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Essays on Race and Political Economy

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Essays on Race and Political Economy

A dissertation submitted in partial satisfaction of the
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by

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DEDICATION

For Mom and all the years that we missed.

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ABSTRACT OF THE DISSERTATION

Essays on Race and Political Economy

by

Desmond Ang

Doctor of Philosophy in Economics

University of California San Diego, 2018

Professor James Andreoni, Chair
Professor Gordon Dahl, Co-Chair

The dissertation contains three essays exploring race and political economy. Leveraging large, novel datasets and applied econometric techniques, it explores the role that government policy may have on racial disparities in civic engagement and education. Chapter 1 examines the impact of federal election oversight restrictions established under the Voting Rights Act on electoral participation and outcomes. I find that these oversight measures led to large long-run gains in minority turnout, but actually decreased Democratic vote share in presidential elections. I provide evidence that this partisan shift was due to political backlash among racially-conservative whites. Chapter 2 investigates how acts of police violence affect the educational and psychological outcomes of Los Angeles public high school students. I find that students living near an officer-involved killing

experience large and significant decreases in academic achievement and increases in emotional disturbance. These effects are concentrated among black and Hispanic students and police killings of other minorities, particularly those in which the individual killed was unarmed. Chapter 3 then explores how police violence influences voter turnout. I find that police killings lead to increased civic engagement among black communities, especially those that were historically critical of the criminal justice system.

Chapter 1

Do 40-Year-Old Facts Still Matter? Long-Run Effects of Federal Oversight under the Voting Rights Act

Abstract

In 2013, the Supreme Court struck down parts of the Voting Rights Act that mandated federal oversight of election laws in discriminatory jurisdictions, prompting a spate of controversial new voting rules. Utilizing difference-in-differences to examine the Act's 1975 revision, I provide the first estimates of the effects of "preclearance" oversight. I find that preclearance increased long-run voter turnout by 4-8 percentage points, due to lasting gains in minority participation. Surprisingly, Democratic support *dropped* sharply in areas subject to oversight. Using historical survey and newspaper data, I provide evidence that this was the result of political backlash among racially conservative whites.

I. Introduction

"But a more fundamental problem remains: Congress did not use that record to fashion a coverage formula grounded in current conditions. It instead re-enacted a formula based on 40-year-old facts having no logical relation to the present day."

— *Chief Justice John Roberts (Shelby v. Holder, 2013)*

The Voting Rights Act (VRA) of 1965 has been hailed as one of the "greatest legislative achievements of the Civil Rights Movement" (Menand, 2013). Passed months after the Selma to Montgomery marches, the Act prohibited the denial or abridgement of "the right to vote on account of race or color." The effects of the VRA on minority enfranchisement were immediate. Between the 1964 and 1968 presidential elections, black voter registration rates increased 67 percent among Southern states (Vallely, 2009).

The Act achieved this through two principal mechanisms. The first was the prohibition of literacy tests, which were used throughout the Jim Crow era to disenfranchise Southern blacks. The VRA's second and more controversial mechanism was a federal oversight process commonly known as preclearance. Jurisdictions subject to preclearance (henceforth called *covered* jurisdictions) were prohibited from implementing any new electoral rule without first obtaining federal approval. While preclearance's geographic purview was limited only to areas that met certain historical criteria, the scope of its protections was expansive and encompassed *all* future changes affecting voting in those areas. Thus, preclearance restrictions, which have been called "the most effective means of preventing racial bias in voting" (Bennett, 2013), were designed as a broad prophylaxis against voter discrimination, shifting onto covered jurisdictions the burden of proving *ex ante* that new voting rules did not have a "discriminatory purpose" and would not have a "discriminatory effect."

Since its inception, preclearance oversight has been alternately praised and criticized as "extraordinary legislation otherwise unfamiliar to our federal system" (Northwest Austin Municipal Utility District No. 1 v. Holder, 2009). These arguments came to a head in Shelby County v. Holder (2013), in which the Supreme Court ruled that continued coverage based on historical — rather than current — measures of discrimination is unconstitutional. As a result, until and unless Congress enacts a new coverage formula, previously covered jurisdictions are no longer subject to federal oversight.

Immediately following the Shelby ruling, lawmakers in several previously covered areas enacted controversial new voting changes, many of which have been challenged in federal courts. Alabama, Mississippi, North Carolina and Texas introduced restrictive voter ID requirements, while

Florida, Georgia and Virginia sought to purge their voter rolls of thousands of eligible minorities. Though Republicans have justified these measures as necessary to combat widespread voter fraud, Senate Democrat Chuck Schumer denounced them as "clear front[s] for constricting the access to vote to poor Americans...and - above all - African-Americans and Latinos." Underpinning this partisan divide is the common belief that the minorities most affected by restrictive voting rules lean heavily Democratic. Indeed, President Donald Trump, a Republican, claimed that, of the "millions" of allegedly illegal ballots cast in 2016, "none of 'em come to me. They would all be for the other side." Given America's growing minority electorate, the legal fate of these voting laws could have lasting implications for future elections.

Despite their relevance to ongoing policy debates, the specific effects of preclearance have never been estimated. While researchers have examined the VRA's impact on turnout (Filer et al., 1991), representation (Besley et al., 2010; Washington et al., 2012; Schuit and Rogowski, 2016), and minority aid (Cascio and Washington, 2014), these studies focus on the 1965 implementation of the Act and are thus unable to disentangle the effects of preclearance from the simultaneous abolition of literacy tests, which were among the most discriminatory tools ever employed in the U.S. election system and are unlikely to ever be reinstated (Springer, 2014). Furthermore, all of these papers, as well as the broader literature exploring the enfranchisement of minorities (Husted and Kenny, 1997), women (Miller, 2008), and the poor (Fujiwara, 2015), examine policies designed to alleviate specific, existing barriers and prohibitions to voting – such as the elimination of literacy tests, the introduction of electronic voting, and the extension of suffrage rights.

Preclearance restrictions differ fundamentally from these interventions. Rather than targeting specific voting barriers already in use, federal oversight was designed to restrict the implementation of any *new* discriminatory measures. Understanding the implications of these blanket protections is especially relevant in light of evidence from Trebbi et al. (2008) and Alesina et al. (2004) of the strategic manipulations that local election officials engage in to maintain power. Indeed, broad preventative oversight encompassing the universe of potential voting changes may be the most effective means of curbing discrimination in settings like the U.S., where electoral rule-making is highly decentralized and opaque.

This paper seeks to better understand the effects of such oversight. Using a flexible difference-in-differences model, I examine the geographic expansion of coverage under the 1975 revision of the Voting Rights Act to estimate the causal impact of preclearance on county-level voter turnout and Democratic vote share from 1960 to 2016. Unlike the 1965 VRA which was "reverse-engineered" by Congress to capture Southern states that employed literacy tests, the 1975 coverage formula relied on noisy measures of voter turnout and minority population share to determine which counties were subject to preclearance (Holder, 2013). Thus, application of the 1975 formula resulted in heterogeneity of coverage within states throughout the country, subjecting 283 counties across nine states to federal oversight. I am able to exploit this heterogeneity to precisely estimate the policy's effects and to demonstrate its plausible exogeneity.

I find that preclearance restrictions led to gradual and significant increases in voter participation and that these gains persisted for over 40 years, bolstering turnout by 4-8 percentage points in recent elections. Examining state-level turnout by race, I demonstrate that these effects were due entirely to increased participation among minorities, who were 17 p.p. more likely to vote in the 2012 election as a result of preclearance coverage. Analyzing electoral rules data, I show that municipalities subject to voter protections were significantly less likely to employ "winner-take-all" election systems, which are commonly believed to dilute minority voting power. Combined with heterogeneity analysis demonstrating larger effects among areas with more historical discrimination, these results suggest that gains in turnout were the result of reduced voter discrimination as opposed to other demographic or political factors.

Surprisingly, I find that preclearance coverage led to significant and immediate *decreases* in the share of Democratic votes cast. These estimates are large – averaging 3.2 p.p. across post-treatment elections – and exceed the 1992 and 1996 presidential margins of victory in the covered states of Texas and Arizona. Using historical survey data, I show that this rightward shift was driven by increased Republican support among whites opposed to government aid for minorities. As demonstration of the political controversy surrounding preclearance, I find a sharp spike in newspaper mentions of the VRA in covered areas beginning in 1975, particularly among those papers that had endorsed President Richard Nixon, whose Republican administration sought to abolish election oversight restrictions. Taken together, these results provide strong evidence that the implementation of minority voter protections triggered political re-alignment among whites.

This paper makes several important contributions. First, it complements the existing literature on voter enfranchisement efforts, which concentrates on targeted, remedial interventions, by demonstrating the efficacy of broader preventative measures. Viewed from a different angle, my findings suggest that strategic manipulations of electoral rules affecting ballot access and voting power can have deleterious effects on voter participation, and demonstrate that these effects are disproportionately received by minorities.

Second, this paper provides new evidence in support of race-based theories of Southern dealignment, which argue that the collapse of the New Deal coalition in the South was due primarily to the Democratic Party's embrace of the Civil Rights Movement in the 1960's (Kuziemko and Washington, 2015). In examining the expansion of preclearance coverage to counties across the nation more than a decade later, I validate the salience of "white backlash" against minority political threats in other settings (Key and Heard, 1950; Tesler and Sears, 2010; Enos, 2016). By demonstrating geographic and partisan differences in local media coverage of preclearance, this study also adds to a growing body of literature exploring the role of media in politics (Besley and Prat, 2006; Snyder Jr and Strömberg, 2010; Gentzkow and Shapiro, 2010; Chiang and Knight, 2011; Gentzkow et al., 2015).

Perhaps most importantly, this paper contributes to current policy debate by deriving the first estimates of preclearance's impact. The Shelby ruling was predicated on the Court's opinion that "a [coverage] formula based on 40-year-old facts" has "no logical relation to the present day." I show not only that preclearance coverage led to historical increases in minority participation but also that the application of these restrictions in 1975 continued to bolster enfranchisement over four decades later. To the extent that the future of the Voting Rights Act hinges on the formulation of new coverage criteria relevant to the "present day," understanding these effects and the role they played in shaping the current political landscape is critical to Congress' ability to craft meaningful legislation capable of protecting voting rights today and into the future.

This paper is organized as follows: Section I provides details surrounding preclearance's history, enforcement and coverage, Section II describes my empirical strategy and data, Section III presents estimation results for voter turnout and Democratic vote share, Section IV includes various robustness analyses, Section V explores mechanisms, Section VI discusses implications of the Shelby ruling and Section VII concludes.

II. Background

Passed at the height of the American Civil Rights movement under a Democratic-controlled government, the Voting Rights Act of 1965 was designed to be, as President Lyndon B. Johnson described, "the goddamndest, toughest voting rights act [possible]" (May, 2013).

Section 2 of the Act broadly reinforced the voting rights guaranteed in the 14th and 15th Amendments and allowed private citizens to sue as means of enforcing prohibitions on discrimination. The Act further banned the use of literacy tests - first in the South, then nationwide in 1970 - and beginning in 1975, mandated the provision of translated election materials and language assistance in minority-heavy areas.

In drafting the Act, members of Congress feared a never-ending cat-and-mouse game would ensue without more expansive protections. Previous efforts by the Department of Justice to strike down discriminatory laws often resulted in the enactment of new discriminatory rules, even within 24 hours (Pitts, 2003). The difficulty of pursuing piecemeal remedies was perhaps best summarized by one Mississippi election official, who explained "what those smart fellows [at the Justice Department] don't realize is that we can still get to these darkies in a whole lot of subtle ways" (Stanley, 1987).

Thus, Congress designed Section 5 of the Act as a counter these "subtle" methods of discrimination. Section 5 essentially froze in place the voting processes of covered jurisdictions, requiring that any and all future changes to those processes be "precleared" by the federal government. Only if and when a proposed change received preclearance could that change be enforced by the jurisdiction. These blanket protections — widely considered the "heart" of the VRA — sought to shift the burden of proof of discrimination off of aggrieved voters *ex post* and onto discriminatory election officials *ex ante* (Tucker, 2006).

A. Enforcement

The scope of preclearance restrictions was extensive. All changes, no matter how minor, pertaining to voting "laws, practices or procedures" and affecting a covered jurisdiction or any of its sub-jurisdictions required federal approval before they could be implemented.

Proposed changes had to be submitted to the Attorney General, who had 60 days to interpose an objection.[1] Proposals were then assessed on a case-by-case basis according to a "retrogression test." This test considered the effects of the proposal on minority enfranchisement in relation to the *status quo*. Any change that was deemed to leave minorities *worse* off was denied preclearance. Regardless of its likely effect, any change that the Attorney General believed was *intended* to harm minorities was also denied preclearance. Importantly, this process was only designed to prevent the implementation of *new* changes with discriminatory intent or effect. It did not reverse existing discriminatory practices.

As Panel A of Figure 1.1 shows, preclearance oversight was actively enforced throughout its existence. From 1965 to 2013, over half a million Section 5 submissions were made to the Attorney General. Of these, 1,134 were objected to, representing over 3,000 blocked voting changes. Notably, more than 85 percent of objections pertained to local or municipal, as opposed to state-level, proposals.[2]

[Figure 1.1 about here.]

As Panel B shows, objections were lodged against a wide range of discriminatory voting

[1]Changes could alternatively be submitted to the U.S. District Court for D.C. However, due to large relative material and time costs of pursuing "judicial" preclearance, over 99 percent of submissions were directed to the Attorney General.

[2]Another several thousand proposals were withdrawn or amended following the issuance of a "more information request" by the Attorney General (Adler and Kousser, 2011)

changes. The majority related to either election system changes or redistricting and annexation. The former primarily refer to attempts by local governments to transition from district-based to election systems, which dilute minority votes in areas of high racial segregation.[3] The latter encompass not only boundaries for national and state legislatures but also those for school districts, city councils and other local governing bodies.[4] Changes pertaining to voter registration, including identification, residency and re-registration requirements, comprised fewer than 20 percent of objections in any decade, less than those regarding the timing and placement of elections and polling locations.

While these rule changes may not appear particularly onerous in the cross-section, the protections afforded under Section 5 become more obvious when considering a historical case study. Table 1.1 lists the entire history of objections lodged against voting changes in Harris, Texas, the state's most populous county. Had they been enacted, these 18 state-level and 15 local-level submissions would have resulted in, among other changes: the purge of all voters that did not re-register in 60 days, the elimination of state-funded primaries for the state's leading Mexican-American party, the creation of a white-controlled school district and criminal court, the elimination of hundreds of polling stations, the institution of gerrymandered districts and at-large elections for school boards, city councils and the state legislature, the allowance of administrative challenges of voter citizenship and the implementation of strict voter identification requirements.

[Table 1.1 about here.]

As both Figure 1.1 and Table 1.1 demonstrate, the number and type of objections interposed by the Attorney General evolved over time. While further exploration of these dynamics is outside the scope of this paper, they nonetheless serve to highlight the expansiveness of preclearance oversight, which protected against ever-shifting efforts to discriminate across a wide range of different electoral rules.[5]

[3]While one commonly cited benefit of at-large systems is that they produce less pork-barrel spending, relative to district-based systems (Persson et al., 2000), Baqir (2002) finds that at-large election systems have little effect on size of government.

[4]Though perhaps less salient, annexations are an important strategic margin employed by local jurisdictions to manipulate demographic heterogeneity. See, for example, Alesina et al. (2004)

[5]It is certainly possible that these enforcement patterns were also influenced by the partisan leanings of the

B. Coverage

Figure 1.2 maps the jurisdictions brought under preclearance coverage by each revision of the VRA. As shown, preclearance coverage was initially limited to areas in the Deep South. The 1965 VRA imposed federal oversight restrictions and banned literacy tests only among those Southern states that employed such tests. This led to preclearance coverage of the entire states of Alabama, Georgia, Louisiana, Mississippi, South Carolina and Virginia, as well as parts of North Carolina. The 1970 VRA then brought under preclearance coverage a handful of jurisdictions in California, New York and New Hampshire that had continued to administer literacy tests.

[Figure 1.2 about here.]

The 1975 revision of the Act expanded preclearance coverage to include those areas where discrimination may have been less overt. Specifically, any jurisdiction where a single language minority group (i.e., Hispanic, Asian, Alaskan or Native Americans) comprised greater than 5 percent of the voting age population in 1970 *and* where voter turnout was less than 50 percent in 1972 was brought under federal oversight.[6] This resulted in the coverage of 283 counties across nine states, only three of which (Texas, Arizona and Alaska) were covered in their entirety.[7]

For all versions of the VRA, preclearance coverage lasted indefinitely (until the 2013 Shelby decision) and included all of a covered jurisdiction's political sub-jurisdictions.[8] Thus, for example, in covering the state of Texas, the 1975 VRA also brought under federal supervision all of its

Executive Branch. However, Posner (2006) notes that the staff attorneys at the Department of Justice responsible for day-to-day enforcement were largely insulated from political pressure since the "legal bases on which the Department could invalidate non-retrogressive change" were "well-set" by the mid-1980's.

[6]Technically, the trigger only applied if a jurisdiction's language minorities experienced illiteracy rates greater than the national average. However, this was true for all cases, except for a few Native American reservations.

[7]Newly covered counties as well as any county with greater than 5 percent language minorities, regardless of 1972 turnout, were also subject to bilingual election requirements. Except as applied to counties subject to preclearance, these requirements were temporary and determined on a rolling basis following each Census.

[8]The VRA included"bail out" provisions, allowing areas to escape preclearance coverage after demonstrating non-discrimination. However, these criteria were designed such that "bail out" was virtually impossible prior to 1985 and remained extremely onerous afterwards. In over 40 years, only a handful of cities in Virginia successfully bailed out of coverage after proving non-discrimination. The VRA also included a "pocket trigger" to "bail in" uncovered jurisdictions. However, this was seldom used and generally imposed only temporary coverage of certain types of voting changes (for example, New Mexico was subject to preclearance only for redistricting plans and only from 1984 to 1994).

counties and cities, even those with high turnout rates or low minority population shares.

C. Response

Given the sovereignty and material costs associated with preclearance coverage, the implementation of Section 5 sparked considerable outrage among local and state officials.[9] After Texas fell under coverage in 1975, the state's governor called preclearance "a fraud", "an insult", and "an administrative nightmare" (Seguiin Gazette, 1975). Other critics claimed that preclearance's selective geographic application represented an "unfair" and "unprecedented breach of federalism" (South Carolina v. Katzenbach, 1966). Preclearance remained a political flashpoint years after its initial implementation. While campaigning in 1980, President Ronald Reagan called the Act "humiliating to the South" and promised to "restore to state and local governments the power that properly belongs to them" (Wolters, 1996).[10]

In Shelby County v. Holder (2013), the Supreme Court agreed with these perspectives and ruled that the continued enforcement of preclearance based on historical coverage formulas was unconstitutional. This freed from federal oversight all states and counties that had been captured under the 1965, 1970 and 1975 coverage formulas. While the Court upheld the constitutionality of preclearance itself, it placed on Congress the onus of drafting a new coverage formula. Though several new formulas have been proposed, none have been enacted.

Since the Shelby decision, previously covered jurisdictions have implemented numerous controversial voting changes. Within hours of the ruling, Texas passed a voter identification law that had previously been rejected under preclearance, while North Carolina enacted registration requirements that federal courts have since found to "target African Americans with almost surgical precision."[11] Though local voting changes have attracted less media attention, many are no less

[9]Expenses related to obtaining preclearance for even minor voting changes were estimated to range from $500-$1000. Over a period of several decades, these costs could become quite burdensome, especially for local governments. Officials in Merced County, California estimated spending over $1 million from 2000 to 2010 alone (Nidever, 2012).

[10]The Reagan administration then attempted to weaken Section 5 by proposing regulatory guidelines that required affirmative evidence of discrimination for changes to be invalidated. However, this proposal was ultimately abandoned under Congressional pressure (Kousser, 2007).

[11]Though the Texas law was overturned in 2014 by federal courts under Section 2 of the VRA, many of its controversial provisions remained in effect during the 2016 presidential election, thus highlighting the challenges of

controversial. In Maricopa County, Arizona, home to over one million Hispanic voters, election officials opted to reduce the number of polling stations for the 2016 presidential primary by over 70 percent, leading to lines up to five hours long. This is part of a larger trend among previously covered counties, 43% of which have closed polling locations since Shelby, resulting in nearly 900 fewer places to vote in the 2016 election (Leadership Conference Education Fund, 2016).

III. Empirical Strategy

Though the majority of covered counties fell under coverage beginning in 1965, this paper focuses only on those covered by the 1975 revision of the VRA. The reasons for this are many.

First, jurisdictions that fell under coverage beginning in 1965 were simultaneously banned from using literacy tests, which were not eliminated nationwide until 1970.[12] Thus, identifying the specific effects of preclearance from the 1965 VRA, which essentially introduced two concurrent interventions to an identical treatment group, would require strict and possibly unrealistic assumptions.[13]

Second, the 1975 VRA's reliance on an objective coverage formula made precise geographic targeting of problematic areas nearly impossible. As former Attorney General Eric Holder noted, the use of noisy estimates for determining coverage meant that "the scope of coverage had the potential to be over- and under-inclusive" (Holder, 2013). Indeed, officials in Kings County have long contended that the county's coverage was the result of the Census Bureau's failure to properly account for a large military population that was ineligible to vote in Kings (Nidever, 2012). Furthermore, the estimates used to determine coverage were not known to Congress — nor had they even been calculated by the Census Bureau — at the time of the Act's passage.

Last, the nature of the 1975 VRA's coverage formula, which took into account county-level demographic measures, resulted in substantial within-state treatment heterogeneity as well as a

ex-post litigation as compared to preclearance's preventative mechanisms

[12]Though the 1975 VRA introduced bilingual election requirements in addition to expanding preclearance coverage, hundreds of counties *not* covered under preclearance were also subject to these language restrictions.

[13]Furthermore, much of this paper's heterogeneity analysis would not possible through examination of the 1965 VRA due to the lack of historical data prior to its implementation. For example, the CPS voter supplement data on state-level turnout by race is only available from 1968 onwards.

diverse regional representation. Indeed, 283 counties from nine states were brought under coverage in 1975, representing three of the nation's four Census regions. This geographic heterogeneity bolsters the study's internal and external validity.

A. Data

The purpose of this paper is to assess the impact of preclearance restrictions on voter enfranchisement and representation. While I would ideally estimate a first-stage effect on direct measures of voter discrimination, comprehensive data on discriminatory incidents and policies does not exist for uncovered counties and is fraught with issues of selection for covered areas. Furthermore, preclearance's very existence was predicated on the notion that it is nearly impossible to enumerate every policy channel by which local officials are able to discriminate against voters. Thus, I instead estimate effects on voter turnout and share of Democratic votes cast in presidential elections, perhaps the most common and direct expressions of political participation and preferences.

County-level voting data for presidential elections come from the Interuniversity Consortium for Political and Social Research (ICPSR) and Dave Leip's Atlas of U.S. Presidential Elections. Estimates of voting age citizens are interpolated from Census and American Community Survey demographic data.[14] These data are used to construct county-level estimates of voter turnout (the share of votes cast to eligible voting population) and Democratic vote share (the share of Democratic votes cast to major party votes cast) in all presidential elections from 1960-2016.[15] To examine changes in political preferences, I also obtain district-level measures of political ideology and party affiliation for the 87th to 113th Congresses from Poole and Rosenthal's DW-NOMINATE data (Poole and Rosenthal, 1985, 2011).

As county-level estimates of historical turnout by ethnicity do not exist, I rely on the Current Population Survey Voting and Registration supplement to examine effects on minority turnout at the

[14] Interpolated estimates were obtained from (Gentzkow et al., 2011) for 1960-2004 and were calculated by the author using the same methodology from 2004 onwards.

[15] Democratic vote share is measured against major party votes cast due to the presence of significant third-party presidential candidates in 1968, 1980, 1992 and 1996. However, as shown in the Appendix, results are virtually identical if Democratic vote share is instead calculated using all presidential votes cast.

state-level.[16] The CPS data contains individual-level self-reports of race and voting participation which I aggregate to construct representative estimates of state-level turnout for whites and non-whites from 1968-2016.[17] Unfortunately, the voting supplement only contains information on whether a respondent voted and not on her ballot choice.

Thus, to examine racial differences in political affiliation, I use data from the American National Election Series (ANES), an in-person survey conducted on a stratified random sample of roughly 2,000 individuals around each presidential and midterm election. Prior to 2000, the survey contains unrestricted access to each respondent's race and county as well as consistently asked questions regarding political preferences.[18] As validation of the ANES data, I also analyze historical Gallup survey data from 1961 to 2003. This data is identified by respondent race and state and has been used by researchers such as Kuziemko and Washington (2015).

To explore mechanisms, I make use of two other sources of data. First, I employ municipality-level data on election rules from the International City/County Municipal Association (ICMA).[19] The ICMA conducts regular surveys of U.S. municipal governments regarding the number and type of council seats in each city. These surveys — which have been employed by Baqir (2002) and Trebbi et al. (2008), among others — were merged to form muncipality-level panel data from 1970 to 2010.

Second, I obtain data on media coverage of the Voting Rights Act. Specifically, I search newspapers.com for articles containing the phrase "Voting Rights Act."[20] To account for non-

[16]The voter supplement is carried out after each federal election using a sample of roughly 100,000-150,000 individuals. Historical micro-data are from CPS Utilities.

[17]I am unable to calculate estimates for 1976, for which state-level identifiers are not available, and for those cases where the voting age population of a group is less than 75,000, the minimum threshold used by the Census Bureau for calculating summary statistics. As microdata from the 1968 supplement is not available, voting estimates for that year are derived from the 1972 supplement, which also asked respondents if they voted in the 1968 election. The Census Bureau only began surveying Hispanic or Latino ethnicity in 1974. Thus, for consistency across pre- and post-treatment samples, non-whites include any individual that did not identify as "White."

[18]Whites are those who self-report as "White non-Hispanic." All others are defined as "non-white."

[19]ICMA data from 1980 onwards is available in electronic format, while data for 1970 and 1975 was hand-coded from hard copies. Further discussion of this data is included in the Appendix.

[20]The search was conducted on "Voting Rights Act" instead of more detailed phrases like "Section 5" or "pre-clearance" for two main reasons. First, the latter are technical terms largely unfamiliar to the public. Thus, many articles that discuss the preclearance do not include those terms. Second, searching on those phrases returns many false matches wholly unrelated to the VRA (i.e., "Section 5" often refers to the section of a newspaper, while "preclearance" primarily references customs and travel requirements). In light of the labor-intensive nature of the data collection

random attrition in the database, I limit my search from 1965 to 1980, returning 85,471 mentions from 502 papers.[21] This information is collapsed to form paper-level panel data containing the number of VRA mentions and the total number of digitized pages in each paper-year. Finally, papers are mapped to counties based on the location of their headquarters and merged with information about historical presidential endorsements using data from Gentzkow et al. (2011).

B. Estimating Equation

This paper employs a flexible difference-in-differences (DD) design to estimate the effects of preclearance requirements on county-level voter turnout and Democratic vote share. This model allows me to estimate average treatment effects across all affected areas and to compare those effects across elections, shedding light on the unique time dynamics of preventative anti-discrimination measures. DD estimation is also readily extended to secondary data sources, even in cases with limited sample sizes (as with historical surveys) or where coverage was determined at other geographic levels (as with state-level turnout).

My sample consists of 2,515 counties in 43 states and the District of Columbia and includes all U.S. counties except those that were subject to Section 5 coverage prior to the enactment of the 1975 VRA and those in Alaska, where county-level turnout is not available.[22] The treatment group is comprised of the 283 counties in nine states captured by the 1975 VRA coverage formula, while the control group is composed of the remaining 2,232 counties in 41 states and the District of Columbia that remained uncovered under all versions of the VRA. A descriptive comparison of the treatment and control groups is presented in Table 1.A1.

With this sample, I estimate the following base model comparing changes over time between treatment and control counties:

(newspapers.com does not allow automated scraping), the search was kept as broad as possible to limit the amount of false positives and negatives.

[21] While newspapers.com contains nearly 300 million digitized pages from over 5,000 newspapers, it does not include the universe of all U.S. papers nor the full set of pages in each sample paper.

[22] Analysis including previously covered areas is discussed in the Appendix and produces consistent results.

$$(1.1) \qquad y_{c,t} = \delta_c + \delta_{s,t} + \sum_{\tau \neq 1972} \beta_\tau I_{\tau,t} \times PC_c + \gamma_1 bilingual_{c,t} + \varepsilon_{c,t},$$

where $y_{c,t}$ is the outcome of interest in county c during the presidential election in year t. δ_c and $\delta_{s,t}$ are county and state-year fixed effects, respectively. The coefficients of interest β_τ correspond to the interaction between a set of year indicators, $I_{\tau,t}$, and a treatment group dummy, PC_c, equal to 1 for those counties covered under the 1975 VRA. I control for the VRA's concurrent introduction of language minority protections by including $bilingual_{c,t}$, a dummy variable set to unity if county c is subject to bilingual election requirements in year t. Observations are weighted by eligible voters for turnout and by ballots cast for Democratic vote share. Standard errors are clustered at the state-level, allowing for correlation of errors between observations within the same state.

The inclusion of county and state-year fixed effects controls for static differences in outcome between counties as well as any time-varying, state-level shocks. The latter are important for accounting for the timing of state elections (such as for governor), for the passage of relevant policies by states (which wield signficant electoral influence in the American federalist system) and for any other threats due to underlying state-level trends (such as in demographics, political advertisement, etc.). In the Section IV, I test alternative specifications replacing state-year fixed effects with year and Census division-year controls and find similar results.

While DD models do not require random assignment or that treatment and control groups "look similar," identification relies crucially on a parallel trends assumption. Though there is no way to prove the existence of parallel trends in the counterfactual, the estimates of β_τ for $\tau < 1972$ allow me to test for common pre-treatment trends between treatment and control groups. As demonstrated below, these estimates are close to zero and insignificant in the majority of the models presented. In Section IV, I further validate my findings using a simple regression discontinuity design, which does not require parallel trends for identification.

IV. Results

A. Voter Turnout

Figure 1.3 depicts the coefficients of interest and confidence intervals from estimation of Equation 2.1 on voter turnout. These results are also shown in Table 1.2, Column 1. Each point estimate represents the weighted-average difference in turnout between treatment and control counties in that year relative to the same difference in 1972, the last presidential election prior to treatment. In support of parallel trends, the pre-treatment estimates (1960-1972) are close to zero in magnitude. These estimates are also statistically insignificant at the 5 percent level, except for 1968, which — though similar in magnitude — has appreciably smaller standard errors than the other pre-treatment estimates. As shown in Table 2.8 of the Appendix, this coefficient is *not* statistically different from zero when estimated with state and year multi-way clustered, county-clustered, heteroskedasticity-robust or wild-t bootstrapped standard errors.[23] Furthermore, all of the pre-1972 coefficients are insignificantly different from each other (Wald test: 1960 = 1968, $p = .927$; 1960 = 1964, $p = .585$; 1964 = 1968, $p = .259$). Taken together, these estimates indicate the similarity of time trends between groups prior to treatment.

[Figure 1.3 about here.]

Following the implementation of oversight restrictions in 1975, average turnout among covered jurisdictions, relative to uncovered counties, gradually increased. These effects are modest at first (2.1 percentage points in 1976), peaking more than twenty years after implementation (8.3 p.p. in 2000) before leveling off. Notably, all post-treatment coefficients are positive and statistically significant at the 5 percent level, demonstrating the persistence of preclearance's impact over several decades. To highlight these broader time dynamics, Table 1.A2 displays the treatment effects averaged across the short- (1976-1988), medium- (1992-2004), and long-runs (2008 onwards).[24]

[23] State-clustered standard errors are smaller for DD coefficients near the treatment date, as compared to other specifications, but larger in later years and are thus the most conservative method of estimating post-treatment effects.

[24] Specifically, I estimate Equation 2.1 replacing the full set of treatment-year interactions with interactions between treatment and a set of period indicators ($I_{\tau_1-\tau_2,t}$), where $I_{\tau_1-\tau_2,t}$ is set to 1 for years between τ_1 and τ_2 and 0 otherwise.

Again, analysis suggests that preclearance led to stable gains in turnout of nearly 8 p.p. over the past two decades.

The gradual and lasting nature of these effects makes sense in context of the policy itself. Because preclearance oversight did not expand the franchise *per se*, there is little reason to expect large increases in voter turnout immediately following its implementation. Instead, the restrictions were designed to limit *future* constrictions of minority voting. Thus, as evidenced here and by the Harris County case study, the benefits of preclearance accumulate in relation to a counterfactual in which new discriminatory changes are enacted over time. Furthermore, because proposed voting changes were only granted preclearance if they were determined not to harm minority representation relative to the *status quo*, any incremental gains in minority turnout that acrued over time served to raise the standard of comparison for all proposed voting changes in the future, essentially locking in the effects from one year to the next.[25]

The long-run effects are large. The 2012 point estimate of 8.1 p.p. represents 15 percent of average turnout in that election (54.9 percent). Though confidence intervals also include more modest gains ranging from less than 1 p.p. to roughly 4 p.p., the estimated treatment effects are in range of those identified by Filer et al. (1991) and Highton (2004) for banning literacy tests (2 to 9 p.p.) and poll taxes (13 p.p. to 15 p.p.). As I will demonstrate later, I find larger effects among minority populations, demonstrating the sizable impact that preventative anti-discrimination measures can have.

As shown in Table 1.2, these estimates are robust to the inclusion of flexible controls for historical demographic predictors of civic engagement (Smets and Van Ham, 2013). Column 2 displays estimates after controlling for pre-treatment differences in income, eduction and minority share (i.e. by including interactions between a full set of year indicators and county-level measures of non-white population share, college-educated population share and average income in 1970) and voter eligibility (i.e, by interacting year with historical shares of 18- to 21-year-olds, who were not enfranchised until the passage of the 26th Amendment in 1971, and of military personnel, who may

[25]These effects are also consistent with habit-formation among those voters newly enfranchised by preclearance protections. See Gerber et al. (2003); Madestam et al. (2013); Fujiwara et al. (2016).

not have been able to vote in their county of residence due to deployment during the Vietnam War). Including these controls decreases the magnitude and significance of all pre-treatment coefficients. Though the post-treatment coefficients also decrease in magnitude by about one-third, they remain highly significant, suggesting large treatment effects even when accounting for unobserved trends in, for example, youth activism or minority mobilization.

Column 3 of Table 1.2 shows similar results after limiting the sample to counties near the coverage cutoffs — specifically, those with 1972 turnout between 40-60% and 1970 language minority share between 0-10%. These estimates are also plotted in Figure 1.A1. Due to noise in the determination measures and non-linearities in the coverage formula, treatment was plausibly exogeneous among this restricted sample. In support of this, all of the pre-treatment coefficients are precise zeros. However, following treatment, I again find gradual increases in voter turnout that persist for several decades. These coefficients are similar in magnitude to those in Column 2 and the majority are statistically significant at the 5 percent level, suggesting that preclearance had lasting effects on covered counties, even when comparing politically and demographically similar areas.

Table 1.A3 corroborates these findings by extending the analysis to include non-presidential elections. Prior to treatment, I find little evidence of differential pre-trends, despite shifting the omitted year to 1974, just one year before the Act's passage. Following treatment, I find larger gains in turnout for midterm elections than for presidential elections. Indeed, the average treatment estimate for the former (6.1 p.p.) is roughly 20% larger than the latter (5.2 p.p.).[26] This is consistent with the fact that most Section 5 objections were lodged against local voting changes and suggests that preclearance's effects on enfranchisement may have extended far beyond presidential turnout.

As I discuss in Section IV, the estimated treatment effects are robust to a host of different specifications and controls and are unlikely to be caused by selection bias or unobserved demographic or political trends. The validity of my model and results are also supported by alternative estimation using regression discontinuity. In sum, these analyses reinforce a causal interpretation of the coefficients of interest, which demonstrate that preclearance protections contributed to long-run

[26]Turnout is defined as the maximum voter participation rate among Presidential, Senate, House and gubernatorial elections taking place in a given county on a given Election Day.

gains in voter turnout of 4 to 8 p.p.

Turnout by Race

Using state-level data from the CPS voter supplement, I assess the policy's effects on minority turnout. Here, I exclude all states that were wholly-covered prior to 1975. This drops those Southern states that fell under coverage in 1965 due to their use of literacy tests.

I then use a state-level DD model to compare changes in race-specific turnout over time between covered and uncovered states. In particular, I estimate the following state-level analogue of Equation 2.1 for white and non-white turnout, separately:

$$y_{s,t} = \delta_s + \delta_{d,t} + \sum_{\tau \neq 1972} \beta_\tau I_{\tau,t} \times PC_s + \varepsilon_{s,t}, \tag{1.2}$$

where, $y_{s,d,t}$ represents turnout among whites (non-whites) in state s at time t. PC_s is a treatment state dummy set to 1 if the entire state was subject to preclearance starting in 1975 (i.e. Texas or Arizona). As observations are at the state-level, I am no longer able to include state-year fixed effects. Instead, I include state and Census division-year indicators, which account for level differences in turnout between states as well as time-varying regional differences due to, for example, trends in migration. Observations are weighted by each state's voting age population of whites (non-whites) and standard errors are clustered at the state-level.

To help mitigate any state-level confounds, I also estimate Equation 1.2 controlling for the presence of other state-wide elections (by including indicators for gubernatorial and Senate elections in a given state-year) as well as for county-level bilingual restrictions (by including a dummy set to 1 if a state contains any county that is subject to bilingual elections in that year).

[Figure 1.4 about here.]

Estimates of β_τ from Equation 1.2 are plotted in Figure 1.4 and displayed in Table 1.3, separately for whites and non-whites.[27] As shown in the right panel of Figure 1.4, I find little

[27]Treatment estimates averaged across period are also shown in Table 1.A4.

evidence of differential pre-trends between minorities in treatment and control states, as the 1968 DD estimate is insignificantly different from zero. However, following implementation, I find that minority turnout increased steadily over time in covered states, relative to uncovered states, before regressing modestly in recent elections. These results are striking both for their size — the 2012 estimate of 17 p.p. represents 30 percent of average minority turnout (48.5 percent) and is bounded below at 7 p.p. — and for their consistency — all post-treatment coefficients are positive and the majority are highly significant (F-test of joint significance yields $F = 3.7 \times 10^5, p < 0.001$).

These estimates complement the county-level results presented earlier. As before, the coefficients increase with time before leveling off. Furthermore, as minorities comprised roughly 30% of the population in treatment states, the average treatment estimate of 14.2 p.p. (averaging all post-treatment coefficients from Column 3 in Table 1.3) translates to a net gain in turnout of 3.7 p.p., roughly similar to the corresponding 4.8 p.p. increase recovered from the county-level analysis (Column 1 of Table 1.2).[28]

As the left panel of Figure 1.4 shows, I find no effect on white turnout. None of the post-treatment coefficients are statistically significant and most are near zero in magnitude. That I find no significant effects on white turnout is consistent with preclearance's objective to bolster minority participation. As whites historically controlled over 98% of city councils, it is also consistent with findings by Trebbi et al. (2008) and Hajnal et al. (2017) that strategic manipulations of electoral rules by local governments are intended to disenfranchise minorities, specifically.[29] Finally, the null effects for whites imply that the minority turnout estimates are not driven by unobserved threats, as such factors would have had to differentially affect not only treatment states relative to control states in the same Census division-year, but also minorities relative to whites within those states.

In Table 2.A7, I validate the above results using a treatment intensity variable equal to the proportion of each state's population residing in covered counties. Again, I find no effects on white

[28]While the county-level results presented in Table 1.2 are essentially identified off of counties in partially-covered states, robustness analysis excluding state-year fixed effects shown in Table 1.B6 demonstrate similar effects among the entire treatment group (i.e. including Texas and Arizona).

[29]Information on the racial make-up city councils comes from 1980 ICMA data, the first year for which breakdowns of city council seats by race are available.

turnout, as the post-treatment estimates are inconsistent in sign and never rise above 0.7 p.p. in magnitude. However, following treatment, I find gradual gains in minority turnout as large as 14 p.p. While these estimates are not individually significant due to the bimodal distribution of the treatment intensity variable, they remain jointly so ($F = 52.7, p < 0.001$).[30] Taken together with the county-level analysis presented earlier, these results suggest that the increased turnout observed under preclearance coverage was driven by large and lasting gains in minority participation.

B. Democratic Vote Share

In line with theories of identity and distributive politics, enfranchisement has been shown to increase political and economic representation for members of marginalized groups (Pande, 2003; Olken, 2010; Cascio and Washington, 2014; Fujiwara, 2015).[31] To the extent that political preferences of whites and minorities differ, changes in minority enfranchisement may alter the net balance of support between political parties. Since to the passage of the Civil Rights Act of 1964, minorities have overwhelmingly voted Democratic (Bositis, 2012).[32] In the 1972 presidential election, Democratic nominee George McGovern received 87 percent of non-white votes, but only 32 percent of white votes. Thus, *ceteris paribus*, one might expect that preclearance coverage, in bolstering turnout among minorities, would also increase Democratic support.

To assess this hypothesis, I examine preclearance's effects on the share of votes cast for Democratic candidates. The coefficients and confidence intervals from estimation of the county-level DD model (Equation 2.1) including demographic controls are displayed in Figure 1.5 and Column 5 of Table 1.2. As with the voter turnout results, I find little evidence of differential trends between treatment and control groups prior to the passage of the 1975 VRA. Two of the three pre-treatment estimates are less than 1 p.p. in magnitude and all are insignificantly different from

[30] $PCintensity_s$ is distributed with a mass point at 1 and all the remaining values clustered below .20, suggesting that the binary treatment variable may be more appropriate.

[31] For theoretical work, see Cox and McCubbins (1986), Lindbeck and Weibull (1987), Osborne and Slivinski (1996), Besley and Coate (1997) and Dixit and Londregan (1998).

[32] While this was part of a larger national trend dating back to the New Deal, data on regional minority voting prior to the 1960's is scarce (Black, 2004) and some historians have credited Southern blacks with helping Dwight Eisenhower win the presidency in 1952 and 1956 (Strong, 1971).

zero, even at the 35 percent level.

[Figure 1.5 about here.]

Following implementation, I find significant *decreases* in Democratic vote share among counties subject to Section 5 coverage. Roughly half of the post-treatment estimates are significant at the 5 percent level, and an F-test of their joint significance rejects the null at the 1 percent level ($F = 36.41, p < 0.001$). The point estimates indicate that, among covered counties, average Democratic support dropped by 2.6 percentage points in 1976 and by 5.3 p.p. in the tightly contested 2000 election before recovering in recent years. Though confidence intervals include more modest effects ranging from 0.8 p.p. in 1976 to 1.5 p.p. in 2000, these magnitudes are nonetheless politically relevant. The mean treatment estimate of 3.2 p.p. is greater than half the average presidential margin of victory since 1975 (5.7 p.p.).

As shown in Table 1.2, I find similar (though larger) effects even without demographic controls. Under the base model (Column 4), all pre-treatment coefficients are statistically insignificant at the 5 percent level. Though the 1960 and 1964 coefficients are relatively large in size, average party support differed by less than one percentage point between treatment and control counties in each election prior to 1972. Rather than indicating differential trends, the pre-treatment coefficients likely reflect large group differences (of 7 p.p.) in Democratic support during the omitted election of 1972, a historical outlier and the largest presidential landslide in U.S. history.[33]

As shown in Column 5, after accounting for the influence of the Vietnam War and 26th Amendment on the 1972 election by controlling for historical military and youth population shares, I find little evidence of differential group pre-trends. These estimates also control for non-white population share in 1970, suggesting that trends in racial polarization are unlikely confounds. Even among a restricted sample of counties near the coverage cutoffs, I find large and significant decreases

[33]Democratic nominee George McGovern received only 3 percent of the electoral vote and 18 million fewer popular votes than Republican incumbent Richard Nixon. As political historians have noted, the Vietnam War and the 1971 ratification of the 26th Amendment made "the 1972 election very different from the [previous] presidential elections" (Miller et al., 1976). Prior to America's withdrawal in 1973, over 2.7 million Americans had been deployed to Vietnam. As legislation guaranteeing absentee ballots for military personnel was not passed until 1986, many of these service members were unable to vote. On the other hand, the 26th Amendment allowed 10 million Americans between the ages of 18 to 21 to cast a presidential ballot for the first time in 1972.

in Democratic vote share immediately after treatment (Column 6 and Figure 1.A1). As I discuss Section IV, the observed effects are robust to numerous alternative specifications — including flexibly controlling for Democratic support in 1972 — and translated to significant changes in Congressional representation. Taken together, these results suggest that preclearance protections meaningfully influenced party support in covered areas. Across the three models, I find immediate effects on Democratic vote share ranging from 2.5 to 3 p.p. that persisted for multiple decades.

The direction and timing of these effects is surprising. Despite increased turnout among Democratic-leaning minorities, I find that oversight restrictions led to *decreased* net support for Democratic candidates. Furthermore, whereas voter protections produced gradual gains in turnout, vote shares immediately shifted in response to coverage. In light of the historical controversy surrounding preclearance's implementation, this partisan swing was not simply a second-order effect of changes in the voter base. Indeed, as I demonstrate in the following subsection, political backlash against Section 5 restrictions likely played a direct role in decreased Democratic support.

Party Affiliation by Race

To examine heterogeneous changes in political preferences by voter race, I rely on individual-level, time-series data from the American National Election Studies. In particular, I investigate responses to a series of questions regarding self-identified party affiliation and support for government aid to minorities.

Thus, I estimate the following DD model — separately for whites and non-whites — comparing political preferences before and after 1975 between individuals in treatment and control counties:

$$y_i = \delta_c + \delta_{d,t} + \beta PC_c \times Post_t + \varepsilon_i. \tag{1.3}$$

Here, y_i is the response of individual i in county c of Census division d during year t, and $Post_t$ is an indicator equal to 1 for years after 1975. Though observations are at the individual-level, I am unable to estimate β if both county and state-year fixed effects are included, because of insufficient within-state treatment heterogeneity in the survey sample. Instead, I include county and Census division-year fixed effects. Standard errors are clustered at the state-level.

[Table 1.4 about here.]

Table 1.4 displays the coefficient and standard error for β from estimation of Equation 1.3 on a number of survey responses. As Column (1) of Panel A shows, following treatment, whites in covered counties were significantly more likely to identify as weakly (7.4 percentage points) or strongly Republican (5.2 p.p). Non-whites, on the other hand, were significantly less likely to identify as Republican. As shown in Column (2), these effects are robust to the inclusion of individual-level demographic controls (i.e., age, education, income and military service). In support of the model's validity, Figure 1.6 displays results using a flexible DD design and shows no evidence of differential pre-trends in party affiliation among either race group.

[Figure 1.6 about here.]

These ethnicity-level results align neatly with the county-level effects presented earlier. As whites comprised roughly 80 percent of voter base in the ANES sample, a 7.4 p.p. increase in Republican vote share among this group would more than offset any concurrent Democratic gains from increased minority turnout or support. Indeed, the partisan shift among whites translates to a roughly 6 p.p. decrease in county-wide Democratic vote share, while the combined mobilization and party preference effects among minorities correspond to a 4 p.p. increase in Democratic support.[34] Thus, on net, the race-level estimates for turnout and party affiliation predict an approximately 2 p.p. decrease in Democratic vote share, in range of the average treatment effect of 3 p.p. found in the county-level analysis (i.e., averaging the 1976-1996 DD coefficients for Democratic vote share from Table 1.2, Column 5).

[34] Roughly, the change in net vote share can be decomposed as the net change due to party-switching among whites ($.80 \times -7.4 = -5.9$) and minorities ($.20 \times 12 = 2.4$), holding turnout constant, plus the differential change due to increased minority turnout of approximately 1.5 p.p. This last estimate uses the 8.8 p.p. (17 percent) average increase in minority turnout from 1976-1996 (Column 4 of Table 1.3) and average pre-treatment Democratic vote share of 35% (Table 1.A1.)

In Panel B of Table 1.4, I examine questions concerning whether the government should help to "improve the social and economic positions" of minorities. Respondents were asked to state their own preferences — scaled from 1 (government should help minorities) to 6 (government should *not* help minorities) — as well as their beliefs about the preferences of the Republican and Democratic parties. Following treatment, I find that whites in covered counties were significantly more likely to *oppose* government aid to minorities, while non-whites were insignificantly more likely to *support* it. However, consistent with the VRA's contentious legislative history, both groups report a growing partisan divide over the government's position towards minorities.

These findings suggest that racial attitudes may have played an important role in explaining the partisan shifts found in covered areas. To test this, I examine changes in Republican identification by interacting preclearance coverage with opposition to minority aid. In particular, I estimate the following equation, separately for whites and non-whites:

$$\begin{aligned} Rep_i = &\delta_c + \delta_{d,t} + \beta_1 NoAid_i + \beta_2 Post_t \times NoAid_i + \beta_3 PC_c \times NoAid_i \\ &+ \beta_4 PC_c \times Post_t + \beta_5 PC_c \times Post_t \times NoAid_i + \varepsilon_i, \end{aligned} \tag{1.4}$$

where Rep_i is an indicator for whether respondent i self-identifies as weakly or strongly Republican and $NoAid_i$ is an indicator equal to 1 if she opposes government aid to minorities.

The results are shown in Column 1 of Table 1.5. Examining whites, the DD coefficient (β_4) on $PC_c \times Post_t$ is small and statistically insignificant, suggesting that preclearance had little effect on the party affiliation of those who favored minority aid. However, both the triple-difference coefficient (β_5) on $PC_c \times Post_t \times NoAid_i$ and the sum of β_4 and β_5 are large, positive and highly significant, indicating that whites opposed to government support for minorities were significantly more likely (12 p.p.) to identify as Republican following coverage. On the other hand, I find that non-whites in covered areas were significantly *less* likely to identify as Republican following treatment and that this effect was largest among those opposed to minority aid. However, given that the pre-treatment sample includes only eight non-whites in the PC_c-$NoAid_i$ cell, this last result is likely spurious.

[Table 1.5 about here.]

Please consider these findings with caveats. As noted, the estimates, particularly for non-whites, are derived from small samples. For this reason, I cross-validate the race-level party affiliation analysis using historical Gallup survey data. These results are shown in Tables 1.A7 and 1.A8 and are consistent with the ANES analysis, demonstrating increased polarization between whites and minorities in covered areas that is largely explained by racial attitudes.[35] One may also be concerned that $NoAid_i$ is merely a proxy for conservatism and contains little information about underlying views on race. However, as shown in Column 2 of Table 1.5, controlling for respondents' self-reported conservatism actually increases the magnitude and significance of β_5, suggesting that racial preferences played an important role in party switching.

Taken together, the observed dynamics strongly complement those discussed by Kuziemko and Washington (2015), who find robust evidence that Southern dealignment, the period spanning the Civil Rights era during which the Deep South transitioned from Democratic to Republican, was caused by political backlash among racially conservative whites.[36] Despite examining a different geographic and temporal setting, I find that white racial attitudes similarly explain Democratic defections following the expansion of federal anti-discrimination oversight. These results suggest a causal link between the application of preclearance restrictions to covered counties and decreased Democratic support in those areas, particularly among whites.

V. Robustness

A. Regression Discontinuity

This paper's primary DD design is able to recover relevant treatment effects across a range of individual-, county- and state-level data, the validity of which are supported by consistent

[35] Additional information regarding the Gallup data is included in the Appendix.

[36] That study builds on a large literature in political science demonstrating the salience of racial preferences on vote choice and turnout, such as Key and Heard (1950); Carmines and Stimson (1989); Kousser (2010); Tesler and Sears (2010); Enos (2016).

evidence of parallel pre-treatment trends. Nonetheless, the model's primary identifying assumption is untestable.

One alternative strategy for estimating county-level effects is a regression discontinuity (RD) design. While RD does not rely on parallel trends assumptions, it is only able to recover *local* average treatment effects around the policy triggers, a limitation that is exacerbated by the scarce historical variation around those triggers (i.e., only 16 counties in uncovered states were within 3 p.p. of both the minority and turnout cutoffs). That being said, RD estimates are consistent and allow me to corroborate my findings under alternative assumptions.

Thus, I employ a simple RD design, which relies only on exogeneity around the historical turnout trigger for identification. In particular, I restrict the sample to counties with greater than 5% language minority share in 1970 and estimate the following equation:

$$y_{c,t} = \beta_1 Less50_c + \beta_2 turnout1972_c + \beta_3 Less50_c \times turnout1972_c + \varepsilon_{c,t}. \tag{1.5}$$

Here, $turnout1972_c$ represents turnout in county c in 1972 (normalized to 0 at the 50% cutoff), which is allowed to have different slopes on either side of the discontinuity. The coefficient of interest β_1 represents the effect of crossing the cutoff from the right, which activated preclearance coverage among high-minority counties. Observations are weighted by voting eligible population and votes cast. Bandwidths include all high-minority counties within 20 percentage points of the turnout trigger, except those in the wholly-covered states of Texas and Arizona, where preclearance was determined by state (rather than county) turnout. To account for limited power, I pool observations across all post-treatment years.

[Figure 1.7 about here.]

Panel A of Figure 1.7 displays the relationship between the running variable and average post-treatment turnout and Democratic vote share. Notably, crossing from just above the 50% cutoff to just below it is associated with a significant jump in post-treatment turnout. The point estimate for

β_1 suggests a 6.9 p.p. increase in turnout at the cutoff (Table 1.A9, Column 3). The coverage trigger is also associated with a significant decrease in Democratic vote share of 5.4 p.p. In both cases, the local effects strongly resemble the average effects recovered from the difference-in-differences estimation.

Panel B estimates the same RD model across all *pre*-treatment years. Prior to 1975, no significant discontinuity in turnout or Democratic vote share exists at the coverage trigger. Since preclearance coverage did not begin until 1975, the null effects corroborate the policy's exogeneity. That is, controlling for the assignment variable, there is little evidence that historical turnout or party support differed between treatment counties just below the threshold and control counties just above it. Table 1.A9 displays results using other bandwidths and including controls for year, bilingual restrictions and historical minority share. Across the models, I find consistent support for post-treatment effects and little evidence of pre-treatment discontinuities.

To examine effects over time, Figure 1.A2 and Table 1.A10 display RD coefficients from estimation of Equation 1.5 separately for each election in the sample. Though none of the coefficients are statistically significant due to limited power, their direction, magnitude and dynamics strongly resemble the DD estimates presented in Figures 1.3 and 1.5 with immediate changes in party support and gradual gains in turnout. The long-run similarities are particularly noteworthy. Indeed, the RD coefficients for 2016 turnout (7.0 p.p.) and vote share (-2.3 p.p.) differ by just around one percentage point from their respective DD estimates (7.8 p.p. and -1.2 p.p.).

Taken as a whole, it seems highly unlikely that unobserved confounds could produce nearly identical treatment estimates across two distinct estimation strategies. Thus, as causal identification under regression discontinuity does *not* require parallel trends, these results help to validate the paper's difference-in-differences model and preclearance's effects on county turnout and vote share.

B. Noisy Determination Measures

Because the VRA's coverage formula primarily captured counties with low turnout and high minority shares, one may be concerned that the estimated effects are merely the result of mean reversion or other political trends affecting areas with large minority populations. However, as I displayed in Table 1.2 and Figure 1.A1, I find similar estimates even when restricting the analysis to counties within 5 p.p. of the minority cutoff and 10 p.p. of the turnout cutoff and when controlling for historical demographic differences.

In Column 1 of Table 1.A11, I show that my findings are robust to even further limitations of the sample. Restricting analysis to the 169 counties within 8 p.p. of the turnout cutoff and 4 p.p. of the minority share cutoff, the significance, sign and magnitude of the treatment estimates are largely preserved. These results suggest that preclearance had lasting effects even among counties with similar political and demographic characteristics. As I demonstrate in Column 2, I find consistent, though less significant, effects when analyzing neighboring counties (i.e., restricting the sample only to those treatment counties that are adjacent to at least one control county and to those control counties that are adjacent to at least one treatment county). Thus, the estimated effects are likely not driven by unobserved local trends or the non-random distribution of treatment counties throughout the country.

Another concern may be that Congress strategically crafted the coverage formula to capture specific areas and that selection bias, rather than the policy itself, accounts for the observed changes. Though precise manipulation was impossible since the Census Bureau did not calculate the determination measures until after the Act's passage, policymakers could have used turnout and population measures from prior years as proxies. Thus, in Column 3 of Table 1.A11, I restrict the sample only to those counties with the highest predicted likelihood of coverage, based on county turnout and minority share from the 1960's. Again, I find similar estimates, indicating that the observed effects are the result of actual preclearance coverage as opposed to confounds related to the criteria determining that coverage.

As an alternative method of addressing potential selection bias, I also estimate models flexibly controlling for the coverage determination measures as well as for each county's probability of coverage and historical party support. These results are shown in Table 1.A12. In Column 1 and Figure 1.A3, I display estimation results after including interactions between year indicators and 1972 turnout and 1970 language minority share. In Column 2, I control for year and 1972 Democratic vote share interactions. In Column 3, I flexibly control for predicted likelihood of coverage.

In all cases, I find similar effects to those presented in the main results, though controlling for the coverage determination figures produces smaller turnout estimates. Prior to 1975, I find little evidence of differential trends. After 1975, I find significant increases (decreases) in turnout (Democratic vote share) among treatment counties that again peak roughly twenty years after implementation. These estimates bolster the validity of my main findings and support preclearance's exogeneity. Historical variation in turnout, minority population and Democratic support cannot fully explain the observed treatment effects. Nor do the estimates appear biased by Congressional targeting.

C. Election Outcomes

Given the large observed effects on party support, one would expect that preclearance coverage also impacted election outcomes. Thus, I examine changes in the political ideology of elected Congressional representatives using DW-NOMINATE data. DW-NOMINATE collapses a representative's legislative roll-call voting in a given Congressional session into a two-dimensional ideal point, the first dimension of which is commonly understood to be a measure of conservatism (scaled from -1 to 1) and has been employed by numerous political scientists and economists to examine changes in political ideology over time (Poole and Rosenthal, 2001; Erikson and Tedin, 2015; Gentzkow et al., 2016).[37]

In particular, I estimate a district-level analogue of Equation 2.1 on the Republican affiliation and first dimension DW-NOMINATE scores of House representatives. Treatment is defined as districts containing at least one covered county. Because district boundaries change over time, I

[37]While the second dimension of DW-NOMINATE historically tracked to policy issues that cut across party lines, such as bimetallism and slavery, Congressional voting since the 1960's has been virtually unidimensional (McCarty et al., 1997). As Bateman and Lapinski (2016) note, "this measure bears little relationship to patterns of voting on civil rights" and, during the 1970's, second dimension scores of Southern Republicans were actually higher than those of Northern Democrats but lower than those of Southern Democrats. Given the difficulty of interpreting these scores, results from their analysis, which reveal no clear or consistent treatment effect, are left to the Appendix.

include fixed effects for the counties that comprise each district as well as for Census division-Congress.[38]

These results are shown in Figure 1.A4 and Table 1.A13. Prior to treatment, I find no evidence of differential trends in conservatism or party affiliation between covered and uncovered districts. After preclearance's expansion, the probability of electing a Republican representative spiked among newly covered districts by as much as 35 p.p. Similar to the Democratic vote share results, this effect recedes over time. However, I observe lasting changes in representative ideology. Immediately following treatment, conservatism jumped significantly in covered districts and continued to increase until the most recent Congress. Taken together, these results corroborate the Democratic vote share estimates and suggest that preclearance coverage may have had large ramifications on policy-making.

D. Other Robustness

The Appendix includes other analyses demonstrating that the observed treatment effects are unlikely to be caused by statistical artifacts or unobserved confounds. As mentioned in Section III, Table 2.8 shows robustness to alternative methods of calculating standard errors. In Table 1.B3 Columns 1 and 2, I use randomized treatment placebos to provide evidence that the effects were not caused by serial correlation. Column 3 and 4 of the same table further demonstrate that treatment and control areas did not experience differential trends in population or minority share over time. In Table 1.B4, I find that placebo tests based on "faux" treatments cutoffs around the actual trigger fail to produce similar post-treatment estimates. Table 1.B5 shows that the inclusion of time-varying demographic controls and alternative controls for bilingual language restrictions does not alter the sign, magnitude or significance of my primary estimates. Table 1.B6 shows that treatment effects are largely invariant to alternative area-time fixed effects. Finally, Table 1.B7 demonstrates similar results when examining other measures of turnout and party vote share.

[38]Counties are mapped to districts using relationship files generously provided by James Snyder, as well as hand-coded information from the Congressional District Atlas. The Appendix includes additional discussion of the DW-NOMINATE data and its construction.

VI. Mechanisms

A. Voter Turnout

Electoral Rules

As preclearance's enforcement depended on the subjective analysis of context-specific voting changes across a wide range of election rules, a comprehensive accounting of its effects on electoral policymaking is impractical and neglects the policy's *raison d'etre*. Nonetheless, better understanding the mechanisms behind minority turnout gains is vital to future policy efforts to safeguard enfranchisement.

Thus, utilizing data from the International City/County Municipal Association, I assess preclearance's effects on the prevalence of one particular type of electoral rule: at-large election systems. In prior research, Trebbi et al. (2008) found that white city councils respond to minority political threats by switching from district-based systems, in which council seats are reserved by neighborhood, to at-large or "winner-take-all" systems, which may dilute minority voting power by awarding council seats based on city-wide voting. As proposed changes to at-large systems comprised the plurality of Section 5 objections until the 1980's, examining their prevalence may provide insight into preclearance's broader effects on voter discrimination.

Using a municipality-level analogue of Equation 2.1, I estimate the effects of preclearance coverage on whether a city employed at-large elections and on the share of a city's council seats awarded by those elections. These results are shown in Figure 1.A5 and Table 1.A14.

From 1970 to 1975, I observe no differential change in the use of at-large elections between treatment and control cities. However, by 1980, covered areas were significantly less likely to employ at-large elections, relative to uncovered areas. After treatment, these cities also exhibited significant decreases in the share of seats elected at-large. Notably, these results mirror those observed in the voting analysis, with gradual effects that peak more than a decade after preclearance's implementation.

The same logic explaining the changes in turnout applies even more directly here. Because preclearance restrictions precluded many covered cities from ever implementing at-large systems, the gradual adoption of "winner-take-all" rules in uncovered cities would produce growing relative differences between the two groups. Though at-large systems are but one margin by which local officials may influence the election process, these findings are strongly suggestive of preclearance's prophylactic effect on voter discrimination and are consistent with the observed increases in minority turnout.

Historical Discrimination

To further investigate the role of voter discrimination, I examine whether preclearance's effects on turnout varied according to historical racial disparities in an area. Specifically, I modify Equation 1 to estimate the following linear triple-differences (DDD) model:

$$\begin{aligned} y_{c,t} = & \delta_c + \delta_{s,t} + \beta_1 PC_c \times Post_t + \beta_2 PC_c \times Post_t \times discriminate_{c,1970} \\ & + \sum_{\tau \neq 1972} \lambda_\tau I_{\tau,t} \times discriminate_{c,1970} + \gamma_1 bilingual_{c,t} + \varepsilon_{c,t}, \end{aligned} \tag{1.6}$$

where $discriminate_{c,1970}$ is a defined as one of three predictors of pre-treatment racial discrimination in county c: the ratio of average white to minority income, the difference between minority and white poverty rates and the difference between minority and white illiteracy rates (Loury, 1977; Williams, 1999; Farkas, 2003). Interactions between $discriminate_{c,1970}$ and year account for time-varying differences due to historical racial disparities, while the DDD coefficient (β_2) represents heterogeneous treatment effects based on discrimination, averaged over all post-treatment years. For ease of comparison, I employ a single pre-post DD coefficient (β_1) demonstrating average treatment effects among areas with no historical discrimination.

Table 1.A15 displays the coefficients and standard errors for β_1 and β_2 from estimation of Equation 1.6 including demographic controls. Consistent with my main findings, preclearance significantly increased voter turnout in covered areas. However, I find larger treatment effects among areas with greater historical racial disparities in income, poverty and education, though

this last differential is not statistically significant. These differences are large relative to the base treatment effects (β_1). Compared to treatment areas with no historical discrimination, predicted turnout gains for counties with mean historical income, poverty and education disparities are 22%, 83% and 20% greater, respectively.[39] In line with preclerance's objective, these results suggest that reduced voter discrimination contributed to the observed turnout effects.

B. Democratic Vote Share

Media Coverage

Though the role of civil rights legislation on party-switching is supported by the ANES analysis presented in Section III as well as by other research (Wattenberg, 1991; Valentino and Sears, 2005; Kuziemko and Washington, 2015), I provide further evidence of preclearance's political salience using historical newspaper data. Throughout the 20th century, newspapers were not only the primary source of information regarding local and state politics but also partisan agents themselves (Hamilton, 2004; Strömberg, 2004; Gentzkow et al., 2006, 2011). Thus, if preclearance was an important political issue for voters in covered areas, one might expect to see differential changes in media coverage of the VRA around its enactment.

To this end, I estimate changes in media mentions of the VRA using a newspaper-level analogue of Equation 2.1 controlling for paper and region-time effects. To account for attrition within the newspapers.com data, the outcome of interest is defined as the ratio of VRA mentions to the total number of digitized pages in a paper-year. These results are displayed in Figure 1.A6 and Table 1.A16.

Notably, all pre-treatment coefficients after 1970 are precise zeros, suggesting common trends in press coverage immediately prior to the expansion of Section 5 restrictions. However, beginning in 1975, the number of VRA mentions increased sharply among newspapers located in newly covered areas. Relative to the control group, treatment newspapers referenced the Act roughly once more per dozen pages in 1975. This increased media attention persisted for several

[39]Based on average treatment income ratio of 2.1, poverty gap of 27.4 p.p. and illiteracy gap of 8.1 p.p.

years, as evidenced by the positive coefficients from 1976 to 1979.

A few points are worth noting here. First, the VRA was a politically salient topic throughout the sample. Prior to 1975, papers mentioned the Act about once every 15 pages, indicating that the public was aware of the civil rights legislation and that it was a non-trivial issue. Second, the increased media coverage due to treatment was quite large, as the point estimate for 1975 (.08) exceeds the pre-treatment mean.[40] Last, the small, negative coefficients for 1965 and 1970 — when the VRA was first enacted and extended, respectively — suggest that the 1975 spike was driven specifically by discussion of preclearance and its application to covered areas, as opposed to more general aspects of the Act.

Though rigorous text analysis is outside the scope of this study, I examine partisan responses to the VRA by exploiting heterogeneity in newspaper endorsements of President Nixon in 1972. During Congressional debate over the Act's 1970 extension, Nixon's administration proposed eliminating preclearance restrictions entirely. Though the proposal ultimately failed, House Republicans overwhelmingly voted in its favor.[41]

[Figure 1.8 about here.]

As shown in Figure 1.8, media coverage of the VRA spiked among all treatment papers, regardless of political affiliation. However, increases in VRA mentions were twice as large among papers that endorsed Nixon as those that did not (i.e., comparing the 1975 point estimates of .146 and .063, respectively). Furthermore, heightened media attention of the VRA persisted for many years among Nixon-endorsing papers, but quickly dissipated among others. Given that partisan media often reflects the preferences of its audience (Mullainathan and Shleifer, 2008; Gentzkow and Shapiro, 2010; Gentzkow et al., 2014), these findings suggest that preclearance remained a political flashpoint years after its implementation and corroborate the existence of conservative backlash against the legislation.

[40] Though it is possible that control papers also served readers in treatment areas (and vice versa), the dataset is primarily comprised of local papers, which distribute a large majority of copies to readers in the same county (Gentzkow and Shapiro, 2010). If spillover effects do exist, they would likely bias my estimates towards zero, suggesting that the observed effects may represent a lower bound of actual disparities in local public and media focus on the VRA.

[41] For a comprehensive legislative history of Section 5, see Kousser (2007)

Political Contact

To further examine the role of political and media exposure on party-switching, I return to the ANES survey data. Specifically, I estimate the simple DD model presented in Equation 1.3 on whether respondents were contacted by a political party "to get them to vote for their candidate" and on the number of forms of media they reported reading, seeing or hearing about an election. These estimates are presented in Table 1.6.

[Table 1.6 about here.]

Examining whites (Column 1), I find no effect of Section 5 coverage on media exposure, suggesting that the observed spike in newspaper mentions of the VRA was not driven by a broader growth in political media. However, whites report significant increases in political contact following treatment. Though outreach by both major parties increased, changes in Republican contact (13.2 p.p.) were more than twice as large as those in Democratic contact (6.1 p.p.). As Column 2 demonstrates, I find no evidence of increased exposure to political targeting or media among minorities. Following treatment, non-whites report significantly *reduced* election media consumption and Republican contact and no change in Democratic contact.

Taken together, these results help to explain both the turnout and vote share effects. That non-whites experienced less political and media exposure after treatment suggest that the observed gains in minority turnout were the result of actual changes in voter protections and ballot access, as opposed to increased political campaigning and outreach. Furthermore, the differential increases in Republican targeting of whites are consistent with the rightward shift observed among treatment areas and accord with a theory of white backlash against federal oversight restrictions.

VII. Discussion

In light of the 2013 Shelby decision, determining whether historical gains in turnout will persist without continued federal oversight of election laws is critical to the future of the Voting Rights Act. I provide preliminary insight into this issue by examining the 2016 presidential election.

In particular, I employ a flexible, county-level DD model similar to that used in the paper's primary analysis (Equation 2.1), except here the treatment group is expanded to capture *all* counties freed from federal oversight by the Shelby ruling — including those initially covered by the 1965 and 1970 VRAs — and the sample period is shifted to examine elections after 1965, when preclearance was first implemented.[42] These estimates are plotted in Figure 1.9 and displayed in Column 1 of Table 1.7. In interpreting the results, note that the omitted year is 2012. Thus, negative DD coefficients for years before 2012 indicate differential *increases* in turnout among treatment counties from prior elections to 2012, while a negative coefficient for 2016 indicates a differential *decrease* in turnout from 2012 to 2016.

[Figure 1.9 about here.]

Prior to the Shelby decision, treatment areas experienced larger historical increases in turnout, relative to control counties. These results are reminiscent of the main estimates presented in Section III and suggest that preclearance had similar effects on areas covered by prior VRAs as on those covered by the 1975 revision, a claim I further support in Table 1.B8 of the Appendix. Following the Shelby decision, treatment counties experienced a significant differential *decrease* in turnout of 1.5 percentage points, the single largest year-to-year drop in the sample. Replicating this analysis on state-level turnout by race (Columns 2 and 3 of Table 1.7), I find that, while white turnout remained unchanged, minority participation dropped by 2.1 p.p. after the Shelby ruling.

These effects are only suggestive and are presented without claims of causality. Nevertheless, the change in direction around the ruling — whereas turnout in covered counties was differentially increasing prior to 2012, it differentially decreased following 2012 — implies that recently enacted election laws may have negated many of the gains made under preclearance. Evidence that participation decreased only among minorities further bolsters federal claims regarding the targeted and discriminatory nature of these laws. While the true impact of the Supreme Court's decision

[42] As the 1965 VRA brought under coverage many states in their entirety, I also modify Equation 2.1 to include Census division-year fixed effects in place of state-year fixed effects, which would otherwise absorb all the treatment variation from those areas.

may not be known for several years, these results provide early evidence that the Shelby ruling may jeopardize decades of voting rights progress.

VIII. Conclusion

This study exploits the 1975 revision of the Voting Rights Act to identify the causal effects of preclearance restrictions. The estimated gains in voter turnout are large — ranging from 4 to 8 percentage points — and lasting — having persisted for 40 years. Importantly, I find that increases in turnout were driven entirely by participation among minorities and provide evidence that these gains were facilitated by reduced voter discrimination. These results are the first estimates of preclearance's impact and demonstrate the effectiveness of broad, prophylactic anti-discrimination measures.

Surprisingly, I find that preclearance led to net *decreases* in Democratic support due to defections among whites, particularly those opposed to government support for minorities. This effect is corroborated by newspaper analysis demonstrating the political controversy surrounding oversight restrictions in newly covered areas. These findings complement the existing literature by illustrating the political importance of "white backlash" against minority threats in other contexts and settings.

This paper suggests several additional areas of research. Most obviously, updating the analysis following future elections would allow researchers to better understand the ramifications of the Shelby County decision. Alternatively, as elections of school boards, sheriffs, judges and mayors are often decided by small blocs of voters, examining local settings could shed more light on preclearance's economic and political implications. A closer investigation of the interplay between race, media and politics would also be an important area of study, especially considering the salience of race in public and political discourse during and after the 2016 presidential election.

The preclearance process itself poses interesting research questions. Descriptive statistics show a shift in the types of changes submitted over time, as well as a decreased propensity of objection. Though Chief Justice Roberts took the latter as "illuminating" evidence that covered counties had become less discriminatory, this interpretation elides the possibility of strategic interactions between local election officials and federal supervisors in the costly submissions and approvals process.

Perhaps most importantly, this paper provides valuable information for policymakers as Congress considers if and how to reinstate preclearance restrictions. In explaining the Court's decision to strike down the previous coverage formulas, Chief Justice Roberts noted that "voter turnout and registration rates now approach parity" between covered and uncovered jurisdictions. Yet, the relevant question in determining preclearance's fate may not be whether covered and uncovered jurisdictions *are* different today, but whether they *would have been* different without preclearance. This paper provides the first insight into just such a counterfactual.

Chapter 1, in full, has been submitted for publication of the material as it may appear in American Economic Journal: Applied Economics, 2018, Ang, Desmond, American Economic Association, 2018. The dissertation author was the primary investigator and author of this paper.

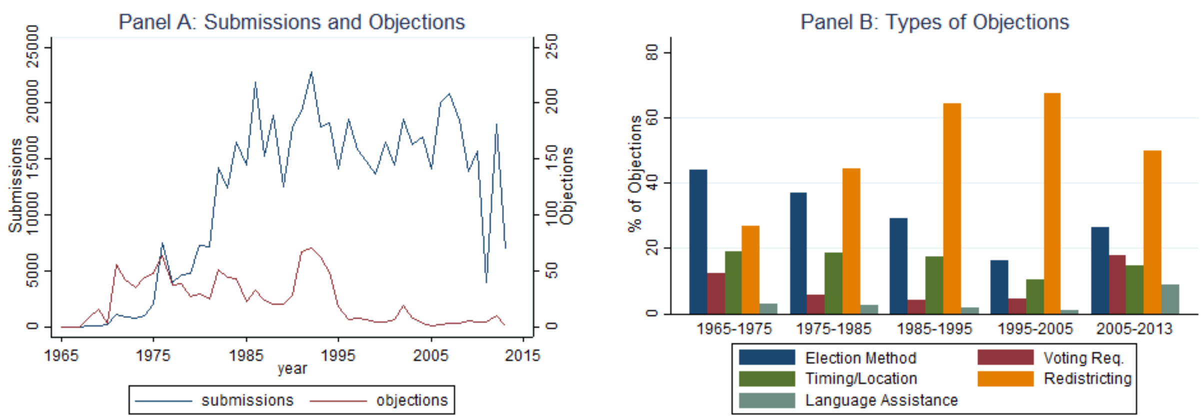

Figure 1.1. Preclearance Enforcement over Time

Notes: Data come from U.S. Department of Justice and author's calculations. Panel A depicts the number of preclearance submissions and objections over time. Panel B depicts the types of objections lodged by decade. For example, over 40 percent of objections from 1965-1975 pertained to changes in election methods.

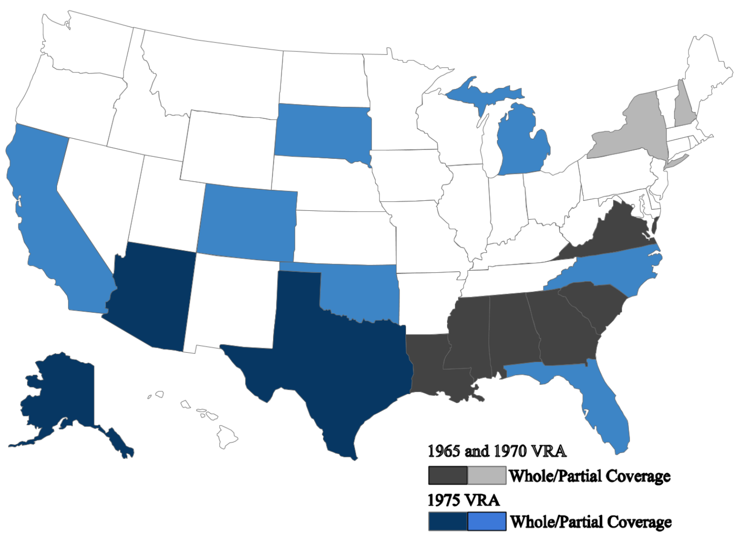

Figure 1.2. Jurisdictions by Year of Preclearance Coverage

Notes: Whole coverage refers to states in which all counties are subject to preclearance, while partial coverage refers to states in which only some jurisdictions are subject to preclearance. Parts of North Carolina and California were also brought under coverage in 1965 and 1970.

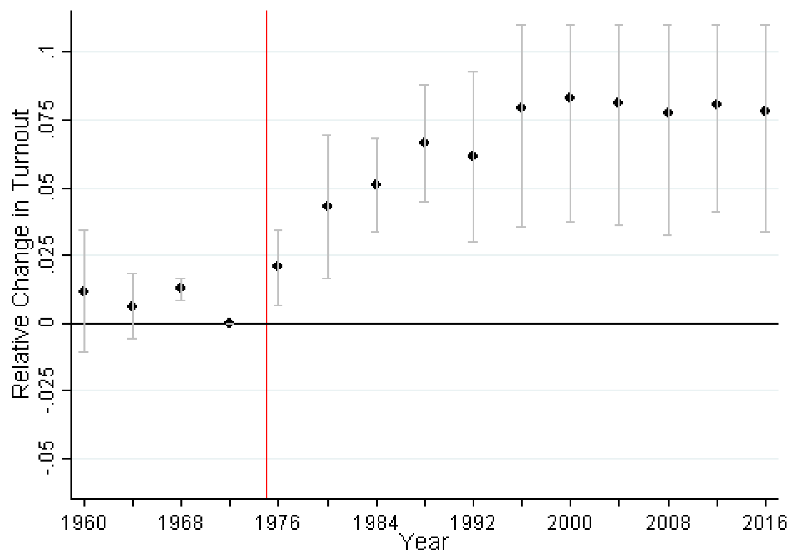

Figure 1.3. Effect on Voter Turnout

Notes: Data come from ICPSR and Dave Leip's Election Atlas. Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1. Observations are at the county-year level and weighted by voting eligibile population. Standard errors clustered at the state-level. Red vertical line represents passage of 1975 VRA. Full results displayed in Table 1.2, Column 1.

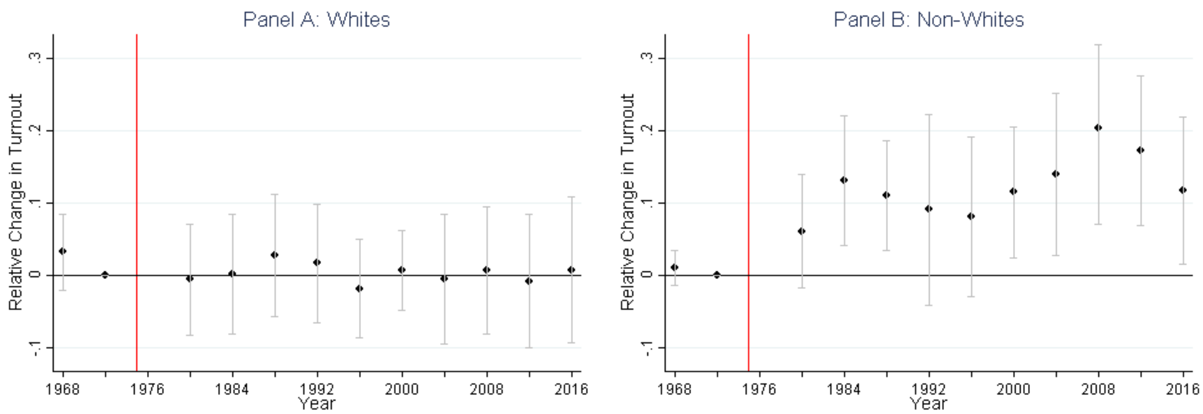

Figure 1.4. Effect on Turnout by Race

Notes: Data come from CPS Voting and Registration Supplement. Graphs show DD coefficients and 95 percent confidence intervals from estimation of Equation 1.2 by race. Observations are at the state-year level and are weighted by voting eligible population. Standard errors clustered at the state-level. Red vertical line represents passage of 1975 VRA. Estimates are unavailable for 1976 due to lack of state-level identifiers in 1976 CPS voter supplement. Full results displayed in Table 1.3, Columns 1 and 3.

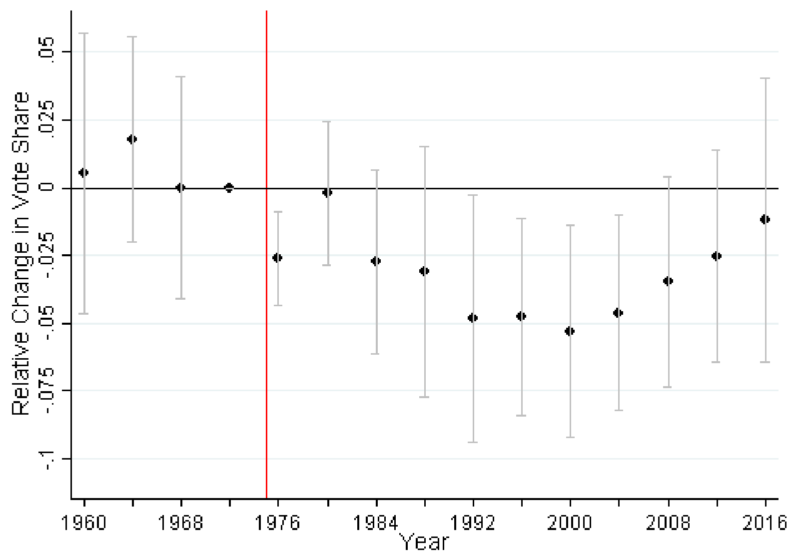

Figure 1.5. Effect on Democratic Vote Share

Notes: Data come from ICPSR and Dave Leip's Election Atlas. Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 including flexible controls for historical demographic and eligibility measures. Observations are at the county-year level and weighted by votes cast. Standard errors clustered at the state-level. Red vertical line represents passage of 1975 VRA. Full results displayed in Table 1.2, Column 5.

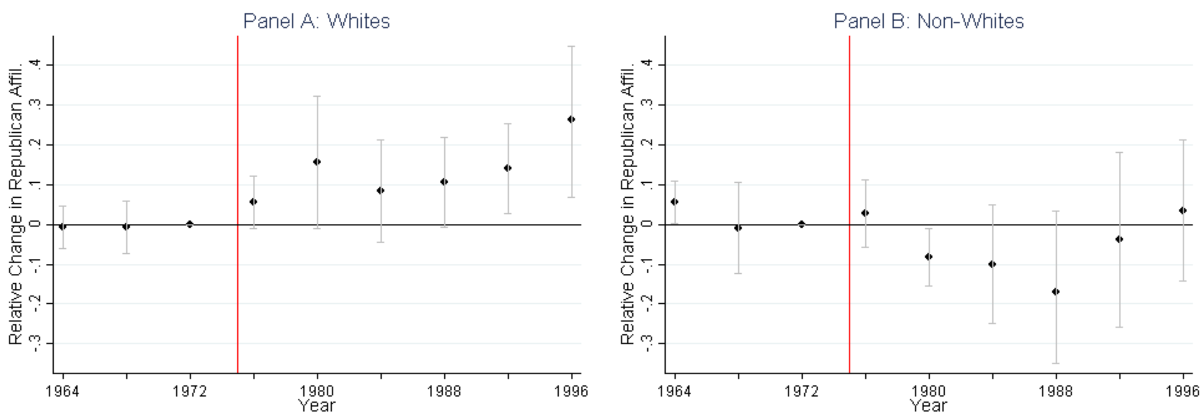

Figure 1.6. Effect on Republican Affiliation by Race

Notes: Data come from ANES. Graphs show DD coefficients and 95 percent confidence intervals from estimation of modified version of Equation 1.3 replacing $Post_t$ with a set of indicators for the most recent presidential election. Given insufficient treatment variation in some cells, Census division-year fixed effects are additionally replaced with Census region-year fixed effects. The outcome of interest is whether a respondent self-identified as strongly Republican. Observations are at individual-level and are weighted by ANES recommended survey weights. Standard errors clustered at the state-level. Red vertical line represents passage of 1975 VRA. Full results displayed in Table 1.A6, Panel B, Columns 1 and 3.

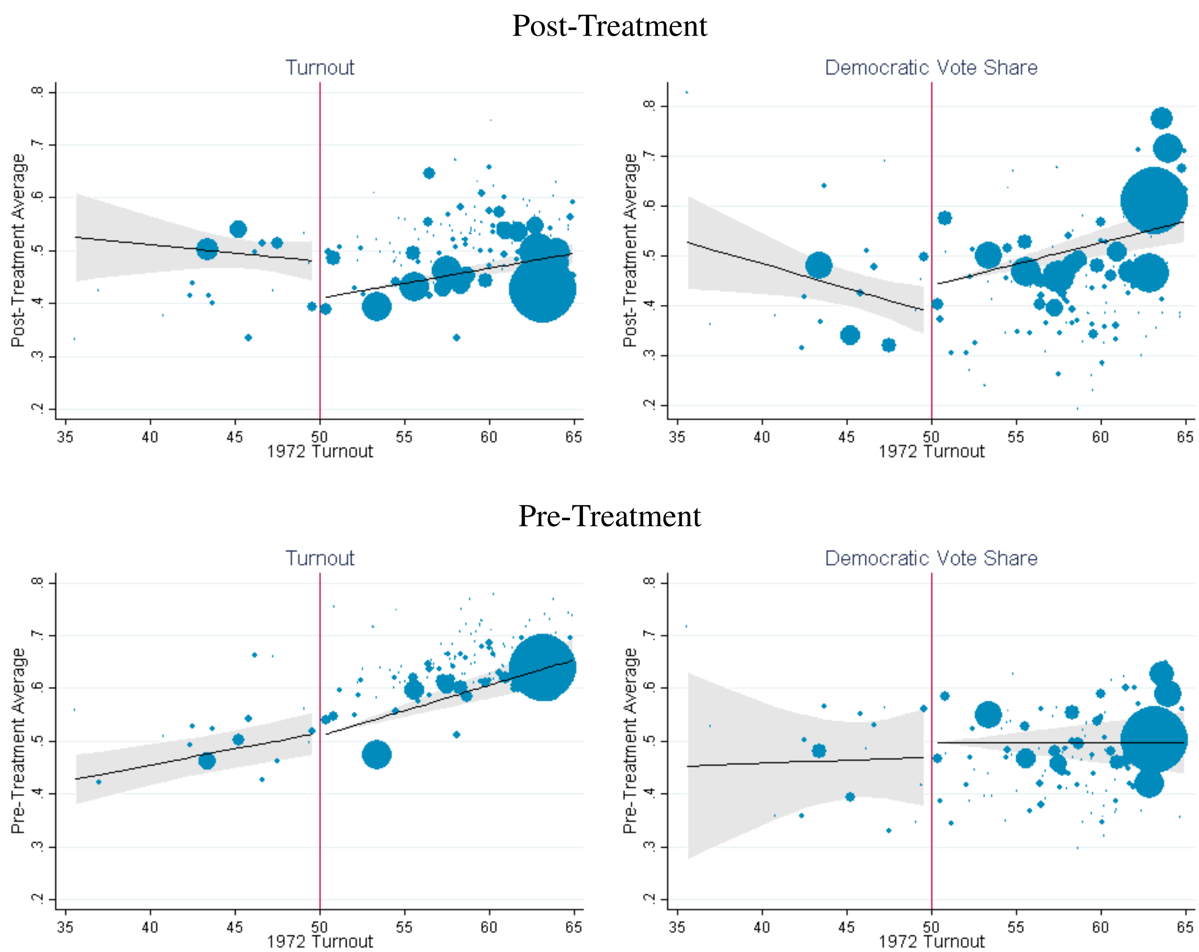

Figure 1.7. Regression Discontinuity

Notes: Data come from ICPSR and Dave Leip's Election Atlas. Graph shows fitted values and confidence intervals from estimation of Equation 1.5 and scatter plot of average post-treatment outcomes for each sample county, weighted by voting eligible population and major party votes cast. Standard errors are heteroskedasticity-robust. Full results displayed in Table 1.A9, Column 3.

Figure 1.8. Newspaper Mentions by Party Affiliation

Notes: Data come from newspapers.com. Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 modified with newspaper and Census region-year fixed effects and omitting year 2012. Left-panel includes newspapers that endorsed Nixon in 1972 according to data from Gentzkow et al. (2011), right-panel includes all other sample papers. Observations are at the newspaper-year level. Standard errors clustered at the state-level. Red vertical line represents passage of 1975 VRA. Full results are displayed in Table 1.A16, Columns 2 and 3.

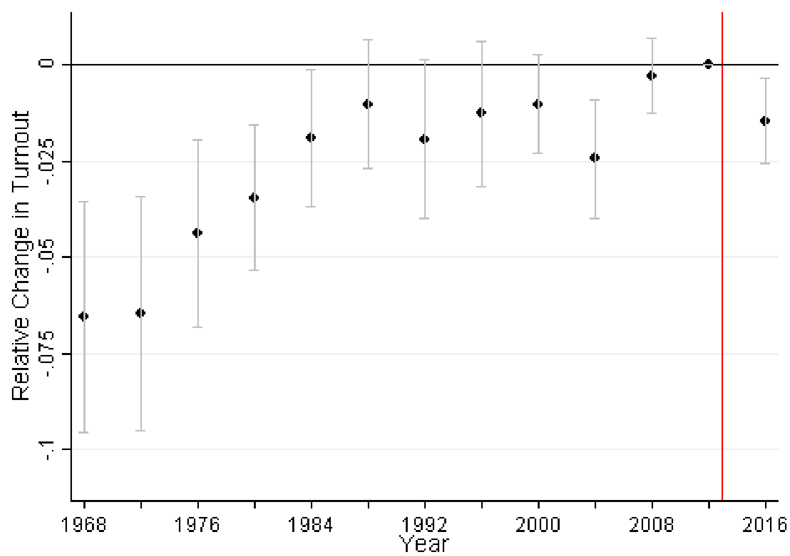

Figure 1.9. Post-Shelby: Voter Turnout

Notes: Data come from ICPSR and Dave Leip's Election Atlas. Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 modified with Census division-year fixed effects and omitting year 2012. Standard errors clustered at the state-level. Observations are at the county-year level and weighted by voting eligible population. Treatment group is comprised of *all* counties freed from preclearance by Shelby ruling, including those brought under coverage by the 1965 and 1970 versions of the VRA. Red vertical line represents 2013 Shelby ruling. Full results displayed in Table 1.7, Column 1.

Table 1.1. Harris County Objections

Date	Proposed Change	Reason for Objection
Dec 1975	Purge all voters that fail to re-register by March 1, 1976*	Re-registration requirements would disproportionately burden minorities, given historical discrimination and poll tax
Jan 1976	Eliminate state-funded primaries for parties with 2- 20% of 1974 vote share*	Would only affect Raza Unida, a Mexican-American nationalist party that received 6% of vote share
Jan 1977	Create new school district in Houston suburbs	Minorities had only gained board majority in Houston after desegregation. Those living outside the city would have little chance at representation in new district
Mar 1978	Consolidate polling station for precincts 55 and 340	Voters in predominantly black precinct 340 would be required to cross a freeway with no pedestrian overpass to vote
Mar 1978	Change school district election date from April to August	Over 3,000 students and faculty at local black university would be out of county during election
May 1978	Change county school trustees election date to January	Would create two separate schooling elections in minority districts (as opposed to one joint election), while reducing the number of polling locations in those areas from 725 to 25
Jun 1979	Annexation of nearby area to Houston	Annexation of predominantly white area would reduce minority population share by 1.7 percentage points
Dec 1979	Implement majority vote requirement; redraw city council districts	Would combine non-adjacent neighborhoods to create a district with less than 50% minority population share
Mar 1989	Implement anti-single-shot and majority vote requirement for at-large elections	Vote rules would make it difficult for minorities to coordinate to elect preferred candidates
Oct 1991	Redistrict Houston city's nine district-based council seats	Redistricting plan would result in only one majority Hispanic district despite Hispanics comprising 30% of population
Mar 1994	Create county criminal court with judge elected via at-large election	Vote rules would make it difficult for minorities to coordinate to elect preferred candidate
Feb 1995	Bilingual election materials contained numerous misspellings and inaccuracies*	Spanish registration card left out required information and would have resulted in invalid Hispanic registrations
Jan 1996	Authorize election officials to invalidate registrations based on citizenship information*	Reliance on out-of-date information would have threatened over 70,000 Hispanics and Asians with pending citizenship applications
Mar 1997	Annexation of nearby area to Webster city	Annexation of predominantly white area would have reduced minority population share by 2.8 p.p.
Nov 2001	Redistrict Texas state legislature*	Would reduce the number of Hispanic majority districts by 11%
May 2006	Reduce polling locations for community college district from 84 to 12	Location with fewest minorities would serve only 6,500 voters, while location with most minorities would serve 67,000 voters
Aug 2008	Require district supervisors to be land-owning registered voters*	Hispanics disproportionately did not own land
Mar 2012	Require state-issued identification in order to vote*	Among registered voters, Hispanics were twice as likely to lack proper identification than whites

Notes: * denotes state-level submissions. Some changes were submitted and objected to multiple times, in which case the earliest submission is noted. In addition to those listed above, objections were also lodged against state, county and judicial redistricting plans in 1976, 1978, 1990 1991, 1992 and 2001.

Table 1.2. Effect on Voter Turnout and Democratic Vote Share

PC x Year	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: DV=Turnout			*Panel B: DV=Dem. Share*		
1960	0.012	0.008	-0.007	0.022	0.005	0.001
	(0.011)	(0.014)	(0.009)	(0.028)	(0.026)	(0.022)
1964	0.006	0.005	-0.007	0.037	0.018	0.012
	(0.006)	(0.008)	(0.007)	(0.019)	(0.019)	(0.017)
1968	0.013	0.012	0.002	0.014	-0.000	-0.012
	(0.002)	(0.003)	(0.004)	(0.023)	(0.020)	(0.014)
1972	-	-	-	-	-	-
	-	-	-	-	-	-
1976	0.021	0.015	0.011	-0.026	-0.026	-0.029
	(0.007)	(0.005)	(0.008)	(0.018)	(0.009)	(0.009)
1980	0.043	0.032	0.026	-0.018	-0.002	0.001
	(0.013)	(0.006)	(0.009)	(0.020)	(0.013)	(0.014)
1984	0.051	0.044	0.037	-0.052	-0.027	-0.018
	(0.009)	(0.008)	(0.013)	(0.026)	(0.017)	(0.018)
1988	0.066	0.054	0.050	-0.058	-0.031	-0.016
	(0.011)	(0.008)	(0.014)	(0.030)	(0.023)	(0.022)
1992	0.061	0.047	0.041	-0.072	-0.049	-0.033
	(0.015)	(0.010)	(0.016)	(0.029)	(0.023)	(0.019)
1996	0.079	0.058	0.044	-0.073	-0.048	-0.033
	(0.022)	(0.008)	(0.014)	(0.025)	(0.018)	(0.017)
2000	0.083	0.064	0.051	-0.081	-0.053	-0.039
	(0.023)	(0.010)	(0.017)	(0.024)	(0.019)	(0.020)
2004	0.081	0.057	0.041	-0.079	-0.046	-0.028
	(0.022)	(0.011)	(0.019)	(0.019)	(0.018)	(0.016)
2008	0.078	0.053	0.039	-0.064	-0.035	-0.016
	(0.022)	(0.012)	(0.021)	(0.016)	(0.019)	(0.013)
2012	0.081	0.057	0.046	-0.056	-0.025	-0.008
	(0.020)	(0.014)	(0.025)	(0.018)	(0.019)	(0.013)
2016	0.078	0.052	0.038	-0.045	-0.012	0.005
	(0.022)	(0.009)	(0.020)	(0.025)	(0.026)	(0.014)
Demo. Ctrls.	-	Yes	Yes	-	Yes	Yes
Near Cutoffs	-	-	Yes	-	-	Yes
Obs.	37,640	37,606	13,892	37610	37567	13884
R-sq.	0.921	0.930	0.892	0.867	0.918	0.920

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 2.1 displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population (turnout) and major party votes cast (vote share). Controls include interactions between year indicators and average income, average education, and county population shares of minorities, military personnel and 18-21 year olds. Near cutoffs restricts analysis to counties with 1972 turnout between 40-60% and 1970 minority share between 0-10%.

Table 1.3. Effect on Voter Turnout by Race

	Whites		Non-Whites	
PC x Year	(1)	(2)	(3)	(4)
	DV=Voter Turnout			
1968	0.032	0.029	0.010	0.014
	(0.026)	(0.023)	(0.012)	(0.011)
1972	-	-	-	-
	-	-	-	-
1980	-0.007	0.005	0.060	0.049
	(0.038)	(0.036)	(0.039)	(0.039)
1984	0.001	0.013	0.131***	0.123**
	(0.041)	(0.040)	(0.044)	(0.045)
1988	0.028	0.033	0.110***	0.106**
	(0.042)	(0.043)	(0.037)	(0.042)
1992	0.016	0.026	0.090	0.086
	(0.041)	(0.039)	(0.065)	(0.069)
1996	-0.019	-0.010	0.081	0.076
	(0.034)	(0.034)	(0.055)	(0.057)
2000	0.006	0.011	0.114**	0.111**
	(0.027)	(0.027)	(0.045)	(0.048)
2004	-0.006	0.003	0.140**	0.135**
	(0.045)	(0.043)	(0.055)	(0.058)
2008	0.007	0.015	0.203***	0.199***
	(0.044)	(0.043)	(0.066)	(0.069)
2012	-0.008	0.001	0.172***	0.184***
	(0.046)	(0.049)	(0.051)	(0.045)
2016	0.007	0.020	0.117**	0.129***
	(0.050)	(0.052)	(0.050)	(0.042)
Election Ctrls.	-	Yes	-	Yes
Obs.	528	528	365	365
R-sq.	0.875	0.877	0.829	0.837

Notes: Data come from CPS Voting and Registration Supplement. DD coefficients from estimation of Equation 1.2 displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Election controls include indicators for the presence of gubernatorial and Senate elections as well as for bilingual restrictions within state-year.

Table 1.4. Party Identification by Race

	Whites			Non-Whites		
	Pre-Treat Mean	(1)	(2)	Pre-Treat Mean	(3)	(4)
Panel A: DV=Republican Affiliation (Indicator)						
Weak	0.366	0.074	0.048	0.082	-0.123	-0.103
		(0.035)	(0.033)		(0.011)	(0.011)
Strong	0.277	0.052	0.026	0.059	-0.054	-0.045
		(0.014)	(0.012)		(0.011)	(0.013)
Panel B: DV=Minority Aid Opposition (Scale: 1-6)						
Respondent	4.480	0.301	0.242	2.565	-0.126	-0.234
		(0.122)	(0.142)		(0.294)	(0.265)
Republicans	4.061	0.318	0.263	4.863	0.902	0.694
		(0.276)	(0.293)		(0.090)	(0.088)
Democrats	3.160	-0.180	-0.174	2.850	0.376	0.364
		(0.158)	(0.180)		(0.070)	(0.140)
Demo. Ctrls.		-	Yes		-	Yes
Obs.(Rep.)		22,612			3,831	
Obs.(Aid)		17,240			3,031	

Notes: Data come from ANES. DD coefficient from estimation of Equation 1.3 displayed. Standard errors clustered at the state-level in parentheses. Observations weighted using ANES recommended survey weights. Demographic controls include respondent's age, income, education and military status. Minority aid responses refer to the respondent's self-reported position regarding government aid to minorities as well as her reported beliefs about the positions of the Republican and Democratic parties.

Table 1.5. Republican Identification by Minority Aid Opposition

	Whites		Non-Whites	
	(1)	(2)	(3)	(4)
	DV=Republican Affiliation			
PC x Post ($\beta4$)	0.013	-0.059	-0.079	-0.087
	(0.053)	(0.044)	(0.018)	(0.031)
PC x Post x NoAid ($\beta5$)	0.109	0.153	-0.134	-0.189
	(0.038)	(0.034)	(0.066)	(0.069)
$\beta4+\beta5$	0.122	0.094	-0.213	-0.276
Conservatism Ctrl.	-	Yes	-	Yes
Obs.	17,240	16,046	3,031	2,884
R-sq.	0.118	0.211	0.201	0.234

Notes: Data come from ANES. DD and DDD coefficients of interest from estimation of Equation 1.4 controlling for demographic variables displayed. Conservatism controls include interactions between year and an indicator for whether the respondent self-identified as weakly conservative. Standard errors clustered at the state-level in parentheses. Observations weighted using ANES recommended survey weights.

Table 1.6. Effect on Campaign Exposure

	Whites		Non-Whites	
	Pre-Treat Mean	(1)	Pre-Treat Mean	(2)
Media Exposure	2.429	0.053	2.298	-0.212
		(0.133)		(0.063)
Contact (Republican)	0.158	0.132	0.085	-0.071
		(0.034)		(0.016)
Contact (Democrat)	0.153	0.061	0.138	0.009
		(0.030)		(0.031)
Obs. (Media)		12,288		1,892
Obs. (Contact)		21,421		3,710

Notes: Data come from ANES. DD coefficient from estimation of Equation 1.3 displayed. Standard errors clustered at the state-level in parentheses. Observations weighted using ANES recommended survey weights. Includes demographic controls. *Media exposure* refers to the number of media forms (zero to four) the respondent saw, read or heard about the political campaign. *Contact* is an indicator for whether the respondent was contacted by Republican (Democratic) party "to get them to vote for their candidate".

Table 1.7. Post-Shelby Analysis

PC x Year	(1)		(2)		(3)	
			DV=Voter Turnout			
1968	-0.066	(0.015)	-0.013	(0.029)	-0.098	(0.063)
1972	-0.065	(0.015)	-0.036	(0.028)	-0.097	(0.068)
1976	-0.044	(0.012)	-	-	-	-
1980	-0.035	(0.009)	-0.017	(0.016)	-0.074	(0.055)
1984	-0.019	(0.009)	-0.021	(0.020)	-0.072	(0.042)
1988	-0.010	(0.008)	-0.001	(0.017)	-0.063	(0.039)
1992	-0.019	(0.010)	0.005	(0.020)	-0.094	(0.034)
1996	-0.013	(0.009)	-0.019	(0.015)	-0.082	(0.043)
2000	-0.010	(0.006)	-0.012	(0.022)	-0.025	(0.041)
2004	-0.024	(0.008)	-0.025	(0.020)	-0.028	(0.028)
2008	-0.003	(0.005)	-0.006	(0.012)	0.010	(0.028)
2012	-	-	-	-	-	-
2016	-0.015	(0.005)	-0.003	(0.010)	-0.021	(0.029)
Sample	County		Whites (State)		Non-Whites (State)	
Obs.	40,402		600		437	
R-sq.	0.836		0.858		0.853	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 2.1 modified with Census division-year fixed effects and omitting year 2012 displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Treatment group is comprised of *all* counties (states) freed from preclearance coverage by Shelby ruling, including those brought under coverage by earlier versions of the VRA.

Appendix A

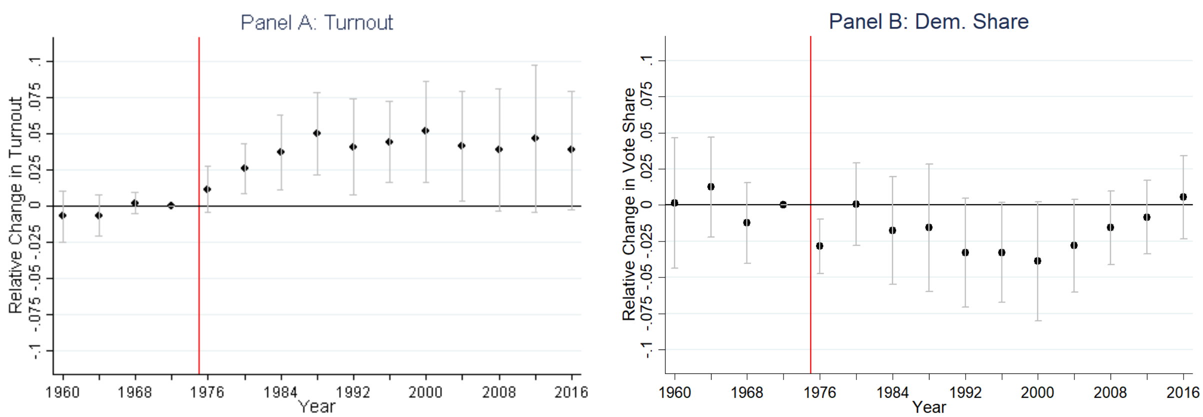

Figure 1.A1. Counties near the Coverage Triggers

Notes: Data come from ICPSR and Dave Leip's Election Atlas. Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 including flexible controls for historical demographic measures. Sample is restricted only to those counties with 40-60% turnout and 0-10% language minority share in 1972. Observations are at the county-year level and weighted by votes cast. Standard errors clustered at the state-level. Red vertical line represents passage of 1975 VRA. Full results displayed in Table 1.2, Columns 3 and 6.

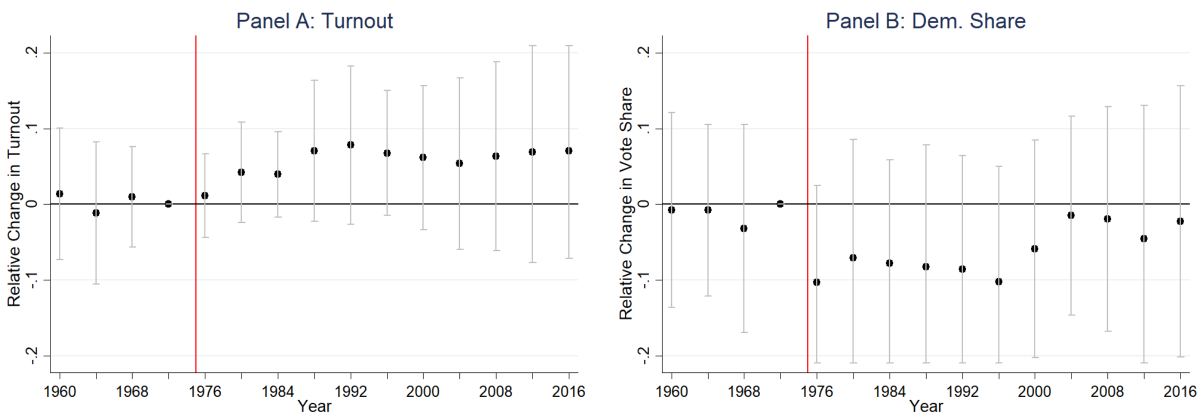

Figure 1.A2. Regression Discontinuity by Year

Notes: Data come from ICPSR and Dave Leip's Election Atlas. Graphs show RD coefficients and 95 percent confidence intervals from estimation of Equation 1.5, separately for each election from 1960 onwards. The sample includes counties with greater than 5% language minority share in 1970 and 1972 voter turnout between 30 and 70%, excluding those in Texas and Arizona. Observations weighted by voting eligible population (for turnout) and major party votes cast (for Dem. share). Heteroskedasticity-robust standard errors are included. Red vertical line represents passage of 1975 VRA. Full results displayed in Table 1.A10.

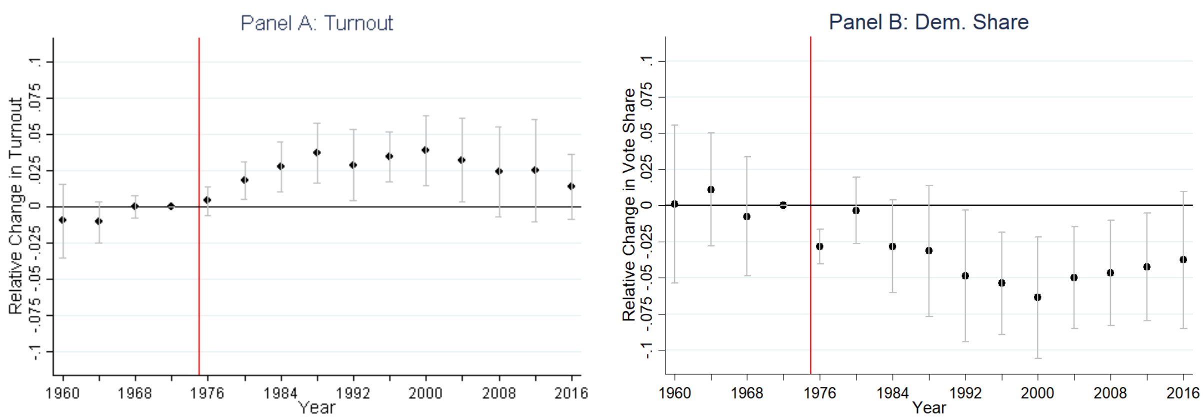

Figure 1.A3. Controlling for Historical Determination Measures

Notes: Data come from ICPSR and Dave Leip's Election Atlas. Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 including interactions between year indicators and 1972 turnout and 1970 language minority share. Observations are at the county-year level and weighted by votes cast. Standard errors clustered at the state-level. Red vertical line represents passage of 1975 VRA. Full results displayed in Table 1.A12, Column 1.

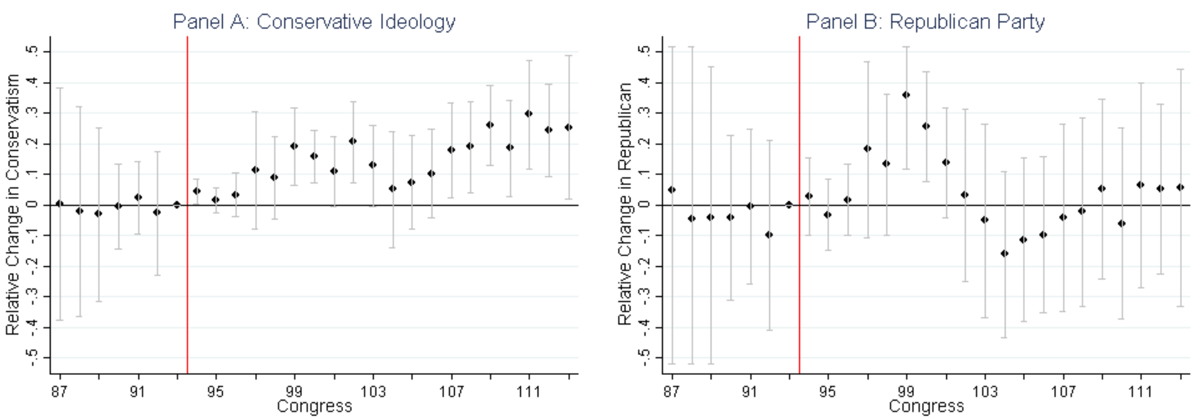

Figure 1.A4. Effect on Congressional Ideology

Notes: Data come from DW-NOMINATE. Graphs show DD coefficients and 95 percent confidence intervals from estimation of district-level analogue of Equation 2.1 on a Republican indicator and DW-NOMINATE first-dimension scores of conservatism (scaled from -1 to 1) including county and Census division-year fixed effects. The omitted group is the 93rd Congress, which ended in January 1975. Observations are at the district-Congress level. Standard errors clustered at the state-level. Red vertical line represents passage of 1975 VRA. Full results are displayed in Table 1.A13.

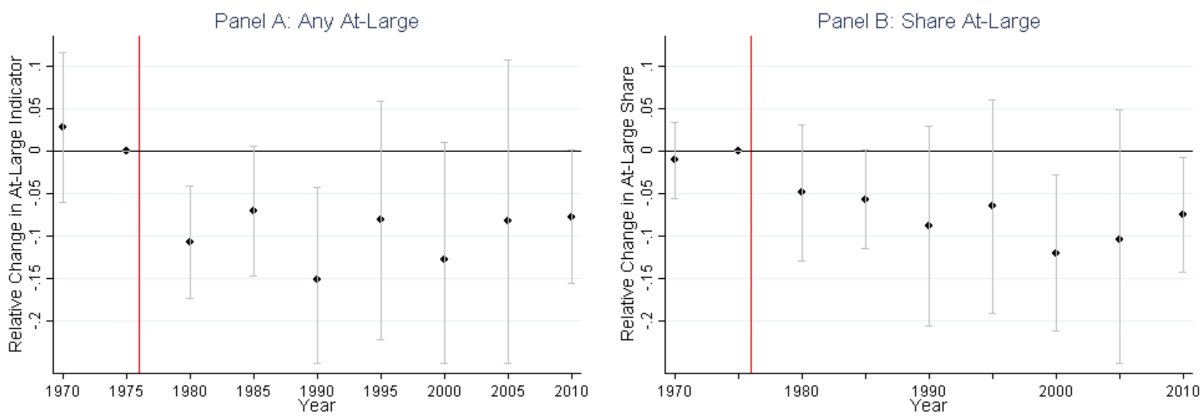

Figure 1.A5. Effect on At-Large Elections

Notes: Data come from ICMA Municipal Yearbooks. Graphs show DD coefficients and 95 percent confidence intervals from estimation of municipality-level analogue of Equation 2.1 substituting municipality fixed effects in place of county fixed effects. The treatment group is comprised of municipalities in covered counties, which were also subject to preclearance. Any At-Large is an indicator set to one if a municipality maintains any council seats elected through at-large, as opposed to district-based, elections in a given year. Share At-Large is the fraction of council seats elected by at-large elections. Observations are at the municipality-year level. Standard errors clustered at the state-level. Red vertical line represents passage of 1975 VRA. Full results are displayed in Table 1.A14

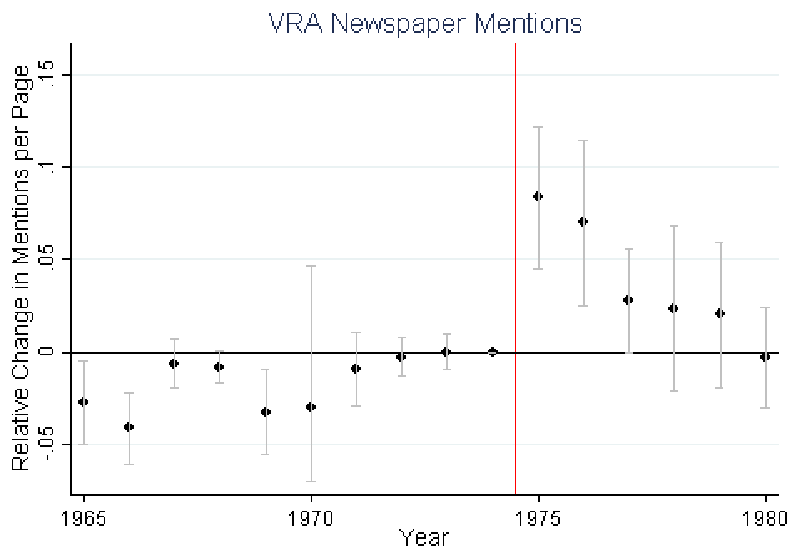

Figure 1.A6. Effect on Newspaper Mentions of the VRA

Notes: Data come from newspapers.com. Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 modified with newspaper and Census region-year fixed effects and omitting year 2012. Observations are at the newspaper-year level. Standard errors clustered at the state-level. Red vertical line represents passage of 1975 VRA. Full results are displayed in Table 1.A16, Column 1

Table 1.A1. Summary Statistics: County Characteristics (1972)

	Control		Treatment	
	Mean	SD	Mean	SD
Total Pop.	73,316	254,507	53,908	171,322
Voting Eligible Pop.	48,611	174,234	34,165	110,878
Income (Avg.)	3,990	898	3,522	716
Black (percent)	3.574	7.848	8.542	10.024
Language Minority (percent)	2.239	7.105	15.303	18.387
College Educated (percent)	7.460	3.962	7.474	3.263
Age 18 to 21 (percent)	5.166	0.984	5.172	1.098
Active Military (percent)	0.388	2.043	0.754	2.924
White-Minority Income Ratio	2.967	7.257	2.121	1.941
Minority-White Poverty Diff.	12.143	20.087	27.430	18.111
Minority-White Uneducated Diff.	2.938	7.468	8.115	7.358
Voter Turnout	0.651	0.133	0.485	0.081
Democratic Vote Share	0.347	0.104	0.313	0.099
Counties	2,232		283	

Notes: Data come U.S. Census Bureau. from White-minority income ratio is the ratio of average white to average non-white income. Minority-white poverty differential is the difference between non-white and white poverty rates. Minority-white uneducated differential is the difference between percent of non-whites with no education and percent of whites with no education.

Table 1.A2. Effect on Voter Turnout and Democratic Vote Share: Period Model

PC x Year	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: DV=Turnout			*Panel B: DV=Dem. Share*		
1960-1968	0.010	0.008	-0.004	0.025	0.008	0.001
	(0.005)	(0.007)	(0.004)	(0.023)	(0.021)	(0.018)
1972	-	-	-	-	-	-
	-	-	-	-	-	-
1976-1988	0.047	0.038	0.033	-0.040	-0.022	-0.015
	(0.009)	(0.005)	(0.009)	(0.023)	(0.015)	(0.016)
1992-2004	0.077	0.057	0.044	-0.077	-0.049	-0.033
	(0.021)	(0.010)	(0.016)	(0.023)	(0.019)	(0.017)
2008-2016	0.079	0.054	0.041	-0.055	-0.024	-0.006
	(0.021)	(0.011)	(0.022)	(0.019)	(0.021)	(0.013)
Demo. Ctrls.	-	Yes	Yes	-	Yes	Yes
Near Cutoffs	-	-	Yes	-	-	Yes
Obs.	37,640	37,606	13,892	37,610	37,567	13,884
R-sq.	0.921	0.930	0.892	0.867	0.918	0.920

Notes: Data come from ICPSR and Dave Leip's Election Atlas. Table shows coefficients from estimation of a period-level analogue of Equation 2.1, which replaces the full set of treatment-year interactions with interactions between treatment and a set of period indicators ($I_{\tau_1-\tau_2,t}$), where $I_{\tau_1-\tau_2,t}$ is set to 1 for years between τ_1 and τ_2 and 0 otherwise. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population (turnout) and major party votes cast (vote share). Controls include interactions between year indicators and average income, average education, and county population shares of minorities, military personnel and 18-21 year olds. Near cutoffs restricts analysis to counties with 1972 turnout between 40-60% and 1970 minority share between 0-10%.

Table 1.A3. Voter Turnout: All Federal Elections

PC x Year	(1)		(2)		(3)	
			DV=Voter Turnout			
1960	-0.001	(0.012)	0.011	(0.019)	-0.009	(0.014)
1962	0.002	(0.008)	0.009	(0.011)	-0.000	(0.014)
1964	-0.007	(0.006)	0.007	(0.010)	-0.009	(0.009)
1966	0.004	(0.003)	0.010	(0.008)	-0.000	(0.007)
1968	-0.001	(0.008)	0.014	(0.007)	0.000	(0.006)
1970	0.004	(0.012)	0.011	(0.011)	0.004	(0.015)
1972	0.002	(0.013)	0.017	(0.013)	-0.004	(0.008)
1974	-	-	-	-	-	-
1976	0.007	(0.010)	0.019	(0.009)	0.013	(0.008)
1978	0.034	(0.013)	0.033	(0.011)	0.030	(0.009)
1980	0.029	(0.014)	0.033	(0.011)	0.024	(0.006)
1982	0.044	(0.014)	0.044	(0.014)	0.045	(0.013)
1984	0.039	(0.013)	0.048	(0.011)	0.036	(0.014)
1986	0.056	(0.012)	0.056	(0.011)	0.050	(0.010)
1988	0.049	(0.014)	0.052	(0.010)	0.045	(0.012)
1990	0.070	(0.015)	0.065	(0.011)	0.059	(0.010)
1992	0.058	(0.021)	0.058	(0.018)	0.050	(0.020)
1994	0.064	(0.019)	0.057	(0.013)	0.046	(0.013)
1996	0.065	(0.022)	0.059	(0.014)	0.042	(0.013)
1998	0.062	(0.017)	0.053	(0.012)	0.042	(0.015)
2000	0.068	(0.024)	0.063	(0.015)	0.046	(0.016)
2002	0.075	(0.016)	0.070	(0.009)	0.055	(0.015)
2004	0.067	(0.024)	0.057	(0.016)	0.037	(0.019)
2006	0.073	(0.019)	0.064	(0.012)	0.049	(0.018)
2008	0.063	(0.025)	0.053	(0.015)	0.035	(0.021)
2010	0.066	(0.018)	0.057	(0.013)	0.043	(0.024)
2012	0.066	(0.023)	0.057	(0.017)	0.042	(0.025)
2014	0.073	(0.018)	0.060	(0.010)	0.047	(0.020)
2016	0.064	(0.023)	0.052	(0.014)	0.033	(0.020)
Demo. Ctrls.	-		Yes		Yes	
Near Cutoffs	-		-		Yes	
Obs.	72,767		72,703		26,858	
R-sq.	0.939		0.946		0.933	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 2.1 including all federal and gubernatorial elections displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Voter turnout measured from highest participation among presidential, gubernatorial, Senate and House elections in given county-year. Controls include interactions between year indicators and average income, average education, and county population shares of minorities, military personnel and 18-21 year olds. Near cutoffs restricts analysis to counties with 1972 turnout between 40-60% and 1970 minority share between 0-10%.

Table 1.A4. Voter Turnout by Race: Period Model

PC x Year	Whites (1)	Whites (2)	Non-Whites (3)	Non-Whites (4)
	DV=Voter Turnout			
1968	0.032	0.029	0.010	0.014
	(0.026)	(0.023)	(0.012)	(0.012)
1972	-	-	-	-
	-	-	-	-
1980-1988	0.008	0.018	0.108	0.102
	(0.039)	(0.038)	(0.039)	(0.040)
1992-2004	-0.001	0.007	0.109	0.105
	(0.036)	(0.035)	(0.053)	(0.057)
2008-2016	0.002	0.013	0.161	0.169
	(0.045)	(0.047)	(0.052)	(0.046)
Election Ctrls.	-	Yes	-	Yes
Obs.	528	528	365	365
R-sq.	0.874	0.876	0.826	0.835

Notes: Data come from CPS Voting and Registration Supplement. Table shows coefficients from estimation of a period-level analogue of Equation 1.2, which replaces the full set of treatment-year interactions with interactions between treatment and a set of period indicators ($I_{\tau_1-\tau_2,t}$), where $I_{\tau_1-\tau_2,t}$ is set to 1 for years between τ_1 and τ_2 and 0 otherwise. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Election controls include indicators for the presence of gubernatorial and Senate elections as well as for bilingual restrictions within state-year.

Table 1.A5. Voter Turnout by Race: Treatment Intensity

	Whites		Non-Whites	
Intensity x Year	(1)	(2)	(3)	(4)
	DV=Voter Turnout			
1968	0.032	0.029	-0.018	-0.014
	(0.027)	(0.024)	(0.041)	(0.038)
1972	-	-	-	-
	-	-	-	-
1980	-0.007	0.005	0.031	0.025
	(0.039)	(0.037)	(0.068)	(0.067)
1984	-0.004	0.008	0.077	0.074
	(0.043)	(0.041)	(0.083)	(0.081)
1988	0.031	0.037	0.053	0.055
	(0.043)	(0.043)	(0.069)	(0.065)
1992	0.013	0.023	0.030	0.031
	(0.042)	(0.040)	(0.076)	(0.080)
1996	-0.019	-0.010	0.023	0.022
	(0.035)	(0.035)	(0.074)	(0.074)
2000	0.005	0.010	0.051	0.051
	(0.029)	(0.028)	(0.075)	(0.071)
2004	-0.006	0.003	0.075	0.073
	(0.046)	(0.045)	(0.080)	(0.083)
2008	0.008	0.016	0.143	0.141
	(0.045)	(0.045)	(0.081)	(0.084)
2012	-0.005	0.005	0.100	0.113
	(0.047)	(0.050)	(0.088)	(0.085)
2016	0.007	0.021	0.051	0.067
	(0.052)	(0.053)	(0.086)	(0.083)
Election Ctrls.	-	Yes	-	Yes
Obs.	528	528	365	365
R-sq.	0.875	0.877	0.827	0.835

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 1.2 replacing treatment dummy PC_s with treatment intensity variable $PCintensity_s$ displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Election controls include indicators for the presence of gubernatorial and Senate elections as well as for bilingual restrictions within state-year.

Table 1.A6. Party Affiliation by Race over Time

	Whites		Non-Whites	
PC x Year	(1)	(2)	(3)	(4)
Panel A: DV=Republican Affiliation (Weak)				
1964	-0.026	-0.020	0.042	0.088
	(0.031)	(0.034)	(0.035)	(0.046)
1968	0.037	0.044	0.005	0.017
	(0.053)	(0.049)	(0.058)	(0.060)
1972	-	-	-	-
	-	-	-	-
1976	0.106	0.093	-0.008	0.029
	(0.056)	(0.053)	(0.051)	(0.052)
1980	0.148	0.162	-0.097	-0.063
	(0.125)	(0.117)	(0.056)	(0.061)
1984	0.088	0.104	-0.041	-0.019
	(0.114)	(0.113)	(0.062)	(0.070)
1988	0.188	0.190	-0.106	-0.078
	(0.098)	(0.094)	(0.098)	(0.100)
1992	0.198	0.189	0.029	0.051
	(0.103)	(0.104)	(0.115)	(0.116)
1996	0.233	0.224	0.111	0.137
	(0.119)	(0.119)	(0.087)	(0.086)
Panel B: DV=Republican Affiliation (Strong)				
1964	-0.007	-0.005	0.056	0.096
	(0.026)	(0.021)	(0.026)	(0.038)
1968	-0.007	-0.004	-0.009	0.005
	(0.032)	(0.031)	(0.056)	(0.059)
1972	-	-	-	-
	-	-	-	-
1976	0.055	0.038	0.026	0.051
	(0.032)	(0.031)	(0.042)	(0.046)
1980	0.155	0.158	-0.081	-0.053
	(0.082)	(0.077)	(0.036)	(0.040)
1984	0.085	0.106	-0.101	-0.087
	(0.064)	(0.062)	(0.073)	(0.083)
1988	0.106	0.118	-0.169	-0.152
	(0.056)	(0.053)	(0.100)	(0.105)
1992	0.142	0.142	-0.040	-0.029
	(0.056)	(0.058)	(0.108)	(0.111)
1996	0.265	0.259	0.035	0.051
	(0.097)	(0.101)	(0.088)	(0.090)
Demo. Ctrls.	-	Yes	-	Yes
Obs.	22,162		3,841	

Notes: Data come from ANES. DD coefficients from estimation of Equation 1.3 including Census region-year fixed effects and replacing $Post_t$ with a set of indicators for the most recent presidential election. Observations are weighted by ANES recommended survey weights. Standard errors clustered at the state-level.

Table 1.A7. Party Affiliation by Race (Gallup)

	Whites			Blacks		
	Pre-Treat Mean	(1)	(2)	Pre-Treat Mean	(3)	(4)
	Panel A: DV=Republican Affiliation (Indicator)					
	0.293	0.149***	0.159***	0.131	-0.098***	-0.110**
		(0.031)	(0.029)		(0.014)	(0.040)
	Panel B: DV=No Black President (Indicator)					
	0.479	0.057	0.048	0.047	0.003	-0.072***
		(0.070)	(0.071)		-0.004	(0.025)
Demo. Ctrls.		-	Yes		-	Yes
Obs.(Rep.)		20,526			1,881	
Obs.(Pres.)		19,050			1,828	

Notes: Data come from Gallup, 1961 to 2003. DD coefficient from estimation of Equation 1.3 with state and divisn-year fixed effects displayed. Standard errors clustered at the state-level in parentheses. Observations weighted using Gallup recommended survey weights. Controls include respondent's age, income, education. *Republican Affiliation* is an indicator for whether the respondent self-identified as Republican, while *No Black President* is an indicator for whether the respondent reported being unwilling to vote for a hypothetical black president.

Table 1.A8. Republican Identification by Racial Conservatism (Gallup)

	Whites		Blacks	
	(1)	(2)	(3)	(4)
	DV=Republican Affiliation			
PC x Post	0.100**	0.112**	-0.118***	-0.133***
	(0.045)	(0.045)	(0.007)	(0.038)
PC x Post x NoBlack	0.063*	0.074**	-	-
	(0.032)	(0.033)	-	-
Demographic Ctrls.	-	Yes	-	Yes
Obs.	19,050		1,828	
R-sq.	0.041	0.064	0.190	0.224

Notes: Data come from Gallup, 1961 to 2003. DD and DDD coefficients of interest from estimation of Equation 1.4 with state and division-year fixed effects displayed. Standard errors clustered at the state-level in parentheses. *NoBlack* is an indicator for whether the respondent was not willing to vote for a black president. Observations weighted using Gallup recommended survey weights.

Table 1.A9. Regression Discontinuity

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: DV=Voter Turnout					
Pre-Treat	0.006	0.006	0.006	0.010	0.006	0.009
	(0.025)	(0.020)	(0.021)	(0.017)	(0.021)	(0.017)
Post-Treat	0.031	0.041	0.069	0.067	0.069	0.068
	(0.023)	(0.018)	(0.021)	(0.017)	(0.021)	(0.017)
	Panel B: DV=Dem. Vote Share					
Pre-Treat	-0.002	-0.000	-0.028	-0.020	-0.027	-0.020
	(0.053)	(0.035)	(0.051)	(0.033)	(0.050)	(0.033)
Post-Treat	0.024	-0.003	-0.054	-0.061	-0.056	-0.064
	(0.030)	(0.026)	(0.027)	(0.024)	(0.026)	(0.024)
Controls	-	Yes	-	Yes	-	Yes
Bandwidth	35-65		30-70		25-75	
Obs. (Pre)	566	562	714	710	762	758
Obs. (Post)	1,555	1,544	1,962	1,951	2,105	2,094
R-sq. (Pre)	0.455	0.708	0.507	0.729	0.508	0.723
R-sq. (Post)	0.024	0.322	0.115	0.415	0.118	0.414

Notes: Data come from ICPSR and Dave Leip's Election Atlas. RD coefficients and standard errors from estimation of Equation 1.5, pooled across all post-treatment and pre-treatment years, respectively. The sample excludes counties in Texas and Arizona, as those areas were covered due to state-level determinations, as well as counties with less than 5% 1970 minority share or 1972 turnout outside of the bandwidths. Observations weighted by voting eligible population (for turnout) and major party votes cast (for Dem. share). Heteroskedasticity-robust standard errors are included.

Table 1.A10. Regression Discontinuity by Year

Less50	(1)		(2)	
	DV=Turnout		*DV=Dem. Share*	
1960	0.014	(0.044)	-0.007	(0.065)
1964	-0.012	(0.048)	-0.008	(0.057)
1968	0.010	(0.034)	-0.032	(0.070)
1972	-	-	-	-
1976	0.012	(0.028)	-0.103	(0.065)
1980	0.042	(0.034)	-0.070	(0.079)
1984	0.040	(0.029)	-0.077	(0.069)
1988	0.071	(0.047)	-0.083	(0.082)
1992	0.078	(0.053)	-0.086	(0.076)
1996	0.068	(0.042)	-0.102	(0.077)
2000	0.062	(0.048)	-0.059	(0.073)
2004	0.054	(0.057)	-0.015	(0.067)
2008	0.064	(0.063)	-0.019	(0.075)
2012	0.069	(0.074)	-0.045	(0.089)
2016	0.070	(0.072)	-0.023	(0.091)
Bandwidth	30-70		30-70	
Counties	178		178	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. RD coefficients and standard errors from estimation of Equation 1.5, separately for each election from 1960 onwards. The sample excludes counties in Texas and Arizona, as those areas were covered due to state-level determinations, as well as counties with less than 5% 1970 minority share or 1972 turnout outside of the bandwidths. Observations weighted by voting eligible population (for turnout) and major party votes cast (for Dem. share). Heteroskedasticity-robust standard errors are included.

Table 1.A11. Restricted Samples

PC x Year	(1)		(2)		(3)	
			Panel A: DV=Voter Turnout			
1960	-0.009	(0.006)	0.026	(0.011)	0.007	(0.013)
1964	-0.003	(0.006)	0.011	(0.009)	0.005	(0.011)
1968	0.007	(0.004)	0.009	(0.006)	0.016	(0.004)
1972	-	-	-	-	-	-
1976	-0.011	(0.004)	0.017	(0.010)	0.005	(0.005)
1980	0.008	(0.008)	0.019	(0.009)	0.016	(0.012)
1984	0.024	(0.015)	0.030	(0.007)	0.036	(0.014)
1988	0.034	(0.016)	0.038	(0.009)	0.043	(0.019)
1992	0.037	(0.020)	0.030	(0.013)	0.037	(0.023)
1996	0.037	(0.016)	0.042	(0.011)	0.042	(0.018)
2000	0.041	(0.019)	0.052	(0.014)	0.048	(0.024)
2004	0.041	(0.019)	0.054	(0.017)	0.038	(0.025)
2008	0.035	(0.020)	0.045	(0.020)	0.037	(0.026)
2012	0.043	(0.026)	0.054	(0.022)	0.047	(0.026)
2016	0.039	(0.021)	0.051	(0.018)	0.039	(0.022)
			Panel B: DV=Dem. Vote Share			
1960	0.008	(0.015)	-0.003	(0.018)	0.030	(0.015)
1964	0.024	(0.011)	0.032	(0.012)	0.033	(0.008)
1968	0.002	(0.010)	0.006	(0.011)	0.014	(0.009)
1972	-	-	-	-	-	-
1976	-0.029	(0.010)	-0.011	(0.019)	-0.034	(0.012)
1980	-0.002	(0.015)	-0.002	(0.019)	-0.007	(0.015)
1984	-0.033	(0.013)	-0.024	(0.024)	-0.045	(0.018)
1988	-0.039	(0.018)	-0.022	(0.029)	-0.045	(0.020)
1992	-0.053	(0.017)	-0.039	(0.031)	-0.058	(0.014)
1996	-0.054	(0.008)	-0.035	(0.032)	-0.065	(0.015)
2000	-0.070	(0.005)	-0.047	(0.030)	-0.075	(0.014)
2004	-0.052	(0.007)	-0.035	(0.024)	-0.059	(0.014)
2008	-0.030	(0.009)	-0.022	(0.017)	-0.046	(0.016)
2012	-0.024	(0.011)	-0.016	(0.016)	-0.038	(0.016)
2016	-0.013	(0.014)	-0.007	(0.018)	-0.019	(0.015)
Sample	42-58% Turnout 1-9% Minority		Neighboring Counties		Prob(PC) > 75th Percentile	
Obs.	2,361		4,315		9,182	
R-sq.	0.931		0.942		0.881	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 2.1 on restricted samples displayed. Column 1 restricts sample to counties within 8 p.p. of the turnout cutoff and 4 p.p. of the minority share cutoff. Column 2 restricts sample to treatment counties bordering at least one control county and control counties bordering at least one treatment county. Column 3 restricts sample to counties in the 75th percentile or higher of predicted preclearance coverage, based on logit of treatment on 1968 turnout and 1960 minority share. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes demographic controls.

Table 1.A12. Selection Controls

PC x Year	(1)		(2)		(3)	
			Panel A: DV=Voter Turnout			
1960	-0.010	(0.013)	0.008	(0.014)	0.007	(0.016)
1964	-0.011	(0.007)	0.005	(0.009)	0.007	(0.007)
1968	-0.000	(0.004)	0.011	(0.003)	0.021	(0.004)
1972	-	-	-	-	-	-
1976	0.004	(0.005)	0.014	(0.004)	0.012	(0.005)
1980	0.018	(0.006)	0.031	(0.005)	0.029	(0.006)
1984	0.028	(0.009)	0.043	(0.008)	0.041	(0.007)
1988	0.037	(0.010)	0.053	(0.008)	0.051	(0.009)
1992	0.028	(0.012)	0.046	(0.010)	0.047	(0.013)
1996	0.034	(0.008)	0.057	(0.008)	0.053	(0.010)
2000	0.038	(0.012)	0.064	(0.010)	0.054	(0.014)
2004	0.032	(0.014)	0.058	(0.011)	0.045	(0.015)
2008	0.024	(0.015)	0.054	(0.012)	0.037	(0.015)
2012	0.025	(0.018)	0.058	(0.015)	0.041	(0.017)
2016	0.014	(0.011)	0.053	(0.010)	0.029	(0.013)
			Panel B: DV=Dem. Vote Share			
1960	0.001	(0.027)	0.004	(0.028)	0.012	(0.030)
1964	0.011	(0.019)	0.017	(0.021)	0.017	(0.019)
1968	-0.008	(0.020)	0.001	(0.019)	0.002	(0.022)
1972	-	-	-	-	-	-
1976	-0.028	(0.006)	-0.028	(0.008)	-0.016	(0.005)
1980	-0.003	(0.011)	-0.004	(0.013)	0.008	(0.010)
1984	-0.028	(0.016)	-0.029	(0.017)	-0.020	(0.015)
1988	-0.031	(0.022)	-0.032	(0.023)	-0.024	(0.022)
1992	-0.049	(0.023)	-0.050	(0.022)	-0.041	(0.022)
1996	-0.054	(0.017)	-0.049	(0.017)	-0.048	(0.019)
2000	-0.064	(0.021)	-0.054	(0.019)	-0.063	(0.024)
2004	-0.050	(0.017)	-0.047	(0.017)	-0.054	(0.022)
2008	-0.047	(0.018)	-0.034	(0.020)	-0.057	(0.026)
2012	-0.042	(0.018)	-0.025	(0.020)	-0.053	(0.028)
2016	-0.038	(0.023)	-0.010	(0.028)	-0.052	(0.034)
Add'l Ctrls.	Turnout1972 x Year Minority1970 x Year		Dem.Share1972 x Year		Pr(PC)xYear	
Obs.	37,565		37,545		37,573	
R-sq.	0.936		0.931		0.932	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 2.1 with additional controls for selection. $Turnout1972$ and $Minority1970$ are historical voter turnout and language minority share measures used to determine preclearance coverage under 1975 VRA. $Pr(PC)$ is county's probability of preclearance coverage based based on logit of treatment on 1968 turnout and 1960 minority share. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes demographic controls.

Table 1.A13. Effect on Congressional Ideology

PC x Cong.	(1)		(2)	
	DV=Conservatism		*DV=Republican*	
87	0.004	(0.188)	0.048	(0.313)
88	-0.020	(0.171)	-0.046	(0.292)
89	-0.031	(0.141)	-0.041	(0.245)
90	-0.003	(0.069)	-0.042	(0.133)
91	0.024	(0.059)	-0.006	(0.126)
92	-0.026	(0.100)	-0.097	(0.154)
93	-	-	-	-
94	0.046	(0.020)	0.027	(0.063)
95	0.017	(0.020)	-0.032	(0.058)
96	0.034	(0.035)	0.018	(0.057)
97	0.114	(0.096)	0.182	(0.142)
98	0.088	(0.067)	0.133	(0.114)
99	0.192	(0.062)	0.359	(0.120)
100	0.159	(0.043)	0.256	(0.089)
101	0.109	(0.056)	0.138	(0.089)
102	0.206	(0.066)	0.032	(0.141)
103	0.129	(0.066)	-0.051	(0.156)
104	0.051	(0.094)	-0.160	(0.135)
105	0.074	(0.076)	-0.113	(0.132)
106	0.102	(0.072)	-0.097	(0.126)
107	0.178	(0.077)	-0.042	(0.151)
108	0.191	(0.074)	-0.022	(0.153)
109	0.261	(0.064)	0.053	(0.145)
110	0.186	(0.078)	-0.060	(0.154)
111	0.297	(0.088)	0.064	(0.166)
112	0.243	(0.075)	0.051	(0.137)
113	0.254	(0.117)	0.058	(0.192)
Obs.	9,449		9,449	
R-sq.	0.721		0.664	

Notes: DD coefficients from estimation of district-level analogue of Equation 2.1 including county and Census division-year fixed effects displayed. Standard errors clustered at the state-level in parentheses. *Conservatism* is the first dimension DW-NOMINATE score of a district's representative in a given session of Congress, scaled from -1 (least conservative) to 1 (most conservative). *Republican* is dummy indicating whether a district's representative is Republican. The omitted period is the 93rd session of Congress, which ended in 1975.

Table 1.A14. Effect on Election Rules

PC x Year	(1)		(2)	
	DV=Any At-Large		*DV=Share At-Large*	
1970	0.028	(0.044)	-0.010	(0.022)
1975	-	-	-	-
1980	-0.107	(0.033)	-0.049	(0.040)
1985	-0.070	(0.038)	-0.057	(0.029)
1990	-0.151	(0.054)	-0.087	(0.058)
1995	-0.081	(0.070)	-0.065	(0.063)
2000	-0.128	(0.069)	-0.119	(0.046)
2005	-0.082	(0.094)	-0.104	(0.076)
2010	-0.077	(0.039)	-0.075	(0.033)
Obs.	29,308		28,837	
R-sq.	0.638		0.758	

Notes: Data come from ICMA Municipal Yearbooks. DD coefficients from estimation of municipality-level analogue of Equation 2.1 including municipality and state-year fixed effects displayed. *Any At-Large* is a dummy indicating whether a city employed any at-large elections and *Share At-Large* is the share of city council seats elected at large. Standard errors clustered at the state-level in parentheses.

Table 1.A15. Effect on Turnout by Historical Discrimination

	Income Gap (1)	Poverty Gap (2)	Education Gap (3)
		DV=Voter Turnout	
PCxPost	0.038***	0.033***	0.041***
	(0.013)	(0.011)	(0.001)
PCxPostxDisc.	0.004**	0.001**	0.001
	(0.002)	(0.000)	(0.001)
Discriminate=	$\frac{\text{avg. white inc.}}{\text{avg. minority inc.}}$	% minority pov. - % white pov.	% minority w/o edu. - % white w/o edu.
Obs.	25,196	25,946	31,133
R-sq.	0.931	0.932	0.931

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DDD coefficient from estimation of Equation 1.6 displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes demographic and eligibility controls.

Table 1.A16. Effect on Newspaper Coverage

PC x Year	(1)		(2)		(2)	
	DV=VRA Mentions per Page					
1965	-0.028	(0.011)	-0.001	(0.011)	-0.042	(0.014)
1966	-0.041	(0.010)	-0.025	(0.018)	-0.046	(0.014)
1967	-0.006	(0.006)	-0.031	(0.013)	-0.001	(0.005)
1968	-0.008	(0.004)	-0.027	(0.009)	-0.003	(0.004)
1969	-0.032	(0.011)	-0.011	(0.008)	-0.038	(0.014)
1970	-0.030	(0.038)	0.004	(0.033)	-0.036	(0.044)
1971	-0.009	(0.010)	-0.009	(0.009)	-0.008	(0.012)
1972	-0.003	(0.005)	-0.016	(0.006)	0.002	(0.007)
1973	0.000	(0.004)	-0.010	(0.010)	0.002	(0.006)
1974	-	-	-	-	-	-
1975	0.083	(0.019)	0.146	(0.059)	0.063	(0.013)
1976	0.070	(0.022)	0.094	(0.049)	0.060	(0.014)
1977	0.028	(0.014)	0.080	(0.034)	0.008	(0.010)
1978	0.023	(0.022)	0.098	(0.061)	0.004	(0.016)
1979	0.020	(0.020)	0.060	(0.025)	0.009	(0.020)
1980	-0.003	(0.013)	-0.004	(0.014)	-0.000	(0.015)
Newspapers	All		Endorse Nixon		No Endorse Nixon	
Obs.	5,682		1,624		4,058	
R-sq.	0.129		0.139		0.556	

Notes: Data come from newspapers.com. DD coefficients from estimation of newspaper-level analogue of Equation 2.1 including newspaper and Census region-year fixed effects displayed. Dependent variable is the ratio of mentions of the phrase "Voting Rights Act" to the total number of digitized pages on newspapers.com in a newspaper-year. Standard errors clustered at the state-level in parentheses.

Appendix B

Reading this Appendix is not necessary to understanding the main analysis. All results discussed here are also referenced in the main paper. This section simply provides a more detailed explanation of the paper's robustness analyses and secondary data sources.

A. Data

Gallup Survey

ANES survey data is one of few available sources of historical information on political preferences identified by respondent race and county. However, given the limited sample size of the data, specifically among minorities, one may be concerned about the robustness of the party identification by race estimates. Thus, to complement those estimates, I replicate the party affiliation by race analysis presented in Section III using historical Gallup survey microdata identified by respondent state and race. These results were displayed in Table 1.A7 and Table 1.A8.

Specifically, I responses regarding self-reported Republican affiliation and hypothetical opposition to a black presidential candidate. The latter was based on a question asking "If your party nominated a generally well-qualified man for president, would you vote for him if he happened to a Negro?" and coded as 0 for individuals responding "Yes" and 1 otherwise. Responses to the same question were employed by Kuziemko and Washington (2015) as a consistently measured proxy for racial conservatism, similar to the ANES question regarding government aid to minorities. Thus, micro-data were acquired from the Roper Center for all surveys administered in the sample period including the black president question (i.e., 1961, 1963, 1965, 1967, 1969, 1971, 1978, 1983, 1984, 1987, 1999, 2003) and contain between 1,000 and 3,500 responses in each year.

Electoral Rules

Municipality-level data employed in Figure 1.A5 and Table 1.A14 comes from International City/County Municipal Association (ICMA), specifically Municipal Yearbooks published in 1972, 1976, 1981, 1986, 1991, 1996, 2001, 2006 and 2011. These data are in turn based on mail-in surveys administered by the ICMA to municipalities in the one or two years prior to publication. Surveys contain questions regarding forms of government and were sent to cities, towns, townships, villages and boroughs with populations larger than 2,500 (5,000 prior to 1980), as well as other municipalities in the ICMA database. Data from 1981 onwards are available in electronic format and are identified by Census FIPS place codes. For earlier years, information was hand-coded from hard copies. Unfortunately, hard copies do not contain contain place codes. Thus, pre-treatment data was first mapped to place codes by name and state then merged with data from latter years by place code. The final dataset contains between 2,000 to 4,000 municipalities in each sample year.

Congressional Ideology

As complement to the Democratic vote share analysis presented in Section III, I estimate the effects of preclearance on Congressional ideology and affiliation using DW-NOMINATE data. DW-NOMINATE is a multidimensional scaling technique which collapses legislative roll-call voting into a two-dimensional ideal point. The first dimension is commonly thought of as a liberal-conservative measure (scaled from -1 to 1) and correctly classifies roll-call votes over 90% of the time. This score is also strongly correlated with ideological measures derived from campaign finances (Bonica, 2014) and Congressional speech, even when controlling for political party (Gentzkow et al., 2016). While the second dimension of DW-NOMINATE historically tracked to policy issues that cut across party lines, Congressional voting since the 1960's has been virtually unidimensional (McCarty et al., 1997). Importantly, DW-NOMINATE scores are specific to a representative-Congress and capture within-person changes over time.

Because preclearance coverage was determined at the county-level, I map each district in each Congress to the county or counties that comprise it. Because district borders frequently change, often drastically, this process was non-trivial. However, it was greatly facilitated by relationship files generously provided by James Snyder. For districts comprised of several counties, this data was supplemented with hand-coded information from the Congressional District Atlas, which publishes detailed district maps overlaid with county borders.

B. Robustness

Standard Errors

The paper's main analysis employs standard errors clustered at the state-level. This has been shown to correct for serial correlation in situations with close to 50 clusters (Bertrand et al., 2004). As my main analysis relies on 44 clusters, assumptions about asymptotic convergence may not hold. Thus, I test the robustness of my results to various corrections. In particular, I estimate Equation 2.1 using heteroskedasticity-robust standard errors, county clustered standard errors, state and year multi-way clustered standard errors, and wild-t cluster bootstrapped standard errors presented in Cameron et al. (2008). These results are shown in Table 2.8.

In nearly all cases, state-clustered standard errors (Column 1) are actually *smaller* than those produced from other methods for pre-treatment estimates. Thus, the 1968 DD estimate for both voter turnout and Democratic vote share is insignificant at the 5% level with all alternative specifications, providing support for the existence of parallel pre-trends. Importantly, post-treatment significance is maintained in nearly all cases.

Placebo Tests: Randomly-Assigned

The fact that standard errors are actually smaller when clustering by state than when clustering by county or not at all suggests that serial correlation is not an overriding concern. Nonetheless, for robustness' sake, I run a series of 500 simulations estimating Equation 2.1 with randomly generated treatment groups. Table 1.B3 lists the estimated coefficients of interest for voter turnout in Column 1 and for Democratic vote share in Column 2. All coefficients are insignificant and near to zero.

Placebo Tests: Faux-Triggers

While the regression discontinuity analysis and flexible determination controls presented in Section IV suggest otherwise, one may be concerned that the cutoffs and variables used to determine preclearance are correlated with differential time trends. To examine this, I estimate "treatment" effects using placebo treatments around the actual coverage cutoffs. In particular, I restrict the analysis to counties with greater than 5% minority population share in 1970 and either 40-50% 1972 turnout or 50-60% 1972 turnout. For the first sample, I then assign treatment according to a "faux"-trigger at 45%. For the second, I assign treatment according to a false trigger at 55%. These estimates are displayed in Columns 1 and 2 of Table 1.B4, respectively.

Notably, none of the post-treatment estimates on voter turnout are significant at the 5%-level and the majority are of the opposite sign (negative) of my main results. Regarding Democratic vote share, four of 22 post-treatment estimates are significant at the 5%-level. However, three of these four estimates, and 11 of the 22 total post-treatment estimates, are positive, while all of the paper's primary post-treatment estimates were negative. Taken together with the extremely limited sample size, these results suggest that my primary estimates are not confounded by differential trends around the true cutoffs.

Falsification Tests

In DD models, there is always concern that the estimated effects reflect some omitted, underlying trend orthogonal to the treatment itself. To test this, I use Equation 2.1 to estimate the impact of treatment on population size and minority share. Unlike other correlates of turnout like income or education which may be directly influenced through political representation, population growth and migration patterns are perhaps more likely to be "pre-determined." But because voter turnout the ratio of votes cast to eligible voters, any unaccounted trends in population could directly bias the outcome of interest.

As shown in Table 1.B3, Columns 3 and 4, this is likely not a concern here. I find little evidence of differential trends between groups in population size or composition, as all coefficients are insignificantly different from zero and small in magnitude.

Alternative Fixed Effects

One strength of this study's design is its ability to control for state-level shocks. In including state-year fixed effects, however, it is possible that the estimated treatment effects are not representative of counties in wholly-covered states (i.e., those in Texas and Arizona). I thus estimate Equation 2.1 replacing state-year indicators with alternative fixed effects.

Column 1 of Table 1.B6 displays the estimated coefficients with a set of year indicators and a dummy variable for the historical use of poll taxes in certain states prior to their nationwide abolition in 1966. Column 2 instead replaces state-year fixed effects with division-year fixed effects. For both models, the results are nearly identical to those including state-year fixed effects, suggesting both the robustness of my findings and their validity with respect to counties in covered states.

Alternative Controls

I also demonstrate robustness to alternative controls for the language restrictions concurrently introduced in the 1975 VRA. Because the presence of bilingual election requirements spanned both treatment and control groups and varied between elections within counties, I included a simply policy indicator to account for their effects in my main estimation. However, one may be concerned that this dummy variable fails to capture the actual policy effect or somehow biases the DD estimates of interest. Thus, I estimate Equation 2.1 with two alternative specifications. One excludes the bilingual indicator entirely, the other includes a full set of bilingual-year interactions, allowing for time-varying effects of bilingual election requirements. As Columns 1 and 2 of Table 1.B5 show, neither method demonstrably changes the sign, magnitude or significance of the coefficients of interest.

Alternatively, one may be concerned that the estimates are biased by unobserved demographic trends, not otherwise captured by the interactions between year and 1970 population measures. To account for this, I further include time-varying controls for these same demographic characteristics. These estimates, which are nearly identical to my main results, are displayed in Column 3.

Alternative Outcomes

This paper's main outcomes of interest are untransformed measures of voter turnout (calculated as the share of votes cast to the voting eligible population) and Democratic vote share (calculated as the share of Democratic votes cast to major party votes cast). Here, I estimate effects on an alternative measure of Democratic support — party vote share measured against *all* presidential votes cast (including those for third-party candidates) — as well as on logged voter turnout and Democratic vote share. These estimates are shown in Table 1.B7. Notably, the direction and significance of all estimates are highly similar to those shown in the paper's primary analysis, supporting the existence of parallel pre-trends and large, persistent post-treatment effects.

C. Other Analysis

Voting Rights Act of 1965

As noted in Section II, identifying preclearance's effects from the 1965 VRA is problematic due to the Act's concurrent prohibition of literacy tests. Though excluding areas covered in 1965 from the analysis bolsters the internal validity of turnout estimates, one may be concerned that it also diminishes their external validity, particularly with relation to the Southern states most commonly associated with racial discrimination.

To address these concerns, I estimate treatment effects upon the universe of counties ever subject to preclearance by employing a "stacked" difference-in-differences model and controlling for the presence of literacy tests. In particular, I estimate:

$$\begin{aligned} y_{c,t} =& \delta_c + \delta_{d,t} + \sum_{\tau \neq -1} \beta_\tau relativetime_{\tau,t} \times PC_c \\ & + \gamma_1 bilingual_{c,t} + \gamma_2 literacytest_{c,t} + \gamma_3 polltax_{c,t} + \varepsilon_{c,t}, \end{aligned} \tag{1.7}$$

where $relativetime_{\tau,t}$ are relative time to treatment dummies equal to 1 if election t is τ elections from the treatment. For example, $relativetime_{1,t}$ is set to 1 in 1968 for counties subject to preclearance starting in 1965, in 1972 for counties subject to preclearance starting in 1970 and in 1976 for counties subject to preclearance starting in 1975, and is set to 0 otherwise. As the inclusion of state-year fixed effects would absorb all the treatment variation from the Southern states wholly-covered by the 1965 VRA, I instead include Census division-year effects $\delta_{d,t}$ and a $polltax_{c,t}$ indicator to control for the historical use of poll taxes in certain states. $literacytest_{c,t}$ accounts for the presence of literacy tests at time t among those discriminatory counties where they were later banned.[43] The coefficients of interest β_τ then represent the average change between an election τ periods from treatment and the election just prior to treatment ($\tau = -1$) pooled across all treatment counties, relative to that same change over time among control counties.

The results from estimating Equation 1.7 with and without flexible demographic and el-

[43] Specifically, $literacytest_{c,t}$ is defined as by Husted and Kenny (1997), such that it equals 1 for all counties covered by the 1965 and 1970 VRAs only in years prior to coverage and is set to 0 otherwise, even among those uncovered counties which employed literacy tests (for example, those in Massachusetts and Maine). The reason for this is that literacy tests varied greatly between areas and were most restrictive in those areas specifically targeted by the earlier VRAs.

igibility controls are displayed in Table 1.B8. Though some of the pre-treatment DD estimates ($\tau < 0$) are significant at the 10% level, they are inconsistent in sign and do not indicate obvious pre-treatment trends. Following treatment, I find that turnout in covered areas increased steadily over time. Despite examining three times as many treatment counties as in Section III, these effects are remarkably similar to those presented earlier, which also found maximum turnout gains of around 8 p.p.

Though this analysis does not control for idiosyncratic state-level shocks and adopts an admittedly simplistic approach to accounting for literacy test bans, the fact that these estimates so closely align with the paper's main results demonstrates both the robustness of my findings as well as the consistent and powerful effects of preclearance coverage as applied throughout the country.

Table 1.B1. Effect on DW-NOMINATE Scores

PC x Cong.	(2)		(3)	
	DV=1st Dim.		*DV=2nd Dim.*	
87	0.004	(0.188)	-0.171	(0.155)
88	-0.020	(0.171)	-0.036	(0.127)
89	-0.031	(0.141)	0.027	(0.129)
90	-0.003	(0.069)	0.059	(0.103)
91	0.024	(0.059)	0.023	(0.093)
92	-0.026	(0.100)	0.095	(0.078)
93	-	-	-	-
94	0.046	(0.020)	0.050	(0.048)
95	0.017	(0.020)	0.125	(0.050)
96	0.034	(0.035)	0.146	(0.047)
97	0.114	(0.096)	-0.000	(0.117)
98	0.088	(0.067)	0.018	(0.115)
99	0.192	(0.062)	-0.068	(0.123)
100	0.159	(0.043)	-0.099	(0.116)
101	0.109	(0.056)	-0.044	(0.116)
102	0.206	(0.066)	-0.083	(0.128)
103	0.129	(0.066)	-0.047	(0.127)
104	0.051	(0.094)	-0.064	(0.126)
105	0.074	(0.076)	-0.098	(0.142)
106	0.102	(0.072)	-0.069	(0.144)
107	0.178	(0.077)	0.132	(0.124)
108	0.191	(0.074)	0.131	(0.119)
109	0.261	(0.064)	0.146	(0.124)
110	0.186	(0.078)	0.228	(0.139)
111	0.297	(0.088)	0.193	(0.128)
112	0.243	(0.075)	0.043	(0.110)
113	0.254	(0.117)	0.133	(0.114)
Obs.	9,449		9,449	
R-sq.	0.721		0.739	

Notes: Data come from DW-NOMINATE. DD coefficients from estimation of district-level analogue of Equation 2.1 including county and Census division-year fixed effects displayed. Standard errors clustered at the state-level in parentheses. DW-NOMINATE scores collapse Congressional voting into a two-dimensional ideal point, scaled from -1 to 1. The first dimension broadly measures a district representative's conservatism in a given session of Congress (higher is more conservative). The second dimension historically captured attitudes surrounding policy issues that cut across party lines, but has provided little additional explanatory power since the 1960's. The omitted period is the 93rd session of Congress, which ended in 1975.

Table 1.B2. Alternative Standard Errors

		Standard Errors				p-values
		cluster state	cluster state, year	cluster county	heterosk. robust	wild-t boot
PC x Year	Coef.	(1)	(2)	(3)	(4)	(5)
		Panel A: DV=Voter Turnout				
1960	0.012	(0.014)	(0.003)	(0.013)	(0.019)	0.116
1964	0.006	(0.008)	(0.010)	(0.010)	(0.022)	0.110
1968	0.013	(0.003)	(0.010)	(0.007)	(0.020)	0.068
1972	-	-	-	-	-	-
1976	0.021	(0.005)	(0.003)	(0.009)	(0.019)	0.000
1980	0.043	(0.006)	(0.006)	(0.013)	(0.017)	0.058
1984	0.051	(0.008)	(0.003)	(0.009)	(0.017)	0.054
1988	0.066	(0.008)	(0.003)	(0.011)	(0.016)	0.000
1992	0.061	(0.010)	(0.009)	(0.015)	(0.016)	0.086
1996	0.079	(0.008)	(0.016)	(0.020)	(0.017)	0.136
2000	0.083	(0.010)	(0.017)	(0.021)	(0.016)	0.014
2004	0.081	(0.011)	(0.017)	(0.021)	(0.016)	0.092
2008	0.078	(0.012)	(0.017)	(0.021)	(0.016)	0.132
2012	0.081	(0.014)	(0.014)	(0.018)	(0.016)	0.162
2016	0.078	(0.009)	(0.016)	(0.021)	(0.017)	0.096
		Panel B: DV=Dem. Vote Share				
1960	0.005	(0.026)	(0.018)	(0.024)	(0.032)	0.076
1964	0.018	(0.019)	(0.016)	(0.016)	(0.027)	0.068
1968	-0.000	(0.020)	(0.019)	(0.018)	(0.032)	0.064
1972	-	-	-	-	-	-
1976	-0.026	(0.009)	(0.009)	(0.012)	(0.022)	0.054
1980	-0.002	(0.013)	(0.007)	(0.015)	(0.019)	0.000
1984	-0.027	(0.017)	(0.014)	(0.019)	(0.020)	0.046
1988	-0.031	(0.023)	(0.018)	(0.025)	(0.021)	0.068
1992	-0.049	(0.023)	(0.018)	(0.025)	(0.022)	0.000
1996	-0.048	(0.018)	(0.014)	(0.022)	(0.020)	0.044
2000	-0.053	(0.019)	(0.017)	(0.021)	(0.020)	0.100
2004	-0.046	(0.018)	(0.014)	(0.020)	(0.018)	0.114
2008	-0.035	(0.019)	(0.015)	(0.021)	(0.017)	0.140
2012	-0.025	(0.019)	(0.015)	(0.024)	(0.019)	0.082
2016	-0.012	(0.026)	(0.020)	(0.030)	(0.024)	0.158

Notes: Data come from ICPSR and Dave Leip's Election Atlas. Standard errors calculated with various methodologies in parentheses, p-values from bootstrapping listed in Column (5). Coefficients and state-clusterd standard errors (shown in Column 1) are derived from main estimation results plotted in Figures 1.3 and 1.5.

Table 1.B3. Placebo Tests: Random Assignment and Population Measures

	Treatment Placebos				Outcome Placebos			
PC x Year	(1)		(2)		(3)		(4)	
	DV=Turnout		*DV=Dem. Share*		*DV=ln(VAP)*		*DV=% Minority*	
1960	0.000	0.008	-0.001	0.008	-0.010	(0.082)	-0.005	(0.010)
1964	0.000	0.008	0.000	0.007	-0.007	(0.050)	-0.002	(0.006)
1968	0.000	0.005	-0.000	0.007	-0.005	(0.024)	-0.001	(0.003)
1972			-	-	-	-	-	-
1976	0.000	0.005	0.000	0.007	-0.004	(0.008)	-0.000	(0.002)
1980	0.001	0.007	0.000	0.008	-0.016	(0.017)	0.001	(0.004)
1984	0.001	0.007	0.000	0.007	-0.003	(0.017)	0.002	(0.003)
1988	0.001	0.008	0.000	0.008	0.011	(0.021)	0.002	(0.003)
1992	0.001	0.010	0.000	0.009	-0.019	(0.027)	0.005	(0.004)
1996	0.001	0.010	-0.001	0.010	-0.024	(0.033)	0.005	(0.003)
2000	0.001	0.009	-0.001	0.011	0.004	(0.025)	0.004	(0.005)
2004	0.001	0.010	-0.001	0.011	0.020	(0.027)	0.003	(0.004)
2008	0.000	0.010	-0.001	0.012	0.020	(0.028)	0.002	(0.004)
2012	0.001	0.009	-0.001	0.013	0.028	(0.026)	0.003	(0.005)
2016	0.000	0.010	-0.001	0.014	0.027	(0.028)	0.002	(0.006)
PC=	Assigned Randomly				Actual Treatment			
Obs.					37,605		37,605	
R-sq.	500 reps		500 reps		0.994		0.965	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 2.1 with randomly assigned treatment status and for outcome placebos displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes demographic controls.

Table 1.B4. Placebo Tests: Faux-Triggers

Less x Year	(1)		(2)	
	Panel A: DV=Voter Turnout			
1960	-0.020	(0.023)	-0.022	(0.015)
1964	-0.017	(0.014)	-0.014	(0.010)
1968	-0.027	(0.019)	-0.020	(0.010)
1972	-	-	-	-
1976	-0.018	(0.012)	0.006	(0.007)
1980	-0.007	(0.015)	0.004	(0.010)
1984	-0.015	(0.014)	0.009	(0.012)
1988	-0.011	(0.016)	0.015	(0.011)
1992	-0.010	(0.018)	0.023	(0.013)
1996	-0.015	(0.014)	0.016	(0.013)
2000	-0.018	(0.019)	-0.002	(0.015)
2004	-0.006	(0.020)	-0.001	(0.014)
2008	-0.001	(0.022)	-0.008	(0.014)
2012	-0.008	(0.025)	-0.019	(0.017)
2016	0.006	(0.025)	-0.007	(0.016)
	Panel B: DV=Dem. Vote Share			
1960	-0.018	(0.021)	0.010	(0.010)
1964	-0.034	(0.021)	-0.019	(0.013)
1968	-0.021	(0.016)	-0.008	(0.008)
1972	-	-	-	-
1976	-0.025	(0.014)	-0.013	(0.009)
1980	-0.006	(0.013)	0.006	(0.007)
1984	-0.012	(0.013)	-0.017	(0.009)
1988	-0.008	(0.015)	-0.026	(0.011)
1992	-0.012	(0.014)	-0.015	(0.012)
1996	0.008	(0.015)	0.021	(0.018)
2000	0.026	(0.014)	0.025	(0.020)
2004	0.027	(0.014)	0.016	(0.021)
2008	0.042	(0.017)	-0.008	(0.020)
2012	0.052	(0.019)	-0.000	(0.021)
2016	0.047	(0.022)	0.048	(0.025)
Turnout	40-50		50-60	
Faux-Trigger	<45		<55	
Obs.	1,530		1,805	
R-sq.	1.000		1.000	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 2.1 examining counties near the turnout cutoff and using "faux" treatment triggers. Column 1 restricts the sample to high-minority counties with 1972 turnout between 40-50% and sets a placebo trigger at 45%. Column 2 restricts the sample to high-minority counties with 1972 turnout between 50-60% and sets a placebo trigger at 55%. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes demographic controls.

Table 1.B5. Alternative Controls

PC x Year	(1)		(2)		(3)	
			Panel A: DV=Voter Turnout			
1960	0.009	(0.015)	0.008	(0.015)	0.007	(0.014)
1964	0.005	(0.008)	0.005	(0.008)	0.003	(0.009)
1968	0.012	(0.003)	0.011	(0.003)	0.011	(0.003)
1972	-	-	-	-	-	-
1976	-0.003	(0.006)	-0.020	(0.014)	0.012	(0.005)
1980	0.014	(0.007)	0.012	(0.008)	0.029	(0.006)
1984	0.025	(0.004)	0.022	(0.006)	0.041	(0.007)
1988	0.035	(0.007)	0.041	(0.011)	0.051	(0.008)
1992	0.033	(0.008)	0.050	(0.011)	0.038	(0.009)
1996	0.044	(0.011)	0.061	(0.008)	0.051	(0.011)
2000	0.050	(0.011)	0.067	(0.011)	0.060	(0.012)
2004	0.043	(0.012)	0.065	(0.012)	0.052	(0.013)
2008	0.039	(0.011)	0.060	(0.013)	0.048	(0.013)
2012	0.046	(0.011)	0.062	(0.015)	0.052	(0.014)
2016	0.048	(0.010)	0.054	(0.009)	0.048	(0.011)
			Panel B: DV=Dem. Vote Share			
1960	0.006	(0.026)	0.006	(0.026)	0.003	(0.030)
1964	0.018	(0.019)	0.018	(0.019)	0.017	(0.021)
1968	0.001	(0.020)	0.000	(0.020)	-0.001	(0.021)
1972	-	-	-	-	-	-
1976	-0.010	(0.005)	-0.013	(0.005)	-0.021	(0.008)
1980	0.015	(0.008)	0.017	(0.009)	0.003	(0.013)
1984	-0.010	(0.013)	-0.011	(0.017)	-0.022	(0.016)
1988	-0.014	(0.022)	-0.011	(0.024)	-0.026	(0.022)
1992	-0.036	(0.022)	-0.038	(0.023)	-0.051	(0.023)
1996	-0.035	(0.016)	-0.048	(0.017)	-0.046	(0.018)
2000	-0.040	(0.017)	-0.060	(0.021)	-0.050	(0.020)
2004	-0.033	(0.019)	-0.045	(0.018)	-0.042	(0.018)
2008	-0.022	(0.022)	-0.033	(0.019)	-0.030	(0.019)
2012	-0.015	(0.021)	-0.031	(0.018)	-0.021	(0.020)
2016	-0.008	(0.027)	-0.023	(0.024)	-0.009	(0.026)
Add'l Ctrls.	-		Bilingual x Year		Demo. (Time-Varying)	
Omitted Ctrls.	Bilingual		-		-	
Obs.	37,606		37,606		37,604	
R-sq.	0.926		0.931		0.933	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 2.1 with alternative control specifications displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes demographic controls.

Table 1.B6. Alternative Fixed Effects

PC x Year	(1)		(2)	
	Panel A: DV=Voter Turnout			
1960	-0.027	(0.025)	0.015	(0.029)
1964	0.018	(0.024)	0.005	(0.019)
1968	-0.021	(0.010)	-0.015	(0.010)
1972	-	-	-	-
1976	0.062	(0.015)	0.040	(0.014)
1980	0.066	(0.015)	0.049	(0.016)
1984	0.099	(0.022)	0.066	(0.013)
1988	0.102	(0.017)	0.079	(0.014)
1992	0.076	(0.012)	0.061	(0.009)
1996	0.065	(0.014)	0.068	(0.013)
2000	0.061	(0.018)	0.075	(0.015)
2004	0.043	(0.015)	0.067	(0.012)
2008	0.037	(0.016)	0.072	(0.016)
2012	0.037	(0.018)	0.073	(0.021)
2016	0.053	(0.023)	0.069	(0.017)
	Panel B: DV=Dem. Vote Share			
1960	0.025	(0.024)	-0.017	(0.017)
1964	0.011	(0.031)	-0.008	(0.031)
1968	0.057	(0.020)	-0.005	(0.011)
1972	-	-	-	-
1976	0.013	(0.019)	-0.080	(0.023)
1980	-0.017	(0.021)	-0.052	(0.016)
1984	-0.039	(0.011)	-0.060	(0.011)
1988	-0.023	(0.011)	-0.068	(0.016)
1992	-0.046	(0.013)	-0.087	(0.026)
1996	-0.051	(0.020)	-0.076	(0.030)
2000	-0.080	(0.025)	-0.075	(0.032)
2004	-0.072	(0.023)	-0.066	(0.026)
2008	-0.072	(0.016)	-0.055	(0.013)
2012	-0.069	(0.019)	-0.045	(0.015)
2016	-0.016	(0.012)	-0.015	(0.014)
Add'l Ctrls.	Year, Polltax State x Year		Division x Year, Polltax State x Year	
Obs.	37,620		37,620	
R-sq.	0.760		0.838	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 2.1 with alternative control specifications displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes demographic controls.

Table 1.B7. Alternative Outcomes

PC x Year	(1)		(2)		(3)	
	DV=ln(Turnout)		*DV=ln(Dem. Share)*		*DV=Dem. Share (All)*	
1960	0.019	(0.027)	-0.005	(0.040)	0.005	(0.026)
1964	0.008	(0.016)	0.021	(0.015)	0.018	(0.019)
1968	0.020	(0.006)	0.001	(0.029)	-0.011	(0.011)
1972	-	-	-	-	-	-
1976	0.013	(0.011)	-0.066	(0.029)	-0.025	(0.009)
1980	0.048	(0.011)	-0.013	(0.038)	0.001	(0.013)
1984	0.073	(0.022)	-0.078	(0.048)	-0.028	(0.016)
1988	0.090	(0.026)	-0.082	(0.059)	-0.031	(0.022)
1992	0.079	(0.030)	-0.112	(0.056)	-0.039	(0.018)
1996	0.095	(0.020)	-0.106	(0.049)	-0.042	(0.018)
2000	0.105	(0.029)	-0.121	(0.052)	-0.051	(0.020)
2004	0.095	(0.032)	-0.101	(0.048)	-0.047	(0.017)
2008	0.090	(0.034)	-0.070	(0.044)	-0.036	(0.018)
2012	0.095	(0.041)	-0.054	(0.045)	-0.027	(0.019)
2016	0.090	(0.028)	-0.022	(0.053)	-0.013	(0.024)
Obs.	37,606		37,576		37,576	
R-sq.	0.998		0.9		0.921	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 2.1 for alternative outcomes displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes demographic controls.

Table 1.B8. Counties Covered under All VRA Versions

PC x Rel.Time	(1)		(2)	
	DV=Voter Turnout			
-4	-0.005	(0.029)	0.000	(0.028)
-3	0.012	(0.012)	0.032	(0.017)
-2	-0.040	(0.020)	-0.027	(0.017)
-1	-	-	-	-
0	0.020	(0.019)	0.035	(0.019)
1	0.006	(0.025)	0.022	(0.025)
2	0.024	(0.026)	0.038	(0.024)
3	0.038	(0.026)	0.048	(0.025)
4	0.041	(0.024)	0.045	(0.024)
5	0.040	(0.026)	0.046	(0.026)
6	0.050	(0.024)	0.058	(0.024)
7	0.056	(0.021)	0.060	(0.022)
8	0.052	(0.023)	0.057	(0.024)
9	0.045	(0.024)	0.048	(0.024)
10	0.059	(0.024)	0.056	(0.024)
11	0.067	(0.023)	0.064	(0.023)
12	0.065	(0.024)	0.068	(0.026)
Demo. Ctrls.	-		Yes	
Obs.	46,591		46,533	
R-sq.	0.837		0.847	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 1.7 displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes all counties brought under preclearance coverage by the 1965, 1970 and 1975 versions of the VRA. Relative time corresponds to the number of presidential elections before/after treatment.

Chapter 2

The Effects of Police Violence on Inner-City Students

Abstract

Roughly a thousand individuals are killed by American law enforcement each year. While use of force is often necessary to protect innocent civilians from imminent harm, survey and anecdotal evidence suggest that acts of police violence may also have unintended consequences. Examining administrative panel data for over 700,000 public high school students and incident-level information for more than 600 officer-involved killings in Los Angeles County from 2002 to 2016, this paper estimates the causal effects of police violence on the educational and psychological outcomes of inner-city students. Employing a difference-in-difference design, I find that students living within half a mile of a police shooting experience significant and persistent decreases in grade point average, relative to others in their neighborhood. Notably, academic changes are driven primarily by minority students and police killings of minority suspects. Drawing on contextual details from county attorney incident reports, I show that the magnitude of these effects increases with the demographic similarity of students and suspects and decreases with the apparent reasonableness of lethal force. Analysis of clinical and survey data reveals that students exposed to police shootings are significantly more likely to be diagnosed with emotional disturbance, a chronic learning disability associated with PTSD and depression, and to report feeling unsafe in their neighborhoods. In the long-run, students that are exposed to police violence in the 9th grade

go on to obtain 0.25 fewer years of schooling, are 15% less likely to graduate from high school and 18% less likely to enroll in college than their peers. Taken together, these findings suggest that acts of police violence may exert large unseen costs on minority communities and that these effects are driven in part by fear and trauma.

I Introduction

A central role of the state is to ensure public safety (Atkinson and Stiglitz, 2015). As means of achieving this, American law enforcement officers are afforded broad discretion over the use of force. Given the prevalence of violent crime in many neighborhoods, use of force is a routine part of daily policing, and roughly a thousand individuals are killed by American law enforcement each year. In addition to protecting innocent lives from imminent harm, these incidents may also serve as important deterrents against future criminal activity (Becker, 1968).

However, acts of police violence may also produce unintended consequences. As Patrick Murphy, former commissioner of the New York police department, stated, these events may have a "woeful impact on citizens' perceptions of the fairness and decency of police agencies." Indeed, a large literature has shown that use of force may have a detrimental effect on attitudes towards law enforcement in many communities (Skolnick and Fyfe, 1993; Weitzer and Tuch, 2004; Brunson and Miller, 2005). Negative perceptions may then spill into other domains, leading to increased fear (Hale, 1996; Renauer, 2007) or legal cynicism (Carr et al., 2007; Kirk and Papachristos, 2011).

In this light, the effects of police violence may be ambiguous, bolstering perceived safety in some areas while diminishing it in others. Investigating these effects is critical not only to understanding police-community relations but also to examining the role that perceptions of fear and inclusion may play in determining other important political and economic outcomes.

Though recent work has sought to explore the consequences of police violence, these efforts rely on retrospective interviews or case studies of well-publicized incidents (Sigelman et al., 1997; Desmond et al., 2016; Gershenson and Hayes, 2017). Thus, while informative, these findings cannot account for the non-random nature of police force and may not apply to the overwhelming majority of incidents that go unreported in media. Additionally, existing studies focus primarily on the effects of police violence on perceptions of and interactions with law enforcement and are unable to provide insight into long-run economic or psychological implications.

This paper thus seeks to provide a comprehensive analysis of the effects of police violence on inner-city students, whose physical and emotional vulnerability is well-documented (Bowen and Bowen, 1999; May and Dunaway, 2000). By examining changes in student attendance, achievement and attainment, I am able to identify the short- and long-run human capital implications of exposure to police violence. Furthermore, by investigating effects on student beliefs and mental health, I shed light on the extent to which these effects are mediated by psychological factors such as trauma and fear.

To address the potential endogeneity of police violence, I employ two novel and highly-detailed datasets. In particular, I combine incident-level data on the known universe of officer-involved killings in Los Angeles County, California from 2002 to 2016 with individual-level panel data for all high school students enrolled in Los Angeles Unified School District, the nation's second largest public school system, over this same period. By geo-coding the precise location of the 627 incidents and 712,954 student home addresses, I am able to calculate each student's geographic proximity to police killings. Employing a flexible difference-in-differences framework, I then exploit variation in the exact timing and location of shootings to generate causal estimates of the effects of police violence exposure on student outcomes.

I find that students are significantly and adversely impacted by police violence. Relative to others in their neighborhood, students living within 0.50 miles — or roughly four city blocks — of an incident demonstrate immediate and significant reductions in academic achievement of 2–4% that persist for several semesters. Examining daily attendance data, I show that exposed students also demonstrate immediate, but short-lived, spikes in absenteeism.

In unpacking these findings, I find that achievement effects vary widely according to the race of students and suspects. Strikingly, the observed changes are driven primarily by black and Hispanic students in response to police shootings of other minorities. I detect no significant effects of minority exposure to police killings of whites. I also find little effect of police killings on white students, though statistical precision is limited in this case. By restricting the analysis to school attendance zones that experienced killings of both whites and minorities, I provide evidence that minority responses to police violence are in fact mediated by the race of the suspect, as opposed to, for example, neighborhood differences in where minority and white fatalities occur.

Exploiting hand-coded information from Los Angeles County Attorney incident reports, I find larger minority effects following police violence against more similar suspects. That is, students are more negatively affected by killings of individuals that "look" like them, as measured by demographic characteristics like gender, age, race and criminal background. I also find evidence that minorities are more negatively influenced by police killings that appear *less* reasonable, as proxied by contextual details such as whether the suspect was armed and whether police attempted non-lethal methods of detaining the suspect prior to the shooting. These findings suggest that student responses to police violence may be driven by perceptions of injustice or institutional distrust.

I explore psychological mechanisms more directly using clinical and survey data. Examining student mental health records, I show that exposed students are 15% more likely to be diagnosed with emotional disturbance — a chronic learning disability associated with depression and PTSD, which "cannot be explained by intellectual, sensory or health factors" — and that this effect persists for several semesters. Evidence of psychological trauma is also reflected in student surveys, which show that minorities report feeling significantly *less* safe in their neighborhoods following officer-involved killings.

Finally, I show that these findings cannot be explained by a simple "violence" effect. Exploring data on all gun-related homicides in Los Angeles, I find that, while similar in direction, the effects of *non-police* killings are roughly half as large as those of *police* killings. Furthermore, in contrast to the stark racial heterogeneity observed for police killings, student responses to *non-police* killings of minorities and whites are nearly identical.

In the long-run, students exposed to police violence in the 9th grade obtain 0.25 years less education, are 15% less likely to graduate and 18% less likely to enroll in college than their peers. Taken together, these findings suggest that acts of police violence have large unseen costs on human capital accumulation and that these costs are borne disproportionately by minorities. Rather than bolstering feelings of safety, police use of force may instead cause students to feel traumatized, insecure or disenfranchised, consistent with the literature on violence (Singer et al., 1995; Sharkey, 2010), discrimination (Abramson et al., 1978), and legal cynicism (Baumer, 2002; Bobo and Thompson, 2006; Sampson, 2012).

This paper makes several contributions. First, it complements Becker's work on the economics of crime and punishment (Becker, 1968). Here, I provide evidence that police activity may exert large externalities on local communities. Extrapolating from this paper's findings suggests that each police shooting in Los Angeles is associated with approximately two million dollars in lost earnings due to foregone schooling. These costs are disproportionately borne by minority students and are accompanied by unquantifiable psychological and emotional tolls.

Second, this paper joins recent work in economics (Callen et al., 2014; Malmendier and Nagel, 2015) drawing on insights from psychology to demonstrate the importance of lived experiences on decision-making. By bringing to bear rich, student-level data, I shed light not only on how exposure to traumatic events impacts children's psychological well-being, but also, how those behavioral responses translate to immediate and long-run educational outcomes. In doing so, I also contribute to literature examining how students are influenced by the neighborhoods they live in (Katz et al., 2001; Harding et al., 2010; Chetty et al., 2016).

Perhaps most importantly, this paper may help to inform school administrators and counselors as they shepherd at-risk students through the education system. Understanding the effects that neighborhood factors like police violence may have on children is critical to the design and implementation of interventions aimed at mitigating those effects. Given the importance of education across a range of domains (Angrist and Krueger, 1991; Kenkel, 1991), such efforts may have large ramifications on the long-run outcomes of underserved communities.

The remainder of this paper is organized as follows: Section II describes the background and setting, Section II details the data, Section IV summarizes my identification strategy and estimating equation, Section V presents estimation results for academic achievement, Section VI explores mechanisms, Section VII examines long-run implications, Section VIII discusses costs and potential policy interventions and Section IX concludes.

II Background

A Use of Force

The ability of police to employ lethal force is long established in American history, dating back to English common law.[1] While Supreme Court rulings in Tennessee v. Garner (1985) and Graham v. Connor (1989) defined legal standards for "objective reasonableness," police officers are afforded broad discretion over the use of force and criminal charges are filed for fewer than half a percent of officer-involved shootings.[2]

Often these incidents represent clear attempts to protect innocent lives from imminent danger. Indeed, nearly 60% of individuals killed by police in recent years had openly brandished or fired a gun, and officer-involved shootings are significantly more likely to occur in areas with high homicide rates (Kania and Mackey, 1977; Jacobs, 1998). Extrapolating from Becker (1968), police shootings may also help to deter *future* criminal activity by demonstrating the high potential price of law-breaking. This prediction is supported by Cloninger (1991), who finds that an area's incidence of police shootings is inversely related to its rate of non-homicide violent crime. In reducing crime, police shootings may thus foster feelings of public safety (Nofziger and Williams, 2005), which are in turn correlated with educational aspirations and academic performance (Spera et al., 2009). Given the large material costs associated with crime, police use of force may thus exert a wide range of tangible and intangible benefits on local communities.

However, these acts are nevertheless a form of violence, exposure to which is associated with

[1] As recently as 1985, twenty-four states maintained "any-felony rules," allowing police to use *any* means necessary to arrest or detain felony suspects, regardless of threat or circumstance.

[2] Official guidelines can vary widely across agencies, and even within departments. A Department of Justice report described Philadelphia Police Department's use of force policy as alternately "vague", "fragmented" and "confusing," while federal investigators found that only one in six Chicago police recruits "came close to properly articulating" the standards for use of force (Department of Justice, 2015, 2017).

detrimental changes in cognitive functioning (Sharkey et al., 2012), propensity towards violence (Miguel et al., 2011; Widom, 1989), and academic performance (Gershenson and Tekin, 2017; Burdick-Will, 2013). In this light, it is possible that acts of police force may have the opposite effect as intended, inciting trauma and fear among nearby citizens. Furthermore, as Brandl et al. (2001) argues, reactions to police actions may depend not only on legal concepts of "reasonableness" but also, on community *perceptions* of "unfair, unjust or otherwise unequal treatment from police." Thus, even when legally defensible, use of force may incite cynicism about law enforcement, the criminal justice system or government as a whole (Kirk and Papachristos, 2011), leading to significant changes in civic and social engagement (Desmond et al., 2016; Ang, 2017a).

In striving towards a full accounting of the effects of police violence, one population of particular interest is juveniles. The reasons for this are many. First, relative to adults, teenagers experience disproportionate contact with police (Leiber et al., 1998). Second, as these interactions occur not simply in the context of law enforcement but also through social workers and schools (Kenney and Pursuit, 1995), young adults are more likely to be emotionally and physically influenced by police actions (Hurst and Frank, 2000). Third, decisions made during an individual's adolescent years — particularly as related to schooling decisions — carry outsized importance across a wide-range of socioeconomic outcomes (Heckman, 2006).

B Los Angeles

A natural setting to examine these effects is Los Angeles. Greater Los Angeles experiences more officer-involved killings — roughly 50 a year — than any other county and a higher per capita rate of fatal police shootings than other urban areas like Chicago and New York.

Many of these events occurred in the inner-city neighborhoods served by Los Angeles Unified School District (LAUSD). As the second largest public school system in the country, LAUSD encompasses Los Angeles City as well as 25 adjoining cities in the county. In total, the District operates 1,132 schools and serves over 700,000 students a year, of whom 72% are Hispanic, 12% are black, and 9% are white.[3] The majority of these students come from disadvantaged

[3]LAUSD demographics differ from those of the county as a whole, which is comprised of approximately 48%

households with 67% qualifying for free or subsidized lunch and only 8% with college-educated parents. LAUSD students are also frequently exposed to violence. A 2012 survey found that 40% of 6th graders in the District had witnessed or been victim to an armed violent encounter in the prior year (Ramirez et al., 2012).

Given the at-risk population it serves, the District employs the country's largest school police force and was the first in the nation to establish a crisis intervention unit. LAUSD also pioneered classroom-based cognitive behavioral therapies for trauma that have since been adopted by schools around the nation. However, despite these efforts, student performance at LAUSD ranks behind most California public school systems. Over the past few years, LAUSD's reported four year graduation rate has ranged from 65-70% and fewer than 15% of juniors demonstrated college preparedness in English and Math.[4]

III Data

A Police Killings

Suspect, Date and Location

Incident-level data on police killings come from the Los Angeles Times Homicide Database, which chronicles all deaths in the county committed by a "human hand." Whether an officer was involved is based on information from the coroner and police agencies as well as from LA Times' own research.[5] For each incident, the LA Times records the name, race, cause of death of the deceased suspect as well as the exact address, time and date of the event. In total, the data contains 627 incidents resulting in 325 Hispanic, 161 black and 117 white fatalities from 2002 to 2016. A summary description of police shootings with respect to media coverage and suspect and officer characteristics is shown in Table 2.1.

Hispanic, 9% black and 28% non-Hispanc white.

[4]Reported graduation rate differs substantially from the 50% matriculation rate found in the data, likely due to variations in calculation.

[5]However, the Times does not report on the majority of these incidents in print.

Contextual Details

For those officer-involved shootings that are officially reported by police departments, the county attorney issues a legal analysis and detailed description of the event based on information compiled from forensic evidence as well as officer and witness testimonies. For each of the 483 reports, I read and hand-coded relevant incident details. These include: the names of the involved police officers, whether officers employed non-lethal force prior to firing their sidearms (14%), whether a suspect was armed with a firearm (47%), and whether a suspect's criminal record is mentioned (31%). In all cases, the county attorney declined to press charges against involved officers.

I also predict the race of involved police officers from their names. To do so, I employed the Consumer Financial Protection Bureau's Bayesian Improved Surname Geocoding (BISG) method, which predicts an individual's race from national surname frequencies and Census block group demographics. I first applied BISG to the set of all registered voters in Los Angeles in 2010, then aggregated the race probabilities by first and last names and matched those to officers. This information is used to determine whether a shooting involved any minority officers (69%).

Media Coverage

Official reports are further supplemented with newspaper data. Specifically, I searched for each suspect by name in one national paper (the Los Angeles Times) and five local papers (the Los Angeles Daily News, Pasadena Star News, San Gabriel Valley Tribune, Torrance Daily Breeze and Whittier Daily News). Combined, the newspapers distribute roughly 1 million copies in the Los Angeles area. In all, 134 shootings (22%) are mentioned in the sample newspapers. Conditional on being reported in a newspaper, the median number of articles is two.

B Student Outcomes

Demographics

Student information comes from LAUSD administrative data and contains individual-level records for all high school students ever enrolled at Los Angeles Unified School District from the 2002-2003 academic year to the 2015-2016 academic year. In total, the dataset contains information for 712,954 unique students. All student information is anonymized. For each student, I have detailed demographic information containing the student's race, gender, date of birth, English literacy, home language, free/subsidized lunch qualification, and parental education. Table 2.2 provides summary statistics, separately for control and treatment students.

Crucially, the data also contains each student's last reported address while enrolled at LAUSD. However, because the data does not track student addresses over time, migration within the District may be of particular concern and is an issue I address in Section IV. The data does, however, contain "leave codes" describing the circumstances under which a student left the District — that is, whether the student moved out of the state or transferred to a school outside of LAUSD.

Academic Performance and Attendance

Semester grade point average is calculated from student transcript data, which lists the name, subject and letter grade of every class a student was enrolled in during a given semester. I code letter grades to numerical scores according to a 4.0 scale. I then average grades in math, science, English and social sciences — the primary subjects used to determine graduation eligibility — by student-semester to produce non-cumulative, semester grade point averages.

Daily attendance for every student is available from the 2009-2010 school year onwards. Each student-date observation contains the number of scheduled classes for which a student was absent on that day. This information is used construct a binary indicator for whether a student was absent on a given day.

Educational Attainment

Educational attainment is measured in a student's matriculation, highest grade and college enrollment. Matriculation is defined as receiving "a high school diploma or equivalent (GED or CHSPE) or a Special Education Certificate of Completion." The data does not distinguish between receipts of diplomas and GEDs.

Highest grade is measured as the highest grade a student was ever enrolled in while at LAUSD with no distinction between whether a student completed that grade or not. While the dataset does contain information on whether a student transferred to another school district, it does not capture any years of schooling or diplomas that a student may have obtained outside of LAUSD.

The administrative data also contains information on whether students enrolled in college. This information is available for students that graduated high school from 2009 to 2014 and comes from the National Student Clearinghouse, which provides enrollment and degree verification information for colleges and universities enrolling over 98% of all post-secondary students in the country.

Psychological Outcomes

Though LAUSD does not conduct universal psychological screenings, the data does contain two sources of information regarding student mental and emotional health. The first is the date students were designated by the District as "emotionally disturbed," a federally certified learning disability that "cannot be explained by intellectual, sensory or health factors" and that qualifies for special education accommodations. I map the dates of designation and re-designation to school semesters to create student panel data.

The second dataset contains student responses from LAUSD's School Experience Survey (SES). While the survey is optional, it is generally administered during school hours leading to response rates above 75%. Of particular interest to this study, the survey includes three questions examining students' feelings of school and neighborhood safety. Students answer each question along a Likert scale ranging from 1-5. Responses are confidential except to LAUSD executive management and are never shared with faculty or staff at a student's school. Unfortunately, individual-level SES responses are only available for the 2014-2015 and 2015-2016 academic years.

C Exposure to Police Killings

To calculate exposure to shootings, I geo-code the home address of every student and the location of every police shooting in the database. I then calculate the coordinate distance between each student's address and every police shooting that occurred during that student's high school tenure at LAUSD.

For a given shooting, students are classified as exposed if they lived within 0.50 (0.25) miles — roughly 4 (2) city blocks — of the incident location. On average, 303 (81) students live within 0.50 (0.25) miles of each shooting. Over the course of their LAUSD tenures, 20% (6%) of sample students are ever exposed. Though it is impossible to identify the precise sample of individuals who are aware of a given incident, these definitions take into account the fact that the vast majority of police shootings are unreported in local media and likely known only among family, neighbors and witnesses living near to the event.

Figure 2.1 provides a graphical description of LAUSD boundaries and the address data. The top panel plots the address of every student in the dataset, color-coded by student race. Notably, neighborhoods are fairly segregated, with blacks residing in urban centers such as Central and South Los Angeles and whites living in more affluent areas in the Northwest of the county. Hispanic students, however, live across the county. The bottom panel then plots the location of every police killing, color-coded by suspect race. As shown, police killings demonstrate a similar geographical distribution as student addresses.

[Figure 2.1 about here.]

IV Identification

A Difference-in-Differences

The primary obstacle to causal identification in this setting is that police shootings are not random and may be more likely to occur in neighborhoods where students are most disadvantaged. To account for this potential endogeneity, I rely on a flexible, difference-in-differences framework. This design leverages the panel data to compare changes in the educational outcomes of treatment students before and after exposure to changes over time among control students. In doing so, the model accounts for any level differences that may exist between students that are exposed to violence and those that are not. The validity of this design is further bolstered by the data's granularity, which allows me to control for unobserved time trends in a student's neighborhood, as defined by their Census block group or tract.

Specifically, I exploit the time and spatial variation of police shootings by estimating the following base equation:

$$y_{i,t} = \delta_i + \delta_{n,t} + \delta_{c,t} + \sum_{\tau \neq -1} \beta_\tau Shoot_{t,\tau} + \varepsilon_{i,t}, \tag{2.1}$$

Where $y_{i,t}$ represents educational outcome of individual i at time t. δ_i are individual fixed effects, $\delta_{n,t}$ are neighborhood-time fixed effects, and $\delta_{c,y}$ are cohort-year fixed effects. $Shoot_{t,\tau}$ are relative time to treatment indicators, which are set to 1 for treatment students if time t is τ periods from treatment. Treatment is defined as students living within .5 (.25) miles of an incident during their high school tenure at LAUSD. Drawing on Bertrand et al. (2004), standard errors are clustered by zip code, allowing for correlation of errors within each of the sample's 219 zip codes.[6] The coefficients of interest β_τ then represent the differential change between relative time τ and the last period before treatment among students exposed to police violence relative to that same change

[6]Results are robust to different methods of calculating standard errors, such as clustering by school or Census tract or additionally by time.

over time among unexposed students in the same neighborhood.

This model relies crucially on a parallel trends assumption. While estimates of β_τ for $\tau < 0$ allow me to test for common trends prior to treatment in the attendance and performance data, identification may still be threatened by selective migration or unobserved post-treatment shocks. As I demonstrate in the next section, it appears unlikely that these potential confounds could fully explain the observed treatment effects.

B Selective Migration

A primary concern with identification under difference-in-differences is the possibility of selective migration between treatment and control groups. Migration could be especially relevant in this setting, as exposure to police violence may cause families to relocate or students to drop out. The latter scenario is an outcome of interest in its own right, which I will examine directly in Section VII.

Of greater concern for identification purposes are students who re-locate within the county while remaining enrolled at LAUSD. Because the data only contains a student's most recent address, some students who were exposed to violence at their previous address may be incorrectly marked as control, and vice versa. In either case, the student's educational outcomes are still contained in the dataset. Thus, if errors are uncorrelated with treatment, classical measurement error or attenuation bias would simply bias my estimates towards zero. Census data suggests that measurement error varies little between exposed and unexposed students. 2006-2010 ACS estimates indicate that 86.6% of individuals living in block groups where a police shooting occurred reported residing at the same house one year prior, nearly identical to the 86.8% tenure rate among those living in block groups that did not experience a shooting ($p = .628$).

However, it is possible that these area-level statistics obscure the existence of non-classical measurement error. For example, exposure to police shootings may disproportionately cause high-achieving students to relocate to safer neighborhoods within the county. These students would then be misclassified as untreated, shifting up the average GPA of the control group and driving down (i.e., making negative) the estimated treatment effects. Though I cannot rule out this possibility, the Census data again suggests otherwise.

In Figure 2.A1, I compare 2006-2010 and 2011-2015 ACS estimates of the share of individuals that reported moving *within* the county in the prior year, broken down by income. The left panel plots average move rates by income for Census tracts that did not experience a sample shooting. The right panel plots those same rates for tracts that experienced a shooting *after* the first ACS survey period. Notably, move rates generally decrease with income. While the income curve flattens over time for control tracts, it steepens for treatment tracts. That is, lower income individuals in treatment areas are relatively more likely to move after police shootings, while higher-income individuals are less likely to move.

Using a simple difference-in-differences design to formally test changes between groups, I find that treatment exposure is associated with an insignificant 0.7 percentage point *decrease* in the move rate of high-income individuals (i.e., those earning $50,000 or more) and a significant 1.2 p.p. *increase* in move rate of lower-income individuals.[7] Full estimation results are displayed in Table 2.A1, which shows positive DD coefficients for each income bracket below $50k and negative coefficients for each income bracket above $50k. As household income is positively correlated with student achievement (Heckman, 2006), these findings suggest that if families are relocating within Los Angeles due to police violence, it would likely bias my treatment estimates upwards by leading lower-achieving treatment students to be misclassified as control.

[Note: This section will be expanded to examine the effects of treatment exposure on students' likelihood of changing schools within the District, which is observed in the data. While transfers may occur for other reasons than address changes, this analysis would nonetheless shed light on the potential magnitude of selective attrition among the student population.]

[7]This comes from estimation of $y_{n,t} = \delta_n + \delta_t + \beta Shoot_n \times Post_t + \varepsilon_{n,t}$.

C Crime and Policing Activity

Another concern is that unobserved trends in local crime or policing activity may explain both the presence of police shootings and changes in academic performance. Using a modified version of Equation 2.1, I directly examine the effects of police violence on an area's homicides, reported crimes and arrests. Here, the unit of observation is a Census block-semester. Treatment and relative time to treatment are based on the occurrence of a police shooting in a block, while the outcomes of interest are the number of homicides, arrests, and reported crimes in the block in a given semester.[8]

Results are displayed in Table 2.A2. Across all three outcomes, I find little evidence of differential trends prior to police shootings. This supports the plausible exogeneity of police killings, after conditioning on neighborhood and time. Following acts of police violence, I also find little evidence of differential changes in homicides, crimes or arrests between the streets where these incidents occur and other areas in the same Census tract.

This does not mean that police violence has no effect on crime. It is certainly possible that the deterrence effects of police shootings are not localized to the specific blocks in which they occur, but are instead distributed throughout an entire precinct or city. These effects would then be absorbed by the neighborhood-time fixed effects in the difference-in-differences model. While a thorough investigation of the relationship between police use of force and crime is outside the scope of this paper, these findings demonstrate that differential trends in local crime or policing activity are unlikely to bias my treatment estimates.

[8]Note that while data on homicides is available for the entire sample, information for arrests and non-homicide crimes are only available from 2010 onwards.

V Academic Performance

A Main Results

I first examine the effects of exposure to police shootings on academic performance. Thus, I estimate Equation 2.1 on semester GPA. The omitted period is the last semester prior to treatment.[9] Estimates from my preferred specification — employing block group-semester fixed effects and a treatment radius of .5 miles — are displayed in Figure 2.2 and Column 6 of Table 2.3.

[Figure 2.2 about here.]

Prior to shootings, I find little evidence of differential group trends. For $\tau < 0$, all treatment coefficients are less than .015 points in magnitude and never reach statistical significance, even at the 10% level. Following shootings, grade point average decreases significantly among exposed students. GPA declines by .04 points in the semester of the shooting and by between .06 and .07 points in the following two semesters. These effects gradually dissipate over time reaching insignificance five semesters after exposure. Though the treatment magnitudes may appear small relative to a 4.0 scale, the point estimates represent as much as 86% of the difference between sample average GPA (2.08) and the minimum grade thresholds (2.0) required for University of California and California State University admissions, suggesting that the effects may have large consequences for long-run educational attainment.

Table 2.3 contains results from a variety of different specifications. In Columns 1 through 3, the treatment group is restricted to students living within 0.25 miles of an incident, while Columns 4 through 6, define treatment as students living within 0.50 miles. In all cases, the control group remains constant — students living between .5 miles and 3 miles from an incident.[10] Time-varying Census block-level controls for the occurrence of non-officer-involved homicides are omitted in

[9]For the 5% (15%) of students who are exposed to multiple shootings within .25 (.5) miles during their high school career, I define treatment according to the date of the first shooting. In robustness analysis, I also restrict the sample to students who only experience one shooting and find similar results.

[10]Given that Census block groups (tracts) average approximately .75 (1.75) square miles, restricting or expanding the 3 mile control bandwidth has virtually no effect on the coefficients of interest due to the inclusion of neighborhood-time fixed effects.

Columns 1 and 4 and included in all others. Finally, in Columns 3 and 6, neighborhood-time fixed effects are defined at the block group-level, while in all other columns, neighborhoods are defined at the Census tract level (each tract contains roughly 2.3 block groups).

Across models, I find consistent support for parallel trends prior to treatment and for significant decreases in academic performance immediately after exposure. Examining students living nearest to shootings and Census tract neighborhoods (Columns 1 and 2), the only significant pre-treatment estimate occurs six semesters prior to exposure, with near zero coefficients in the semesters directly prior to a shooting. Though β_{-3} becomes significant once neighborhoods are tightened to Census block groups (Column 3), this coefficient is much smaller in magnitude than the post-treatment estimates. Indeed, as shown in Figure 2.A3, the sharp drop in GPA coinciding with the exact timing of exposure supports the exogeneity of police shootings.

Notably, controlling for incidence of violent crime on a student's street in given semester has virtually no impact on the coefficients of interest (i.e., comparing Columns 1 and 2 and Columns 4 and 5). This suggests that the observed treatment estimates are not confounded by unobserved changes in local crime. These findings also demonstrate that the students are in fact responding to police killings and not, for example, criminal homicides that may have brought police to the scene.

Examining results across treatment exposure (i.e., comparing Columns 1 and 4, Columns 2 and 5 and Columns 3 and 6), estimated effects are smaller for the expanded treatment group including students further from incidents. This is consistent with the limited visibility of police shootings and suggests that the effects of exposure increase with proximity, which will also examine more closely in the following subsections.

As the above estimates pool across students exposed at different grades, I replicate the analysis based on semester of treatment. That is, I restrict the sample to students that entered LAUSD in the 9th grade and estimate Equation 2.1 separately for treatment students that were exposed in their 1st semester, 2nd semester, and so on. These estimates are displayed in Figure 2.A2 and Table 2.A3 for my preferred specification and strongly resemble the pooled effects found above.

Finally, I replicate the primary analysis restricting the treatment group to those students who were only exposed to one shooting. These results are qualitatively similar to my main findings (Table 2.A4). However, if shootings cause some students to drop out, then whether a student experiences multiple shootings is endogenous. For this reason, my preferred specification retains the entire sample of students, defining treatment based on first exposure.

B Secondary Analysis

Attendance

To better understand the academic performance effects, I estimate Equation 2.1 on daily absenteeism. Here, relative time to treatment is measured in school days. Given that a student's attendance decision is made in the morning and that shootings primarily occur in the evening, the omitted period is set to the date of the shooting, rather than the day before. These estimates are shown in Figure 2.A4 and Table 2.A5.

As shown, absenteeism jumps significantly in the immediate aftermath of police homicides. Relative to others in their neighborhood, students exposed to a shooting are .7 p.p. or 15% more likely to miss school the following day. These effects linger for one to two days before quickly receding.

These effects are noteworthy for three reasons. First, they reinforce the exogeneity of police shootings. By examining date-level data, I provide evidence in support of parallel trends even within a semester. For the observed changes to be caused by some unobserved shock, one would need to believe that its effects coincided with the precise calendar date of the police shooting. Second, as researchers have shown attendance to be particularly sensitive to neighborhood violence and concerns for personal safety (Bowen and Bowen, 1999), immediate changes in school attendance suggest that police killings may incite fear among nearby students, something I will examine more directly in Section VI. Third, the short-lived spike in absenteeism is unlikely to explain the large and persistent decreases in grade point average, suggesting that achievement effects are largely driven by changes in student effort and motivation.

Geographic Proximity

To examine how GPA effects are moderated by a student's proximity to an incident, I modify Equation 2.1 replacing relative time to treatment indicators with interactions between a pre-post dummy and a categorical treatment variable based on the distance between a student's home and a shooting (i.e., 0-.1 miles, .1-.2 miles, etc.). I restrict this analysis to those shootings that were *not* reported in Los Angeles newspapers. To better examine changes over distance, I widen the neighborhood-time fixed effects to Census tracts and zip codes.

These results are shown in Figure 2.A5 and Table 2.A6. Notably, I find the largest performance effects among students living nearest to a shooting. These effects then decrease steadily over distance with negative but insignificant estimates for those living more than .2 miles away. In Table 2.A7, I return to my primary flexible specification, replacing the binary treatment group dummy with a treatment intensity variable set to $min[0, 1-(distance \times 2)]$, which decreases linearly with distance. As before, I find that the effects of police violence dissipate over time, with the strongest impact felt among those living nearest to the incident.

These findings can be interpreted in two possible ways. In light of the unreported nature of police shootings, the localized effects may simply be due to differences in information — that is, individuals living more than a few blocks from a shooting may be completely unaware of the incident. However, it is also possible responses to police killings are strongest among those who may have known the person killed or directly witnessed the event.

Media Exposure

As further investigation of the specific role of information, I then exploit variation in media coverage of police shootings. To do this, I merge the newspaper mentions data with zip code-level circulation information from Audited Media. I then estimate a modified version of Equation 2.1 where treatment is an indicator for whether a newspaper that mentioned the shooting circulated in the student's zip code. Because the Los Angeles Times is distributed to every zip code in the county, I restrict the sample to the 40 incidents that were mentioned in local papers but *not* the

Times in order to preserve identifying variation.[11] These results are displayed in Table 2.A8 and demonstrate significant decreases in academic achievement among students living in circulation areas of covering newspapers.

While these findings point to the role that information may play in community responses to police violence, it is important to note that newspaper coverage is likely correlated with other formal and informal information networks — such as television, social media and community relationships — and that the impact of those channels would also be reflected in the above estimates. Another limitation of this analysis is that it necessarily excludes the types of nationally-publicized incidents that may be of greatest public interest and consequence.

Thus, to shed light on high-profile acts of police violence, I exploit temporal variation to examine student attendance around the March 24, 2012 killing of Kendrec McDade, which received more newspaper coverage than any other shooting in the sample. [12]

The top panel of Figure 2.A6 plots the share of students absent throughout the entire District for each calendar day before and after the incident. The bottom panel then plots the number of newspaper articles mentioning the incident published each day. The first vertical line represents the date of the incident, while the second represents the date of the first Los Angeles Times article referencing the event (March 28, 2012). Notably, absenteeism changes little in the days immediately following McDade's death, but spikes as media coverage of the incident emerges, particularly after the first article by the Times. Absenteeism and newspaper mentions then appear to co-move over the following weeks, suggesting that the effects of police violence may be mediated by media coverage.

[11]As treatment varies at the zip code-level, I can no longer include neighborhood fixed effects and instead control for time trends using either semester or semester-District fixed effects, where districts refer to LAUSD's seven Board of Education Districts.

[12]Nearly 250 local articles were written about McDade. Only one other shooting in the sample generated more than 30 articles, that of Ezell Ford, which occurred during the summer.

VI Mechanisms

A Incident Context

Racial Differences

As others have noted, "racial differences in attitudes toward police have been among the most robust findings in criminal justice research" (Taylor et al., 2001). Even after controlling for other socioeconomic predictors, race remains the strongest predictor of perceptions of law enforcement (Weitzer and Tuch, 2002; Leiber et al., 1998). Thus, I extend the primary analysis to examine heterogeneous effects by the race of students and suspects.

As a first step, I estimate Equation 2.1 on GPA, separately for each student-suspect race subsample, replacing the relative time to treatment indicators with a simple post-treatment dummy. These results are shown in Table 2.4, which is split into 12 cells with rows representing the race of the student (black, Hispanic and white) and columns representing the race of the deceased (all races, black, Hispanic and white).

[Table 2.4 about here.]

Examining rows 1 and 2, black and Hispanic students are significantly and adversely affected by police shootings (Column 1). Notably, these effects are driven entirely by shootings of other minorities (Columns 2 and 3) with significant effects for black student-black suspect, Hispanic student-black suspect and Hispanic student-Hispanic suspect pairings.[13] However, exposure to police shootings involving white fatalities has no significant effect on minority outcomes (Column 4). Though standard errors are larger due to reduced power, those coefficients are also close to zero in magnitude and, in the case of Hispanic students, positive in direction.

[13]While a closer examination of differences between Hispanic and black responses is outside the scope of this paper, evidence from LAUSD administrators and McLaughlin et al. (2007) suggest that black students are more likely to externalize behavioral problems (for example, by acting out), while Hispanic students are prone to internalizing symptoms (for example, by becoming withdrawn or depressed). Thus, one possible explanation for why Hispanic students are affected by black killings but blacks are not affected by Hispanic killings is that the former incidents incite more visible community and student reactions. However, this is an area that requires more study.

Among white students (row 3), I find no significant effects of police shootings on performance, either in aggregate or when broken down by suspect race. While coefficients go in the opposite direction of those for minorities, it is difficult to interpret these estimates due to limited power. Thus, for the remainder of this subsection, I restrict the analysis to black and Hispanic students.

One concern in interpreting these results is that the neighborhoods in which whites are killed may differ drastically from those in which minorities are killed and that the observed treatment differences may reflect factors correlated with race rather than race itself. Thus, the above treatment estimates, while internally consistent, may not be comparable across suspect race.

To better understand the racial dynamics, I then limit the analysis to school attendance zones ("catchments") that experienced both minority and white police shootings during the sample period. Assuming students living within a catchment are similar across time, treatment effects between shootings of whites and those of minorities are comparable. To estimate these effects, I employ the following modified version of Equation 2.1 with distinct treatment indicators for minority and white killings:

$$
\begin{aligned}
y_{i,t} = &\delta_i + \delta_{n,t} + \delta_{c,t} + \beta_m ShootMinority_i \times PostMinority_t \\
&+ \beta_w ShootWhite_i \times PostWhite_t + \varepsilon_{i,t},
\end{aligned}
\tag{2.2}
$$

where $ShootMinority_i$ and $ShootWhite_i$ are set to 1 for students that were exposed to police killings of minorities and whites, respectively, and $PostMinority_t$ and $PostWhite_t$ are their respective post-treatment dummies.

Estimation results from this analysis for minority students are shown in Table 2.A9. Columns 1 display results from examining the entire sample of minority students. Column 2 restricts the analysis to those school catchments (28 out of 87) that experienced police shootings of both minorities and whites. As streets where minorities and whites are shot may differ even within a catchment, Column 3 further restricts the sample to students that come from disadvantaged households — i.e., those that qualify for free or subsidized lunch and whose parents did not attend college. In Column 4, I leverage the full sample and employ a propensity score weighting method, weighting each observation by the student's inverse probability of being exposed to both minority and white police shootings (Imbens, 2004).[14]

As shown, results mirror those found in Table 2.4 with negative and significant treatment effects for minority students following minority shootings and insignificant, near zero estimates for white shootings. Notably, the difference between the two treatment coefficients is virtually constant across all four models and significant or borderline significant when assessing the full sample.

One interpretation of these effects is that students react more strongly when they personally identify with the person killed. To explore this possibility, I construct a social distance index to capture the level of interpersonal similarity between minority suspects and minority students. Specifically, the similarity measure is coded from 0 to 4 and is a cumulative score that increments by 1 if the exposed student and suspect are of the same gender (male or female), ethnicity (black or Hispanic), age group (suspect was under 25) or criminal background (suspect had a criminal record).[15] Thus, for a black male student, $similarity_{i,s}$ would be set to 4 for shootings involving black male suspects under 25 without a criminal record and set to 0 for shootings involving older Hispanic female convicts.

[Figure 2.3 about here.]

Results from estimation of Equation 2.2 extended to a five category treatment intensity variable based on exposure within .5 miles and controlling for neighborhood time trends at the Census block group-level are displayed in Figure 2.3. Full estimation results are included in Table 2.A10 for treatment radii of .25 and .5 miles. As shown, treatment effects on GPA increase monotonically in magnitude with interpersonal similarity. In fact, exposure to police shootings of dissimilar suspects ($similarity_{i,s} \leq 1$) have no effect on academic performance with treatment

[14]These probabilities come from a logistic regression of joint exposure on neighborhood diversity, poverty, high school graduation rate and homicides, as well as student poverty status and parental education, correcting for the rarity of joint exposure as per (King and Zeng, 2001).

[15]These characteristics are drawn from Parrillo and Donoghue (2005) and Bogardus (1933), who identify the primary demographic determinants of social distance using national surveys.

coefficients near zero, while shootings of demographically similar suspects lead to large and highly significant decreases in GPA of 8.8 p.p (Wald test of difference: $p = .001$).

Taken together, these results suggest that the specific relationship between student and suspect race plays a direct mediating role on responses to police violence. In particular, minority students are most adversely impacted following police killings of individuals that "look" like them. These findings are consistent with theoretical (Tajfel, 2010) and experimental (Chen and Li, 2009) work on social identity demonstrating that individuals may be more sensitive to the welfare of other in-group members.

Suspect and Officer Characteristics

To better understand minority responses, I exploit heterogeneity in contextual details surrounding police killings. Drawing on information from Los Angeles county attorney reports, I examine differential responses depending on the characteristics of the suspect killed and of the involved police officers. Given evidence that minority students respond only to shootings of other minorities, I focus the analysis on that particular sample.

First, I compare treatment effects based on the threat posed by the suspect — i.e., whether the suspect was armed with a firearm or was a convicted criminal. I also compare effects based on the characteristics of involved officers. Specifically, whether police attempted non-lethal means of arresting the suspect (ex., bean bag gun, Taser, etc.) before discharging their firearm and whether any of the involved officers were minority.[16] In each case, I use Equation 2.2 to estimate treatment effects on the sample of catchments that experienced both forms of shootings (i.e, armed and unarmed, criminal record and non-criminal record, etc.).

[Table 2.5 about here]

These results are displayed in Table 2.5. Though none of the treatment pairs are significantly different from each other at the 5 percent level, I find larger coefficients for incidents involving

[16]Though officer race is a poor predictor of excessive force (Brandl et al., 2001; Worden, 1995), the federal government's 1968 "Kerner Report" — as well as more recent ethnographic studies (Mastrofski et al., 1996, 2002) — have nonetheless found that many minorities associate police with "white power", suggesting that officer race may be highly salient to those groups.

unarmed suspects, for incidents in which a minority officer was *not* involved and for incidents in which non-lethal methods were *not* deployed. Interestingly, I find little evidence of differential effects depending on the suspect's criminal background. One possible explanation is that, unlike the other characteristics, a suspect's criminal record may not be immediately salient to witnesses of an incident.[17]

To improve precision, I sum all four of the previous characteristics. Thus, the combined index is set to 0 for incidents involving suspects that were unarmed and did not possess a criminal record and non-minority officers that employed lethal force without first attempting non-lethal measures, and set to 4 for shootings that involving armed suspects with criminal records and minority officers that first deployed a Taser or bean bag gun. As shown in the last row of Table 2.5, responses to these types of incidents differ drastically.[18] Specifically, treatment effects are highly significant and large (-0.063 points) for the former but precisely zero (-0.008 points) for the latter (Wald test of difference: p-value=0.013).

In interpreting these effects, note that the incident details analyzed here may fail to accurately capture the threat posed by the suspect or the reasonableness of police actions. For example, it is possible that unarmed suspects may have been killed while attempting to grab an officer's firearm. Indeed, the LA county attorney conducted a detailed incident review of each of these cases and deemed police actions lawful in all instances. Thus, the observed heterogeneity cannot be explained by differences in the legality of police behavior. Nonetheless, these contextual details often inform citizens' *perceptions* of the appropriateness of force (Brandl et al., 2001; Hall et al., 2016). In this light, the findings suggest that academic performance is strongly and negatively affected by police shootings that are *perceived* to be less reasonable.

[17] Another possibility is that the criminal record is a noisy indicator, both due to variance in the severity of individual's criminal behavior and because a suspect's record may be not be accurately captured in the dataset. That is, it is possible that reports may not mention a suspect's criminal record even if he or she had been arrested or convicted in the past.

[18] Estimation results for all five treatment categories are shown in Table 2.A10, Column 4.

B Psychological Trauma

Safety

To examine psychological mechanisms directly, I turn to LAUSD's clinical and survey data. First, I examine changes in self-reported feelings of safety using data from LAUSD's School Experience Surveys (SES). As individually-identified responses are only available for the 2015 and 2016 academic years, I estimate Equation 2.1 using a simple post-treatment dummy, which is set to one for students exposed to shootings between the end of the survey's 2015 administration period in April 2015 and the beginning of the 2016 administration period in January 2016. To bolster internal validity, I also restrict the sample to students that had not been treated prior to the administration of the first survey.

Though the SES questionnaire contains nearly a hundred questions across twelve different categories, I restrict the analysis to the three survey questions concerning perceptions of safety. This is both because changes in public safety are the most natural outcome of law enforcement actions and because doing so limits concerns with multiple hypothesis testing. For each question, students are asked to report how safe they feel along a 1-5 scale with higher scores indicating stronger feelings of safety.

Table 2.8 displays estimated treatment effects for each of the questions as well as for the average of all three. As shown, when controlling for neighborhood trends at either the Census tract (Column 1) or block group (Column 2) level, I find highly significant and negative effects of treatment exposure on average feelings of safety, indicating that students feel *less* safe following police shootings.

Examining each question separately, I find negative coefficients for all three, two of which are significant at either the 5 or 10 percent-level — those for "How safe do you feel in the neighborhood around the school" and for "How safe do you feel when you are at school," respectively. Notably, however, the treatment coefficient for neighborhood safety is statistically significant across both models and is substantially larger than those for school safety, both in absolute and percentage terms. This is consistent with the fact that exposure to violence is calculated based on home addresses and suggests that safety concerns sparked by police violence are felt most strongly outside of school.

These findings are presented with caveats. First, the survey is voluntary. While response rates are relatively high (around 75%), the results may suffer from biases due to selective participation. Second, student responses are self-reported and correspond to narrowly defined questions that may reveal little about student's actual behaviors or beliefs. Last, the analysis includes only two years of data. Thus, to the extent that the survey does capture actual changes in beliefs, those effects are necessarily short-term and may not correlate with the longer-run changes on academic performance.

Nonetheless, these findings provide important and novel insight into the effects of police violence on student beliefs and emotions, especially given that the existing literature in psychology and sociology relies on priming or contextually specific questions (Brunson and Miller, 2005; Broman et al., 2000). Here, I find evidence that students exposed to police violence report feeling *less* safe without artifactual devices other than the survey itself.

[Note: This section will be updated to explore biases due to selective responses.]

Emotional Disturbance

Increased fear suggests that police violence may have significant impacts on student psychological well-being. I next explore these effects using data on clinical diagnoses of "emotional disturbance." Emotional disturbance (ED) is a federally certified disability defined as a "general pervasive mood of unhappiness or depression," "a tendency to develop physical symptoms or fears," or "an inability to learn which cannot be explained by intellectual, sensory, or health factors." While there exists no single specific cause of emotional disturbance, its symptomatology and incidence are strongly linked with post-traumatic stress disorder (Mueser and Taub, 2008).

[Figure 2.4 about here.]

Figure 2.4 and Table 2.7 display results from estimation of Equation 2.1 on incidence of ED. As shown, students exposed to police violence are significantly more likely to be classified as emotionally disturbed. Though the treatment estimates are small, ranging from 0.04 p.p. to 0.07

p.p., they are highly significant and represent an 8-15% increase over the mean (0.5% of sample students are classified with ED in a given year).

While it is possible that these effects are driven by changes in school reporting or detection of ED rather than actual incidence of it, the spike in *self-reported* fear presented earlier suggests that exposure does impact students' underlying emotional well-being. Given that students are not regularly screened for ED and diagnoses are only made after an intensive referral process, these estimates may in fact represent a lower bound of the true psychological effects.[19] Indeed, epidemiological studies estimate that between 8% and 12% of all adolescents suffer from some form of emotional disturbance (U.S. Department of Education, 1993) — more than fifteen times the actual rate of diagnosis among LAUSD students. Furthermore, research in child psychiatry has found that exposure to even a single violent event is associated with a 150% increase in PTSD among school-age children (McCloskey and Walker, 2000).

Changes in emotional disturbance are also highly persistent with little drop-off several semesters after exposure. This can be explained in two ways. The first is that emotional disturbance and psychological trauma are chronic conditions and have been shown to persist for several years after the inciting incident (Friedman et al., 1996; Famularo et al., 1996). The second, more mechanical, explanation is that ED *designations* are sticky. While designations are reviewed by the District each year, comprehensive re-evaluations are only required every three years. Thus, the drop-off in effect observed seven semesters after treatment coincides precisely with the timing of triennial re-evaluations for students diagnosed immediately after exposure.

As children with ED have been shown to fare worse in school and on the labor market, relative even to those with identifiable physical and mental disabilities (Wagner, 1995), these findings hint at the large economic costs of use of force. These results may also help to explain the observed declines in academic performance.[20] One common symptom of depression are feelings of

[19]Students are only classified as ED after 1) pre-referral interventions have failed, 2) referral to LAUSD Special Education and 3) a comprehensive meeting between the student's parent, teachers and school psychologist. This process is also extremely costly to the District, as students with ED often receive their own classrooms and are sometimes transferred to private schools or residential facilities at the District's expense.

[20]While I cannot rule out the possibility that changes in ED designations are simply a knock-on effect of underlying changes in academic performance, anecdotal evidence indicates otherwise. LAUSD mental health staff stated that,

worthlessness, suggesting that exposed students may adjust downwards the their expectations for themselves and the value of schooling (Beck et al., 1996). Emotional disturbance is also strongly associated with reduced levels of self-determination (Carter et al., 2006). Thus, even if student aspirations remain unchanged, their optimism about achieving those goals and knowledge of the types of behavior required to do so may be affected. Lastly, students with emotional disturbance may hallucinate or suffer from limited attention spans (McInerney et al., 1992) suggesting impaired cognitive functioning due to biochemical changes (Yehuda et al., 2004).

C Violence Effects

A host of recent studies have sought to unpack the effects of neighborhood violence on educational outcomes, demonstrating that exposure to violent crime is associated with significant reductions in academic performance. While Burdick-Will (2013) finds that violence has little effect on grades, that study and others (Burdick-Will et al., 2011; Sharkey et al., 2014; Gershenson and Tekin, 2017) note a strong negative relationship with student test scores. In this context, comparing the effects of police violence to those of other violent acts may help to shed light on an important mechanism.

However, identifying the effects of exposure to neighborhood violence is hindered by the prevalence of crime in many inner-city communities. From 2002 to 2016, Los Angeles county experienced nearly 9,500 shooting-related homicides. Among the sample's four-year high school students, 80% were exposed to at least one non-police shooting, with the average student experiencing 4.5 such incidents over the course of their high school career. Given the frequency of treatment exposure, I am no longer able to employ an event-study model.

Instead, I estimate the following equation on grade point average:

$$y_{i,t} = \delta_i + \delta_{n,t} + \delta_{c,t} + \beta_p Police_{i,t} + \beta_n NonPolice_{i,t} + \varepsilon_{i,t}, \tag{2.3}$$

unless accompanied by other behavioral changes, poor academic performance is unlikely to warrant a student being classified as emotionally disturbed.

where $Police_{i,t}$ and $NonPolice_{i,t}$ are the number of police and non-police shootings that a student was exposed to in the current and previous semester. This model is similar to my main difference-in-differences approach in that it exploits temporal and spatial variation in exposure to violence, accounting for level differences between students and time-varying differences between neighborhoods.

Results are shown in Table 2.6. The first column estimates the base equation above, while the second restricts the sample to those Census tracts where students were exposed to at least one police shooting. Columns 3 and 4 then control for recent (non-homicide) crime in the student's block, while Columns 5 and 6 further account for recent arrests.[21]

All forms of crime and police activity are associated with significant reductions in grade point average. However, killings — and in particular those committed by police — are linked to larger academic changes. Indeed, exposure to police killings leads to decreases in GPA that are roughly twice as large as non-police killings (.030-.032 versus .014-.016 grade points). Notably, the difference between these estimates is virtually constant across models and is statistically significant at around the one percent-level in all cases.

I find similar relative effect sizes for police and non-police killings when examining changes in daily absenteeism using my primary event-study model (Table 2.A11). I also find qualitatively similar effects if I instead control for a student's total exposure to violence and regress GPA on the share of those killings that were committed by police (Table 2.A12).

Taken at face value, these results suggest that the educational impacts of police shootings are roughly double those of non-police shootings. One explanation would be that police killings are somehow "more" violent than criminal homicides. However, given that student responses to police force are more pronounced following incidents where one would expect less gunfire or collateral damage (such as when the suspect was unarmed), this seems unlikely.

Furthermore, if police violence acted primarily through direct exposure to violent trauma, one would not expect these effects to differ drastically based on the specific relationship of student

[21] In all cases, the sample is limited to data from 2010 onwards, as crime and arrests controls are not available for prior years. Results for the base model without controls are similar when examining all sample years.

and suspect race. Indeed, as I show in 2.A13, non-police killings of whites and minorities — as well as police killings of whites — are associated with nearly identical changes in grade point average (-0.013, -0.016 and -0.013 points, respectively), but police killings of minorities lead to decreases that are more than twice as large (-0.037 points). Thus, while violent trauma may explain some of the observed effects of police killings, these findings suggest that police use of force against minorities, specifically, may impact students in ways distinct from other forms of violence.

[Note: This section will be expanded to compare police killings to more "similar" non-police killings, by exploiting contextual details from arrest records and by employing propensity score matching methods. A closer comparison of police killings to alternative police actions — such as arrests — will also be included.]

VII Long-Run Implications

A Estimating Equation

The estimated effects on student performance and mental health suggest that exposure to police shootings may have large ramifications for long-run educational outcomes. However, when examining long-run attainment — which is measured only once per student at the end of their high school careers — I am no longer able to estimate Equation 2.1, as individual fixed effects would fully absorb variation in outcomes. Instead, I estimate the following equation for entering 9th graders:

$$y_i = \delta_s + \delta_{n,c} + \beta Shoot9_i + \varepsilon_i. \tag{2.4}$$

Here, δ_s are street-level fixed effects that control for the level differences between students living on different streets, while $\delta_{n,c}$ are neighborhood-cohort effects accounting for a changes over time between cohorts in a neighborhood. To account for correlations between a student's tenure at LAUSD and their likelihood of being exposed to a shooting, treatment is defined only according to a student's exposure in the 9th grade, $Shoot9_i$.[22] Thus, the model is analogous to a block-level DD, where the first difference is that between students exposed to shootings in the 9th grade and students from other cohorts that lived on the *same* street, and the second difference is between students of different cohorts living in the surrounding neighborhood.

B Main Results

As a first step to understanding long-run effects, Figure 2.5 plots the raw data for educational attainment. In particular, the plot displays average attainment (i.e. highest grade enrolled at LAUSD) by *expected* grade of exposure for students living within .5 miles of an incident location and for those living between .5 miles and 3 miles of an incident, where, expected grade is based on the year students enrolled in the 9th grade.

[Figure 2.5 about here.]

Among students that had left high school by the time of the shooting (expected grade $>$ 12), I find that average attainment is similar in trend, if not level, between treatment and control areas, indicating the exogeneity of police shootings. However, differences in attainment widen when comparing students that may have still been enrolled at the time of the shooting (expected grade between 9 and 12).

Turning to Equation 2.4, I formally test the effects of exposure on educational attainment, high school graduation and college enrollment. The results are presented in Table 2.9.

[Table 2.9 about here.]

Columns 1 through 4 examine a treatment radius of 0.25 miles, excluding students living between 0.25 and 0.50 miles from a shooting. The base model (Column 1) includes tract-cohort fixed effects and a treatment group dummy that is set to 1 if a student's address is within the treatment radius of *any* sample shooting, regardless of whether the student was exposed to that

[22] Students that remain in the dataset for several semesters are also more likely to be exposed to a shooting while enrolled.

shooting in the 9th grade. Column 2 then introduces demographic controls to account for level differences in educational attainment due to student race, gender, parental education, poverty status and home language. Column 3 tightens neighborhoods from Census tracts to Census block groups. Column 4 then replaces the treatment group dummy with Census block dummies, effectively limiting comparisons between exposed students and unexposed students that lived on the same street but entered high school in different years. Columns 5 through 8 display estimates from replicating this analysis using a treatment radius of 0.50 miles.

Across all models, I find that police violence has highly significant adverse effects on educational attainment. Relative to others in their neighborhoods, students exposed to police violence in the 9th grade go on to obtain between 0.20 and 0.35 fewer years of schooling. These students are 4.6 to 9.2 percentage points less likely to graduate from high school and 4.6 to 8.8 percentage points less likely to enroll in college.

These effects are quite large. The point estimates suggest that exposure to police shootings is linked to a 9-18% decrease in graduation rate (mean of 50%) and a 14-27% decrease in post-secondary enrollment rate (mean of 32.6%). The magnitudes are validated by epidemiological studies examining correlates of psychiatric disorders and trauma (Harris, 1983; Broberg et al., 2005). Drawing on a nationally representative survey, Porche et al. (2011) find that childhood trauma is associated with a 50% increase in dropout rate.[23] As an alternative comparison, Oreopoulos et al. (2017) finds that eligibility for Pathways to Education, an educational support program involving daily tutoring and financial incentives, raises high school graduation by 35% and post-secondary enrollment by 60%. Thus, exposure to a single police shooting is associated with adverse effects on attainment that are roughly half as large as the gains made under an intensive, four-year educational intervention.

[23]While their definition of trauma encompasses a range of chronic or repeated incidents like domestic violence, they also find similarly large correlations between dropout rates and one-off events — such as witnessing a killing, being in a car crash or experiencing a natural disaster.

C Secondary Analysis

Grade of Treatment

In Table 2.A14, I replicate the above analysis to examine differential effects based on grade of exposure. Column 1 replicates the analysis from Column 8 of Table 2.9 on 9th graders. Column 2 examines 10th grade exposure, restricting the sample to students who had not been exposed beforehand, and so on. As shown, effects on highest grade decrease with grade of exposure but remain significant even among those exposed in the 11th grade. Interestingly, effects on high school graduation and college enrollment are slightly larger among students exposed in 11th grade than among those exposed in 10th grade, suggesting that exposure to police violence may impact those schooling decisions in ways other than its mechanical effect on dropouts. Drawing on Hacker et al. (2006), who find that neighborhood violence is more predictive of suicide rates among 11th graders than 9th graders, one possible explanation is that emotional sensitivity to trauma varies with age.

Differences by Academic Performance

In Table 2.A15, I separate the sample based on students' academic performance. In particular, Column 1 examines students that demonstrated below basic proficiency on the 8th grade California Standards Tests, while Column 2 examines those that performed at or above a basic level.[24] Consistent with Arcidiacono (2004) and Eckstein and Wolpin (1999), who show that academic ability is inversely correlated with dropout rate, I find that treatment effects on highest grade and graduation rate are significantly larger among students that demonstrated below basic proficiency. This suggests that lower-performing students are more at-risk of dropping out due to police violence. Differential drop-outs among lower-performing students may also help to explain why treatment effects on GPA dissipate over time.

[24]The California Standards Test is graded from 150 to 600, with basic proficiency normalized to 300 in each year and subject. Students are classified with respect to the same threshold based on average performance across English, Math, History and Science.

Attrition

One concern with this analysis is that the data does not track the educational outcomes of students who leave the District. Thus, if police violence causes students to transfer to another school district, those students would be marked as dropouts, whether or not they went on to graduate from high school or enroll in college. However, the LAUSD data does include information about the immediate circumstances under which students leave the District. Thus, I estimate Equation 2.4 to examine changes in whether a student "transfers to another public school within the state of California but outside LAUSD (including juvenile correction facilities and community colleges)," "transfers to a non-public school including home schooling," or "leaves the state of California."

Results are shown in Table 2.A16 with treatment defined as 0.25 miles (Column 1) and 0.50 miles (Column 2). In both cases, I find little evidence of differential attrition through private school transfers or out-of-state relocations, as treatment estimates are near zero. While also insignificant at the 5 percent level, estimates for public transfers are larger, ranging from .6 to 1.3 p.p., suggesting possible attrition due to intra-state relocations or juvenile detention. These results suggest that transfers explain between 8% and 14% of the estimated change in graduation rates (i.e., comparing to Columns 4 and 8 of Table 2.9).

VIII Discussion

A Understanding Costs and Behavior

While comprehensive cost-benefit analysis is outside the scope of this paper, the effects documented here suggest the existence of large externalities of police violence on nearby students. Extrapolating from Levitt and Venkatesh (2001), who estimate total annualized returns to a year of schooling of nearly 20%, or $6,000, among minorities living in Chicago public housing, and assuming a working life of 30 years, the treatment effect of -0.25 years of schooling corresponds to foregone lifetime earnings of nearly $25,000 (net present value). Projecting those costs over the approximately 80 ninth graders affected by each incident suggests that a single police killing produces about \$2 million in collateral damage due to reduced human capital accumulation.

From the perspective of an individual student, these costs can be thought of in two ways. Based on per capita income in Los Angeles of \$30,000, lost wages are equivalent to a reduction in expected working life of 3.5 years. Alternatively, drawing on the literature on statistical value of life, where estimates of VSL range from \$4 million to \$9 million (Viscusi and Aldy, 2003), the foregone earnings correspond naively to a .3 to .6 p.p. increase in instantaneous probability of death, a 20- to 40-fold increase over the actual likelihood of a 17 year-old dying over a quarter-year period. In contrast, the objective odds of being killed by Los Angeles police in a given year are exceedingly rare, roughly one in 200,000. Even among blacks, this probability is still less than one in 50,000.

Thus, the effects are perhaps better rationalized by psychological mechanisms. Increases in emotional disturbance suggest non-trivial cognitive and behavioral implications of exposure to police violence, including excessive fear and reduced self-determination. Research in psychology has shown that loss of agency may also be symptomatic of feelings of discrimination, such as those suggested by the stark racial and contextual differences discussed in Section VI and by a large literature exploring minority perceptions of law enforcement (Feagin, 1991; Cole, 1999). Considering recent work by Glover et al. (2017) showing that perceived racial bias can lead to "self-fulfilling prophecies" of decreased minority effort, these findings point to the key role that trauma and perceptions of discrimination may play in explaining the observed effects on student behavior.

B Potential Policy Interventions

Given the important and multi-faceted role that law enforcement play in local communities, determining optimal levels of police force is a question left to others. Instead, I explore potential measures that may help to mitigate the unintended consequences of police activity, which may be of particular interest to families, educators and policymakers.

As mentioned in Section II, LAUSD was the first public school district in the nation to establish a trauma and crisis intervention team. In the years since, the District has made numerous efforts to establish support programs for traumatized and at-risk students. These include the placement of on-site Wellness and Mental Health Clinics at various campuses to provide intensive psychiatric and counseling services and the integration of social and emotional learning (SEL) into classroom curricula to promote resiliency and self-efficacy.

Thus, as a first step towards identifying possible mitigating factors, I estimate Equation 2.1 to examine heterogeneous effects on academic performance based on student access to these support services (i.e., whether a student's school of enrollment provided these programs).[25] These results are shown in Table 2.10 including either tract-semester or block group-semester fixed effects.

[Table 2.10 about here.]

Notably, I find no effect of police violence on students that were enrolled at schools that either had a mental health clinic or that implemented SEL on a school-wide basis (Columns 3 and 4). These treatment coefficients are near zero and relatively precise. However, students at schools that did not provide psychiatric support continued to experience significant decreases in GPA following exposure to police violence (Columns 1 and 2).

Given the non-random provision of services, the above estimates are not directly comparable. For example, better educated parents may be more likely to send their children to schools with psychiatric support. Nonetheless, these findings are consistent with recent evidence by Heller et al. (2017) demonstrating the efficacy of social and emotional learning programs among Chicago youth and suggest that similar programs may help to limit the toll inflicted by traumatic events.

IX Conclusion

This study makes use of a novel and highly detailed dataset to identify the effects of police violence on the educational outcomes of Los Angeles public high school students. I find large, negative impacts on student achievement. These effects are concentrated among minority students

[25] School program information is drawn from staff responses on the 2015-2016 School Experience Survey. A school is coded as providing social and emotional learning if the school's principal answered the SES question "To what extent is teaching students social and emotional skills happening in your school?" with "happening on a programmatic basis school-wide."

following police killings of other minorities and are accompanied by up-ticks in psychological trauma and fear, as measured both through clinical diagnoses and student self-reports. Taken together, these findings document the large collateral damage that acts of police violence may cause and suggest that these incidents may lead some citizens to feel insecure or disenfranchised.

This paper presents several additional areas of research. Given the significance of police behavior to academic achievement, a natural question to consider is how youth may be affected by other types of state interactions. Thus, I explore the consequences of Immigration and Customs Enforcement activity on LAUSD elementary and middle school students in another study (Ang, 2017b).

In light of the stark racial heterogeneity presented in Section VI, a deeper investigation into how legal cynicism and perceptions of discrimination may influence motivation and engagement is also warranted. While this is an area well-covered in psychology, empirical evidence is scarce and largely interpretive. Consistent with this paper's results, Desmond et al. (2016) find that blacks disengaged from local institutions following a controversial police beating of a black man. However, in Ang (2017a), I show that police killings of blacks actually *increased* civic engagement among black registered voters. Examining other contexts and settings may help to reconcile these findings and illuminate the circumstances under which aggrieved communities choose "voice" over "exit" (Hirschman, 1970).

Finally, while the results shown in Section VIII provide suggestive evidence that mental health services may help to offset the adverse effects experienced by nearby students, additional work identifying effective methods of mitigating the unintended consequences of police actions could be of great value to school administrators and counselors. One example of this would be to test the efficacy of social and emotional learning techniques through randomized control trials.

Motivating each of these extensions is the common understanding that law enforcement officers are a vital part of local communities. As the first line of defense and one of the most visible arms of government, interactions with police have wide-ranging implications across numerous domains. Measuring these effects and understanding how they may be mitigated or amplified is thus essential not only to determining optimal levels of law enforcement but also to the promotion of safety, education and inclusion.

Chapter 2, in part is currently being prepared for submission for publication of the material. Ang, Desmond. The dissertation author was the primary investigator and author of this material.

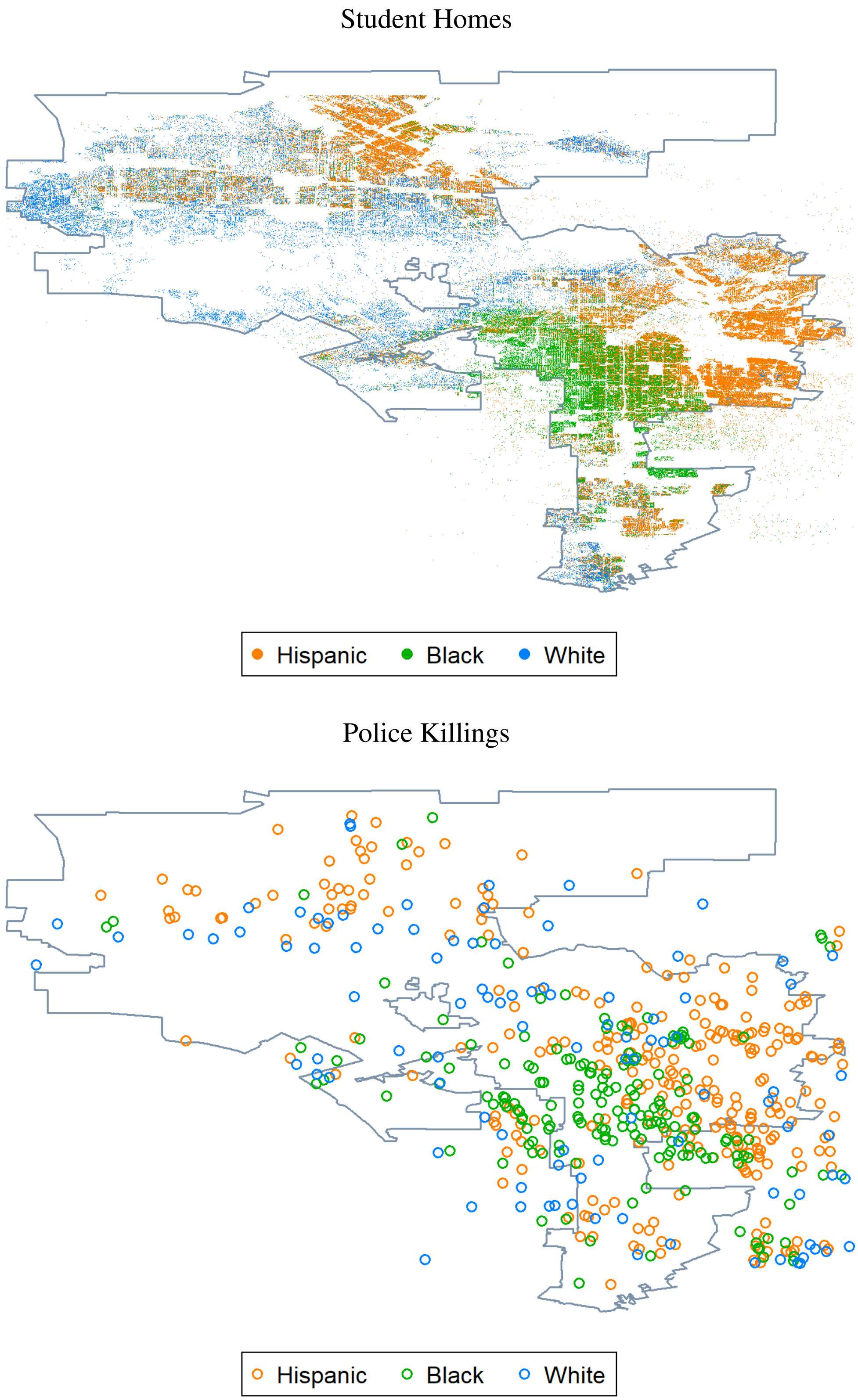

Figure 2.1. Geo-coded Addresses

Notes: Top panel plots the home address of every student in the dataset, color-coded by race of the student. Bottom panel plots location of every police killing in the dataset, color-coded by race of the suspect killed.

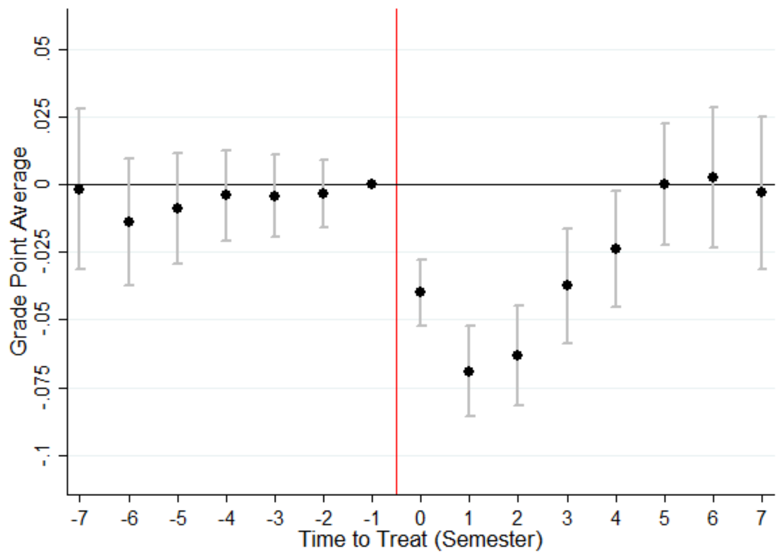

Figure 2.2. Effects on Grade Point Average

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 on semester grade point average. Standard errors clustered by zip code. Includes Census block group-semester fixed effects. Treatment defined as students living within 0.50 miles of an incident. Red vertical line represents time of treatment. Full results displayed in Table 2.3, Column 6.

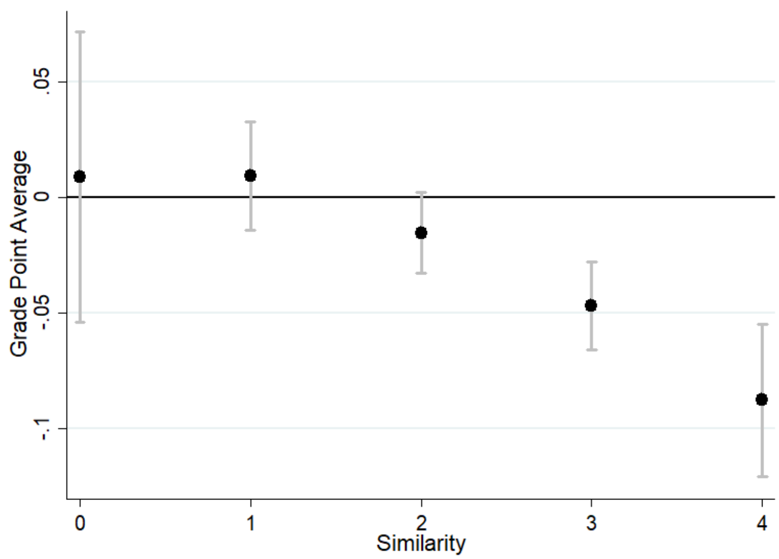

Figure 2.3. Effects on GPA by Student-Suspect Similarity

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 on semester grade point average for minority (black and Hispanic) students and minority shootings, replacing time to treatment indicators with interactions between a student-suspect similarity index and a post-treatment dummy. Standard errors clustered by zip code. Includes Census block group-semester fixed effects. Treatment defined as students living within 0.50 miles of an incident. Similarity increments by 1 if the exposed student and suspect are of the same gender (male or female), ethnicity (black or Hispanic), age group (suspect was under 25) or criminal background (suspect had a criminal record). Full estimation results are shown in Table 2.A10, Column 2.

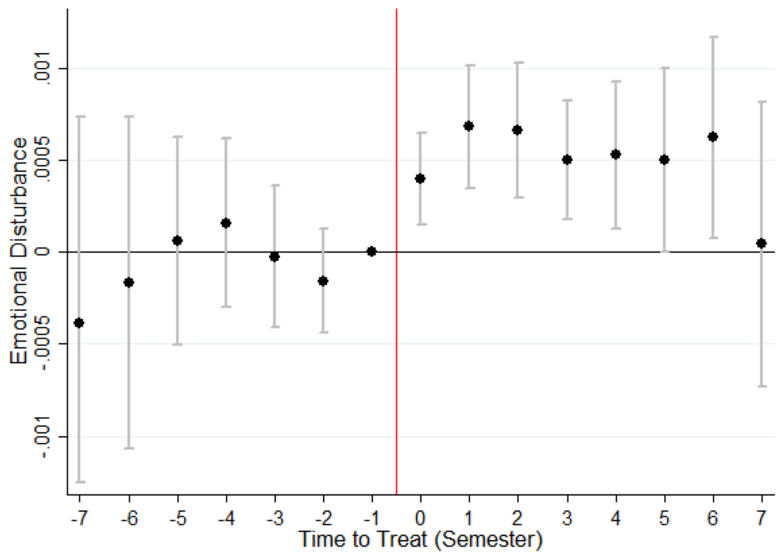

Figure 2.4. Effects on Emotional Disturbance

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 on emotional disturbance. Standard errors clustered by zip code. Includes Census block group-semester fixed effects. Treatment defined as students living within 0.50 miles of an incident. Red vertical line represents time of treatment. Full results displayed in Table 2.7, Column 6.

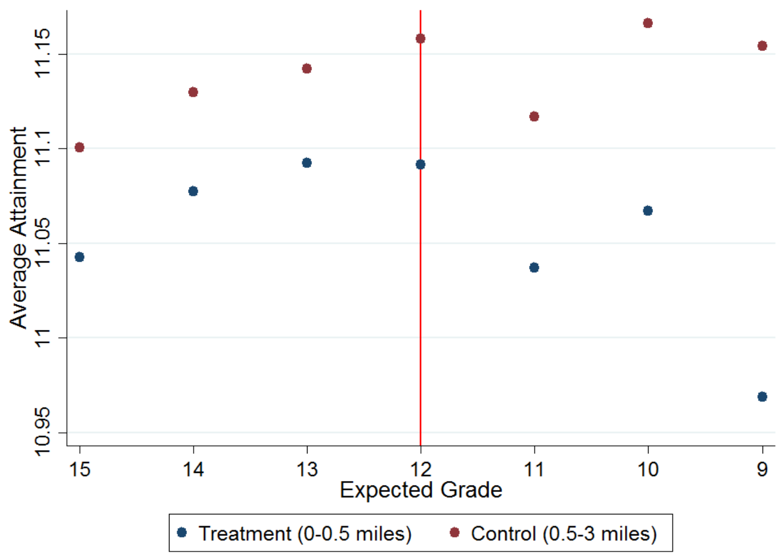

Figure 2.5. Attainment by Expected Grade at Time of Police Killing

Notes: Figure plots raw average attainment based on the expected grade of exposure for students living in areas within 0.50 miles of an incident and for those living in areas between 0.50 and 3 miles from an incident. Expected grade is calculated based on the year students enrolled in the 9th grade.

Table 2.1. Summary Statistics: Police Killings

	Mean	SD
Suspect Characteristics		
Age	32.33	12.29
Black	0.26	0.44
Hispanic	0.52	0.50
White	0.19	0.39
Male	0.97	0.18
Armed	0.47	0.50
Criminal Record	0.31	0.46
Police Characteristics		
Number Officers	2.31	2.23
Black (any)	0.14	0.35
Hispanic (any)	0.63	0.48
White (any)	0.56	0.50
Non-Lethal	0.14	0.34
Media Coverage		
Total Mentions	1.48	12.96
Any Mentions	0.22	0.41
Lawsuit	0.09	0.28
Total Incidents	627	

Notes: Table provides summary statistics for police killings that occurred in Los Angeles county between the beginning of the 2002-2003 academic year and the end of the 2015-2016 academic year. Armed is an indicator for whether the suspect killed was armed with a firearm. Criminal record is set to 1 if the the suspect's criminal record was mentioned in the incident report. For police, Black, Hispanic and White refer to whether any involved officers were of those races.

Table 2.2. Summary Statistics: Students

	Control		Treatment	
	Mean	SD	Mean	SD
Student Characteristics				
Black	0.12	0.32	0.11	0.32
Hispanic	0.72	0.45	0.82	0.39
White	0.09	0.03	0.03	0.17
Male	0.49	0.50	0.50	0.50
Household Characteristics				
Free/Reduced Lunch	0.67	0.47	0.77	0.42
English Home Language	0.30	0.46	0.23	0.42
College or Higher (Parents)	0.08	0.28	0.06	0.23
Students	571,326		141,628	

Notes: Table provides summary statistics for all LAUSD high schoolers in the dataset. Treatment refers to students that were exposed to a police shooting (based on a treatment radius of .5 miles) during their high school tenure.

Table 2.3. Effects on Grade Point Average

Time to Treat	(1)	(2)	(3)	(4)	(5)	(6)
-7	-0.011	-0.011	0.000	0.000	0.000	-0.002
	(0.020)	(0.020)	(0.023)	(0.014)	(0.014)	(0.015)
-6	-0.044**	-0.044**	-0.024	-0.014	-0.014	-0.014
	(0.019)	(0.019)	(0.023)	(0.010)	(0.010)	(0.012)
-5	-0.008	-0.008	0.014	-0.009	-0.009	-0.009
	(0.015)	(0.015)	(0.020)	(0.010)	(0.010)	(0.010)
-4	0.010	0.010	0.028*	-0.007	-0.007	-0.004
	(0.012)	(0.012)	(0.016)	(0.008)	(0.008)	(0.009)
-3	0.018	0.018	0.032**	-0.006	-0.006	-0.004
	(0.011)	(0.011)	(0.016)	(0.007)	(0.007)	(0.008)
-2	0.006	0.006	0.013	-0.005	-0.005	-0.003
	(0.010)	(0.010)	(0.013)	(0.006)	(0.006)	(0.006)
-1	-	-	-	-	-	-
	-	-	-	-	-	-
0	-0.035***	-0.035***	-0.081***	-0.032***	-0.032***	-0.040***
	(0.010)	(0.010)	(0.015)	(0.005)	(0.005)	(0.006)
1	-0.069***	-0.069***	-0.130***	-0.052***	-0.052***	-0.069***
	(0.014)	(0.014)	(0.021)	(0.007)	(0.007)	(0.008)
2	-0.054***	-0.054***	-0.104***	-0.051***	-0.051***	-0.063***
	(0.015)	(0.015)	(0.022)	(0.008)	(0.008)	(0.009)
3	-0.036**	-0.036**	-0.069***	-0.032***	-0.032***	-0.037***
	(0.017)	(0.017)	(0.023)	(0.009)	(0.009)	(0.011)
4	-0.008	-0.008	-0.035	-0.019**	-0.019**	-0.024**
	(0.016)	(0.016)	(0.021)	(0.010)	(0.010)	(0.011)
5	0.011	0.011	-0.015	0.003	0.003	0.000
	(0.017)	(0.017)	(0.023)	(0.011)	(0.011)	(0.011)
6	0.018	0.018	-0.015	0.007	0.007	0.003
	(0.019)	(0.019)	(0.025)	(0.012)	(0.012)	(0.013)
7	0.008	0.008	-0.022	0.003	0.003	-0.003
	(0.023)	(0.023)	(0.028)	(0.014)	(0.014)	(0.014)
Crime Ctrls.	-	Yes	Yes	-	Yes	Yes
Neighborhood	Tract	Tract	Block Group	Tract	Tract	Block Group
Treatment	0.25	0.25	0.25	0.5	0.5	0.5
Control	0.5-3.0	0.5-3.0	0.5-3.0	0.5-3.0	0.5-3.0	0.5-3.0
Obs.	3,019,161	3,019,161	3,012,242	3,584,479	3,584,478	3,577,564
R-sq.	0.699	0.699	0.706	0.695	0.695	0.700

Notes: DD coefficients from estimation of Equation 2.1 on semester grade point average displayed. Standard errors clustered by zip code. Crime controls include indicators for the presence of a non-officer involved homicide in a Census block-semester.

Table 2.4. Effects on GPA by Student and Suspect Race

		Suspect Race		
Student Race	Any (n=627)	Black (159)	Hispanic (318)	White (113)
Black	-0.033**	-0.051**	-0.008	-0.012
(n=88,725)	(0.015)	(0.024)	(0.021)	(0.059)
Hispanic	-0.026***	-0.034**	-0.025**	0.005
(517,642)	(0.007)	(0.015)	(0.010)	(0.015)
White	0.013	0.044	0.034	0.024
(46,642)	(0.034)	(0.088)	(0.054)	(0.061)

Notes: DD coefficients from estimation of Equation 2.1 on semester grade point average displayed, separately by student and suspect race. Standard errors clustered by zip code. Includes Census block group-semester fixed effects. Treatment defined as students living within 0.50 miles of an incident.

Table 2.5. Effects on GPA by Suspect and Officer Characteristics

	(1)	(2)	$p(\beta_{no} = \beta_{yes})$
Suspect Characteristics	No	Yes	
Armed	-0.048***	-0.029**	0.386
(No=256; Yes=227)	(0.016)	(0.012)	
Criminal Record	-0.031***	-0.043***	0.450
(No=333; Yes=150)	(0.011)	(0.013)	
Police Characteristics	No	Yes	
Minority Officers	-0.048***	-0.020*	0.113
(No=150; Yes=333)	(0.013)	(0.011)	
Non-Lethal Attempt	-0.043***	-0.021	0.442
(No=418; Yes=65)	(0.014)	(0.024)	
Combined	None	All	
Sum of Above	-0.063***	-0.008	0.013
	(0.021)	(0.012)	

Notes: DD coefficients from estimation of Equation 2.2 on semester grade point average for minority (black and Hispanic) students and killings. Standard errors clustered by zip code. Includes Census block group-semester fixed effects. Treatment defined as students living within 0.50 miles of an incident. Armed refers to whether the suspect was armed with a firearm. Criminal record refers to whether the suspect had a prior criminal conviction. Minority officers refers to whether any of the involved police officers was a minority. Non-lethal attempt refers to whether involved officers attempted non-lethal means of arresting the suspect (such as using a Taser or bean bag gun) prior to discharging their firearm. Analysis in first four rows is limited to students in school attendance zones that experienced police killings of both types (ex., armed and unarmed suspects) during sample period.

Table 2.6. Effects on GPA of Police and Non-Police Violence

	(1)	(2)	(3)	(4)	(5)	(6)
Police Killings	-0.032***	-0.032***	-0.030***	-0.030***	-0.030***	-0.030***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Non-Police Killings	-0.016***	-0.017***	-0.014***	-0.015***	-0.014***	-0.015***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Crimes			-0.001***	-0.001***	-0.001***	-0.001***
			(0.000)	(0.000)	(0.000)	(0.000)
Arrests					-0.001***	-0.000***
					(0.000)	(0.000)
Sample	All	Restricted	All	Restricted	All	Restricted
$\beta_p - \beta_n$	-0.016	-0.015	-0.016	-0.015	-0.016	-0.015
$p(\beta_p = \beta_n)$	0.009	0.009	0.011	0.011	0.011	0.011
Obs.	2,009,604	1,350,012	2,009,604	1,350,012	2,009,604	1,350,012
R-sq.	0.715	0.710	0.715	0.710	0.715	0.710

Notes: Coefficients from estimation of Equation 2.3 on semester grade point average. Independent variables are the number of police and non-police shootings a student experienced (within 0.50 miles) in the current and prior semester and the number of crimes and arrests that occurred in the student's Census block in the current and prior semester. Restricted sample limits the analysis to those Census tracts in which students were exposed to both forms of violence. Standard errors clustered by zip code. Includes Census block group-semester fixed effects.

Table 2.7. Effects on Emotional Disturbance

Time to Treat	(1)	(2)	(3)	(4)	(5)	(6)
-7	0.00024	0.00024	0.00020	-0.00022	-0.00022	-0.00039
	(0.00095)	(0.00095)	(0.00099)	(0.00054)	(0.00054)	(0.00057)
-6	0.00016	0.00015	0.00013	-0.00008	-0.00008	-0.00017
	(0.00062)	(0.00062)	(0.00071)	(0.00044)	(0.00044)	(0.00046)
-5	0.00041	0.00041	0.00038	0.00013	0.00013	0.00006
	(0.00045)	(0.00045)	(0.00048)	(0.00026)	(0.00026)	(0.00029)
-4	0.00050	0.00050	0.00042	0.00024	0.00024	0.00016
	(0.00036)	(0.00036)	(0.00037)	(0.00021)	(0.00021)	(0.00023)
-3	0.00036	0.00036	0.00020	0.00006	0.00005	-0.00002
	(0.00031)	(0.00031)	(0.00034)	(0.00018)	(0.00018)	(0.00019)
-2	0.00016	0.00015	0.00004	-0.00009	-0.00009	-0.00016
	(0.00026)	(0.00026)	(0.00025)	(0.00014)	(0.00014)	(0.00014)
-1	-	-	-	-	-	-
	-	-	-	-	-	-
0	0.00039*	0.00039*	0.00038	0.00033***	0.00033***	0.00040***
	(0.00022)	(0.00022)	(0.00023)	(0.00012)	(0.00012)	(0.00013)
1	0.00054*	0.00054*	0.00057*	0.00056***	0.00056***	0.00068***
	(0.00030)	(0.00030)	(0.00033)	(0.00016)	(0.00016)	(0.00017)
2	0.00052	0.00052	0.00066	0.00052***	0.00052***	0.00066***
	(0.00035)	(0.00035)	(0.00042)	(0.00017)	(0.00017)	(0.00019)
3	0.00059*	0.00059*	0.00066*	0.00036**	0.00036**	0.00050***
	(0.00031)	(0.00031)	(0.00039)	(0.00014)	(0.00014)	(0.00016)
4	0.00069*	0.00069*	0.00065	0.00044**	0.00044**	0.00053**
	(0.00036)	(0.00036)	(0.00050)	(0.00017)	(0.00017)	(0.00020)
5	0.00075*	0.00075*	0.00077	0.00045**	0.00045**	0.00050**
	(0.00044)	(0.00044)	(0.00053)	(0.00023)	(0.00023)	(0.00025)
6	0.00102**	0.00101**	0.00101*	0.00054**	0.00054**	0.00062**
	(0.00049)	(0.00049)	(0.00059)	(0.00025)	(0.00025)	(0.00028)
7	-0.00013	-0.00013	-0.00020	-0.00009	-0.00009	0.00004
	(0.00063)	(0.00063)	(0.00064)	(0.00038)	(0.00038)	(0.00039)
Crime Ctrls.	-	Yes	Yes	-	Yes	Yes
Neighborhood	Tract	Tract	Block Group	Tract	Tract	Block Group
Treatment	0.25	0.25	0.25	0.5	0.5	0.5
Control	0.5-3.0	0.5-3.0	0.5-3.0	0.5-3.0	0.5-3.0	0.5-3.0
Obs.	2,988,591	2,988,591	2,988,172	3,561,898	3,561,897	3,561,498
R-sq.	0.647	0.647	0.651	0.652	0.652	0.656

Notes: DD coefficients from estimation of Equation 2.1 on emotional disturbance displayed. Standard errors clustered by zip code. Crime controls include indicators for the presence of a non-officer involved homicide in a Census block-semester.

Table 2.8. Effects on Perceptions of Safety

Question (Scale 1-5, higher is safer)	Mean	(1)	(2)
How safe do you feel in the neighborhood around the school?	3.68	-0.133**	-0.137**
		(0.054)	(0.054)
How safe do you feel when you are at school?	3.74	-0.096*	-0.053
		(0.051)	(0.056)
I feel safe in my school	3.57	-0.057	-0.048
		(0.052)	(0.055)
Safety (Average)	3.66	-0.093***	-0.092**
		(0.034)	(0.042)
Neighborhood		Tract	Block Group
Observations		91,790	91,358

Notes: DD coefficients from estimation of Equation 2.1 on student survey responses. Standard errors clustered by zip code. Includes Census tract-year fixed effects. Treatment defined as those living within 0.50 miles of a shooting that occurred between the 2015 and 2016 survey administrations.

Table 2.9. Effects on Educational Attainment

DV	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Highest Grade	-0.200***	-0.219***	-0.351***	-0.353***	-0.183***	-0.190***	-0.245***	-0.250***
	(0.021)	(0.020)	(0.026)	(0.027)	(0.012)	(0.012)	(0.015)	(0.015)
Graduated	-0.046***	-0.050***	-0.092***	-0.092***	-0.054***	-0.054***	-0.074***	-0.075***
	(0.008)	(0.008)	(0.011)	(0.012)	(0.005)	(0.005)	(0.007)	(0.007)
College	-0.046***	-0.048***	-0.083***	-0.088***	-0.051***	-0.050***	-0.067***	-0.070***
	(0.012)	(0.011)	(0.013)	(0.015)	(0.007)	(0.007)	(0.007)	(0.008)
Demo Ctrls	-	Yes	Yes	Yes	-	Yes	Yes	Yes
Neighborhood	Tract	Tract	Blk Group	Blk Group	Tract	Tract	Blk Group	Blk Group
Street	Treat	Treat	Treat	Block	Treat	Treat	Treat	Block
Treatment	0.25	0.25	0.25	0.25	0.5	0.5	0.5	0.5
Obs.	563,108	563,108	560,185	556,555	593,453	593,453	590,589	586,935
R-sq.	0.074	0.107	0.140	0.181	0.073	0.107	0.138	0.177

Notes: DD coefficients from estimation of Equation 2.4. Standard errors clustered by zip code. Demographic controls include indicators for student race, gender, parental education, poverty status and home language. Street refers to fixed effects controlling for the student's home address (either at the Census block-level or with a simple treatment group dummy).

Table 2.10. Effects on GPA by Access to Support Services

	(1)	(2)	(3)	(4)	$p(\beta_N = \beta_Y)$
	No		*Yes*		
Wellness Clinic	-0.019***	-0.024***	0.006	-0.002	0.270
(No=276; Yes=18)	(0.005)	(0.006)	(0.015)	(0.019)	
Emotional Learning	-0.011*	-0.020***	-0.002	0.005	0.055
(No=128; Yes=102)	(0.006)	(0.007)	(0.008)	(0.011)	
Neigborhood	Tract	Blk Group	Tract	Blk Group	Blk Group

Notes: DD coefficients from estimation of Equation 2.1 on semester grade point average. Standard errors clustered by zip code. Treatment defined as students living within 0.50 miles of an incident. Number of schools with/without services are shown in parentheses. The sum of these figures are not equal between rows due to incomplete faculty responses on School Experience Survey. Wellness clinic refers to schools that maintain an on-site mental health wellness center, emotional learning are those schools that feature integrate social and emotional learning on a school-wide basis.

Appendix A

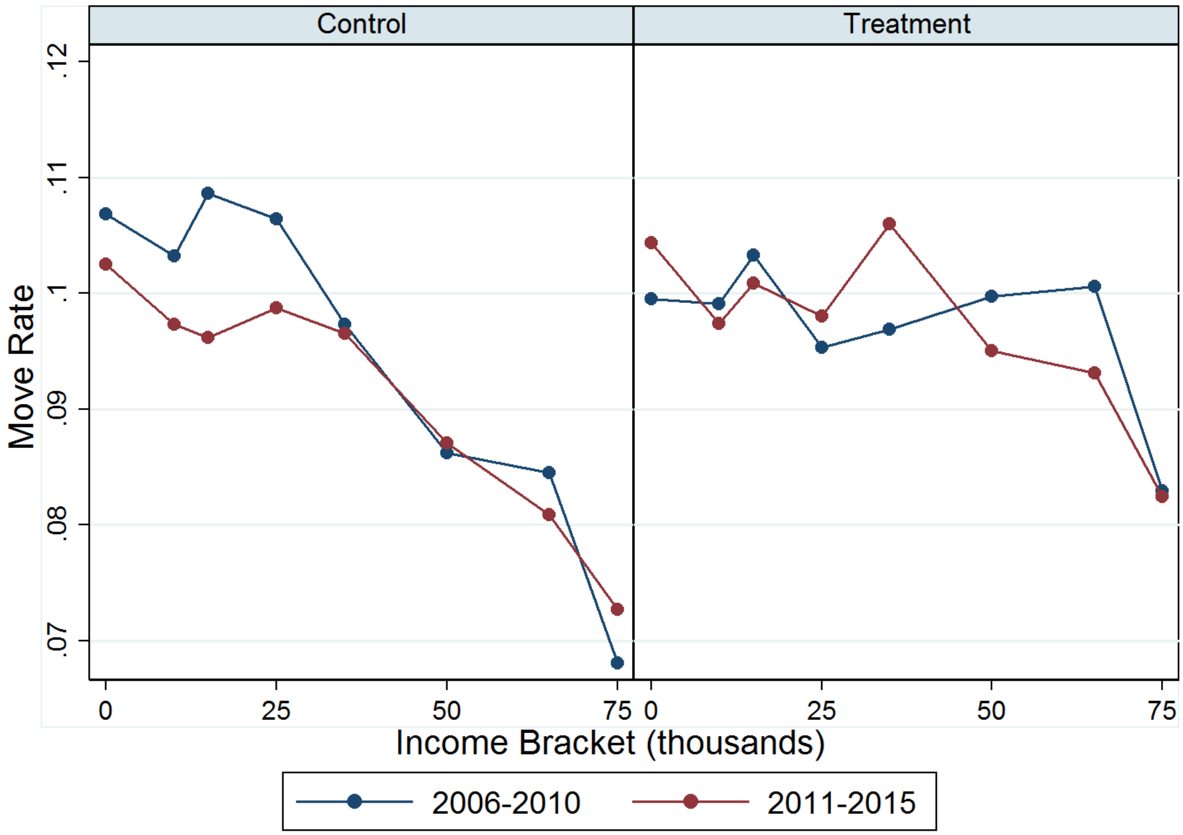

Figure 2.A1. Within-County Move Rates by Income

Notes: Plots depict the share of individuals that reported moving within the county in the prior year, broken down by reported income. Blue line represents 2006-2010 ACS estimates, red line represents 2011-2015 ACS estimates. Left panel includes Census tracts that did not experience a sample shooting. Right panel includes Census tracts that experienced a shooting after 2010.

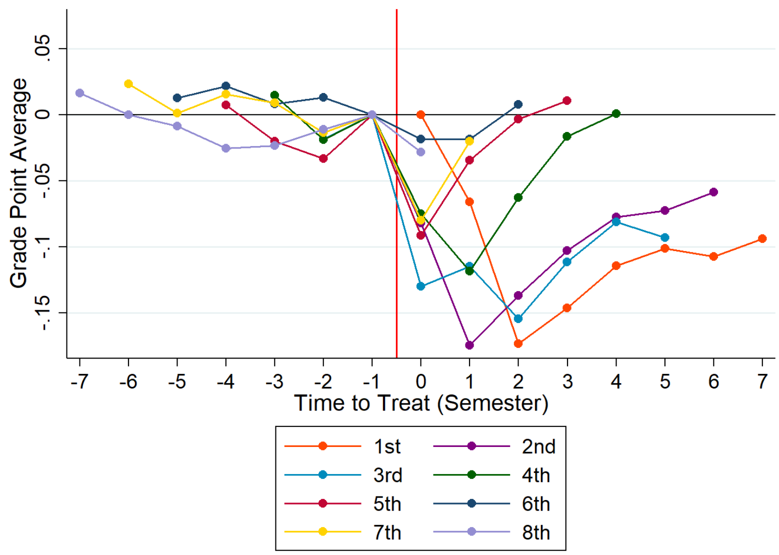

Figure 2.A2. Effects on Grade Point Average by Semester of Treatment

Notes: Graph shows DD coefficients from estimation of Equation 2.1 on semester grade point average, separately for students that were treated in their 1st semester, 2nd semester, and so on. Analysis is restricted to students that entered LAUSD in the 9th grade and that remained enrolled until the given semester of treatment. Standard errors clustered by zip code. Red vertical line represents time of treatment. Treatment defined as those living within 0.50 miles of an incident. Full results displayed in Table 2.A3.

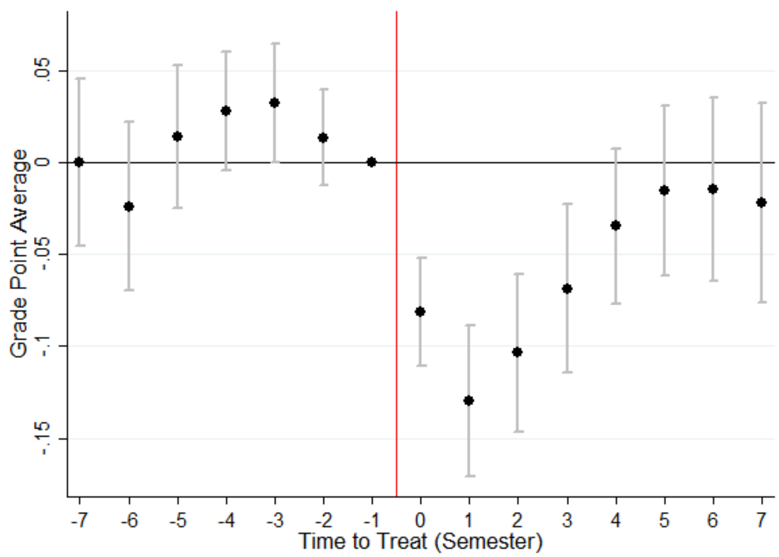

Figure 2.A3. Effects on Grade Point Average (Restricted Treatment)

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 on semester grade point average. Standard errors clustered by zip code. Red vertical line represents time of treatment. Treatment defined as those living within 0.25 miles of an incident. Full results displayed in Table 2.3, Column 3.

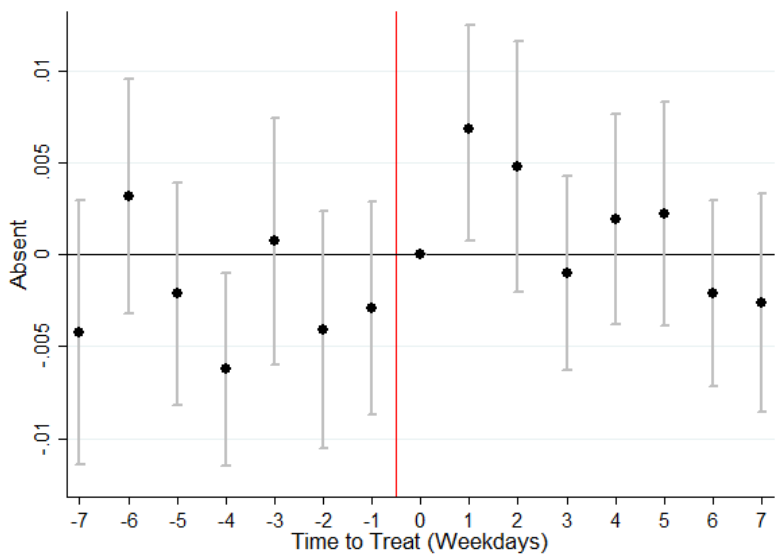

Figure 2.A4. Effects on Absenteeism

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 on daily absenteeism. Standard errors clustered by zip code. Red vertical line represents time of treatment. Treatment defined as those living within 0.50 miles of an incident. Full results displayed in Table 2.A5, Column 4.

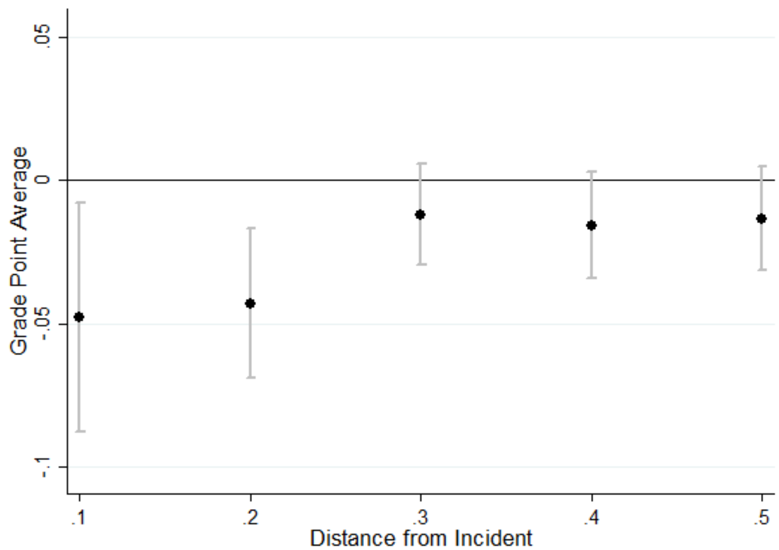

Figure 2.A5. Effects on GPA by Distance

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 on semester grade point average, replacing time to treatment indicators with interactions between distance from shooting indicators and a post-treatment dummy. Full results displayed in Table 2.A6, Column 2.

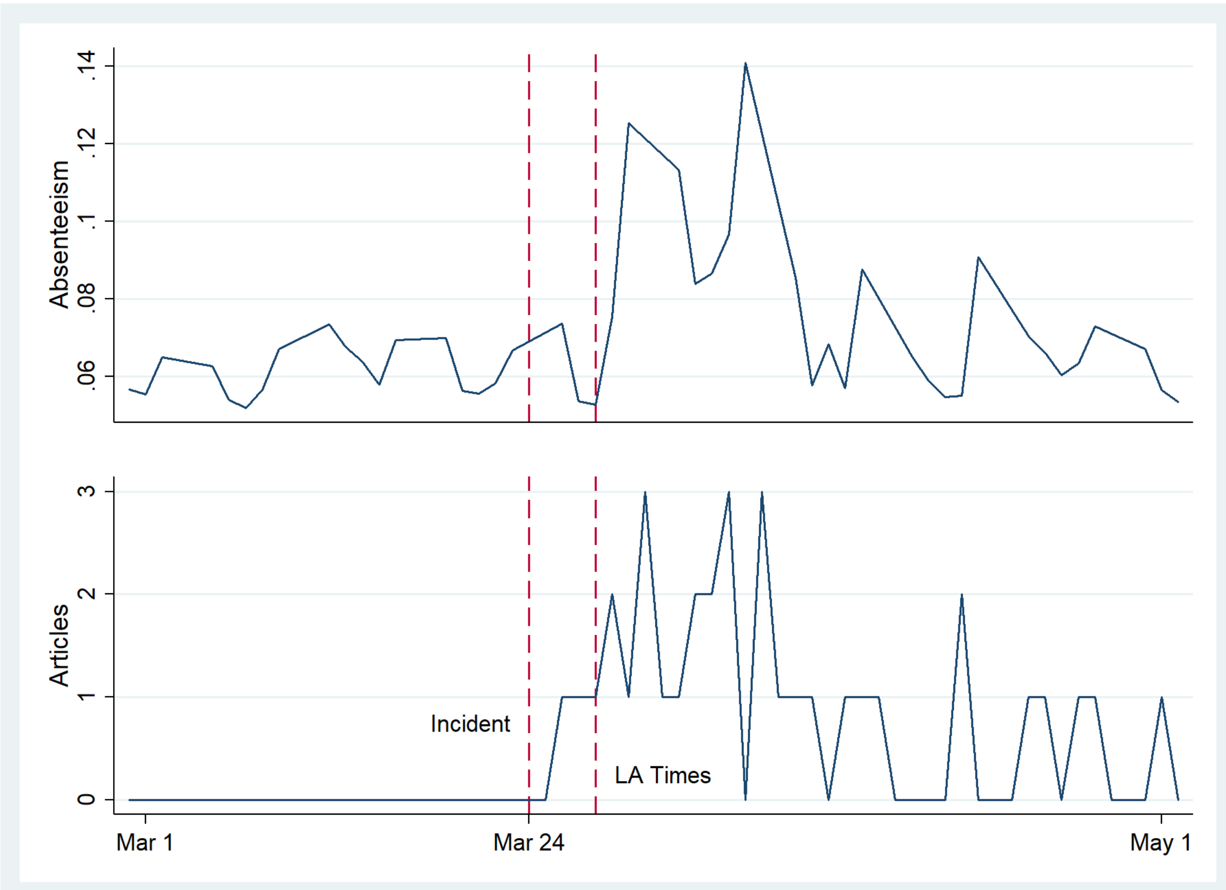

Figure 2.A6. Absenteeism and Media Coverage surrounding Death of Kendrec McDade

Notes: Top panel plots absenteeism by calendar day, averaged across all LAUSD high schoolers. Bottom panel plots the number of newspaper articles mentioning Kendrec McDade by date of publication. Left vertical line marks the date of the incident (March 24, 2012), while the right vertical line marks the publication date of the Los Angeles Times' first article on the incident (March 28, 2012).

Table 2.A1. DD Estimates for Within-County Move Rates

Income Bracket	(1)	
0-10k	0.0086	(0.0069)
10-15k	0.0055	(0.0081)
15-25k	0.0093	(0.0081)
25-35k	0.0154*	(0.0086)
35-50k	0.0154*	(0.0083)
50-65k	-0.0016	(0.0110)
65-75k	-0.0139	(0.0153)
75k+	-0.0005	(0.0119)
0-50k	0.012**	(0.005)
50k+	-0.007	(0.008)
Obs.	5,108	

Notes: DD coefficients from estimation $y_{n,t} = \delta_n + \delta_t + \beta Shoot_n \times Post_t + \varepsilon_{n,t}$ using Census tract-level panel data from 2006-2010 and 2011-2015 ACS estimates of one-year within-county move rates. Each row represents separate regression comparing income bracket move rate between tracts that did not experience a sample shooting to those that experienced a shooting after the 2006-2010 ACS survey period. Standard errors clustered by tract.

Table 2.A2. Effects on Crime

Time to Treat	(1)	(2)	(3)	(4)	(5)	(6)
	DV=Crime		*DV=Arrests*		*DV=Homicides*	
-7	-0.493	-0.408	-2.555	-1.770	0.008	0.008
	(0.939)	(0.915)	(3.055)	(2.537)	(0.008)	(0.008)
-6	-0.881	-0.483	-1.288	-1.174	0.002	-0.001
	(0.807)	(0.686)	(1.965)	(2.047)	(0.011)	(0.011)
-5	-0.049	0.295	0.175	0.426	0.029**	0.027**
	(0.686)	(0.652)	(1.120)	(1.348)	(0.014)	(0.014)
-4	-0.061	0.085	0.268	0.104	-0.005	-0.004
	(0.728)	(0.642)	(0.706)	(0.748)	(0.009)	(0.010)
-3	-0.124	-0.020	0.398	0.767	0.000	0.001
	(0.642)	(0.573)	(0.664)	(0.784)	(0.010)	(0.010)
-2	-0.079	-0.045	0.219	0.114	-0.006	-0.003
	(0.336)	(0.353)	(0.379)	(0.407)	(0.010)	(0.010)
-1	-	-	-	-	-	-
	-	-	-	-	-	-
0	-0.236	-0.305	0.608	0.039	0.004	0.001
	(0.434)	(0.408)	(1.124)	(0.598)	(0.011)	(0.011)
1	0.147	-0.225	2.209	1.116	0.010	0.010
	(0.629)	(0.530)	(2.196)	(1.452)	(0.012)	(0.012)
2	0.670	0.286	1.799	0.686	-0.003	-0.001
	(0.829)	(0.666)	(2.168)	(1.402)	(0.010)	(0.010)
3	0.338	0.104	0.034	-0.662	0.004	0.003
	(0.549)	(0.643)	(0.894)	(0.744)	(0.010)	(0.010)
4	-0.038	-0.202	0.199	-0.187	-0.001	-0.002
	(0.527)	(0.626)	(0.920)	(0.670)	(0.009)	(0.010)
5	-0.548	-0.560	-0.106	-0.631	-0.004	-0.006
	(0.552)	(0.622)	(1.001)	(0.933)	(0.010)	(0.010)
6	-0.653	-0.649	-0.508	-1.104	-0.001	0.003
	(0.581)	(0.629)	(1.163)	(1.035)	(0.010)	(0.011)
7	-0.570	-0.610	-0.068	-0.529	-0.003	-0.005
	(0.591)	(0.618)	(1.211)	(0.964)	(0.008)	(0.009)
Neighborhood	Tract	Blk Group	Tract	Blk Group	Tract	Blk Group
Obs.	491,647	483,028	491,647	483,028	983,294	966,056
R-sq.	0.859	0.841	0.825	0.872	0.115	0.198

Notes: DD coefficients from estimation of Equation 2.1 on crime displayed. Standard errors clustered by zip code. Outcomes are the number of reported crimes, arrests and homicides in a block-semester.

Table 2.A3. Effects on GPA by Semester of Treatment

Time to Treat	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
-7								0.016
								(0.040)
-6							0.023	0.000
							(0.029)	(0.040)
-5						0.013	0.001	-0.009
						(0.028)	(0.030)	(0.032)
-4					0.007	0.022	0.016	-0.025
					(0.021)	(0.033)	(0.034)	(0.029)
-3				0.015	-0.020	0.008	0.009	-0.023
				(0.021)	(0.019)	(0.030)	(0.036)	(0.027)
-2			-0.018	-0.019	-0.033**	0.013	-0.013	-0.011
			(0.015)	(0.021)	(0.016)	(0.024)	(0.022)	(0.026)
-1	-	-	-	-	-	-	-	-
	-	-	-	-	-	-	-	-
0	-	-0.082***	-0.130***	-0.075***	-0.091***	-0.018	-0.080***	-0.028
	-	(0.018)	(0.022)	(0.016)	(0.023)	(0.024)	(0.028)	(0.024)
1	-0.066***	-0.175***	-0.115***	-0.118***	-0.034*	-0.019	-0.020	
	(0.015)	(0.024)	(0.026)	(0.020)	(0.020)	(0.027)	(0.026)	
2	-0.174***	-0.137***	-0.155***	-0.063***	-0.003	0.008		
	(0.020)	(0.025)	(0.023)	(0.018)	(0.030)	(0.029)		
3	-0.147***	-0.103***	-0.112***	-0.016	0.011			
	(0.020)	(0.025)	(0.021)	(0.025)	(0.028)			
4	-0.115***	-0.077***	-0.081***	0.001				
	(0.020)	(0.026)	(0.023)	(0.023)				
5	-0.101***	-0.073***	-0.093***					
	(0.020)	(0.027)	(0.024)					
6	-0.107***	-0.059**						
	(0.020)	(0.026)						
7	-0.094***							
	-0.025							
Treat Semester	1	2	3	4	5	6	7	8
Obs.	1,698,946	1,727,990	1,620,843	1,592,457	1,419,220	1,369,728	1,141,061	1,067,277
R-sq.	0.714	0.713	0.701	0.694	0.678	0.670	0.658	0.651

Notes: DD coefficients from estimation of Equation 2.1 on semester grade point average, separately for students that were treated in their 1st semester, 2nd semester, and so on. Analysis is restricted to students that entered LAUSD in the 9th grade and that remained enrolled until the given semester of treatment. Standard errors clustered by zip code. Treatment defined as those living within 0.50 miles of an incident. Includes Census block-group semester fixed effects.

Table 2.A4. Effects on GPA (without Multiple Treaters)

Time to Treat	(2)	(3)	(5)	(6)
-7	-0.015	-0.000	0.001	-0.002
	(0.020)	(0.024)	(0.015)	(0.016)
-6	-0.046**	-0.023	-0.015	-0.015
	(0.020)	(0.024)	(0.011)	(0.013)
-5	-0.008	0.019	-0.008	-0.007
	(0.016)	(0.020)	(0.010)	(0.011)
-4	0.009	0.031*	-0.005	-0.003
	(0.013)	(0.017)	(0.009)	(0.010)
-3	0.018	0.038**	-0.002	0.001
	(0.011)	(0.017)	(0.007)	(0.009)
-2	0.006	0.020	0.002	0.005
	(0.010)	(0.014)	(0.006)	(0.008)
-1	-	-	-	-
	-	-	-	-
0	-0.034***	-0.088***	-0.030***	-0.040***
	(0.010)	(0.016)	(0.006)	(0.008)
1	-0.068***	-0.134***	-0.047***	-0.066***
	(0.013)	(0.020)	(0.007)	(0.009)
2	-0.049***	-0.099***	-0.038***	-0.050***
	(0.015)	(0.023)	(0.008)	(0.009)
3	-0.026	-0.056**	-0.014*	-0.019*
	(0.017)	(0.024)	(0.008)	(0.010)
4	0.002	-0.023	-0.000	-0.001
	(0.015)	(0.020)	(0.008)	(0.010)
5	0.021	-0.004	0.018*	0.019
	(0.018)	(0.025)	(0.011)	(0.012)
6	0.028	-0.002	0.033***	0.032***
	(0.020)	(0.026)	(0.012)	(0.012)
7	0.011	-0.015	0.023	0.022
	(0.025)	(0.030)	(0.015)	(0.016)
Neighborhood	Tract	Block Group	Tract	Block Group
Treatment	0.25	0.25	0.5	0.5
Control	0.5-3.0	0.5-3.0	0.5-3.0	0.5-3.0
Obs.	3,006,083	2,999,179	3,451,026	3,444,129
R-sq.	0.700	0.706	0.696	0.702

Notes: DD coefficients from estimation of Equation 2.1 on semester grade point average displayed, dropping students that were exposed to multiple shootings during their LAUSD high school careers. Standard errors clustered by zip code. Includes indicators for the presence of a non-officer involved homicide in a Census block-semester.

Table 2.A5. Effects on Daily Absenteeism

Time to Treat	(1)	(2)	(3)	(4)
-7	-0.009	-0.001	-0.002	-0.004
	(0.007)	(0.008)	(0.004)	(0.004)
-6	-0.006	0.000	-0.008**	0.003
	(0.007)	(0.006)	(0.003)	(0.003)
-5	-0.002	-0.004	-0.001	-0.002
	(0.008)	(0.005)	(0.003)	(0.003)
-4	-0.006	-0.009	-0.004	-0.006**
	(0.007)	(0.006)	(0.004)	(0.003)
-3	-0.001	0.001	0.000	0.001
	(0.007)	(0.008)	(0.004)	(0.003)
-2	0.004	0.001	-0.000	-0.004
	(0.006)	(0.006)	(0.004)	(0.003)
-1	-0.001	-0.001	-0.004	-0.003
	(0.008)	(0.006)	(0.003)	(0.003)
0	-	-	-	-
	-	-	-	-
1	0.014*	0.012*	0.008*	0.007**
	(0.008)	(0.006)	(0.004)	(0.003)
2	0.009	0.004	0.007*	0.005
	(0.007)	(0.007)	(0.004)	(0.003)
3	-0.004	-0.001	0.001	-0.001
	(0.007)	(0.006)	(0.003)	(0.003)
4	0.003	0.010*	0.006*	0.002
	(0.007)	(0.006)	(0.003)	(0.003)
5	0.006	-0.003	-0.001	0.002
	(0.008)	(0.005)	(0.003)	(0.003)
6	0.008	-0.000	0.001	-0.002
	(0.008)	(0.006)	(0.004)	(0.003)
7	0.000	0.004	0.004	-0.003
	(0.008)	(0.006)	(0.004)	(0.003)
Time	Days	Weekdays	Days	Weekdays
Treatment	0.25	0.25	0.5	0.5
Control	0.5-3.0	0.5-3.0	0.5-3.0	0.5-3.0
Obs.	6,755,882	9,400,436	7,172,353	9,982,137
R-sq.	0.310	0.297	0.306	0.294

Notes: DD coefficients from estimation of Equation 2.1 on absenteeism displayed. Standard errors clustered by zip code. Includes Census tract-date fixed effects and crime controls for the presence of a non-officer involved homicide in a Census block-date.

Table 2.A6. Effects on GPA by Distance

Post x Distance	(1)		(2)	
0 to .1 miles	-0.028*	(0.016)	-0.048**	(0.020)
.1 to .2 miles	-0.023***	(0.009)	-0.030**	(0.013)
.2 to .3 miles	-0.010	(0.007)	-0.012	(0.009)
.3 to .4 miles	-0.013**	(0.006)	-0.016*	(0.009)
.4 to .5 miles	-0.004	(0.007)	-0.013	(0.009)
Neighborhood	Zip		Tract	
Obs.	3,553,185		3,548,614	
R-sq.	0.690		0.694	

Notes: DD coefficients from estimation of Equation 2.1 on semester grade point average, replacing time to treatment indicators with interactions between distance from shooting indicators and a post-treatment dummy. Standard errors clustered by zip code.

Table 2.A7. Effects on GPA by Distance (Treatment Intensity)

Time to Treat	(1)	(2)	(3)
-7	0.025	0.019	0.020
	(0.030)	(0.030)	(0.033)
-6	-0.032	-0.045	-0.036
	(0.026)	(0.028)	(0.032)
-5	-0.009	-0.008	-0.003
	(0.022)	(0.025)	(0.030)
-4	0.005	0.017	0.017
	(0.018)	(0.019)	(0.024)
-3	0.006	0.017	0.021
	(0.012)	(0.016)	(0.021)
-2	-0.006	-0.004	-0.004
	(0.012)	(0.014)	(0.016)
-1	-	-	-
	-	-	-
0	-0.019**	-0.041***	-0.055***
	(0.008)	(0.013)	(0.018)
1	-0.043***	-0.078***	-0.116***
	(0.013)	(0.018)	(0.023)
2	-0.040***	-0.078***	-0.110***
	(0.013)	(0.019)	(0.022)
3	-0.030**	-0.061***	-0.071***
	(0.015)	(0.020)	(0.024)
4	0.005	-0.016	-0.018
	(0.016)	(0.019)	(0.023)
5	0.038**	0.028	0.036
	(0.015)	(0.022)	(0.025)
6	0.036*	0.031	0.032
	(0.021)	(0.025)	(0.030)
7	-0.005	0.008	0.010
	(0.026)	(0.031)	(0.034)
Neighborhood	Zip	Tract	Block Group
Treatment	Intensity	Intensity	Intensity
Control	0.5-3.0	0.5-3.0	0.5-3.0
Obs.	3,553,185	3,548,614	3,541,966
R-sq.	0.690	0.694	0.700

Notes: DD coefficients from estimation of Equation 2.1 on semester grade point average, replacing treatment group dummy with treatment intensity dummy $intensity_i = min[0, 1 - (distance_i \times 2)]$. Standard errors clustered by zip code.

Table 2.A8. Effects on GPA by Media Coverage

Time to Treat	(1)	(2)	(3)	(4)
-7	-0.065***	-0.065***	-0.064***	-0.064***
	(0.020)	(0.020)	(0.019)	(0.019)
-6	-0.006	-0.009	0.002	-0.000
	(0.009)	(0.009)	(0.009)	(0.009)
-5	0.005	0.003	0.013*	0.011
	(0.007)	(0.007)	(0.007)	(0.007)
-4	0.001	0.001	-0.001	-0.001
	(0.004)	(0.004)	(0.004)	(0.004)
-3	0.007*	0.007*	0.010***	0.011***
	(0.004)	(0.004)	(0.004)	(0.004)
-2	-0.002	-0.001	0.000	0.001
	(0.003)	(0.003)	(0.003)	(0.003)
-1	-	-	-	-
	-	-	-	-
0	-0.013***	-0.012***	-0.019***	-0.019***
	(0.004)	(0.004)	(0.004)	(0.004)
1	-0.025***	-0.026***	-0.042***	-0.042***
	(0.007)	(0.007)	(0.008)	(0.008)
2	-0.022***	-0.023***	-0.042***	-0.042***
	(0.006)	(0.006)	(0.008)	(0.008)
3	-0.010	-0.010	-0.029***	-0.029***
	(0.007)	(0.007)	(0.008)	(0.008)
4	-0.018***	-0.018***	-0.029***	-0.029***
	(0.007)	(0.006)	(0.008)	(0.008)
5	-0.000	-0.001	-0.009	-0.010
	(0.007)	(0.006)	(0.008)	(0.008)
6	0.020**	0.021***	0.012	0.013
	(0.008)	(0.008)	(0.008)	(0.008)
7	0.041***	0.040***	0.023**	0.023**
	(0.010)	(0.010)	(0.011)	(0.011)
Sample	All	Dist ¿ 3 mi.	All	Dist ¿ 3 mi.
Neighborhood	None	None	District	District
Obs.	3,806,200	3,791,408	3,675,211	3,660,974
R-sq.	0.692	0.692	0.704	0.704

Notes: DD coefficients from estimation of Equation 2.1 on semester grade point average, where treatment group dummy is set to 1 for students residing in circulation areas of newspapers that mentioned a police shooting. Standard errors clustered by zip code. Treatment sample includes only incidents that were mentioned in at least one local newspaper but not in the Los Angeles Times.

Table 2.A9. Effects on GPA by Suspect Race: Restricted Catchments

Post x	(1)	(2)	(3)	(4)
$Shoot_{minority}$	-0.029***	-0.039***	-0.047***	-0.021**
	(0.008)	(0.012)	(0.011)	(0.009)
$Shoot_{white}$	0.003	-0.010	-0.015	0.013
	(0.013)	(0.015)	(0.020)	(0.015)
$\beta_m - \beta_w$	-0.032	-0.029	-0.032	-0.034
$p(\beta_m = \beta_w)$	0.066	0.175	0.219	0.044
Catchments	All	Restricted	Restricted	All
Students	All	All	Poor	All
Weights	None	None	None	1/Prob
Obs	3,072,817	1,636,999	1,114,097	3,072,708
Rsq	0.683	0.689	0.691	0.681

Notes: DD coefficients from estimation of Equation 2.1 on semester grade point average for minority (black and Hispanic) students and killings. Standard errors clustered by zip code. Includes Census block group-semester fixed effects. Treatment defined as students living within 0.50 miles of an incident. Restricted catchments are school attendance zones that experienced police killings of minorities and of whites during sample period. Poor students are those that qualify for free or reduced lunch and whose parents never attended college. Probability weights based on logit of joint exposure on block group diversity, poverty rate, high school graduation and homicides as well as student lunch status and parental education.

Table 2.A10. Effects on GPA by Suspect Similarity and Incident Context

Treat x Post x Index==	*Similarity* (1)	(2)	*Reasonableness* (3)	(4)
0	0.022	0.009	-0.028	-0.008
	(0.043)	(0.032)	(0.036)	(0.012)
1	-0.039	0.009	-0.053	-0.042**
	(0.025)	(0.012)	(0.047)	(0.017)
2	-0.049**	-0.015*	-0.026	-0.011
	(0.021)	(0.009)	(0.031)	(0.013)
3	-0.056**	-0.047***	-0.116***	-0.045***
	(0.023)	(0.010)	(0.032)	(0.012)
4	-0.133***	-0.088***	-0.117**	-0.063***
	(0.023)	(0.017)	(0.056)	(0.021)
$\beta_4 - \beta_0$	-0.155	-0.097	-0.089	-0.055
$p(\beta_4 = \beta_0)$	0.001	0.004	0.176	0.013
Treatment	0.25	0.5	0.25	0.5
Obs.	2,563,592	3,023,133	2,563,592	3,023,133
R-sq.	0.687	0.683	0.687	0.683

Notes: DD coefficients from estimation of Equation 2.1 on semester grade point average for minority (black and Hispanic) students and minority killings, replacing time to treatment indicators with interactions between a similarity and context indexes and a post-treatment dummy. Standard errors clustered by zip code. Includes Census block group-semester fixed effects. Similarity increments by 1 if the exposed student and suspect are of the same gender (male or female), ethnicity (black or Hispanic), age group (suspect was under 25) or criminal background (suspect had a criminal record). Reasonableness decrements by 1 if a suspect was armed, if a suspect had a criminal record, if none of the involved officers were minority and if the police did not employ non-lethal methods.

Table 2.A11. Effects of Police and Non-Police Violence on Absenteeism

Post x Treat x	(1)	(2)	(3)	(4)
Police	0.006**	0.005**	0.007**	0.007*
	(0.003)	(0.003)	(0.003)	(0.004)
Non-Police	0.003***	0.002***	0.003***	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)
$\beta_p - \beta_n$	0.003	0.003	0.004	0.005
$p(\beta_p = \beta_n)$	0.244	0.305	0.255	0.269
Sample	All	Restricted	All	Restricted
Neighborhood	Tract	Tract	Blk Group	Blk Group
Obs.	38,762,819	20,337,840	38,694,704	20,311,523
R-sq.	0.257	0.255	0.267	0.265

Notes: DD coefficients from estimation of Equation 2.1 on daily absenteeism, separately for police and non-police killings. Standard errors clustered by zip code. Treatment radius set at 0.50 miles.

Table 2.A12. Effects of Share of Police Violence on GPA

	(1)	(2)	(3)	(4)	(5)	(6)
All Killings	-0.016***	-0.018***	-0.015***	-0.016***	-0.015***	-0.016***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Share Police	-0.022***	-0.021***	-0.022***	-0.021***	-0.022***	-0.021***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Crimes			-0.001***	-0.001***	-0.001***	-0.001***
			(0.000)	(0.000)	(0.000)	(0.000)
Arrests					-0.000***	-0.000***
					(0.000)	(0.000)
Sample	All	Restricted	All	Restricted	All	Restricted
Obs.	2,009,604	1,350,012	2,009,604	1,350,012	2,009,604	1,350,012
R-sq.	0.715	0.710	0.715	0.710	0.715	0.710

Notes: Coefficients from estimation of Equation 2.3 on semester grade point average. Independent variables are the total number of police and non-police shootings a student experienced (within 0.50 miles) in the current and prior semester and the share of those shootings committed by police. Standard errors clustered by zip code. Includes Census block group-semester fixed effects.

Table 2.A13. Effects of Police and Non-Police Violence on GPA by Race of Deceased

	(1)	(2)	(3)
Police Minority	-0.0367***	-0.0349***	-0.0348***
	(0.0066)	(0.0066)	(0.0066)
Non-Police Minority	-0.0161***	-0.0143***	-0.0143***
	(0.0024)	(0.0024)	(0.0023)
Police White	-0.0130	-0.0117	-0.0119
	(0.0170)	(0.0169)	(0.0168)
Non-Police White	-0.0133**	-0.0110*	-0.0106*
	(0.0059)	(0.0059)	(0.0059)
Crimes		-0.0016***	-0.0013***
		(0.0002)	(0.0002)
Arrests			-0.0006***
			(0.0001)
$p(\beta_{P_M} = \beta_{N_M})$	0.003	0.003	0.003
$p(\beta_{P_W} = \beta_{N_W})$	0.986	0.972	0.948
Obs.	1,725,228	1,725,228	1,725,228
R-sq.	0.6994	0.6995	0.6995

Notes: Coefficients from estimation of Equation 2.3 on semester grade point average for minority (black and Hispanic) students. Independent variables are the number of police and non-police shootings involving minority and white fatalities a student experienced (within 0.5 miles0) in the current and prior semester and the number of crimes and arrests that occurred in the student's Census block in the current and prior semester. Standard errors clustered by zip code. Includes Census block group-semester fixed effects.

Table 2.A14. Attainment by Grade of Exposure

DV	(1)	(2)	(3)	(4)
Highest Grade	-0.250***	-0.203***	-0.133***	-
	(0.015)	(0.016)	(0.008)	-
Graduated	-0.075***	-0.046***	-0.061***	0.002
	(0.007)	(0.010)	(0.010)	(0.010)
College	-0.070***	-0.032***	-0.038***	-0.024*
	(0.008)	(0.011)	(0.010)	(0.014)
Grade Treated	9th	10th	11th	12th
Obs.	543,015	426,643	337,116	269,577
R-sq.	0.185	0.185	0.192	0.204

Notes: DD coefficients from estimation of Equation 2.4. Standard errors clustered by zip code. Includes demographic controls and block and block group-cohort fixed effects. Grade treated refers to the grade at which treatment exposure (based on 0.50 mile radius) is defined. For example, 10th grade restricts the sample to students who had not been exposed prior to the 10th grade and defines treatment based on exposure in that grade.

Table 2.A15. Attainment by Academic Performance

DV	(1)	(2)	$p(\beta_b = \beta_a)$
Highest Grade	-0.303***	-0.130***	0.000
	(0.022)	(0.027)	
Graduated	-0.103***	-0.063***	0.011
	(0.009)	(0.013)	
College	-0.054***	-0.053***	0.465
	(0.007)	(0.009)	
Performance	Below Basic	Basic or Above	
Obs.	185,828	205,645	
R-sq.	0.212	0.243	

Notes: DD coefficients from estimation of Equation 2.4. Standard errors clustered by zip code. Treatment defined as students living within 0.50 miles of an incident. Includes demographic controls and block and block group-cohort fixed effects. 8th grade performance is classified according to student's average California Standards Test score, relative to a score of 300, which denotes Basic proficiency.

Table 2.A16. Selective Migration

Dependent Var	(1)	(2)
Public Transfer	0.013	0.006
(includes juvenile detention)	(0.009)	(0.004)
Private Transfer	-0.002	-0.001
(includes home schooling)	(0.002)	(0.001)
Leave California	-0.003	0.002
	(0.005)	(0.002)
Any of Above	0.006	0.006
	(0.010)	(0.005)
Treatment Radius	0.25	0.5
Obs.	447,356	473,066
R-sq.	0.115	0.111

Notes: DD coefficients from estimation of Equation 2.4. Standard errors clustered by zip code. Treatment defined as students living within 0.50 miles of an incident. Includes Census block and Census block group-cohort fixed effects as well as demographic controls (race, poverty status, parental education and home language).

Chapter 3

The Effects of Police Violence on Inner-City Students

Abstract

In recent years, officer-involved shootings of minorities have generated widespread controversy – sparking social and political movements like Black Lives Matter among historically under-represented minority groups. Utilizing detailed individual-level records for over four million registered voters in Los Angeles county, I examine the effects of police shootings on civic engagement. I find that exposure to police killings involving black victims increases voter turnout among blacks by 3-5%. However, I find no effects on white or Hispanic turnout, or for shootings involving white or Hispanic victims. Exploiting historical referenda voting, I show that black turnout increased specifically among those neighborhoods opposed to harsher penalties for drug and gang-related crimes. Furthermore, I find that gains in turnout are larger following police killings of unarmed individuals and during midterm elections, when the county sheriff is chosen. Taken together, these results suggest that blacks may strategically respond to police killings by engaging with democratic institutions in order to reform the criminal justice system.

I Introduction

In recent years, police shootings of minorities have garnered increasing public attention. The high-profile killings of Michael Brown, Laquan McDonald and Philando Castile prompted widespread claims of institutional discrimination, leading to federal inquiries of local police departments and sparking a range of social and political responses – from the Black Lives Matter movement to the revenge killings of Dallas police officers. Yet, these cases represent merely tip of the iceberg. Since 2000, over 20,000 people have been killed by police. The vast majority of these incidents involved minority victims and received little or no media coverage (Burghart, 2016).

As one of the most visible arms of government, the actions of law enforcement agencies may have widespread social, political and economic ramifications. Indeed, numerous studies have shown that trust in government is strongly correlated with attitudes towards police (**?**). Those attitudes, in turn, may be shaped by negative interactions interactions with police (**?**) or even second-hand accounts of police misconduct (Hurst and Frank, 2000). Nonetheless, little empirical evidence exists demonstrating how local civic engagement is affected by police violence.

In line with work by Desmond et al. (2016), who find that a high-profile police beating of a black man in Milwaukee led to reduced 911 calls in black neighborhoods, police violence may lead community members to disengage from democratic institutions. On the other hand, political outrage and concerns about government misconduct may instead spur individuals to voice their displeasure at the ballot-box. Better understanding the nature and direction of these effects may be critical to addressing long-standing racial disparities in voter turnout and electoral representation.

This paper provides the first evidence of the causal effects of police violence on voter turnout, perhaps the most common form of civic engagement and a broad measure of social connectedness (**?**). To estimate these effects, I draw on two novel and highly-detailed datasets. The first contains incident-level information for over 400 officer-involved killings that occurred in Los Angeles County from 2008 to 2016. The second includes individual-level demographic and turnout information for the universe of Los Angeles registered voters. By geo-coding the exact locations of each killing and home address, I am able to calculate each voter's exposure (or geographic proximity) to police violence. Employing a generalized difference-in-differences methodology, I exploit the precise timing and location of shootings to generate causal estimates of the impact of exposure to police killings on voter turnout.

I find that exposure to officer-involved killings involving black fatalities increases voter turnout among blacks by 3-5%. However, I find no effects on white or Hispanic turnout nor for shootings involving white or Hispanic victims. In early work, I show that black turnout increased specifically among those neighborhoods that were historically opposed to harsher penalties for drug and gang-related crimes. Furthermore, I show that gains in turnout are larger following police killings of unarmed individuals and during midterm elections, when the L.A. County Sheriff is chosen. Taken together, these results suggest that blacks strategically respond to police killings by engaging with democratic institutions in an effort to hold governments accountable for perceived injustices.

II Data

A Police Killings

Suspect, Date and Location

Incident-level data on police killings come from the Los Angeles Times Homicide Database, which chronicles all deaths in the county committed by a "human hand." Whether an officer was involved is based on information from the coroner and police agencies as well as from LA Times' own research.* For each incident, the LA Times records the name, race, cause of death of the deceased suspect as well as the exact address, time and date of the event. In total, the data contains 414 incidents resulting in 219 Hispanic, 100 black and 77 white fatalities from November 2008 to November 2016.

Contextual Details

For those officer-involved shootings that are officially reported by police departments, the county attorney issues a legal analysis and detailed description of the event based on information compiled from forensic evidence as well as officer and witness testimonies. For each of the 483 reports, I read and hand-coded relevant incident details. These include: the names of the involved

*However, the Times does not report on the majority of these incidents in print.

police officers, whether a suspect was armed with a firearm (47%) or another weapon (24%). In all cases, the county attorney declined to press charges against involved officers.

B Voter Turnout

Individual-level turnout information comes from voter registration files obtained from the California Secretary of State shortly after the 2008, 2010, 2012, 2014 and 2016 elections. Each file contains a list of all valid, registered voters in Los Angeles county at the time of extraction. Across the five datasets, there exist 6,658,538 unique individuals. For each individual, the file includes their full name, date of birth and gender, as well as their home address and party affiliation at the time of extraction. Individuals are identified across files using a unique registrant identification number provided by the state. Crucially, the files include individuals' voting history in national and state elections while residing in California. Because home addresses are current only as of the date of file extraction (ex., individuals in the 2008 voter file may not have resided at the same address, or even the same city, in 2006), I limit the analysis to those elections from November 2008 onwards. I also limit the analysis to participation in even-year general elections (i.e. those that occur on Election Day). In total, the data set

I also predict the race of each individual using surnames and home addresses. To do so, I employed the Consumer Financial Protection Bureau's Bayesian Improved Surname Geocoding (BISG) method, which predicts an individual's race from national surname frequencies and Census block group demographics. 42% of voters are predicted to be white, 37% Hispanic, and 10% black.

C Exposure

To calculate exposure to shootings, I geo-code the home addresses of every voter and the location of every police shooting in the database. I then calculate the coordinate distance between each voter's address and every police shooting that occurred while the individual resided at that address.[†]

[†]Note, each voter file contains home address information as of the date of extraction. Thus while I do know if individuals changed addresses between extraction dates, I do not observe the exact date the individual moved. In those cases, I assume the individual resided at the first address until the beginning of the calendar year of the next extraction.

Individuals are classified as treated or "exposed to police violence" if they lived within 0.50 (0.25) miles — roughly 4 (2) city blocks — of an incident location. Though it is impossible to identify the precise sample of individuals who are aware of a given incident, these definitions take into account the fact that the vast majority of police shootings are unreported in local media and likely known only among family, neighbors and witnesses living near to the event.

The final merged dataset contains voter participation, registration and shooting information for all five statewide and national general elections from January 2000 to November 2014 for each of the 76,450 Census blocks in Los Angeles County.

III Empirical Strategy

A Estimating Equation

The primary obstacle to causal identification in this setting is that police shootings are not random and voter turnout in neighborhoods where these events occur may differ greatly from those where they do not. To account for this potential endogeneity, I rely on a flexible, difference-in-differences framework. This design leverages the panel data to compare changes in voter turnout among individuals that live close to police violence, before and after exposure, to changes over time among control individuals, who live further away. In doing so, the model accounts for any level differences that may exist between individuals that are exposed to violence and those that are not. The validity of this design is further bolstered by the data's granularity, which allows me to control for unobserved time trends at the neighborhood-level, as defined by Census block group or tract.

Specifically, I exploit the time and spatial variation of police shootings by estimating the following base equation:

$$y_{i,t} = \delta_i + \delta_{n,t} + \sum_{\tau \neq -1} \beta_\tau Shoot_{t,\tau} + \varepsilon_{i,t}, \tag{3.1}$$

For example, an individual with address A in voter file 2008 and address B in voter file 2010 is assumed to have lived at address A until January 1, 2010.

Where $y_{i,t}$ represents turnout (0 or 1) of individual i at election t. δ_i are individual fixed effects and $\delta_{n,t}$ are neighborhood-time fixed effect, where neighborhood is defined by Census block group. $Shoot_{t,\tau}$ are relative time to treatment indicators, which are set to 1 for treatment individuals if election t is τ elections from treatment (e.g., $\tau = -1$ corresponds to the last election prior to exposure and $\tau = 0$ the first election after). In my primary specification, treatment is defined as individuals living within 0.25 (0.50) miles of an incident. The control group includes individuals living between half a mile and one mile from an incident.[‡] Drawing on Bertrand et al. (2004), standard errors are clustered by zip code, allowing for correlation of errors within each of the sample's 219 zip codes. The coefficients of interest β_τ then represent the differential change between relative time τ and the last period before treatment among individuals exposed to police violence relative to that same change over time among unexposed individuals in the same neighborhood.

This model relies crucially on a parallel trends assumption. While estimates of β_τ for $\tau < 0$ allow me to test for common trends prior to treatment, identification may still be threatened by selective migration or unobserved post-treatment shocks.

IV Results

A Main Findings

A large literature demonstrates that race is the single strongest predictor of attitudes towards police (Weitzer and Tuch, 2002; Leiber et al., 1998). Furthermore, responses to police use of force depend greatly on the race of the victim of such violence (Ang, 2018; Desmond et al., 2016). Thus, I first estimate a simple pre-post version of Equation 3.1 for each voter-victim race pair. These results are shown in Table 3.1 controlling for neighborhood time trends at the Census block group-level.

[Table 3.1 about here.]

As shown, I find little evidence of changes in turnout among white and Hispanic voters following exposure to police violence. Across killings of whites, blacks and Hispanics, estimates are

[‡]Due to the inclusion of small neighborhood-time fixed effects, expanding the control group to include those living further away produces similar results.

close to zero and never exceed 0.5 percentage points. Examining black voters, I also find near-zero coefficients for police killings of whites and Hispanics. However, exposure to police killings of blacks leads to increases in black turnout. Compared to other voters in their neighborhood that were not exposed to a police killing, turnout among blacks living within a quarter-mile of an incident increases by 1.7 p.p., or roughly 3.5%, in following elections. Despite black voters comprising the smallest race group, this estimate is statistically significant at the 5%-level and no less precise than corresponding estimates for whites and Hispanics.

I next examine changes in black turnout following black killings using the non-parametric model presented in Equation 3.1. This model allows me to better compare the pre- and post-treatment trends between exposed and unexposed groups. Results are displayed in Figure 3.1 and Table 3.2.

[Figure 3.2 about here.]

In support of the parallel trends assumption and the exogeneity of police killings, I find little evidence of differential trends in turnout between blacks exposed to violence and those that were not. Treatment estimates for turnout in elections prior to exposure are insignificant and never rise above 0.7 p.p. in magnitude. In contrast, turnout among increases significantly among treatment individuals in elections following the incident. Estimates indicate that nearby black turnout increases by about 1.6 p.p. in the election immediately following a police killing of another black person. These gains then persist through the following two elections.

Taken together, these findings indicate that police killings may serve to spur civic engagement, but that these effects are limited to black voters in response to incidents involving other blacks. In light of robust survey evidence demonstrating that blacks are much more likely than whites or Hispanics to believe that police use of force is excessive or racially-biased (Taylor et al., 2001), these results suggest that these effects may be driven in part by a desire to express displeasure with local policing tactics and to reform the criminal justice system.

B Robustness

To examine the validity of these results, I intend to conduct a series of robustness tests. First, I will test alternative specifications, such as by including time-varying controls for potential confounds like local crime or poverty and by employing different definitions of treatment and control. I also hope to validate that exposure is a function of geographic proximity by using a treatment intensity variable based on distance.

Second, I will test if responses to police violence differ according to demographic characteristics other than race, like age and gender.

Lastly, as complement to the primary analysis on voter turnout, I also plan to examine effects on voter registration. Because the registration file contains only individuals that were registered to vote at a given election, this analysis is necessarily conducted at the area-level, as opposed to individual-level. Thus, I will instead estimate an analogue of Equation 3.1 on the share of voting age citizens that are registered to vote in each Census block. The outcome measure will be constructed using the registration data and ACS estimates. This analysis will allow me to examine whether gains in turnout are due to changes on the intensive (increased turnout among registered voters) or extensive margins (increased registration and turnout among previously unregistered individuals) of civic engagement.

C Mechanisms

To explore mechanisms, I intend to conduct a series of additional analysis. First, I plan to estimate differential effects on black turnout for police killings of armed and unarmed individuals. Drawing on (Braga et al., 2014; Brandl et al., 2001), these contextual details are critical determinants in community perceptions of police use of force. By comparing responses to killings of more or less threatening individuals, I hope to then shed light on the extent to which changes in voter engagement are driven by perceptions of police misconduct and the "reasonableness" of force.

Second, I plan to estimate differential effects based on historical referendum voting in local neighborhoods. In 2008, two propositions regarding criminal justice reform were voted on in

California. The first, California Proposition 5, sought to reduce the severity of criminal punishments for non-violent drug-related offenses. The second, California Proposition 6, sought to increase the penalties for gang-related crimes. While neither proposal was approved, both generated significant debate around the California criminal justice system. By examining heterogeneous responses to police violence across neighborhoods for and against these propositions, I hope to better understand whether blacks are turnout out in support of or opposition to police use of force.

Third, I plan to estimate differential effects between midterm and Presidential elections. As both the California Attorney General and the Los Angeles County Sheriff are elected in midterms, this will provide insight into whether voters are strategically attempting to influence the local criminal justice system or if they are instead spurred on by a more general sense of political activism.

V Conclusion

This paper seeks to provide the first causal evidence of the impact of police violence on civic engagement. Preliminary results indicate that, while police killings have no effect on turnout among whites and Hispanic voters, killings of blacks lead to large and significant increases in participation among black communities.

This research contributes to a burgeoning literature on collateral effects on local communities of policing (Ang, 2018; Legewie et al., 2018) and violence (Karol and Miguel, 2007; Callen et al., 2014; Imas et al., 2015). Perhaps more importantly, this paper hopes to inform policymakers seeking to improve police-community relations, particularly among minority neighborhoods. As civic engagement is vital to political and economic representation (Miller, 2008; Cascio and Washington, 2014; Fujiwara, 2015), understanding the effects of police shootings on voter turnout may yield important insight to persistent racial inequities in crime, income, health and education.

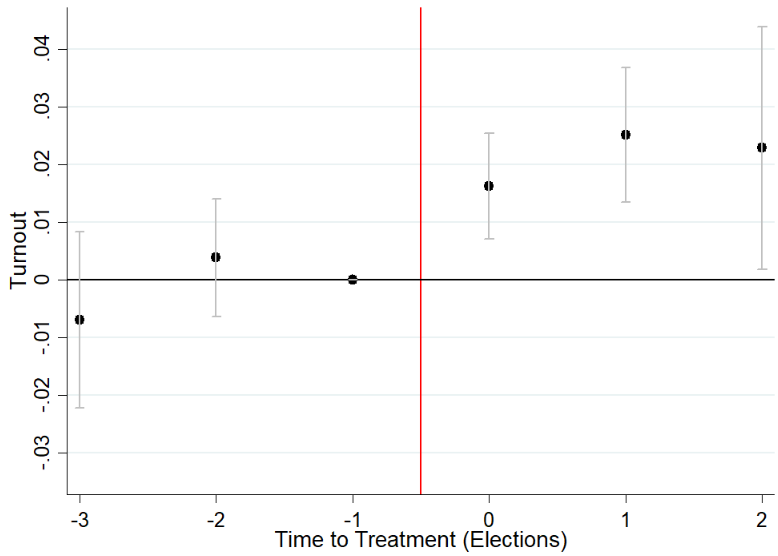

Figure 3.1. Effects of Black Killings on Black Turnout

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 3.1 on black turnout. Standard errors clustered by zip code. Includes Census block group-election fixed effects. Treatment defined as individuals living within 0.25 miles of a police killing of a black suspect. Red vertical line represents time of treatment. Full results displayed in Table 3.2, Column 1.

Table 3.1. Effects on Turnout by Voter and Suspect Race

	Suspect Race		
Voter Race	White (77)	Black (100)	Hispanic (219)
White	0.004	-0.004	0.001
(n=2,832,977)	(0.003)	(0.005)	(0.004)
Black	0.001	0.017***	0.003
(687,104)	(0.008)	(0.005)	(0.005)
Hispanic	-0.005	0.005	0.001
(2,430,326)	(0.004)	(0.004)	(0.002)

Notes: DD coefficients from estimation of Equation 3.1 on turnout displayed, separately by voter and suspect race. Standard errors clustered by zip code. Includes Census block group-election fixed effects. Treatment defined as individuals living within 0.25 miles of a police killing of given suspect race.

Table 3.2. Effects of Black Killings on Black Turnout

Time to Treat	(1)	(2)
-3	-0.007	0.005
	(0.008)	(0.006)
-2	0.004	0.003
	(0.005)	(0.004)
-1	-	-
	-	-
0	0.016***	0.012***
	(0.005)	(0.004)
1	0.025***	0.011**
	(0.006)	(0.005)
2	0.023**	0.010*
	(0.011)	(0.006)
Treatment	0.25	0.50
Obs.	333,430	424,199
R-sq.	0.699	0.682

Notes: DD coefficients from estimation of Equation 3.1 on black turnout displayed. Standard errors clustered by zip code. Includes Census block group-election fixed effects. Treatment defined as individuals living within 0.25 (0.50) miles of a police killing of a black suspect.

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