5,385	5,386	5,394	5,392	5,397	5,222
R-squared	0.842	0.890	0.875	0.850	0.941	0.901

Note: Table 2.2 presents difference-in-differences regression estimates for the number of student in grades 9 to 12 and the number of teachers. The coefficient of interest is After*Shooting School. We use clustered standard errors at the district level. Coefficients for school and year fixed effects are not shown. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.
Source: Common Core of Data (CCD) from the National Center for Education Statistics (NCES).

Table 2.3: The Effect of Homicidal Shootings on Test Results and Behavioral Variables

	Fraction Proficient in Math	English	Graduation	Rate of Attendance	Suspension
After	-3.48	-3.52**	0.81	-1.29***	1.02
	(2.31)	(1.58)	(1.34)	(0.46)	(1.34)
After*Shooting School	-4.92***	-3.93***	0.40	0.62	-2.28
	(1.18)	(1.07)	(1.19)	(0.39)	(1.55)
Observations	1,412	1,425	566	501	462
R-squared	0.606	0.668	0.254	0.366	0.669

Note: Table 2.3 presents difference-in-difference regression estimates for math and English proficiency rate, graduation, attendance and suspension rates. The coefficient of interest is After*Shooting School. We use clustered standard errors at the district level. Coefficients for school and year fixed effects are not shown. *** p<0.01, ** p<0.05, * p<0.1.
Source: Test results and other variables are extracted from each school's report card and from data files posted by each state's Department of Education.

Table 2.4: The Effect of Suicidal Shootings

	Enrollment in Grade			
	9	10	11	12
After	-2.04	-8.50	-1.16	2.02
	(10.31)	(6.99)	(5.02)	(3.51)
After*Shooting School	22.70	-2.69	3.77	0.88
	(17.61)	(14.03)	(14.41)	(8.52)
	Total Number of		Fraction Proficient in	
	Students	Teachers	Math	English
After	-13.25	0.48	-3.39	0.64
	(17.19)	(0.78)	(10.18)	(7.08)
After*Shooting School	26.59	0.09	7.50	-5.59
	(39.16)	(1.49)	(10.59)	(6.29)

Note: Table 2.4 investigates the effect of suicidal shootings. We run regressions for enrollment per grade, number of teachers, proficiency in math and English and behavioral outcomes for suicidal shootings. The coefficient of interest is After*Shooting School. We use clustered standard errors at the district level. Coefficients for school and year fixed effects are not shown. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.
Source: Enrollment data are from the Common Core of Data (CCD) from the National Center for Education Statistics (NCES). Test results and other variables are extracted from each school's school report card and from data files posted by each state's Department of Education.

Table 2.5: The Effect of Shootings using California Student Level Data - 2007-2011

	Probability of Proficiency Level in	
	Math(Level 4 or 5)	English (Level 4 or 5)
After	-0.079***	-0.015
	(0.010)	(0.009)
After*Shooting School	-0.042**	-0.102***
	(0.017)	(0.017)
Observations	246,864	270,114
Number of Students	120,924	125,949

Note: Table 2.5 investigates the impact of shootings on students using student-level data from the California Department of Education. Using conditional fixed effects logit models with student-level fixed effects, we study the probability of students achieving level 4 or 5 in math and English. The sample is restricted to students who took tests both before and after a shooting. The level of math and English proficiency for students in the 7 districts are: far below basic (1), below basic (2), basic (3), proficient (4), and advanced (5).To correct for autocorrelation, we cluster errors at the district level. Estimates for student and year fixed effects are not shown. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.
Source: Student-level data from California provided by the California Department of Education (CDE).

Table 2.6: The Effect of Shootings on Cumulative Level of Achievement

	Probability of Student Reaching Achievement Level			
	2, 3, 4, or 5	3, 4 or 5	4 or 5	5
Math Test	0.005	-0.012	-0.042**	-0.104***
	(0.015)	(0.012)	(0.017)	(0.039)
English Test	-0.106***	-0.116***	-0.102***	-0.104***
	(0.020)	(0.017)	(0.017)	(0.019)

Note: Table 2.6 investigates the distributional impact of shootings on students using student-level data from the California Department of Education. Using conditional fixed effects logit models with student-level fixed effects, we study the probability of students achieving various levels on math and English tests after a shooting. The sample is restricted to students who took tests both before and after a shooting. The level of math and English proficiency for students in the 7 districts are: far below basic (1), below basic (2), basic (3), proficient (4), and advanced (5). Column 1 estimates the probability of reaching achievement level 2, 3, 4, or 5 after the shooting. As we move right from column 1, the remaining columns restrict the outcome to higher levels of achievement. The coefficient of interest is After*Shooting School for math and English tests. Estimates for student and year fixed effects are not shown. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.
Source: Student-level data from California provided by the California Department of Education (CDE).

Table 2.7: The Effect of Shootings by Gender using Student-Level Data

Male Students	Probability of Achieving Proficiency in	
	Math (Level 4 or 5)	English (Level 4 or 5)
After	-0.062***	-0.010
	(0.014)	(0.013)
After*Shooting School	-0.030	-0.094***
	(0.023)	(0.024)
Observations	125,649	138,731
Number of Students	62,238	65,190

Female Students	Probability of Achieving Proficiency in	
	Math (Level 4 or 5)	English (Level 4 or 5)
After	-0.098***	-0.020
	(0.015)	(0.013)
After*Shooting School	-0.054**	-0.110***
	(0.024)	(0.024)
Observations	121,215	131,383
Number of Students	58,755	60,841

Note: Table 2.7 investigates the impact of shootings on students by gender using student-level data from the California Department of Education. Using conditional fixed effects logit models with student-level fixed effects, we study the probability of students reaching level 4 or 5 on math and English tests. The sample is restricted to students who took tests both before and after a shooting. Estimates for student and year fixed effects are not shown. *** p<0.01, ** p<0.05, * p<0.1.
Source: Student-level data from California provided by the California Department of Education (CDE).

Table 2.8: The Effect of Shootings on enrollment for future years

VARIABLES	Grade10	Grade11	Total	Total
After	1.201	-2.881	3.982	-9.118
	(6.251)	(9.900)	(24.09)	(34.38)
After*Shooting School	-41.40*	-35.92*	-130.2*	-196.0**
	(22.28)	(21.81)	(75.40)	(83.04)
EXCLUDED 1^{st} Yr	Y	Y	Y	Y
EXCLUDED 2^{nd} Yr		Y		Y
Observations	5,173	4,335	5,192	4,345
R-squared	0.437	0.438	0.462	0.468

Note: Table A.1 presents difference-in-differences regression estimates for the number of students in grades 10 and 11, and the total number of students, by excluding the first year or the first two years after a shooting. The coefficient of interest is After*Shooting School. We use clustered standard errors at the district level. Coefficients for school and year fixed effects are not shown. *** p<0.01, ** p<0.05, * p<0.1.
Source: Common Core of Data (CCD) from the National Center for Education Statistics (NCES).

Table 2.9: The Effect of Homicidal Shootings on Test Results - Matching Estimates

	(1)	(2)
	Fraction Proficient in	
	Math	English
Kernel	-10.17***	-8.43***
	(2.47)	(2.40)
Caliper	-10.75***	-9.17***
	(2.48)	(2.41)
Nearest Neighbor	-7.79***	-5.15*
	(3.58)	(3.13)

Note: Table 2.9 presents matching regression estimates for math and English proficiency rate. Matching regressions are based on state, area (city, suburb, town or rural), size of school and number of teachers. Table 2.9 presents three type of matching estimates: Kernel, Caliper and Nearest Neighbor. The reported coefficient is the variable of interest: the shooting variable. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.
Source: Test results and other variables are extracted from each school's report card and from data files posted by each state's Department of Education.

Chapter 3

Ill Communication: Mobile Phones & Student Performance

Authors: Louis-Philippe Béland and Richard Murphy

3.1 Introduction

Technological advancements are commonly viewed leading to increased productivity. Numerous studies document the benefits of technology on productivity in the workplace (Aral et al. (2007), Ding et al. (2009) and Chakraborty and Kazarosian (1999)) and on human capital (Malamud and Pop-Eleches (2011)). There are, however, potential drawbacks to new technologies, when they can be used for multiple purposes. Then they may provide distractions, and reduce productivity, such as is the case with teenagers and mobile phones at school.

Mobile phones can be a source of great distraction in the classroom, as they provide students with access to chat software, texting, games, social media and the Internet. Given these features, a mobile phone can be a great temptation to the teenage mind during lessons.

A large majority of teenagers use mobile phones. In the United States, 72% of teens owned a mobile phone in 2012, while in England, the proportion was 90.3.%[1] There is a debate in both countries as to how schools should address the issue of mobile phones. Some advocate for a complete ban, while others promote the use of mobile phones as a teaching tool in classrooms (eg. Barkham and Moss (2012), Drury (2012), O'Toole (2011), Johnson (2012) and Carroll (2013)). Schools differ on how they have reacted to this phenomenon, with some applying strict bans and others allowing their usage. Despite their prevalence, the consequences of mobile phones for high school student performance has not been studied to date.

In this paper, we estimate the effect of implementing a mobile phone ban on student test scores. We generate a unique dataset on mobile phone policies from a survey of high schools in four cities in England (Birmingham, Leicester, London and Manchester), which we combine with administrative data from U.K. school performance tables and the National Pupil Database. We use differences in mobile phone policies and differences in the timing of the implementation of these policies to measure the impact on student performance, first at the school level, then at the student level. The nature of the National Pupil Database allows us to study the impact of mobile phones on different types of high-school students: by gender, race and their prior achievement in elementary school at age 11.

Our results indicate that there is an improvement in school and student performance after introducing a mobile phone ban. We find no significant impact on mobile phone bans that are not widely complied with and that the impact of banning of mobile phones improves the outcomes of low-achieving students the most. Thus, our results suggest that banning mobile phones from school premises reduces educational inequality.

The rest of the paper is organized as follows: Section 2 discusses mobile phone use in England; Section 3 provides a description of the data, survey and descriptive statistics; Section 4 presents the methodology; Section 5 is

[1] According to eMarketer (2012).

devoted to results; and Section 6 concludes with policy implications.

3.2 Mobile Phones in England

Mobile phones are ubiquitous in England; 94% of adults and 90.3% of teenagers owned a mobile phone in 2012.[2] Figure 1 shows the percentage of teenagers and adults who owned a mobile phone in England, from 2000 to 2011, and shows a steady increase in mobile phone ownership, from 60% in 2000 to 94% in 2012. Teenagers and adults had similar ownership rates over this period.[3]

In addition to ownership, the usage of mobile phones changed drastically between 2000 to 2011. The primary use of mobile phones has evolved from calls and text messages to, in recent years, the Internet and social networking. This change was brought about with the introduction of smartphones between 2004 and 2007. In 2004, prior to their introduction 60% had a mobile phone, but the number of texts sent per day was 1-2 on average, with limited Internet usage. In contrast, in 2007, more than 70% had mobile phones, and the average mobile phone owner sent more than five texts per day[4]. Internet usage on mobile phones has increased drastically, as many games and applications have emerged on smartphones in recent years. Over a quarter of adults and almost half of teenagers in England owned a smartphone in 2012, with estimates that by 2017, 96% of teenagers will have a smartphone.[5]

[2]According to MobilePhone operators (2012) and eMarketer (2012), respectively.

[3]According to reports by Ofcom (2011).

[4]According to reports by Ofcom communication market reports (2012).

[5]According to reports by Ofcom communication market reports (2012) and eMarketers (2012), respectively.

3.3 Data and survey

3.3.1 Mobile phone policies

The U.K. Department of Education does not have any official policy or recommendation regarding mobile phone usage in schools. Therefore, schools are free to decide how to regulate their use on school property, which means there is a variation in how mobile phones are treated.

In the spring of 2013, we conducted a survey of high schools' mobile phone policies in four cities in England (Birmingham, Leicester, London and Manchester). We emailed every high school in each of the four cities with a set of questions on their mobile phone policies. The survey had questions about the current policy toward mobile phones, when it was implemented, whether there was a previous mobile phone policy and, if so, when it was implemented. This was complemented by questions relating to punishments for violating the policy and how well complied with answered by the head-teacher (equivalent to a principal) considered the policy to be. In addition to questions relating to mobile phone policy, we also asked if there were any other policy or leadership changes occurring over the same time period,to account for any general shifts in educational policy at the school[6].

We received complete answers from 90 schools, which represents nearly 20% of the high schools in the four cities in our sample. Table 3.1 presents statistics on when mobile phone policies were put into effect and the type of policy introduced (a ban with a high level of compliance vs. low-compliance ban). It shows that many schools implemented a mobile phone ban between 2004 and 2010, and that most of the bans are widely complied with.

Table 3.2 presents descriptive statistics for key variables for the whole sample and pre- and post-policy by ban type. It shows that permanent stu-

[6]The school could either reply to our email directly, or visit a website to respond to our survey. The survey website is http://mobilephoneatschool.weebly.com. We sent up to three emails if we did not receive an answer. Finally, if we did not hear back from schools from our emails, a research assistant called the schools to gather survey responses orally.

dent characteristics are similar pre- and post-ban, implying there is minimal sorting by parents according to mobile phone policy. It also indicates an increase in standardized test scores at age 16 after the introduction of the policy. Table 3.9 presents summary statistics on the schools that responded to our mobile phone survey and schools that refused to answer or did not provide complete information on their school mobile policy. Schools that responded and schools that did not provide a complete response are similar along a number of dimensions (proportion of Special Educational Needs (SEN) students, average test scores (GCSE) and number of pupils), though schools that answered our survey have a statistically higher proportion of SEN students.

We also categorize mobile phone restrictions into high-compliance bans and low-compliance bans, based on the head teacher's assessment of how widely the policy is being adhered to. Once a school implements a mobile phone ban, we observe very few changes to the policy over time.

3.3.2 Student Performance

All students in England's publicly funded schools follow the National Curriculum. They progress through a series of five "Key Stages." Our paper focuses on secondary school students. Students start secondary school at age 11 after completing Key Stage 2 in primary school. Key Stage 3 covers the first three years of secondary school and Key Stage 4 leads to subject-specific exams at age 16, called a General Certificate of Secondary Education (GCSE).

U.K. Performance Table — School-level data

We use the school performance tables published by the U.K. government in November of each year. The performance tables detail the achievement of each school's pupils in the most recent national externally marked exams.

The key measure is the proportion of students achieving a grade of A*-C on at least five GCSEs, which is often seen as the basic benchmark of attainment by employers and academic institutions.

National Pupil Database — Student-level data

The National Pupil Database (NPD) is a rich education dataset of the complete state school population of England. It contains information on student performance as well as student characteristics (gender, age, race, ethnicity, whether they are eligible for the Free School Meals (FSM), and whether they are a Special Educational Needs (SEN) student). From this additional school features can be derived such as the percentage of students with particular characteristics.

The measure of student achievement that we use is the standardized score of a student at Key Stage 4. This score gives us more information than the dichotomous variable: whether or not the student earned at least a C on five GCSEs. We also use an alternative measure of student performance as an outcome variable, which reflects the differences in difficulty in attaining certain grades and student performance at Key Stage 3. These test scores are standardized by year to account for any potential grade inflation.

3.4 Empirical Strategy

3.4.1 School-level data

To estimate the impact of a mobile phone ban, we use the difference in timing for the introduction of the policy, using high schools in our sample that had not yet imposed a ban as our comparison group. Equation (1) represents our main specification.

$$Y_{st} = \beta_0 + \beta_1 MobileBanOn_{st} + \beta_2 X_{st} + \mu_s + \gamma_t + \epsilon_{st} \tag{3.1}$$

where Y_{st} is the outcome variable of interest, the percentage of students that achieve at least a C on five GCSEs, for school s in year t; X_{st} are controls for school characteristics (total pupils and the proportion of Key Stage 4 pupils with SEN). The variable of interest is $MobileBanOn_{st}$, which takes a value of one for every year after the implementation of the mobile ban. It captures the impact of the introduction of the mobile phone ban on student performance. Accordingly, the coefficient of interest is β_1 which represents that increase in the proportion of students attaining 5 A-C GCSE at school s. We include school fixed effects, μ_s, for school s, to control for any time-invariant school-level factors that may be correlated with outcome variables. We also include year fixed effects, γ_t, to control for any trends in student attainment from 2002 to 2012.

A potential threat to the study's validity arises if other policies were implemented at the same time as a mobile phone ban. To address this, we use information on whether any leadership or policy changes occurred during the period of analysis. Therefore, in the most demanding specifications, we control for such changes. This is open to recall bias, but we would expect that head teachers would be very familiar with school-level policies and leadership changes.

3.4.2 Student-level data

We use a similar empirical strategy for student-level data as for school-level data, but now we can exploit the panel aspect of the data at the student level as well as conditioning on individual characteristics. In the specifications that control for prior test score we are accounting for individual ability and all inputs up until age 11 and therefore the coefficients can be interpreted as growth terms between age 11 and age 16. Moreover as we are allowing for school effects the variation that we are using is within school, i.e. did students in school s have significantly higher learning growth after a ban was introduced. Again the primary outcome variable of interest is student performance at age 16, but now β_1 represents the standard deviation gains in test score due to a mobile phone ban. We use interaction terms to study the impact of the mobile ban on students with different characteristics: SEN students, FSM students, males, minority groups and achievement level at age 11. Results using the achievement level at age 14 are very similar to our main findings, but with a smaller sample size.

3.5 Results

3.5.1 School-level data

Table 3.3 presents regression results for the fraction of students achieving at least a C on five GCSEs, which represents school performance. There is a substantial increase in the fraction of students attaining the five-C benchmark after the introduction of a mobile phone ban. Estimates of our preferred specification, column (4) in the table, show an increases of 3.64% points after the introduction, significant at the 1% level. Table 3.3 indicates that a higher proportion of students achieve at least a C on five GCSEs after a mobile phone ban is imposed. Results are robust across specifications.

Table 3.4 investigates heterogeneity of the mobile phone ban. It separates bans into high-compliance bans and low-compliance bans using the head teacher's assessment of the degree to which the ban is complied with.[7] In Table 3.4, we examine whether high-compliance mobile phone bans have a more positive impact on student performance than low-compliance bans. Table 3.4 indicates that only high-compliance bans have a significant impact on the proportion of students achieving at least a C on five GCSEs. The coefficient associated with the low-compliance variable is positive but not significant. Estimates from our preferred specification, column (4) in the table, show an increases of 3.88% after the introduction of an high-compliance

[7]We define high-compliance bans as those with a score from the head teacher of 4 or higher out of 7.

mobile ban, significant at the 1% level.

3.5.2 Student-level data

Tables 3.5 and 3.6 present estimates of the impact of a mobile phone ban on individual student performance.

Table 3.5 presents the impact of imposing a mobile phone ban on student performance, controlling for student characteristics (achievement at age 11, gender, SEN students and FSM students). Results of our preferred specification (4) show an improvement in student performance after a school bans mobile phones of 6.09%, significant at the 5% level. Results are robust across specifications.

In Table 3.6, we examine whether high-compliance mobile bans have a more positive impact on student performance than low-compliance mobile bans. As in the aggregate data, we find that only high-compliance mobile bans have a positive and significant impact on student achievement (7.45%). The coefficient associated with a low-compliance mobile ban is positive but not significant.

Table 3.7 studies the heterogeneity of a ban on students with different characteristics, using interaction terms for SEN students, FSM students, males and minority groups. This is in addition to any baseline effects. The results indicate that a mobile phone ban has a positive and significant impact on FSM-eligible students (column (1)), SEN students (column (2)) and males (column (3)). In columns (1), (2) and (3) the baseline effect of a mobile phone ban is not significant when controlling for student characteristics and ban interaction, which indicates that results are driven by a part of the distribution and that not all students are positively affected by mobile phone bans. Table 3.7 also investigates whether mobile phone bans have different effects on high-achieving students than they do on low-achieving students (columns (5), (6) and (7)), using an interaction term between mobile phone bans and test scores at age 11. The interaction term is negative and significant, which indicates that the ban is more effective for low-achieving students.

Table 3.8 studies in more detail the impact of the ban on students with different achievement levels at age 11. Students are grouped into five categories based on their achievement level at age 11, where group 1 means lowest achievement group and group 5 is the highest achievement group. Tables 3.8 shows that the ban has a positive and significant impact on low-achieving students and no significant impact on high-achieving students.[8] Figure 3.2 shows the density of standardized student test scores before and after a mobile phone ban. It shows that the density of test scores shifts right after the imposition of a ban.

3.6 Robustness

We do several tests to ensure that our results are robust and valid.

One possible concern is the potential for pre-existing trends that could potentially affect the mobile phone ban impact presented in this paper. Figure 3.3 presents the event study graph for student achievement before and after the implementation of a mobile phone ban. It shows no pre-existing trends before the implementation of a ban and an increase in achievement after schools ban mobile phones. The figure provides confidence that pre-existing trends are not a concern.

Another potential concern is that students might sort into schools based on the mobile phone policy in place. We can test for this by using student characteristics as outcome variables and investigate if the variable Mobile Ban is significant. Table 3.10 shows that the variable Mobile Ban is never significant for test scores at age 11, males, minorities, SEN students and FSM students. This suggests that students are not sorting into schools based on the mobile phone policy in place.

We also consider the period post-2005 differently. During that period,

[8] Results using achievement at age 14 instead of age 11 are similar

smartphones became very popular and mobile phones became more of an issue for schools. Results of Table 3.11 once again show a positive impact of banning mobile phones on the period 2005 to 2011.

Another related test is to only use students in schools that imposed a ban between Key Stage 3 (with a test at age 14) and Key Stage 4 (with a test at age 16), for which we have fewer observations but for which sorting is not an issue. Tables 3.12 and 3.13 show very similar results to our main findings. This provides confidence that the increase in student performance after the implementation of a mobile phone ban is indeed caused by the ban.

The National Pupil Database contains several measures of student attainment. We use several outcome measures of student performance, such as GCSE and an alternate point-scoring system that reflects the differences in difficulty in attaining certain grades, in addition to the standardized score of a student at Key Stage 4. Results presented in Tables 3.14, 3.15, 3.16 and 3.17 are very similar to our main findings. Moreover, using standardized test scores at age 14 as the outcome variable once again leads to similar results to using test scores at age 16, though the coefficients are smaller and not all specifications are significant. Results are available in Table 3.18.

Overall, results are very robust to alternative specifications and a rich set of time-varying state characteristics. These numerous robustness checks provide confidence that mobile phone bans play a role in determining school and student performance.

3.7 Conclusion

We estimate the effect of mobile phone bans on student performance. We combine survey data on mobile phone policies for schools in four cities with administrative data from the U.K. school performance table and the National Pupil Database. We use differences in mobile phone policies and differences in the timing of the implementation of the policy to measure the impact on student performance, first at the school level, then at the student level.

Our results indicate that there is an increase in school and student performance after introducing a mobile phone ban. We find that banning mobile phones improves the outcomes of the low-achieving students the most and has no significant impact on high achievers. The results suggest that low-achieving students are more likely to be distracted by the presence of mobile phones, while high achievers can focus in the classroom regardless of the mobile phone policy. Our results suggest that mobile phones create a distraction from learning and that introducing a ban limits this problem. Our results also suggest than banning mobile phones can reduce educational inequality.

While technology improves the standard of living for many, our findings suggest at least one negative impact of technology on productivity. Further research should be done to better understand the potential negative impact of new technology on productivity in the workplace and in schools.

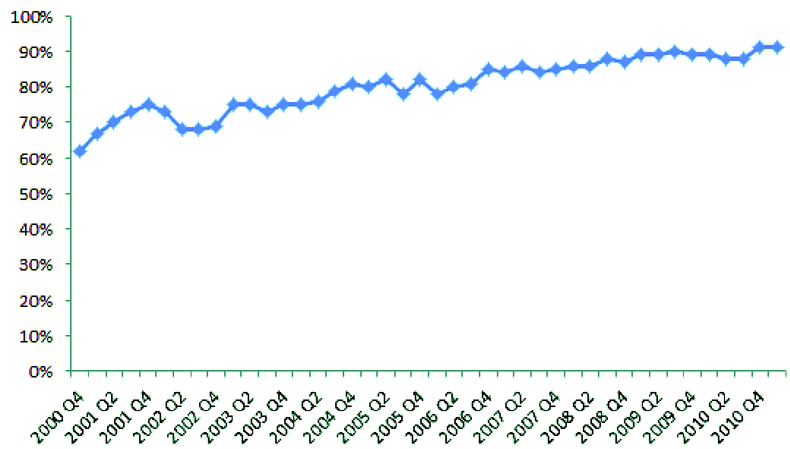

Figure 3.1: Mobile Phones Take-up Rates in England
Source: Oftel/ Ofcom, Based on face to face survey data, 2011

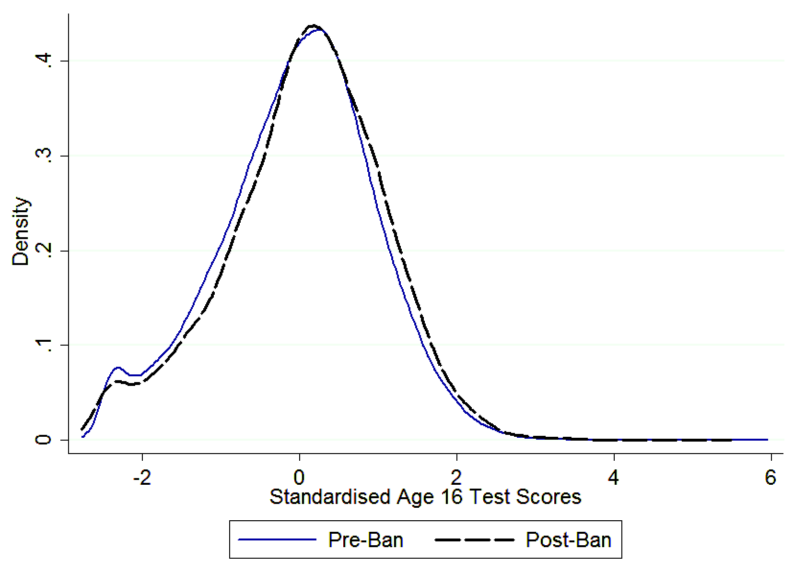

Figure 3.2: KS4 density pre and post ban
Source: National Pupil database

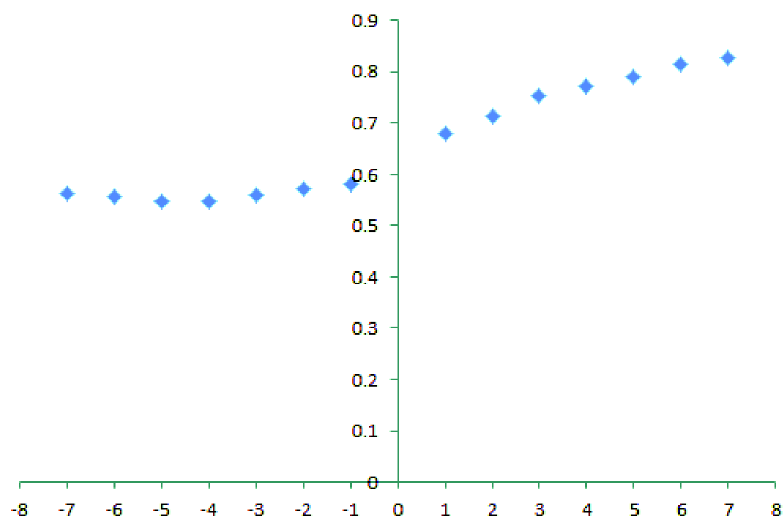

Figure 3.3: Event study graph for student proficiency and mobile ban
Source: UK School Performance Table

Table 3.1: Descriptive statistics on Mobile Phone policies in effect per year

Year	High-compliance Ban	low-compliance Ban	Mobile Ban
2000	0	0	0
2001	0	0	0
2002	2	1	3
2003	5	1	6
2004	7	2	9
2005	13	6	19
2006	20	9	29
2007	31	12	43
2008	38	20	58
2009	47	24	71
2010	54	31	85
2011	55	33	88
2012	56	34	90

Source: Mobile phone policy survey of schools in four cities in England: Birmingham, Leicester, London and Manchester

Table 3.2: Descriptive statistics on key variables Pre and Post policy

Student Characteristics	Sampled	Pre-Policy	Post-Policy	Difference
Test Scores - Age 16 (Kk4tot)	0.00	-0.03	0.03	0.06
	(1.00)	(1.00)	(1.00)	(0.04)
Test Scores - Age 11 (Kk2tot)	0.00	-0.01	0.01	0.01
	(1.00)	(1.00)	(1.00)	(0.04)
Male	0.47	0.47	0.46	0.00
	(0.50)	(0.50)	(0.50)	(0.02)
Minority	0.59	0.55	0.63	0.08
	(0.49)	(0.50)	(0.48)	(0.03)
SEN	0.18	0.15	0.21	0.05
	(0.39)	(0.36)	(0.40)	(0.01)
FSM	0.31	0.28	0.35	0.07
	(0.46)	(0.45)	(0.48)	(0.02)
Total Students	130,659	62,273	65,097	
Total School * Years	817	404	394	

Table 3.2 presents descriptive statistics on key variable Pre and Post policy. SEN means Special Educational needs student and FSM means Free School Meal students.
Source: National Pupil database (NPD) and mobile phone policy from survey.

Table 3.3: The Effect of Mobile Phone ban on school performance - using UK School Performance Table

School Performance- GCSE	(1)	(2)	(3)	(4)
Mobile Ban	4.48***	4.42***	3.65***	3.64***
	(1.24)	(1.22)	(1.16)	(1.14)
School characteristics				
Time invariant	yes	no	no	no
Time variant	yes	no	yes	yes
Year fixed effect	yes	yes	yes	yes
School fixed effect	no	yes	yes	yes
Leadership changes	no	no	no	yes
Schools	90	90	90	90
Observations	907	907	907	907

Note: Table 3.3 presents regression estimates for proportion of student who pass the GCSE test. We use robust clustered standard error at the school level. The time variant controls are number of pupils enrolled at school and proportion of KS4 pupils with Special Educational Needs (SEN). Time invariant controls are dummy for city and type of schools (boys, girls or mixed). The leadership changes variable control if there was a leadership or policy changes occuring at the time of the introduction of the policy. *** $p<0.01$, ** $p<0.05$, * $p<0.1$
Source: UK Performance Table and Mobile phone policy from survey.

Table 3.4: The Effect of Mobile Phone ban on school performance by ban efficiency - using UK School Performance Table

School Performance- GCSE	(1)	(2)	(3)	(4)
High-compliance Mobile Ban	3.95***	4.94***	3.95***	3.88***
	(1.54)	(1.62)	(1.54)	(1.50)
Low-compliance Mobile Ban	1.83	2.37	1.83	1.82
	(1.82)	(2.06)	(1.82)	(1.81)
School characteristics				
Time invariant	yes	no	no	no
Time variant	yes	no	yes	yes
Year fixed effect	yes	yes	yes	yes
School fixed effect	no	yes	yes	yes
Leadership changes	no	no	no	yes
Schools	90	90	90	90
Observations	907	907	907	907

Note: Table 3.4 presents regression estimates for proportion of student who pass the GCSE test. It separates ban into high-compliance (principal assesment score above or eqal to 4 out of 7) and low-compliance mobile ban. We use robust clustered standard error at the school level. The time variant controls are number of pupils enrolled at school and proportion of KS4 pupils with Special Educational Needs (SEN). Time invariant controls are dummy for city and type of schools (boys, girls or mixed). The leadership changes variable control if there was a leadership or policy changes occuring at the time of the introduction of the policy. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Source: UK Performance Table and Mobile phone policy from survey.

Table 3.5: The Effect of Mobile Policy on student performance - Using the NPD

Std Student Performance - age 16	(1)	(2)	(3)	(4)	(5)
Mobile Ban	5.93**	6.35**	6.70**	6.09**	5.06*
	(2.91)	(2.91)	(2.94)	(2.92)	(2.86)
School & student characteristics					
Kk2tot	no	yes	yes	yes	yes
Male	no	no	yes	yes	yes
Minority	no	no	yes	yes	yes
SEN	no	no	yes	yes	yes
FSM	no	no	yes	yes	yes
Leadership changes	no	no	no	yes	yes
School characteristics	no	no	no	no	yes
Observations	130,595	130,595	130,595	130,595	130,595

Note:Table 3.5 presents regression estimates for student performance. Outcome variable is standardized test score at age 16. We use robust clustered standard error at the school level with school and year fixed effect. SEN means Special Educational Needs student and FSM means Free School Meal students. Test Score- age 11 is standardized student test score at age 11 (before high school). *** $p<0.01$, ** $p<0.05$, * $p<0.1$
Source: National Pupil database (NPD) and mobile phone policy from survey.

Table 3.6: The Effect of Mobile Policy on student performance by ban efficiency - Using the NPD

Std Student Performance - age 16	(1)	(2)	(3)	(4)	(5)
High-compliance Mobile Ban	6.42**	6.81**	7.12**	7.45**	6.60**
	(3.00)	(2.99)	(3.03)	(3.04)	(2.96)
Low-compliance Mobile Ban	2.12	2.67	3.42	5.87	8.60
	(6.58)	(6.53)	(6.56)	(5.29)	(5.40)
School & student characteristics					
Kk2tot	no	yes	yes	yes	yes
Male	no	no	yes	yes	yes
Minority	no	no	yes	yes	yes
SEN	no	no	yes	yes	yes
FSM	no	no	yes	yes	yes
Leadership changes	no	no	no	yes	yes
School characteristics	no	no	no	no	yes
Observations	130,659	130,659	130,659	130,659	130,659

Note:Table 3.6 presents regression estimates for student performance. It separates ban into high-compliance (principal assesment score above or eqal to 4 out of 7) and low-compliance mobile ban. Outcome variable is standardized test score at age 16. We use robust clustered standard error at the school level with school and year fixed effect. SEN means Special Educational Needs student and FSM means Free School Meal students. Kk2tot is standardized student test score at age 11 (before high school). *** p<0.01, ** p<0.05, * p<0.1
Source: National Pupil database (NPD) and mobile phone policy from survey.

Table 3.7: The Effect of Mobile Policy on student performance by student characteristics - Using the NPD

Std Student Performance - age 16	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mobile Ban	3.98	4.26	4.17	8.96***	5.87**	4.49	3.61
	(2.98)	(2.99)	(2.95)	(3.43)	(2.91)	(2.96)	(3.02)
Mobile Ban * FSM	7.37***					4.88**	4.63**
	(2.02)					(1.94)	(1.93)
Mobile Ban * SEN		10.89***					5.85**
		(2.36)					(2.39)
Mobile Ban * male			4.13**				
			(2.10)				
Mobile Ban * minority				-4.90*			
				(2.63)			
Mobile Ban * Kk2tot					-5.89***	-5.42***	-4.65***
					(1.05)	(1.02)	(1.06)
School & student characteristics							
Kk2tot	yes	yes	yes	yes	yes	yes	yes
Male	yes	yes	yes	yes	yes	yes	yes
SEN	yes	yes	yes	yes	yes	yes	yes
FSM	yes	yes	yes	yes	yes	yes	yes
minority	yes	yes	yes	yes	yes	yes	yes
Leadership changes	yes	yes	yes	yes	yes	yes	yes
Observations	130,595	130,595	130,595	130,595	130,595	130,595	130,595

Note: Table 3.7 presents regression estimates for student performance. Outcome variable is standardized test score in student 8 best subject. We use robust clustered standard error at the school level with school and year fixed effect. SEN means Special Educational Needs student and FSM means Free School Meal students. Kk2tot represents standardized test score at age 11. *** p<0.01, ** p<0.05, * p<0.1
Source: National Pupil database (NPD) and mobile phone policy from survey.

Table 3.8: The Effect of Mobile Policy on student performance with preachievement group - Using the NPD

Std Student Performance - age 16	(1)	(2)	(3)	(4)
Mobile Ban * Kk2tot - 1	13.29***	14.40***	13.68***	12.36***
	(3.11)	(3.14)	(3.13)	(3.05)
Mobile Ban * Kk2tot - 2	8.59***	9.94***	9.30***	8.02***
	(3.07)	(3.11)	(3.08)	(3.05)
Mobile Ban * Kk2tot - 3	5.78*	6.72**	6.22**	4.97
	(3.12)	(3.15)	(3.13)	(3.07)
Mobile Ban * Kk2tot - 4	2.98	2.68	2.14	1.06
	(3.15)	(3.18)	(3.16)	(3.11)
Mobile Ban * Kk2tot - 5	-0.82	-1.97	-2.63	-2.70
	(3.42)	(3.47)	(3.49)	(3.38)
School & student characteristics				
Kk2tot categorical	yes	yes	yes	yes
Male	no	yes	yes	yes
Minority	no	yes	yes	yes
SEN	no	yes	yes	yes
FSM	no	yes	yes	yes
Leadership changes	no	no	yes	yes
School characteristics	no	no	no	yes
Observations	130,595	130,595	130,595	130,595

Note: Table 3.8 presents regression estimates for student performance. Outcome variable is standardized test score in student 8 best subject. We use robust clustered standard error at the school level with school and year fixed effect. SEN means Special Educational Needs student and FSM means Free School Meal students. Kk2tot represents standardized test score at age 11. In this table, student performance at age 11 (Kk2tot) are grouped in 5 category based on their achievement level at age 11, where group 1 means lowest achivement group and group 5 are highest achievement group. *** p<0.01, ** p<0.05, * p<0.1
Source: National Pupil database (NPD) and mobile phone policy from survey.

Table 3.9: Descriptive statistics for key variables for schools in sample and not in sample

Variable	In sample	Not in sample
Total Pupil	971.34	938.38
	(9.14)	(24.02)
Proportion SEN students	9.75	7.92
	(0.32)	(0.13)
School Performance - GCSE	59.51	60.34
	(0.65)	(0.33)

Note: Table 3.9 Descriptive statistics. Schools in our sample are similar to schools not in sample for those variables: Total Pupil, Proportion of Specifal Educational Needs (SEN) students, Student Performance using the GCSE measure. Using t-test, only the proportion of SEN students is significantly different between the two samples.
Source: UK Performance Table and mobile phone policy from survey.

Table 3.10: Balancing test- Using the NPD

Variables	Kk2tot	Male	Minority	SEN	FSM
Mobile Ban	-0.74	-0.39	0.01	0.81	1.07
	(1.25)	(0.44)	(0.72)	(1.00)	(0.70)

Note: Table 3.10 presents regression estimates for different outcome variables to investigates if schools that impose a ban are different and if students are sorting into schools based on student characteristics. SEN means fraction of Special Educational Needs student, FSM means fraction Free School Meal students. Kk2tot means standardized average test score at age 11 of the school. Male and Minority are fraction of students that are male and from a minority group respectively. We use robust clustered standard error at the school level with school and year fixed effect. *** $p<0.01$, ** $p<0.05$, * $p<0.1$
Source: National Pupil database (NPD) and mobile phone policy from survey.

Table 3.11: The Effect of Mobile Phone ban assessment on student performance after 2005 - using UK School Performance Table

School Performance- GCSE	(1)	(2)	(3)	(4)
Mobile Ban	3.84***	3.67**	3.19**	3.18**
	(1.44)	(1.49)	(1.48)	(1.48)
School characteristics				
Time invariant	yes	no	no	no
Time variant	yes	no	yes	yes
Year fixed effect	yes	yes	yes	yes
School fixed effect	no	yes	yes	yes
Leadership changes	no	no	no	yes
Schools	90	90	90	90
Observations	585	585	585	585

Note: Table 3.11 presents regression estimates for proportion of student who pass the GCSE test from 2005 to 2011. We use robust clustered standard error at the school level. The time variant controls are number of pupils enrolled at school and proportion of KS4 pupils with Special Educational Needs (SEN). Time invariant controls are dummy for city and type of schools (boys, girls or mixed). The leadership changes variable control if there was a leadership or policy changes occuring at the time of the introduction of the policy. *** $p<0.01$, ** $p<0.05$, * $p<0.1$
Source: UK Performance Table and Mobile phone policy from survey.

Table 3.12: The Effect of Mobile Policy on student performance - Using the NPD

Std Student Performance - age 16	(1)	(2)	(3)	(4)	(5)
Mobile Ban	6.86**	5.51**	6.14**	6.38**	5.30**
	(2.71)	(2.59)	(2.60)	(2.64)	(2.67)
School & student characteristics					
Kk3tot	no	yes	yes	yes	yes
Male	no	no	yes	yes	yes
Minority	no	no	yes	yes	yes
SEN	no	no	yes	yes	yes
FSM	no	no	yes	yes	yes
Leadership changes	no	no	no	yes	yes
School characteristics	no	no	no	no	yes
Observations	83,211	83,211	83,211	83,211	83,211

Note: Table 3.12 presents regression estimates for student performance. Outcome variable is standardized test score at age 16 and control for standardized test score at age 14. We use robust clustered standard error at the school level with school and year fixed effect. SEN means Special Educational Needs student and FSM means Free School Meal students. Kk3tot means standardized test score at age 14. *** p<0.01, ** p<0.05, * p<0.1
Source: National Pupil database (NPD) and mobile phone policy from survey.

Table 3.13: The Effect of Mobile Policy on student performance with preachievement group - Using the NPD

Std Student Performance - age 16	(1)	(2)	(3)	(4)	(5)
Mobile Ban * Kk3tot - 1	10.43***	11.21***	11.39***	11.39***	10.06***
	(3.05)	(3.02)	(3.04)	(3.04)	(3.03)
Mobile Ban * Kk3tot - 2	9.28***	10.58***	10.79***	10.79***	9.64***
	(3.11)	(3.08)	(3.12)	(3.12)	(3.16)
Mobile Ban * Kk3tot - 3	5.53*	6.21**	6.44**	6.44**	5.20*
	(3.05)	(3.08)	(3.13)	(3.13)	(3.13)
Mobile Ban * Kk3tot - 4	2.49	2.67	2.91	2.91	1.75
	(3.09)	(3.09)	(3.13)	(3.13)	(3.14)
Mobile Ban * Kk3tot - 5	-0.22	0.39	0.68	0.69	-0.07
	(3.42)	(3.47)	(3.49)	(3.49)	(3.49)
School & student characteristics					
Kk3tot categorical	yes	yes	yes	yes	yes
Male	no	yes	yes	yes	yes
Minority	no	yes	yes	yes	yes
SEN	no	yes	yes	yes	yes
FSM	no	yes	yes	yes	yes
Leadership changes	no	no	yes	yes	yes
School characteristics	no	no	no	yes	yes
Observations	83,211	83,211	83,211	83,211	83,211

Note: Table 3.13 presents regression estimates for student performance. Outcome variable is standardized test score. We use robust clustered standard error at the school level with school and year fixed effect. SEN means Special Educational Needs student and FSM means Free School Meal students. Kk3tot represents test score at age 14. In this table, results are grouped in 5 category based on their achievement level (Kk3tot) at age 14, where group 1 means lowest achivement group and group 5 are highest achievement group. *** p<0.01, ** p<0.05, * p<0.1 Source: National Pupil database (NPD) and mobile phone policy from survey.

Table 3.14: The Effect of Mobile Policy on student performance - Using the NPD

Std Student Performance - age 16 - alt	(1)	(2)	(3)	(4)	(5)
Mobile Ban	5.57**	6.01**	6.33**	5.77**	4.81*
	(2.69)	(2.68)	(2.71)	(2.69)	(2.63)
School & student characteristics					
Kk2tot	no	yes	yes	yes	yes
Male	no	no	yes	yes	yes
Minority	no	no	yes	yes	yes
SEN	no	no	yes	yes	yes
FSM	no	no	yes	yes	yes
Leadership changes	no	no	no	yes	yes
School characteristics	no	no	no	no	yes
Observations	130,659	130,659	130,659	130,659	130,659

Note:Table 3.14 presents regression estimates for student performance. Outcome variable is standardized test score at age 16. We use robust clustered standard error at the school level with school and year fixed effect. SEN means Special Educational Needs student and FSM means Free School Meal students. Kk2tot means test score at age 11. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Source: National Pupil database (NPD) and mobile phone policy from survey.

Table 3.15: The Effect of Mobile Policy on student performance by student characteristics - Using the NPD

Std Student Performance - age 16 - alt	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mobile Ban	3.82	4.40	4.20	8.33***	5.58**	4.27	3.75
	(2.74)	(2.76)	(2.74)	(3.11)	(2.68)	(2.72)	(2.78)
Mobile Ban * FSM	6.80***					4.61***	4.46**
	(1.86)					(1.78)	(1.78)
Mobile Ban * SEN		8.13***					3.44
		(2.10)					(2.15)
Mobile Ban * male			3.37*				
			(1.95)				
Mobile Ban * minority				-4.37*			
				(2.43)			
Mobile Ban * Kk2tot					-5.21***	-4.76***	-4.31***
					(0.99)	(0.96)	(1.01)
School & student characteristics							
Kk2tot	yes	yes	yes	yes	yes	yes	yes
Male	yes	yes	yes	yes	yes	yes	yes
SEN	yes	yes	yes	yes	yes	yes	yes
FSM	yes	yes	yes	yes	yes	yes	yes
minority	yes	yes	yes	yes	yes	yes	yes
Leadership changes	yes	yes	yes	yes	yes	yes	yes
Observations	130,659	130,659	130,659	130,659	130,659	130,659	130,659

Note: Table 3.15 presents regression estimates for student performance. Outcome variable is standardized test at age 16. We use robust clustered standard error at the school level with school and year fixed effect. SEN means Special Educational Needs student and FSM means Free School Meal students. Kk2tot means standardized test score at age 11. *** p<0.01, ** p<0.05, * p<0.1
Source: National Pupil database (NPD) and mobile phone policy from survey.

Table 3.16: The Effect of Mobile Policy on student performance with preachievement group - Using the NPD

Std Student Performance - age 16 - alt	(1)	(2)	(3)	(4)
Mobile Ban * Kk2tot - 1	11.05***	12.11***	11.44***	10.22***
	(2.85)	(2.88)	(2.86)	(2.79)
Mobile Ban * Kk2tot - 2	9.03***	10.30***	9.72***	8.53***
	(2.84)	(2.87)	(2.84)	(2.81)
Mobile Ban * Kk2tot - 3	6.00**	6.89**	6.44**	5.27*
	(2.90)	(2.92)	(2.91)	(2.86)
Mobile Ban * Kk2tot - 4	2.86	2.56	2.06	1.05
	(2.94)	(2.96)	(2.94)	(2.90)
Mobile Ban * Kk2tot - 5	-0.78	-1.87	-2.47	-2.57
	(3.21)	(3.25)	(3.27)	(3.18)
School & student characteristics				
Kk2tot categorical	yes	yes	yes	yes
Male	no	yes	yes	yes
Minority	no	yes	yes	yes
SEN	no	yes	yes	yes
FSM	no	yes	yes	yes
Leadership changes	no	no	yes	yes
School characteristics	no	no	no	yes
Observations	130,659	130,659	130,659	130,659

Note: Table 3.16 presents regression estimates for student performance. Outcome variable is standardized test score in student 8 best subject. We use robust clustered standard error at the school level with school and year fixed effect. SEN means Special Educational Needs student and FSM means Free School Meal students. Kk2tot represents test score at age 11. In this table, results are grouped in 5 category based on their achievement level at age 11, where group 1 means lowest achivement group and group 5 are highest achievement group. *** p<0.01, ** p<0.05, * p<0.1 Source: National Pupil database (NPD) and mobile phone policy from survey.

Table 3.17: The Effect of Mobile Policy on student performance GCSE-EM - Using the NPD

Student Performance age 16 - GCSE - EM	(1)	(2)	(3)	(4)
Mobile Ban	1.98**	2.24**	2.23**	1.92**
	(0.93)	(0.91)	(0.92)	(0.89)
School & student characteristics				
Kk2tot	no	yes	yes	yes
Male	no	no	yes	yes
SEN	no	no	yes	yes
FSM	no	no	yes	yes
Leadership changes	no	no	yes	yes
School characteristics	no	no	no	yes
Observations	130,659	130,659	130,659	130,659

Note: Table 3.17 presents regression estimates for student performance. Outcome variable is passing GCSE - EM. We use robust clustered standard error at the school level with school and year fixed effect. SEN means Special Educational Needs student and FSM means Free School Meal students. Kk2tot means test score at age 11. *** $p<0.01$, ** $p<0.05$, * $p<0.1$
Source: National Pupil database (NPD) and mobile phone policy from survey.

Table 3.18: The Effect of Mobile Policy on student performance at age 14 - Using the NPD

Std Student Performance - age 14	(1)	(2)	(3)	(4)	(5)
Mobile Ban	0.99	1.59	2.53*	2.98*	2.34
	(1.77)	(1.49)	(1.50)	(1.54)	(1.53)
School & student characteristics					
Kk2tot	no	yes	yes	yes	yes
Male	no	no	yes	yes	yes
Minority	no	no	yes	yes	yes
SEN	no	no	yes	yes	yes
FSM	no	no	yes	yes	yes
Leadership changes	no	no	no	yes	yes
School characteristics	no	no	no	no	yes
Observations	112,339	112,339	112,339	112,339	112,339

Note:Table 3.18 presents regression estimates for student performance at age 14. Outcome variable is standardized test score at age 14. We use robust clustered standard error at the school level with school and year fixed effect. SEN means Special Educational Needs student and FSM means Free School Meal students. Kk2tot means standardized test score at age 11. *** $p<0.01$, ** $p<0.05$, * $p<0.1$
Source: National Pupil database (NPD) and mobile phone policy from survey.

General Conclusion

In this thesis, I answer three important questions in public and labor economics. In Chapter 1, I investigate whether party allegiance (Democrat vs. Republican) of governors has an impact on the labor market outcomes. I document a strong impact from Democrat governors, especially for black workers. They work more and more hours under Democratic administrations and there is a decrease in the black-white wage gap. In Chapter 2, I analyze how shootings in high schools affect schools and students. Chapter 2 shows that homicidal shootings significantly decrease the enrollment of students in Grade 9, and substantially decrease test scores in math and English. It also demonstrates the lack of a statistically significant effect from suicidal shootings on all outcome variables of interest. Using student-level data from California, we confirm that part of the effects on student performance occur as a result of students remaining enrolled and not solely through a composition effect. My third chapter investigates the impact of school mobile phone policy on student performance. Combining a unique dataset on autonomous mobile phone policies from a survey of schools in four cities in England with administrative data, I investigate the impact of introducing a mobile phone ban on student performance. The results indicate that there is an increase in student performance after a school bans the use of mobile phones. This suggests that mobile phones cause distraction from learning and introducing a ban limits this problem.

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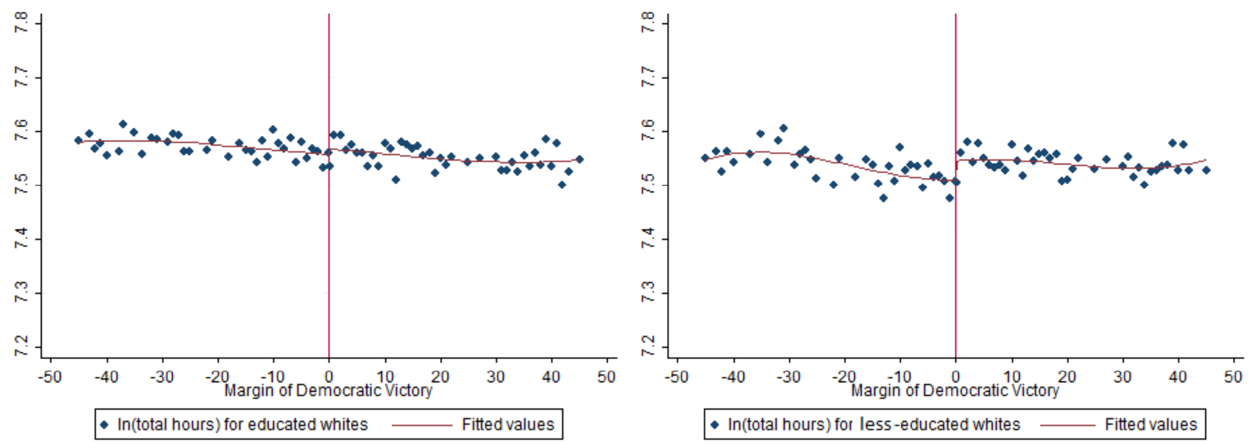
Appendix

Appendix A

Figures based on different covariates

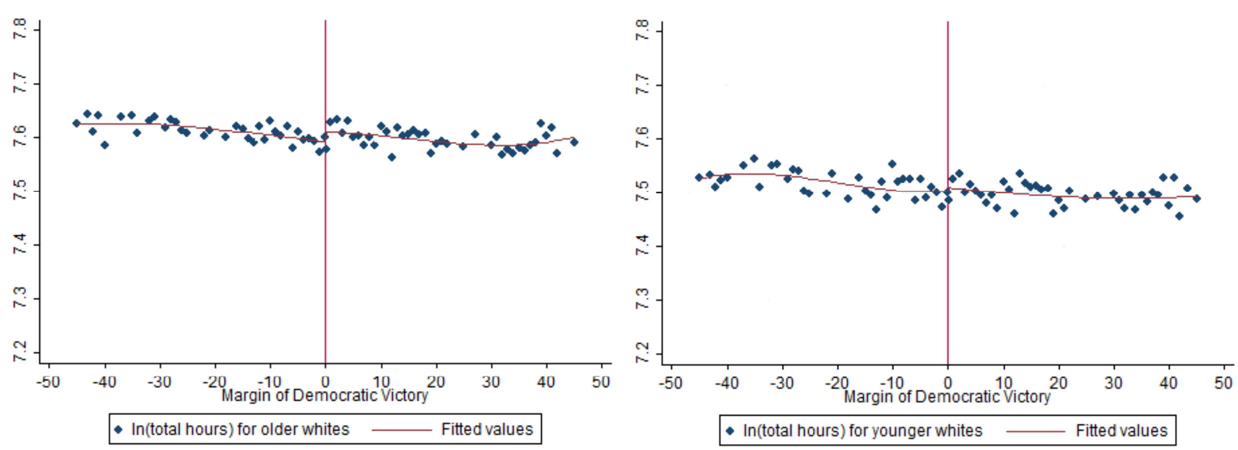
A.1 White

A.1.1 Whites: Educated vs. less-educated for total hours



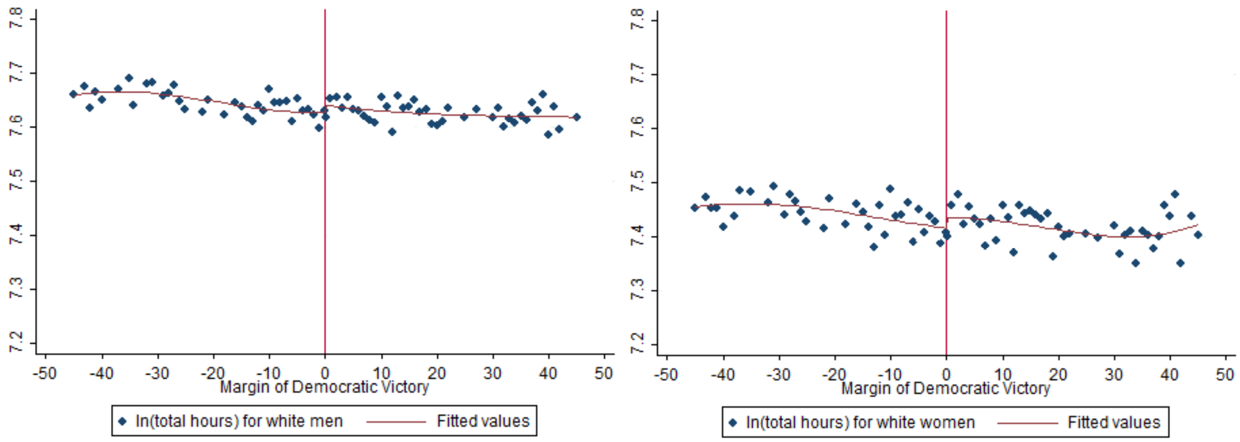
In Figure A.1.1, educated workers are defined as having some college, a college diploma or more.

A.1.2 Whites: Older vs. younger for total hours

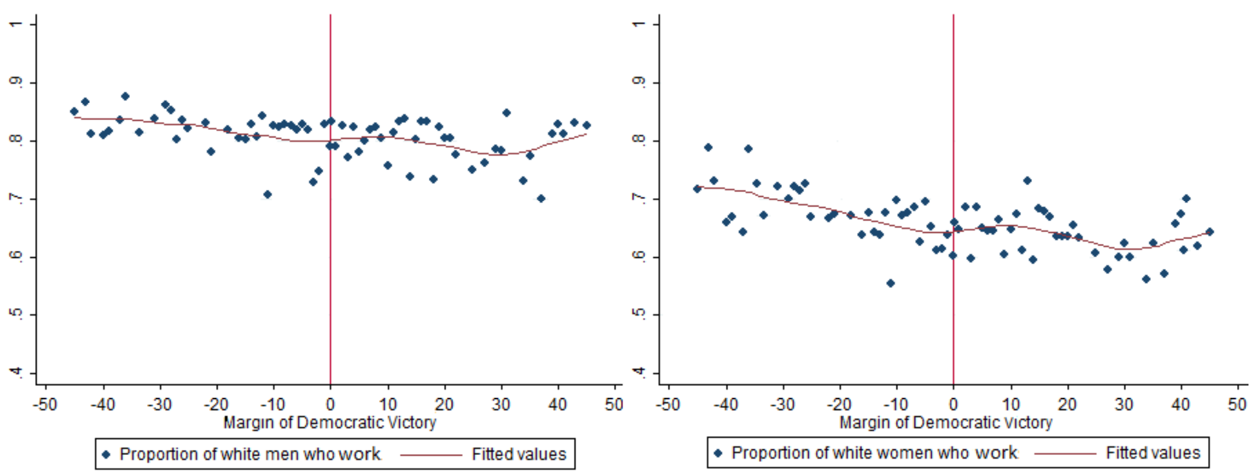


In Figure A.1.2, older workers are defined as being 40 or older.

A.1.3 Whites: Men vs. Women for total hours

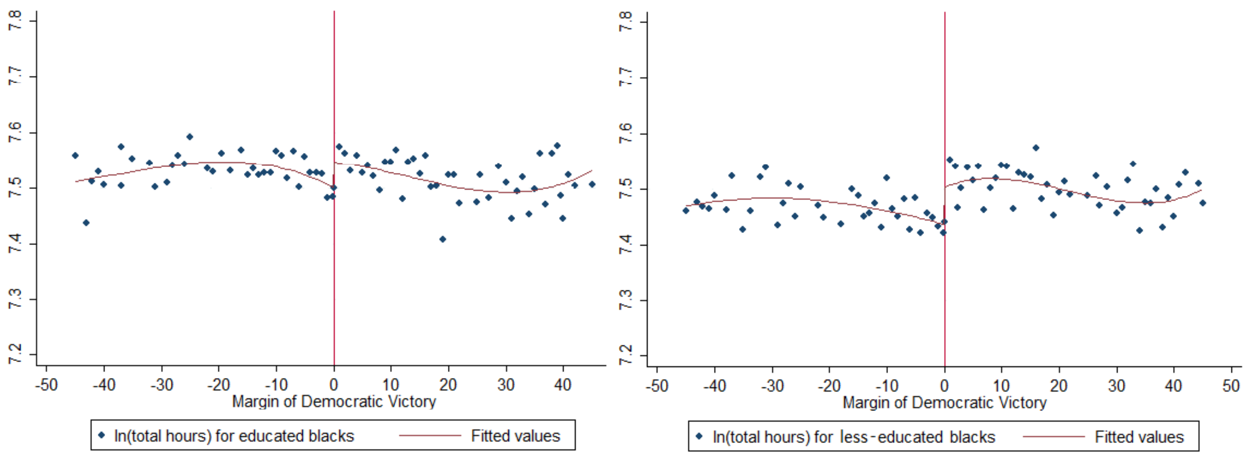


A.1.4 Whites: Men vs. women for employment



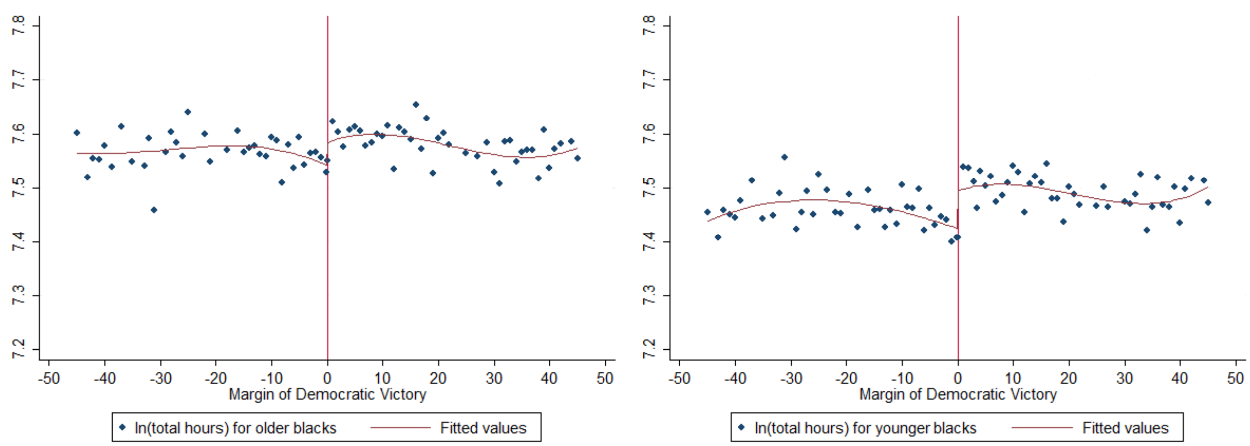
A.2 Blacks

A.2.1 Blacks: Educated vs. less-educated for total hours



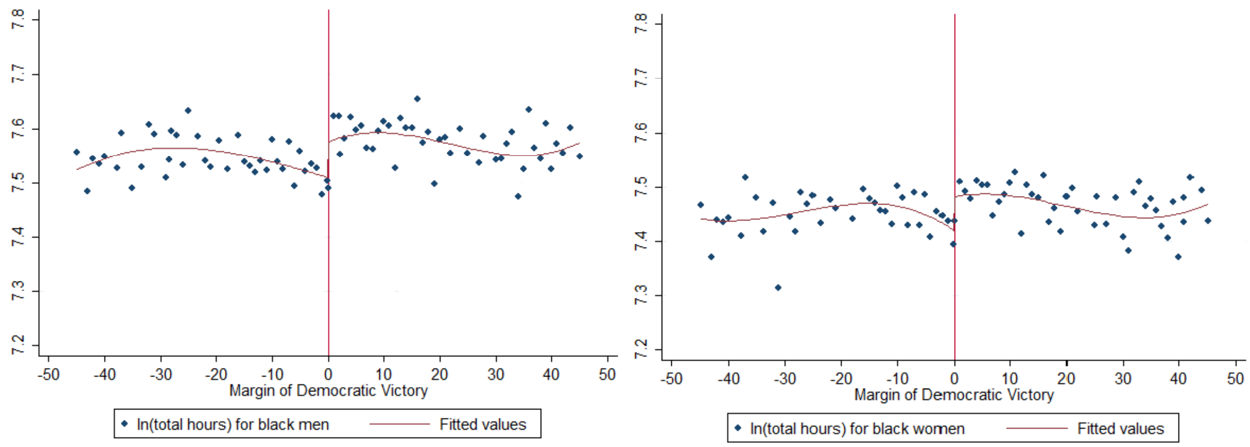
In Figure A.2.1, educated workers are defined as having some college, a college diploma or more.

A.2.2 Blacks: Older vs. younger for total hours

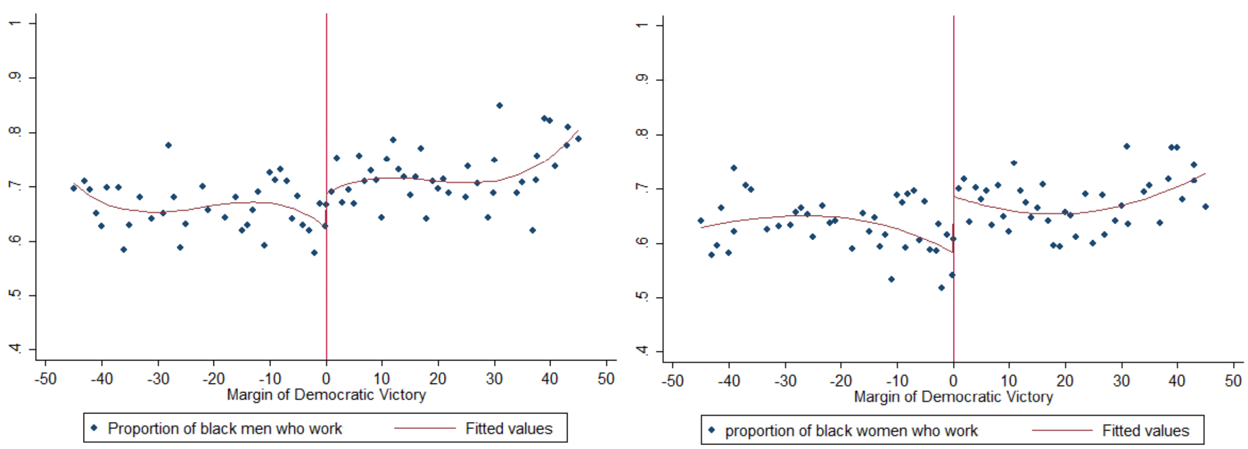


In Figure A.2.2, older workers are defined as being 40 or older.

A.2.3 Blacks: Men vs. women for total hours



A.2.4 Blacks: Men vs. women for employment



RD for overall impact on outcomes (white and black workers combined)

Table A.1 – RD for overall impact on outcomes

Outcomes		All	Men	Women
Annual earnings	Democrat	-2.11*	-2.71**	-1.32
		(1.12)	(1.28)	(1.40)
Total hours	Democrat	-0.36	-0.88	0.28
		(0.65)	(0.89)	(0.78)
Weeks worked	Democrat	0.08	-0.37	0.64
		(0.47)	(0.62)	(0.57)
Employed	Democrat	-0.51	-0.31	-0.65
		(0.49)	(0.58)	(0.59)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. Estimates are generated using specification (1) without the $F_b(MV_{st}) \times Black_{ist}$ interactions. It presents the impact of Democratic governors on black and white workers. Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Source: March CPS data.*

Total impact of Democratic governors (β1 + β2) on blacks

Table A.2 - Total impact for blacks (β1 + β2)

Outcomes		All	Men	Women
Annual earnings	Democrat	2.62	2.88	1.12
		(2.68)	(3.42)	(3.19)
Weekly earnings	Democrat	-0.13	0.79	-1.97
		(2.07)	(2.60)	(2.55)
Hourly wages	Democrat	-0.53	-0.35	-1.34
		(1.81)	(2.22)	(2.00)
Total hours	Democrat	3.15**	3.34*	2.41
		(1.46)	(2.05)	(2.03)
Weeks worked	Democrat	2.76**	2.20*	3.03**
		(1.23)	(1.12)	(1.43)
Usual hours	Democrat	0.39	1.14	-0.62
		(0.66)	(0.81)	(1.13)
In labor force	Democrat	1.79*	1.12	2.58**
		(1.03)	(1.45)	(1.29)
Employed	Democrat	1.82*	1.10	2.71**
		(1.11)	(1.61)	(1.28)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. It presents the total impact of Democratic governors (β1 + β2) on black workers for regressions of Tables 2, 3 and 4 (not relative to white workers). Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Source: March CPS data.*

Summary of Estimated Labor Market Impacts (from Tables 2-3-4, A.1 and A.2)

Table A.3 - Summary of Estimated Labor Market Impacts (RD Estimates)

	All Workers	Main Effect	Black Interaction	Blacks Only
Employment rate	-0.51	-0.77	2.59*	1.82*
	(0.49)	(0.54)	(1.34)	(1.11)
Weeks worked	0.08	-0.15	2.91**	2.76**
	(0.47)	(0.45)	(1.18)	(1.23)
Total Hours	-0.36	-0.63	3.79**	3.15**
	(0.65)	(0.68)	(1.61)	(1.46)
Annual Earnings	-2.11*	-2.42*	5.03**	2.62
	(1.12)	(1.23)	(2.57)	(2.68)
Annual Earnings -- adjusted for composition effects	-0.39			
	(0.53)			

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. It presents the summary of Estimated Labor Market Impacts (RD Estimates). Columns 1 and 4 are results from Table A.1 and A.2, respectively. Last row of Column 1 is from Table 8. Columns 2 and 3 are results from Table 2, 3 and 4. Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Source: March CPS data.*

Appendix B: Validity of the RD Design

Table B.1 - Testing for a discontinuity in baseline covariates

Variables	Different party at T-1	Campaign Spending % Dem	House Democrat	Senate Democrat
Democrat	-8.27	2.76	-9.53	1.91
	(8.83)	(3.69)	(7.22)	(7.64)

*Table B.1 investigates the validity of the RD design. Outcome variables are different party at T-1, fraction of campaign spending by the Democratic candidate, whether the state house has a Democratic majority, and whether the state senate has a Democratic majority. *** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Source: ICPSR 7757, Atlas Presidential Elections, Jenson and Beyle (2003) data and UKCPR data.*

Table B.2 – Placebo test: Regression for outcomes at T-1

	Variables	Annual earnings at T-1	Total hours at T-1	Total weeks at T-1	Employed at T-1	In labor force at T-1
White only	Democrat	0.56	1.89	-0.10	-0.39	-0.23
		(1.89)	(0.73)	(0.57)	(0.77)	(0.58)
Black only	Democrat	-12.68	0.33	-3.18	-4.37	7.29
		(13.48)	(4.54)	(3.15)	(5.15)	(5.10)
All	Democrat	0.46	0.21	-0.15	-0.17	-0.25
		(1.94)	(0.74)	(0.58)	(0.82)	(0.46)

*Table B.2 investigates discontinuity in key outcomes variables using outcomes at time T-1. *** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Source: March CPS data.*

Figure B.1: Density of margin of Democratic victory around the threshold

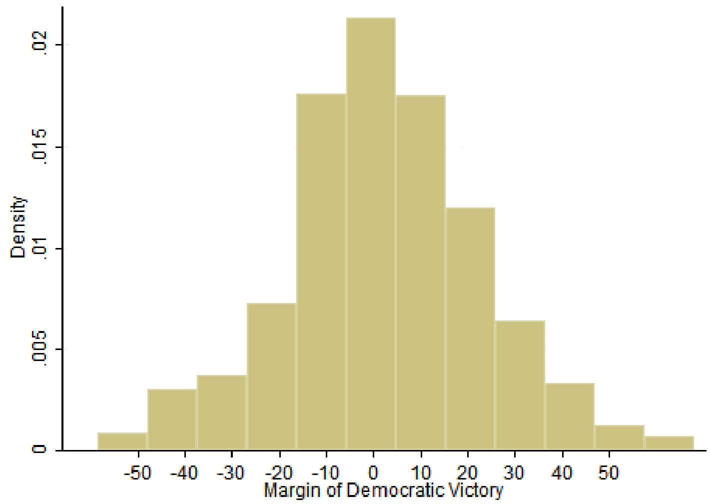
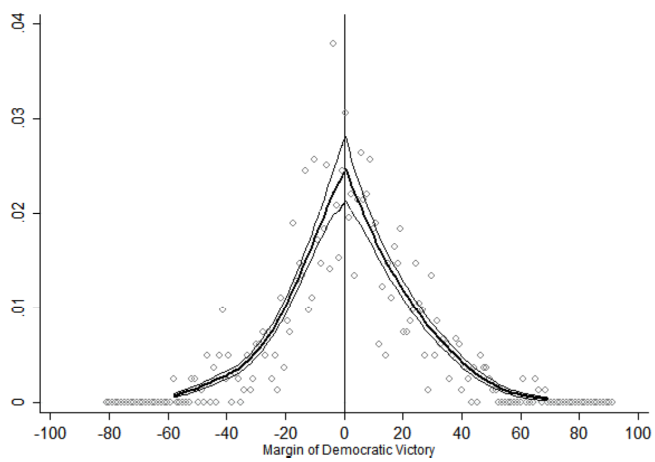
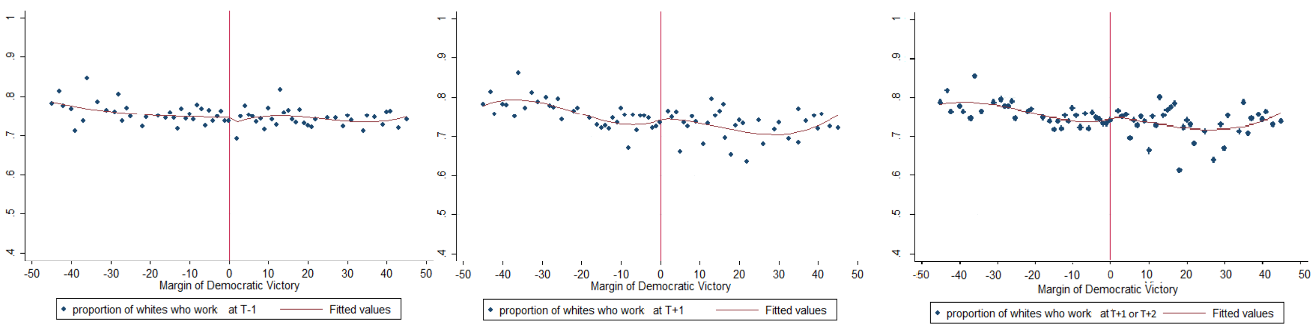


Figure B.2: McCrary Test

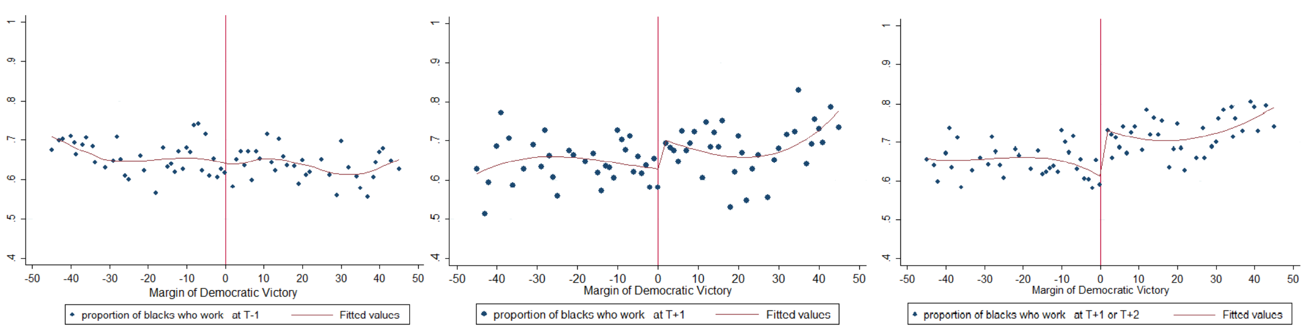


Appendix C: Investigation of discontinuity in employment at T-1

C.1 White



C.2 Black



Appendix D: Different specifications

Table D.1 - RD for Total Hours using different polynomials

Polynomial	1st Order	2nd Order	3rd Order	4rd Order
Democrat	0.18	0.15	-0.63	-0.61
	(0.27)	(0.39)	(0.49)	(0.60)
Democrat x Black	1.89**	2.91***	3.79***	2.44*
	(0.78)	(1.03)	(1.27)	(1.43)
Black	-5.04***	-6.09***	-6.49***	-6.59***
	(0.52)	(0.69)	(0.85)	(0.85)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. The controls are the same as in Table 2. Results are presented for different polynomials (1^{rst}, 2^{nd}, 3^{rd} and 4^{th}). Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Source: March CPS data.*

Table D.2 - RD for weighted and unweighted grouped data at the state-year level

		Variables	White	Black
Weighted	Nonparametric	Democrat	0.64	2.97**
	CCT, 2012		(0.48)	(1.52)
	Parametric	Democrat	0.31	2.60*
			(0.46)	(1.49)
Unweighted	Nonparametric	Democrat	0.57	2.06*
	CCT, 2012		(0.46)	(1.39)
	Parametric	Democrat	0.18	2.42*
			(0.44)	(1.27)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. The table presents regressions without controls for weighted and unweighted grouped data at the state-year level for both parametric and non-parametric estimation. The non-parametric estimation uses Calonico et al. (CCT) (2012). Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Source: March CPS data.*

Table D.3 - Local-linear regressions for total hours

Model		White	Black
Optimal bandwidth, CCT 2012	Democrat	0.64	2.97**
h = 12. 24 (W) & h = 11.41 (B)		(0.48)	(1.52)
	observations	783	745
Optimal bandwidth, IK 2012	Democrat	0.49	4.90*
h = 28.33 (W) & h = 30.12 (B)		(1.02)	(3.62)
	observations	1291	1311
Different bandwidth			
h = 1	Democrat	-0.63	5.09
		(3.21)	(5.36)
	observations	86	86
h = 2	Democrat	-3.88	6.83**
		(2.49)	(3.86)
	observations	141	141
h = 3	Democrat	1.82	6.25**
		(2.86)	(3.27)
	observations	209	208
h = 4	Democrat	1.89	5.52**
		(2.37)	(2.86)
	observations	291	290
h = 5	Democrat	1.88	4.92**
		(2.09)	(2.52)
	observations	342	341
h = 8	Democrat	0.01	3.23**
		(0.70)	(2.08)
	observations	552	547
h = 15	Democrat	0.74	2.30**
		(0.54)	(1.70)
	observations	897	883

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. Results for total hours are presented for different bandwidths, including the optimal bandwidth procedure set out in Calonico et al. (CCT) (2012) and Imbens and Kalyanaraman (2012). Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Source: March CPS data.*

Appendix E: Replication using MORG data for employment and hours in reference week

Table E.1.A - RD for hours last week & being employed (MORG data, State-term Clustering)

Outcomes	Variables	All	Men	Women
Hours last week	Democrat	-0.38	-0.16	-0.70
		(0.58)	(0.64)	(0.70)
	Democrat × Black	1.48	1.95	0.13
		(1.61)	(1.61)	(2.04)
	Black	-2.73**	-7.47***	0.71
		(1.26)	(1.07)	(1.49)
Employed	Democrat	-0.11	-0.12	-0.10
		(0.39)	(0.39)	(0.49)
	Democrat × Black	3.39**	2.74*	3.69**
		(1.52)	(1.54)	(1.77)
	Black	-8.59***	-12.15***	-7.03***
		(1.27)	(1.16)	(1.49)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. The controls are the same as in Table 2 using MORG data. The employed variable is 1 if an individual is employed, and is 0 if the individual is unemployed or out of the labor force. Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Results are clustered at the state-term level. Source: MORG data.*

Table E.1.B – RD for hours last week & being employed (MORG data, State Clustering)

Outcomes	Variables	All	Men	Women
Hours last week	Democrat	-0.38	-0.16	-0.70
		(0.52)	(0.61)	(0.61)
	Democrat × Black	1.48	1.95	0.13
		(1.31)	(1.23)	(1.86)
	Black	-2.73**	-7.47***	0.71
		(1.21)	(0.99)	(1.46)
Employed	Democrat	-0.11	-0.12	-0.10
		(0.41)	(0.39)	(0.53)
	Democrat × Black	3.39*	2.74	3.69**
		(1.72)	(1.88)	(1.84)
	Black	-8.59***	-12.15***	-7.03***
		(1.71)	(1.63)	(1.95)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. The controls are the same as in Table 2 using MORG data. The employed variable is 1 if an individual is employed, and is 0 if the individual is unemployed or out of the labor force. Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Results are clustered at the state level. Source: MORG data.*

Appendix F: Robust to different clustering levels

F1 - Clustering at State-year level

Table F1.1 - RD for hours worked, weeks worked and usual hours

Intensive	Variables	All	Men	Women
	Democrat	-0.63	-1.13**	0.08
Total hours		(0.49)	(0.56)	(0.65)
Worked	Democrat x Black	3.79***	4.47***	2.33
		(1.27)	(1.63)	(1.79)
	Black	-6.49***	-14.00***	-0.91
		(0.85)	(1.06)	(1.18)
	Democrat	-0.15	-0.51	0.35
Weeks		(0.34)	(0.40)	(0.47)
worked	Democrat x Black	2.91***	2.71***	2.68**
		(0.98)	(1.32)	(1.35)
	Black	-6.00	-8.81	-4.13***
		(0.68)	(0.85)	(0.95)
	Democrat	-0.48**	-0.62**	-0.27
Usual		(0.24)	(0.26)	(0.35)
hours	Democrat x Black	0.87	1.76***	-0.35
		(0.54)	(0.66)	(0.91)
	Black	-0.49	-5.19***	3.22***
		(0.34)	(0.46)	(0.60)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. The controls are the same as in Table 2. Outcome variables are expressed in log form and coefficients and standard errors (in parentheses in the table) are multiplied by 100. Results are clustered at the state-year level. Source: March CPS data.*

Table F1.2 - RD for being in labor force and employed

Extensive	Variables	All	Men	Women
In labor force	Democrat	-0.69**	-0.26	-1.08**
		(0.27)	(0.27)	(0.46)
	Democrat x Black	2.48***	1.37	3.65***
		(0.89)	(1.24)	(1.08)
	Black	-4.71***	-8.75***	-2.61***
		(0.63)	(0.75)	(0.79)
Employed	Democrat	-0.77**	-0.40	-1.11**
		(0.37)	(0.40)	(0.50)
	Democrat x Black	2.59**	1.50	3.82***
		(1.03)	(1.38)	(1.19)
	Black	-8.65***	-13.15***	-6.23***
		(0.72)	(0.86)	(0.88)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. The controls are the same as in Table 2. The in-labor-force variable is 1 if an individual is in the labor force and 0 otherwise. The employed variable is 1 if an individual is employed, and is 0 if the individual is unemployed or out of the labor force. Estimates are generated using a linear probability model. Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Results are clustered at the state-year level. Source: March CPS data.*

Table F1.3 - RD for Earnings

Earnings	Variables	All	Men	Women
Annual	Democrat	-2.42***	-3.00***	-1.44
		(0.83)	(0.98)	(0.98)
	Democrat x Black	5.03***	5.88**	2.56
		(1.88)	(2.32)	(2.56)
	Black	-16.37***	-29.62***	-6.18***
		(1.20)	(1.46)	(1.64)
Weekly	Democrat	-2.24***	-2.49***	-1.78**
		(0.67)	(0.77)	(0.77)
	Democrat x Black	2.11	3.28*	-0.19
		(1.47)	(1.81)	(1.96)
	Black	-10.35***	-20.92***	-1.99
		(0.92)	(1.11)	(1.28)
Hourly	Democrat	-1.76***	-1.87***	-1.51**
		(0.57)	(0.66)	(0.66)
	Democrat x Black	1.23	1.52	0.16
		(1.28)	(1.69)	(1.60)
	Black	-9.86***	-15.73***	-5.21***
		(0.83)	(1.04)	(1.06)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. The controls are the same as in Table 2. Outcome variables are expressed in log form and coefficients and standard errors (in parentheses in the table) are multiplied by 100. Results are clustered at the state-year level. Source: March CPS data.*

F2 - Clustering at State level

Table F2.1 - RD for hours worked, weeks worked and usual hours

Intensive	Variables	All	Men	Women
Total hours worked	Democrat	-0.63	-1.13	0.08
		(0.60)	(0.75)	(0.82)
	Democrat x Black	3.79**	4.47*	2.33
		(1.81)	(2.54)	(1.99)
	Black	-6.49***	-14.00***	-0.91
		(1.16)	(1.29)	(1.48)
Weeks worked	Democrat	-0.15	-0.51	0.35
		(0.42)	(0.51)	(0.64)
	Democrat x Black	2.91**	2.71	2.68**
		(1.38)	(2.09)	(1.29)
	Black	-6.00***	-8.81***	-4.13***
		(0.86)	(1.24)	(0.99)
Usual hours	Democrat	-0.48*	-0.62**	-0.27
		(0.27)	(0.28)	(0.38)
	Democrat x Black	0.87	1.76**	-0.35
		(0.64)	(0.74)	(1.39)
	Black	-0.49	-5.19***	3.22***
		(0.57)	(0.47)	(1.15)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. The controls are the same as in Table 2. Outcome variables are expressed in log form and coefficients and standard errors (in parentheses in the table) are multiplied by 100. Results are clustered at the state level. Source: March CPS data.*

Table F2.2 – RD for being in labor force and employed

Extensive	Variables	All	Men	Women
In labor force	Democrat	-0.69**	-0.26	-1.08**
		(0.30)	(0.30)	(0.49)
	Democrat x Black	2.48**	1.37	3.65***
		(1.24)	(1.59)	(1.35)
	Black	-4.71***	-8.75***	-2.61*
		(1.06)	(0.99)	(1.45)
Employed	Democrat	-0.77*	-0.40	-1.11*
		(0.45)	(0.53)	(0.58)
	Democrat x Black	2.59*	1.50	3.82**
		(1.52)	(1.81)	(1.69)
	Black	-8.65***	-13.15***	-6.23***
		(1.13)	(1.13)	(1.44)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. The controls are the same as in Table 2. The in-labor-force variable is 1 if an individual is in the labor force and 0 otherwise. The employed variable is 1 if an individual is employed, and 0 if the individual is unemployed or out of the labor force. Estimates are generated using a linear probability model. Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Results are clustered at the state level. Source: March CPS data.*

Table F2.3 – RD for Earnings

Earnings	Variables	All	Men	Women
Annual	Democrat	-2.42**	-3.00**	-1.44
		(1.12)	(1.22)	(1.55)
	Democrat x Black	5.03*	5.88*	2.56
		(2.53)	(3.38)	(3.05)
	Black	-16.37***	-29.62***	-6.18***
		(1.58)	(1.82)	(2.20)
Weekly	Democrat	-2.24**	-2.49**	-1.78
		(0.93)	(0.97)	(1.19)
	Democrat x Black	2.11	3.28	-0.19
		(1.99)	(2.37)	(2.70)
	Black	-10.35***	-20.92***	-1.99
		(1.42)	(1.29)	(2.14)
Hourly	Democrat	-1.76**	-1.87**	-1.51
		(0.83)	(0.90)	(1.02)
	Democrat x Black	1.23	1.52	0.16
		(1.91)	(2.08)	(2.23)
	Black	-9.86***	-15.73***	-5.21***
		(1.30)	(1.29)	(1.62)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. The controls are the same as in Table 2. Outcome variables are expressed in log form and coefficients and standard errors (in parentheses in the table) are multiplied by 100. Results are clustered at the state level. Source: March CPS data.*

Appendix G – Policies

State Public Sector & Health and Education sector

Table G.1 - RD estimates for the probability of working in the public or related sector

Extensive	Variables	All	Men	Women
State public sector	Democrat	0.23**	0.03	0.42***
		(0.10)	(0.12)	(0.14)
	Democrat × Black	-0.15	-0.08	-0.21
		(0.30)	(0.39)	(0.43)
	Black	1.25***	0.65***	1.73***
		(0.18)	(0.25)	(0.25)
Health sector	Democrat	0.25**	0.08	0.42**
		(0.13)	(0.12)	(0.19)
	Democrat × Black	0.27	0.02	0.47*
		(0.17)	(0.14)	(0.24)
	Black	0.60***	0.35***	0.80***
		(0.11)	(0.08)	(0.16)
Education sector	Democrat	0.17*	0.10	0.25*
		(0.10)	(0.10)	(0.14)
	Democrat × Black	0.15	0.04	0.21
		(0.09)	(0.09)	(0.14)
	Black	-0.09	0.06	-0.15
		(0.07)	(0.06)	(0.11)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. The controls are the same as in Table 2. Estimates are generated using a linear probability model for working in the state public, health and education sectors. Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Source: March CPS data.*

Table G.2 - RD estimates for the intensity of work (total hours) in the public or related sector

Total hours worked	Variables	All	Men	Women
State public sector	Democrat	1.14	-1.13	1.10
		(4.00)	(1.93)	(2.17)
	Democrat × Black	1.24	-2.32	3.17
		(4.01)	(5.57)	(5.21)
	Black	2.36	0.97	0.70
		(2.62)	(3.88)	(3.33)
Health sector	Democrat	2.37*	-0.96	3.36**
		(1.30)	(2.19)	(1.51)
	Democrat × Black	0.46	-1.58	0.98
		(1.42)	(2.98)	(1.58)
	Black	5.19***	-2.50	6.17***
		(0.98)	(2.05)	(1.12)
Education sector	Democrat	2.19	-2.61	4.01*
		(1.79)	(2.50)	(2.42)
	Democrat × Black	3.07	1.02	3.63
		(2.02)	(3.93)	(2.37)
	Black	9.20***	-6.43**	12.63***
		(1.46)	(2.92)	(1.71)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. The table measures the intensity of work (total hours worked) in the state public, health and education sectors. The controls are the same as in Table 2. Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Source: March CPS data.*

Table G.3 – RD with occupation and industry

	Variables	All	Men	Women
Annual earnings	Democrat	-1.78**	-2.35**	-1.11
		(0.86)	(1.09)	(1.20)
	Democrat × Black	3.15*	4.81*	0.21
		(2.02)	(2.68)	(2.98)
	Black	-10.58***	-23.29***	-0.52
		(0.87)	(1.36)	(1.51)
Total hours	Democrat	-0.37	-1.00	0.34
		(0.56)	(0.76)	(0.78)
	Democrat × Black	2.77*	3.94*	1.03
		(1.52)	(2.05)	(1.84)
	Black	-4.24***	-11.99***	1.43
		(0.94)	(1.15)	(1.15)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. Control variables are the same as in Table 2. Outcome variables are expressed in log form and coefficients and standard errors (in parentheses in the table) are multiplied by 100. Source: March CPS data.*

Business Sector

Table G.4 – RD for impact of business dynamics

Outcomes	Variable	All
Establishment entry rate	Democrat	-20.32
		(19.10)
Establishment exit rate	Democrat	-8.35
		(14.43)
Job creation rate	Democrat	-14.91
		(25.72)
Job destruction rate	Democrat	-26.60
		(32.92)
Net job creation rate	Democrat	-38.70
		(56.37)
Top corporate tax rate	Democrat	-2.14
		(1.98)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. Estimates are generated from Business Dynamics Statistics, using a regression of the aggregate version of specification (1) for the outcome variables: establishment entry rate, establishment exit rate, job creation rate, job destruction rate and net job creation rate. Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Source: Business Statistics Dynamics (BDS) and Leigh 2008 data.*

Table G.5 – RD impact of businesses by firm size

Firm Size		0-99	100-499	500-999	1,000-2,499	2,500 or more
Outcomes	Variables	All	All	All	All	All
Establishment entry rate	Democrat	-17.62	-14.94	-17.65	-17.47	-24.56
		(14.97)	(23.38)	(38.43)	(42.30)	(16.77)
Establishment exit rate	Democrat	-2.63	9.11	15.76	-6.99	4.67
		(10.84)	(17.19)	(31.98)	(40.46)	(13.47)
Job creation rate	Democrat	1.74	-18.05	-97.49	-79.46	-43.94
		(24.54)	(34.08)	(61.45)	(71.87)	(43.79)
Job destruction rate	Democrat	-4.83	-12.51	-84.46	-72.94	-95.10
		(23.69)	(33.27)	(58.27)	(79.66)	(58.40)
Net job creation rate	Democrat	6.57	-5.53	-13.02	-6.52	51.16
		(33.65)	(46.11)	(75.54)	(107.96)	(73.89)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. Estimates are generated from Business Dynamics Statistics, using a regression of specification (1), aggregating by firm size for establishment entry rate, establishment exit rate, job creation rate, job destruction rate and net job creation rate. The firm size corresponds to the number of employees, where 0-100 means 0 to 100 employees. Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Source: Business Statistics Dynamics (BDS).*

Displaced Worker Surveys

Table G.6 – RD - probability of being displaced

Variables		All	Men	Women
Displaced worker	Democrat	0.17	0.03	0.23
		(0.16)	(0.24)	(0.20)
	Democrat × Black	-0.53	-2.11**	0.82
		(0.47)	(0.85)	(0.58)
	Black	1.54***	2.40***	0.87**
		(0.30)	(0.61)	(0.36)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. The variables come from DWS, which is available from 1984 to 2008 every two years. The table presents the propensity of having been laid off, using a linear probability model specification. The controls are the same as in Table 2. Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Source: January CPS data (Displaced Worker Surveys).*

Table G.7 – RD estimates by earnings categories, contingent on being displaced

Outcomes	Variables	All	Men	Women
Low-earnings workers	Democrat	-10.65**	-7.46	-18.16***
		(4.17)	(5.12)	(5.89)
	Democrat × Black	5.46	5.59	16.57
		(8.75)	(10.39)	(11.36)
	Black	8.19	8.91	4.27
		(6.21)	(6.75)	(9.09)
Medium-earnings workers	Democrat	1.87	-0.46	6.83*
		(3.17)	(3.60)	(3.97)
	Democrat × Black	-2.03	-5.62	-0.05
		(5.67)	(7.44)	(6.48)
	Black	-1.72	2.86	-6.78*
		(3.23)	(3.83)	(3.77)
High-earnings workers	Democrat	8.79***	7.92*	11.32**
		(3.21)	(4.08)	(5.43)
	Democrat × Black	-3.43	0.04	-6.52
		(6.69)	(6.98)	(9.81)
	Black	-6.47	-11.77***	2.51
		(5.43)	(5.19)	(8.69)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. The variables come from DWS, which is available from 1984 to 2008 every two years. Estimates are generated using a linear probability model for low-, medium- and high-earnings workers who have been laid off, conditional on having been displaced. The controls are the same as in Table 2. Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Source: January CPS data (Displaced Worker Surveys).*

Household earnings, before and after tax

Table G.8 - RD for household earnings

Outcomes	Variables	All
Household earnings before tax	Democrat	-1.70***
		(0.64)
	Democrat × Black	3.35*
		(2.03)
	Black	-23.21***
		(1.42)
Household earnings after all tax	Democrat	-1.33**
		(0.56)
	Democrat × Black	4.43**
		(1.75)
	Black	-19.22***
		(1.23)
Household earnings after state tax	Democrat	-1.60***
		(0.63)
	Democrat × Black	3.56*
		(2.01)
	Black	-22.94***
		(1.40)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. The controls are the same as in Table 2. Outcome variables are in log form. Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Source: March CPS data and NBER TAXSIM simulator.*

Minimum Wage, Incarceration Rate & State EITC Rate

Table G.9 – RD estimates for labour market outcomes of low-earnings workers and low-earnings women

RD estimates for labor market outcomes of low-earning workers, controlling for minimum wage

Variable	Total hours	Labor force participation	Employment
Minimum wage	0.75**	0.32**	0.47***
	(0.35)	(0.15)	(0.16)

RD estimates for labor market outcomes of low-earning workers, controlling for incarceration rate

Variable	Total hours	Labor force participation	Employment
Incarceration rate	0.34	-0.46***	-0.32**
	(0.36)	(0.15)	(0.16)

RD estimates for labor market outcomes of low-earning women, controlling for state EITC rate

Variable	Total hours	Labor force participation	Employment
State EITC rate	7.04***	5.97*	6.75**
	(3.22)	(3.10)	(3.04)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10% level. The controls are the same as in Table 2. Estimates are generated by regressing total hours on the linear probability model for being in the labor force and being employed, controlling subsequently for minimum wage, incarceration rate (per 1.000 habitants and up to 1998, taken from Leigh (2008)) and state EITC rate (starting in 1990). Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Source: Leigh (2008) data and UKCPR data.*

Table G.10 – RD estimates of the impact of partisan allegiance on state EITC rate

Variables	Has a state EITC Rate	Level of state EITC Rate
Democratic governor	9.60***	1.49***
	(2.76)	(0.56)

**** denotes statistically significant results at the 1% level, ** denotes statistically significant results at the 5% level and * denotes statistically significant results at the 10%. The controls are the same as in Table 2. Estimates are generated by regressing partisan allegiance on the presence of a state EITC rate (using a linear probability model) and on the level of the state EITC rate. Coefficients and standard errors (in parentheses in the table) are multiplied by 100. Source: UKCPR data.*