Title
Essays on Political and Economic Inequality

Author
Aneja, Abhay Pal

Publication Date
2019

Peer reviewed|Thesis/dissertation

Essays on Political and Economic Inequality

by

Abhay Pal Aneja

A dissertation submitted in partial satisfaction of the

requirements for the degree of

Doctor of Philosophy

in

Business Administration

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor Ernesto Dal Bo, Co-Chair
Professor W. Reed Walker, Co-Chair
Professor Noam Yuchtman
Professor Prasad Krishnamurthy

Summer 2019

Essays on Political and Economic Inequality

Abstract

Essays on Political and Economic Inequality

by

Abhay Pal Aneja

Doctor of Philosophy in Business Administration

University of California, Berkeley

Professor Ernesto Dal Bo, Co-chair

Professor W. Reed Walker, Co-chair

This dissertation examines how the democratic participation and representation of disadvantaged minority groups affect various dimensions of social and economic wellbeing—including labor market performance, protection from violence, and access to redistributive social programs. Across three chapters, I examine diverse forms of minority political inclusion. In one part of the dissertation (Chapter 1), I examine the effects of "top-down" political empowerment, stemming from the formal extension of franchise protections to a disadvantaged minority. In the second part of the dissertation (Chapters 2 and 3), I examine "bottom-up" political empowerment; specifically, I examine how the political organization of minorities into identity-based political parties allows minorities to redirect public resources in ways that are beneficial to these subpopulations.

In Chapter 1 (co-authored with Carlos Avenancio-Leon), I examine whether a minority group's access to the franchise is capable of not only protecting the group's right to participate politically, but also improving its substantive socioeconomic wellbeing. Although a central concern for racial and ethnic minorities is having an equal opportunity to advance group interests via the political process, there is surprisingly little evidence about whether policies that advance political equality are connected causally to social and economic equality. In this paper, we examine whether and how the expansion of minority voting rights contributes to advances in minorities' economic interests. Specifically, we consider how the political re-enfranchisement of black Americans in the U.S. South, stemming from the passage of the 1965 Voting Rights Act (VRA), contributed to improvements in their relative economic status during the 1960s and 1970s. Using spatial and temporal variation arising from the federal enforcement provision of the VRA, we document that counties where voting rights were more strongly protected experienced larger reductions in the black-white wage gap between 1950 and 1980. We then show how the VRA's effect on the relative wages of black Americans operates through two demand-side channels. First, the VRA contributed to the expansion of public employment opportunities for black workers and afforded these workers existing public-sector wage premia. Second, in line with previous work on the importance of civil rights laws, the VRA contributed to and complemented the enforcement of labor market policies such as affirmative action and anti-discrimination laws.

In Chapter 2, I (along with S. K. Ritadhi) examine whether improving the political voice of historically-marginalized minority groups can reduce human and civil rights abuse. To answer this question, we examine the impact of political parties dedicated to representing India's Scheduled Castes and Tribes (SC/STs) on caste-based violence. We address the endogenous selection of minority-favored politicians using state-level variation in aggregations of close election outcomes. We find that a 10 percentage-point increase in the fraction of SC/ST politicians reduces the incidence of SC/ST-targeted violence by three percentage points. Improved attitudes of SC/ST citizens toward state institutions suggest that our results are not the product of negative reporting bias. Further analysis suggests that the crime reductions are consistent with deterrence induced by politicians influencing the operation of the law enforcement bureaucracy in favor of SC/ST citizens.

Finally, in Chapter 3, I (along with S. K. Ritadhi) explore the impact of minority political parties on redistributive spending. Specifically, we study how caste-based political parties affect the welfare of low caste citizens in India – including Scheduled Castes and Tribes (SC/ST groups). These caste-based parties emerged in response to the perceived inadequacy of electoral quotas, and were unique in their singular focus on improved low-caste welfare. We use the outcome of close elections between caste-based and mainstream parties for exogenous variation, and document two novel findings. We show that low caste party legislators increase low caste households' consumption from the PDS along both the extensive and intensive margins.

This dissertation is dedicated to my kind and patient family. My parents (Arun and Bela) and siblings (Ashima, Sid, and Marlon) have provided unwavering support as I tried for over a decade years to make my way as an academic researcher. And finally, to Anjali, there is no sufficient way to say thank you for everything you've done for me. I've wanted to reach this point in my professional life since the day we met — and you supported me resolutely at each step. You helped me find my dream job. I hope we can spend the next years building a dream life. I also dedicate this dissertation to the memory of my friend Ana, as well to her husband (and my friend), Will, whose optimism in the face of such loss continues to inspire me.

Contents

List of Figures

List of Tables

Acknowledgments

Coauthor information: Chapter 1 of this dissertation is coauthored with Professor Carlos Avenancio-Leon of UC Berkeley. Chapters 2 and 3 were coauthored with S.K. Ritadhi of the Reserve Bank of India.

Acknowledgments: I finished graduate school because of my coauthors and advisors. Indeed, I think that finding the right shoulders to stand on is perhaps my greatest strength as a researcher. My two main student collaborators over the past 6 years—Ritadhi Chakravarti and Carlos Avenancio-Leon—were entirely essential to me finishing my PhD. I learned from them deep institutional knowledge, technical skills, and how to be as ruthless as I could in pushing against priors when doing empirical work. I am honored they chose me as a colleague. They are amazing scholars and true friends.

I was fortunate to start at Berkeley-Haas at a moment when several new faculty members were joining/had joined. Ernesto Dal Bo took a chance on me as a graduate student, even though my *only* qualification for admission was my shared interest in political economy and development. He taught me to be a more careful, systematic thinker. He was also as loyal an advisor as they come. Noam spent 5 years (more than anyone) doing the hard work of advising — ruthlessly killing my bad ideas, and pushing me to think bigger. I'll probably live out my days trying to find an idea that he believes in, and I'll be a better researcher for it. Reed Walker helped me at every single stage of becoming an applied researcher. When he approached me in Fall 2014 (just to introduce himself), I was lost as a grad student (having no ideas and no data is a bad place for a 3rd year PhD student). When he offered to help me find my way, I grabbed on like I was being thrown a a life raft. He taught me the big and small things I would need to finish papers, filtered my ideas, and read countless drafts. My dissertation simply would not have happened without his support. I am so grateful to have been been taught and supported over the past 7 years by these three.

I am equally grateful to my advisors at Stanford Law School. Jacob Goldin convinced me that I had something interesting to say to legal audiences. Even though he wasn't in my field, he vouched for me with people who *were* in my field. He read numerous drafts of my job market paper, even while was balancing the competing demands of being both a new professor and a new father. Prasad Krishnamurthy taught me how to harness my social science focus Michelle Anderson remains my primary role model of a scholar whose research is directed at addressing the most challenging social problems. I hope to follow in her footsteps.

Finally, my journey as a researcher begins with John Donohue, who took a chance on me as an RA 11 years ago. The chance to learn from one of the giants of social science was the

turning point and defining experience of my professional life to date. That I could continue to count on John's support more than 10 years later as one of my most forceful champions gave me the confidence I needed to weather the academic job market. In more ways than one, I aspire to be like him as a resarcher.

There are many more people (at Berkeley, Stanford, and elsewhere) who helped me reach this point. I can't hope to name them all, but I will mention a few. First, many other faculty at both Berkeley and SLS were gracious with their time and support. At Berkeley, Rui de Figuereido, Guo Xu, Fred Finan, Conrad Miller, and several others provided valuable feedback on my work (including participants in the BPP Student Seminars, Political Economy Research Lunch, Labor Lunch, and OEW Seminar). Ned Augenblick, Marty Olney, and Raul Sanchez de la Sierra deserve special mention because of how much they cared about grad students, both as researchers and as people with complex problems that deserved attention. My colleague Paulette Cha was indispensable to getting through years 1 and 2. She would always checked in on me, and sent along her good wishes before every major assignment. More recently, Nate Atkinson was a near-constant sounding board for legal ideas, as were Oren Reshef and Gauri Subramani for social science ideas. Just as importantly, I appreciated their constant good cheer. Tarek Ghani, Mehmet Seflek, Laura Boudreau, Thiago Scot, Petr, Santiago Truffa, and many other students/former students in the BPP group were smart, kind, and collegial colleagues; I'm grateful to you all. At SLS, numerous additional people were instrumental in helping me find a job and sharpen my research. Dan Ho and Pam Karlan read numerous drafts of my work, and offered much constructive feedback. Barbara Fried, Bernie Meyler, and Alison Morantz generously helped me strategize for the academic job market. I would not be here without the support of these esteemed members of Stanford Law School.

Chapter 1

The Effect of Political Power on Labor Market Inequality: Evidence from the 1965 Voting Rights Act

Chapter abstract: A central concern for racial and ethnic minorities is having an equal opportunity to advance group interests via the political process. There remains limited empirical evidence, however, whether democratic policies designed to foster political equality are connected causally to social and economic equality. In this paper, we examine whether and how the expansion of minority voting rights contributes to advances in minorities' economic interests. Specifically, we consider how the political re-enfranchisement of black Americans in the U.S. South, stemming from the passage of the 1965 Voting Rights Act (VRA), contributed to improvements in their relative economic status during the 1960s and 1970s. Using spatial and temporal variation arising from the federal enforcement provision of the VRA, we document that counties where voting rights were more strongly protected experienced larger reductions in the black-white wage gap between 1950 and 1980. We then show how the VRA's effect on the relative wages of black Americans operates through two demand-side channels. First, the VRA contributed to the expansion of public employment opportunities for black workers and afforded these workers existing public-sector wage premia. Second, in line with previous work on the importance of civil rights laws, the VRA contributed to and complemented the enforcement of labor market policies such as affirmative action and anti-discrimination laws.

1.1 Introduction

A half-century ago, the United States Supreme Court Chief Justice Earl Warren hailed the right to vote as one that is "preservative of [all] other basic rights," social, civic, and economic.[1] The chief justice's view reflects a widely shared belief in the franchise's power to

[1] *Reynolds v. Sims*, 377 U.S. 533 (1964).

protect vulnerable citizens and help them achieve a better standard of living. Guided by this belief, black Americans during the 1960s made voting rights a centerpiece of the civil rights movement for social equality. Leaders of this movement viewed political representation as necessary to adequately address economic problems related to poverty, labor market disparities, and other aspects of minority disadvantage that plagued black communities during the first half of the 20th century (Button 1989). Reverend Martin Luther King, Jr., for example, called access to the ballot box "the foundation stone for political action...[w]ith it the Negro can eventually vote out of office public officials who bar the doorway to decent housing, public safety, jobs and decent integrated education."

This paper examines whether and how the political incorporation of a disadvantaged minority generates individual economic benefits for that group. We bring evidence to bear on this question by examining one of the most significant episodes of minority enfranchisement in the past century: the passage of the 1965 Voting Rights Act (VRA) in the United States (U.S.). The VRA outlawed discrimination at the voting booth directed primarily against racial minorities (and black Americans in particular). As a consequence of this key civil rights statute, the size of the black American electorate increased almost overnight – particularly in the southern U.S., where voting rights had been most heavily restricted.[2]

By making politicians responsible to black voters, the statute was intended to increase the responsiveness of local, state, and federal-level representatives to racial minorities' policy interests. During the civil rights era, these interests centered on the alleviation of poverty and economic inequality (perpetuated and maintained via black political suppression). Yet, there remains relatively little evidence that political power furthered these interests. We bring evidence to bear on this question. Given that the aspect of economic status most important to blacks during this time was equal access to opportunities in the labor market (Button 1989), we examine the impact of political enfranchisement on the Southern black-white wage gap. We also seek to understand the mechanisms through which the VRA promoted racial economic equality. Understanding these downstream effects of minority voting power is particularly important given that key protections of the VRA were effectively struck down by the Supreme Court in its 2013 *Shelby County v. Holder* ruling.

To identify the causal effect of voting rights on labor market inequality, we exploit the temporal and spatial variation in minority voter protection and participation afforded by the targeted application of the VRA. While the blanket ban on voting discrimination applied nationwide, the statute's strictest requirements – enumerated in Section 5 – were for jurisdictions where discrimination had been particularly severe. In these places, federal authorities took active steps to register minority voters and protect against discrimination at the polls. Section 5 applied to only a subset of counties and states primarily in the South and Southwest United States (see Figure 2).

The problem for causal inference using Section 5 is a familiar one. Congress did not choose which political jurisdictions would be subject to federal election oversight randomly. Several factors likely influenced this decision. While some of these factors are observable to

[2]In the remainder of the paper, we will refer to the southern region of the United States as "the South." For our purposes, we define this region to include Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia. For ease of the exposition, we also include Arizona, as it was one of the major areas affected by the treatment variation.

the researcher (such as discriminatory laws or economic conditions), many are not (such as culture or racial animus). Such factors were also potentially correlated with economic outcomes. Rather than comparing outcomes across all covered counties and uncovered counties, which would largely amount to a comparison of South to North, we focus on adjacent county pairs (both across states and within states) – where one county is protected under the VRA, and the other is not.[3] The benefit of this approach is that it allows us to control for smoothly-varying unobservable conditions (such as cultural, political, or economic differences) that may confound estimates obtained using more coarse data (e.g., at the state level). This approach better approximates a quasi-experimental setting where the researcher compares "like" treated counties with "like" control counties. This design and the VRA coverage variation require more geographically-detailed location data than is typically available in public-use data samples. We thus use confidential administrative data from the U.S. Census Bureau that includes detailed information on workers' residence and mobility.

The central contribution of our paper is to test whether there exists a link between minority political empowerment and economic well-being – in other words: does the franchise have instrumental value for excluded minorities? We find that it does. The VRA reduced the conditional wage gap between black and white workers by around 5.5 percentage points between 1950 and 1980 (statistically significant at the 5 percent level), a result that is robust to several alternative specifications. This effect is driven primarily by rising black wages within VRA-covered counties, with no statistically significant cost to overall employment. Moreover, these effects are consistent with the VRA making government more responsive to black voters' interests in socioeconomic inequality. To show this discrete shift in political power in favor of black Americans, we also show that the stronger voter protections of the VRA led to a sustained increase in both political participation, as well as legislative actions favorable to minority interests.

As numerous scholars have noted, a concern for isolating the effect of any single American civil rights reform on economic outcomes is separating the effects of a single policy, such as the VRA, from the other major civil rights reforms from this period (Donohue and Heckman 1991; Smith and Welch 1977). The 1960s ushered in several major federal reforms designed to ameliorate economic inequality – including the Equal Opportunity Act (1963), the Civil Rights Act (1964), the Fair Labor Standards Act Amendments (1966), and the Fair Housing Act (1968).[4] To alleviate concerns about concurrent civil rights laws, unobserved state institutional differences, or differential trends, we exploit both the temporal variation arising from the expansion of voting rights protection as well as unique within-state variation. We also analyze a separate 1975 expansion of the VRA across over 200 counties in Texas, Arizona, and other counties within the Southwest. Additionally, we limit our analysis to one state (North Carolina) where approximately half of all counties were covered, and the remainder uncovered. Across these numerous subsamples and specifications, we observe effects of similar directions and magnitudes.

Having documented a causal relationship between political power and economic status, we then proceed to probe various potential economic channels through which black political incorporation reduced labor market inequality. We first focus on a form of direct redistribu-

[3]Figure 2 presents the nationwide variation in VRA coverage, discussed at length in Section 1.2.

[4]Note, however, that unlike the VRA, all of these laws applied with equal force nationwide.

tion that elevated the wages of black Americans: government employment. We find that in VRA-covered counties, black Americans were between 2 and 4 percent more likely to receive a government job relative to white workers. Two stylized facts highlight the plausibility of this channel. First, public-sector workers commanded a substantial premium over their private-sector counterparts – and this premium was particularly high for black workers, who earned 20 percent more in the public sector than in the private. Second, the size of the government workforce (particularly at the local and state levels) grew steadily during the second half of the twentieth century, providing new job vacancies for employment that could be affected by politics.

Both the increase in black public sector hiring as well as the public wage premium have the potential to put significant upward pressure on wages in the private sector. We demonstrate that was indeed the case; in addition to the direct benefits of the VRA for those workers who become employed by government, the VRA's impact on black public sector employment also contributed to the rise in black relative incomes observed in the *private* sector.[5] We show that in occupations where private firms face greater competition with the government for black labor, minority workers experience sharper improvements in wages within the private sector. By improving the bargaining power of the black labor force, the changing composition of the government labor force (influenced by minority political power) contributed to wage equality in markets where discrimination existed previously. We consider these private sector wage-bargaining effects through the lens of a search model (Beaudry, Green, and Sand 2012), both to clarify conceptually the connection between the private and public sector, as well as to quantify the civil rights era's public sector effect on labor market outcomes.

Political participation and influence can also affect the labor market performance of minorities through other economic channels. We provide evidence consistent with voting rights contributing to and/or complementing existing labor market inventions that aimed to reduce black-white income disparities, such as anti-discrimination and affirmative action policies. We cast doubt, though, on channels related to improved human capital, such as improvements in the education of black workers. Finally, we consider *how* the right to vote affects political power – whether it is by changing the incentives faced by all politicians (i.e., spatial competition), or by increasing the presence of black elected officials. In our view, the weight of the evidence favors the former.

1.1.1 Contributions & Roadmap

Our paper lies at the intersection of research within labor economics, political economy, and economic history. First, we contribute to research on what factors affected declining labor market inequality over the twentieth century. Several studies on this area generally emphasize one of two general hypotheses regarding the causes of black-white economic convergence. On one hand, research by Smith and Welch (1989) and others show that the improvement of black economic status was driven by changes operating through supply-side mechanisms, such as the changing quantity/quality of schooling, out-migration, and the crowding-out effects

[5]The increased public sector employment of black Americans put upward pressure on previously low black wages by improving their bargaining power – both by dampening private labor supply and improving the outside-option wage.

of expanding social welfare programs.[6] Alternatively, on the demand side, Freeman (1973), Leonard (1990), Donohue and Heckman (1991), Chay (1998), Derenoncourt and Montialoux (2018), and others argue that legislation passed during the civil rights era contributed measurably to the improved relative economic status of black workers. These studies examine the effects of labor market regulations, such as the 1964 Civil Rights Act (CRA), the creation of affirmative action requirements, and the expansion of the minimum wage. However, while Donohue and Heckman (1991) allude to the importance of black political power to enforce the civil rights-era policy agenda to reduce economic inequality, there has been no formal test of the role played by the VRA. This paper thus provides the first empirical evidence that minority political power may also have contributed to the reductions in black-white economic inequality observed during the period.

Second, we contribute to work examining public sector employment in the U.S. Numerous researchers have shown that government jobs provided a source of economic mobility for black Americans beginning in the mid-20th century. Minorities in the public sector historically earned more than their private sector peers, in part due to the fact that government provided greater opportunities for white-collar employment and upward occupational mobility (Eccles 1975; Freeman 1976; Hout 1984; Pitts 2011). Raw employment statistics suggest that the public sector became an important occupational niche for black workers beginning in the 1960s, as suggested by labor historians Katz, Stern, and Fader (2005). We highlight this trend in Figure 4, which shows the drastic movement of black workers into the public sector beginning in the mid-1960s, particularly within the South. We provide evidence of one causal factor – political influence – related to the increasing importance of the public sector for black employment after 1960.[7] The closest paper to ours in this regard comes from Henderson (2017), who documents that restricting immigrant voting rights at the turn of 19th century decreased the public-sector employment of such immigrants.

Third, we contribute to literature on the effects of minority political incorporation and representation. Theoretical work such as Romer (1975), Roberts (1977), and Meltzer and Richard (1981) suggest that that extending the franchise should, by shifting the median voter toward the poor, increase pro-poor redistribution and in turn reduce inequality. Despite such predictions about the relationship between political and economic inequality, there is relatively little empirical evidence on the economic value of political voice for disadvantaged minorities. Existing studies focus on how minority voting rights shape government spending and redistribution (Cascio and Washington 2014; Husted and Kenny 1997). Research also documents individual-level benefits for the children of voters that stem from enabling poor and women voters – in particular, improvements in child health and education (Miller 2008;

[6]Smith and Welch (1989), for example, show that increasing quantities of schooling can explain about 20-25 percent of the black-white wage gap narrowing in the late 1960s. Card and Krueger (1992) document similar findings, but also argue for a substantial role of anti-discrimination laws. Finally, President Lyndon Johnson's Great Society expansion of social welfare programs may have put upward pressure on wages. For example, marginal workers have responded to increased public assistance by reducing their participation in the labor force; this was particularly plausible for black workers with low levels of education. Donohue and Heckman (1991), however, show that this factor can explain only about 10-20 percent of wage convergence.

[7]The black share of public employment grew particularly sharply in the Deep South post-VRA (Figure **??**). The time path of white public-sector employment, on the other hand, is unchanged. These raw statistics provide *prima facie* evidence that the VRA was associated with the changing racial composition of the government workforce.

Naidu 2012; Carruthers and Wanamaker 2017; Fujiwara 2015; Kose, Kuka, and Shenav 2019). To date, however, little evidence has been offered on the topic of whether democratic participation and accountability concretely improve the material circumstances of minority voters themselves. We demonstrate that enfranchisement and political influence can in fact confer direct benefits to the marginalized group receiving political rights.

Our study also relates to work on the employment effects of the changing supply of minority politicians (i.e., "descriptive representation"). Eisinger (1982) and Nye, Rainer, and Strat (2015), for example, document how increases in minority city council members and mayors improve minority employment outcomes in both the private and public sectors (i.e., better jobs and better pay). Our study differs from these studies by examining the effect of citizen political empowerment more generally, rather than on the effect of descriptive representation.

The remainder of the paper continues as follows. In Section 2, we describe the institutional setting, including a discussion of the Voting Rights Act and the variation we exploit to generate credible empirical estimates. We discuss conceptually the expected effects of minority voting power in Section 3. In Sections 4 and 5, we discuss our empirical strategy and present our primary results. We empirically analyze different labor market mechanisms in Section 6, and political mechanisms in Section 7. Finally, we offer brief concluding remarks in Section 8.

1.2 Context & Historical Background

In this section, we briefly discuss the context for our empirical study, including the history of black voter suppression and the structure of the VRA. The VRA sought to both facilitate the inclusion of black Americans in politics, as well as to address racial disparities in economic status left by decades of civic exclusion concentrated primarily in the American South.

1.2.1 Pre-1965 American South: *De Facto* Disenfranchisement of Black Americans

After the Civil War and the end of slavery, American states ratified the Fifteenth Amendment to the U.S. Constitution. The Fifteenth Amendment guaranteed the right to vote to all men regardless of "race, color, or previous condition of servitude." With the vote and its concomitant political power, black Americans in the South prospered for the first time in the country's history, during the period known as the Reconstruction (Logan 2018). This period of political and economic progress was short-lived, though, as formerly Confederate political jurisdictions sought to reimpose the racial hierarchy that allowed for white Americans' social, political, and economic dominance. To this end, Confederate states and counties responded to the expansion in black Americans' political rights by imposing policies between 1870 and 1910 that, while ostensibly neutral, completely restricted their political participation in practice. These *de facto* franchise restrictions were commonly referred to as Jim Crow laws).[8] As a result of Jim Crow-era political restrictions, most eligible black adults could

[8]Examples included whites-only primaries, poll taxes, and literacy tests. Poll taxes are straightforward to understand. Whites-only primaries prohibited eligible black voters from participating in primaries rather

not register to vote during the first half of the 20th century.[9]

The denial of voting rights contributed in several ways to economic inequality along racial lines. With a monopoly on political power, whites excluded blacks from receiving local public goods or forms of redistribution. Research by Margo (1982), Kousser (1980), and Pritchett (1989), for example, shows that Jim Crow political restrictions significantly reduced the quantity and quality of schools attended by black children. Southern governments also enacted regulations that segregated blacks and whites on most dimensions of social and economic life. Legal segregation reduced black citizens' access to public transportation and reduced access to many services. As a consequence, Jim Crow segregation reduced the competitiveness of black labor in at least two ways: (1) by lowering the returns available to black workers from participating in the labor market and (2) by raising the costs borne by establishments that employed black workers (Anderson and Halcoussis 1996). More generally, the system of segregation maintained by political suppression served as a reminder to blacks that they were second-class citizens in all dimensions of civil, economic, and political life. Jim Crow disenfranchisement and segregation also led to the outmigration of black families from the South (Naidu 2012).

1.2.2 Passage of the The Voting Rights Act & the Importance of Section 5

It is now widely acknowledged that the widespread political exclusion of racial minorities between Reconstruction and the civil rights era was a major driver of Southern black-white economic disparities between 1890 and the early 1960s (Roback 1984; Sundstrom 2007; Wanamaker 2017). The right to participate in elections and shape policy thus became a centerpiece of the Civil Rights movement for socioeconomic equality during the 1950s and 1960s. Policymakers and activists viewed voting rights (and the political representation and influence that would come with an equally-weighted vote) as a necessary step toward improving minority socioeconomic status. In 1965, months after civil rights activists' famous march from Selma to Montgomery, President Lyndon Johnson signed the VRA into law, restoring for black Americans (and all racial/ethnic minorities) the right to vote in the South. The sections of the VRA that are still intact remain today the key federal statutory tools for attacking discrimination against racial minorities in politics.

The key enforcement provisions of the VRA are Sections 2 and 5. Section 2(a) prohibits the use of voting qualifications that deny the right to vote on account of race or color. Section 2(b) is the main instrument to combat political discrimination *nationwide*. Enacted to give life to the Fifteenth Amendment, Section 2 forbids all electoral structures that deny racial minorities the "opportunity...to participate [equally] in the political process and to elect

---

than elections, since primaries were administered by semi-private party organizations that fell outside government regulation, but controlled who would contest many elections. Literacy test requirements usually mandated that individuals read and explain a portion of a state's constitution in order to vote, with performance on such tests being left to the discretion of a local (white) election official. By 1904, every Southern state except Kentucky had passed some form of Jim Crow suffrage restriction. See Perman (2001) for a history of Southern minority disenfranchisement.

[9]Florida and Tennessee were the only Southern states in which as many as half of all eligible black voters were registered. Other states of the South were considerably worse off.

representatives of their choice." This provision is commonly used to challenge vote-denying practices (such as voter identification requirements), as well as vote-diluting practices (such as gerrymandered districts) (Ho 2018; Karlan 1989).

Section 5, however, was long considered the strongest provision of the VRA.[10] This provision of the statute sought to *affirmatively* give black Americans political voice in the areas of the country (primarily in the South) where their voting rights had been most suppressed. The provision applied only to a subset of states and counties (until 2013, when the Supreme Court effectively struck the provision down in the famous *Shelby County v. Holder* decision).[11] Counties and states covered under Section 5 were required to preclear any change to electoral procedures with the U.S. Attorney General or the U.S. District Court for D.C.[12] Furthermore, Section 5 also provided for the appointment of federal examiners to covered jurisdictions, and required that applicants certified by examiners be placed on voter registration lists immediately. Election law scholarship suggests that this latter part of the VRA, while discussed less often by researchers, was crucial for ensuring that previously discriminatory jurisdictions could no longer "foot drag" to register eligible black voters (Karlan 1989). Within two years of the VRA's passage, the Lyndon Johnson administration had used civil service employees to register more black Americans than had been registered in the entire century since the Fifteenth Amendment had been ratified.[13]

Section 5's "covered jurisdictions" were originally defined in the VRA's coverage formula (Section 4(b)) to include any city, county, or state that used a test or device (e.g., a literacy test) and had less than a 50 percent turnout in the 1964 presidential election. The coverage formula thus initially applied to counties in Alabama, Georgia, Louisiana, Mississippi, South Carolina, and Virginia, nearly half of North Carolina's 100 counties, and one county in Arizona.[14] Amendments to the VRA in 1975 (henceforth, the "VRA Amendments" or "Amendments") extended coverage to several more counties in the South and Southwest, including counties in Florida, Oklahoma, Arizona, and New Mexico, as well as all counties in Texas.[15]

Our main analysis will compare economic outcomes for individuals residing along the county and state borders that divide VRA-covered from uncovered jurisdictions. Before proceeding to our empirical analysis, we discuss briefly why we might expect changes in the economic fortunes of blacks after the massive political shock to the South brought about by

[10]Maccoon (1980), for example, argues Section 5 was "one of the most useful statutory tools for the enforcement of voting rights," and Motomura (1983) describes it "as perhaps the most important for the continuing protection of minority voting rights."

[11]133 S. Ct. 2612 (2013). The Supreme Court technically struck down Section 4(b) which provided the formula for determining which states would be subject to Section 5.

[12]To obtain federal approval of voting changes, preclearance jurisdictions were required to demonstrate that a proposed change would not have a "discriminatory effect" or "discriminatory purpose." The election law case *Beer v. United States* defined "discriminatory effect" as "retrogression:" any change that reduced the opportunity for minority voters to elect their candidates of choice.

[13]For a discussion, see Davidson and Grofman (1992)).

[14]The original coverage formula looked at whether jurisdictions imposed discriminatory procedural devices at the time of passage, whether less than 50 percent of the voting-age population was registered to vote as of that date, or if less than 50 percent of eligible voters voted in the November 1964 presidential election.

[15]A handful of jurisdictions in California, New York and New Hampshire that had continued to administer literacy tests were also brought under Section 5 preclearance in 1970. Because our focus is on the effects of eliminating Jim Crow inequality, we do not consider these jurisdictions.

the VRA.

1.3 Conceptual Discussion: Economic Effects of Minority Political Power

Theoretical work such as Cox and Mccubbins (1986), Dixit and Londregan (1996), and Lindbeck and Weibull (1987) hypothesizes that elected officials distribute resources to clearly identifiable constituent groups in order to maximize votes. The VRA created such incentives for Southern politicians to respond to the needs of black American communities, since these constituents tended to be geographically compact and relatively homogeneous in their political preferences (Keech 1968).[16]

Whether and how minority political influence affected individual economic outcomes, however, depended on the policy preferences of the newly enfranchised group. The central political concern for black Americans was equal access to employment opportunities, free of discrimination (Schwartz 1967). Figure 1 presents data from a nationwide survey conducted in 1963 by the National Opinion Research Center (NORC). The data indicate that equal opportunity for advancement within the labor market was by far the central policy concern for black Americans at the time the VRA was passed. As such, to the extent that newly-enfranchised black voters expected government to respond to particular policy interests, dedicating state resources to ensure equal labor market opportunities was a plausible policy area for government action.

In principle, minority-preferred politicians may have improved minority workers' labor market outcomes (such as wages and employment) by altering either the demand for or supply of black American workers. Black political empowerment may have contributed to economic equality, including within the labor market, because investments in human capital-building institutions (such as schools and hospitals used by minority children) would lead to an increasingly skilled minority workforce. Qualitative and quantitative historical evidence suggests that the political representation gained under the VRA did indeed lead to such institutional improvements (Cascio and Washington 2014).

Minority-preferred politicians may also have increased the demand for black workers. Politicians exercise significant authority over the distribution of contracts, which could be used to discourage discriminatory practices and increase minority hiring. Minority political power may also have influenced the regulatory and legal enforcement power of the state. For example, research by Stainback, Robinson, and Tomaskovic-Devey (2005, Shulman (1984) shows that political pressures affected the enforcement of civil rights equal employment opportunity law and affirmative action mandates. Politicians can also directly affect the demand for minority labor through government hiring. Several studies have demonstrated how political factors affect the size and composition of the government workforce in the U.S. (Alesina, Baqir, and Easterly 2000; Glaeser and Shleifer 2005; Enikolopov 2014; Chen, Henderson, and Cai 2017). Relative to private firms, which are typically understood as profit-

[16]That blacks were not a majority of a locality's electorate would not necessarily preclude a relationship between their voting strength and policy outcomes, since black voters could form *de facto* political coalitions with sympathetic whites.

maximizers, government agencies may be guided by factors beside efficient production. For example, politicians use the employment/promotion of minorities and women as a tactic to reverse historical patterns of discrimination (Blank 1994).

While new opportunities for government employment offered black workers opportunities to earn a higher income, the compositional change of the minority workforce (toward greater public-sector employment) may also have exerted indirect pressure on private-sector employers. Because government agencies often comprised a meaningful share of the local labor market (often around 20 percent of local employment), both the reduction in labor supply from the positive sectoral demand shock, as well as the better outside option wage, may have exerted upward pressure on private sector wages.[17] Such inter-sector general equilibrium effects have recently been formalized and estimated in the labor economics literature. In the context of changes to local industrial composition, Beaudry, Green, and Sand (2012) document that sectoral demand shocks have substantial cross-sectoral general equilibrium effects on worker wages. When accounting for such effects, total wage effects are 3-4 times larger than the effects as measured by just considering the direct effects.[18] Consequently, a complete evaluation of the economic impact of a public sector channel (caused by an exogenous political shock) may require accounting for such equilibrium effects.

To summarize, in this study we provide evidence that one of the effects of political empowerment via the VRA was the redistribution of labor income to racial minorities who long suffered from private-sector employment discrimination. Empirically we show how labor market equality improved, the labor mechanisms through which these changes took place, and the political triggers that explain such improvements in black labor market status. In showing this, our study directly contributes to different lines of research – in particular, to research on political participation, on labor markets, and on racial inequality.

1.4 Research Design & Methodology

In this section, we explain the details of our empirical approach. Our goal is to evaluate whether blacks' right to vote improved the economic status of black workers (both in absolute terms and relative to their white counterparts). We exploit the temporal and spatial variation in federal voting rights protection under the VRA's Section 5 (which we will refer to as simply the VRA) to test this hypothesis in the American South. Racial economic disparities were believed to have been most acute here prior to the civil rights movement (Sundstrom 2007). The VRA created sharp, discontinuous changes in whether political participation by minorities would be protected by the federal government. These changes provide quasi-exogenous variation that we use to measure the role of political rights in remedying economic disparities.

[17] This upward pressure on private-sector wages due to changes in the public sector can operate regardless of whether the outside option is exercised – in other words, regardless of whether private workers transition into the public sector.

[18] To formalize similar labor market dynamics in our setting, which incorporate political pressure and the interaction of public and private sectors wages, we provide a model in Appendix A.2.

1.4.1 Data Construction

We rely on several sources of data in this paper. Our main data are the restricted-access United States Decennial Censuses (DEC) from 1950, 1960, 1970, and 1980.[19] The long-form Censuses are a 20 percent representative sample of the U.S. population (except for 1950, for which only a 1 percent sample is available). We restrict our sample to working-age adult males working full-time, to remain consistent with previous research on the racial wage gap.(Smith and Welch 1989; Donohue and Heckman 1991; Card and Krueger 1992).[20] The time period we analyze includes fifteen years before and after the peak of the civil rights movement, 1965, when the VRA was passed. This period of time covers the primary period during which black progress in the labor market was observed (Bound and Freeman 1992). The advantage of using the restricted-use Censuses is that they contain detailed information on a respondent's county of residence, allowing us to compare wages for individuals that reside only in neighboring covered and uncovered *counties*. Our primary variable of interest is an individual's hourly wage.[21] The long-form DEC also contains individual information on demographic variables such as individual race, gender, and age. Moreover, DEC also has additional individual and household-level information that allows us to explore other outcomes and potential mechanisms. These include type of employer (public vs. private), occupation, county/state of work (separate from county of residence), migration status, and educational attainment.

The set of counties covered under the VRA comes from the U.S. Department of Justice's Civil Rights Division. While we are primarily interested in the socioeconomic impact of empowerment, we also validate that the VRA had its intended impact on black political empowerment. To this end, we also make use of county-level voting data from the Interuniversity Consortium for Political and Social Research (ICPSR) and Dave Leip's Atlas of U.S. Presidential Elections. Voting age population estimates are based on interpolation from Census demographic data.[22] These data are used to construct county-level estimates of voter turnout (the share of votes cast to eligible voting population) in all presidential elections from 1948-1980.[23] To examine changes in politician responsiveness preferences, we use district-level measures of political ideology and party affiliation for the 87th through the 100th Congresses from DW-NOMINATE data constructed by Poole and Rosenthal (2001). DW-NOMINATE is a multidimensional scaling technique which collapses legislative roll-call voting into a twodimensional ideal point. While the first dimension is commonly considered to be the contemporary liberal-conservative measure (scaled from -1 to 1), the second dimension of DW-NOMINATE, which we examine here, has historically tracked policy issues that cut across party lines and relate to support for civil rights and other race-related issues.[24]

[19] We access these data within the Berkeley Research Data Center (RDC).

[20] Future work will consider the impact of political empowerment on black women.

[21] We use the log transformation of this outcome as our dependent variable.

[22] Interpolated estimates were obtained from (Gentzkow et al., 2011)

[23] We do not examine party vote shares given the concurrent period of Southern partisan realignment (Kuziemko and Washington 2018).

[24] Research suggests that after 1980, there is 2nd dimension is no longer useful due to Southern Realignment. This is not a problem for our purposes, though. Moreover, for corroboration, we also compare these results to a coding of all congressional roll-call votes (by district and year) in favor of civil rights-related issues, produced by Schuit and Rogowski (2017). Results are available upon request.

Finally, we also make use of several other sources to probe robustness and mechanisms further. County-level control variables are based on public-use Decennial Census estimates. Overall levels of government hiring and expenditures at the county level are based on data from the U.S. Census of Governments (COG). To examine political mechanisms, such as the impact of the VRA on the election of black politicians, we digitize over ten years of original data from annual reports produced by the Joint Center for Political and Economic Studies (JCPES). The JCPES produces an annual listing of every black politician in the country (except for 1970), beginning in 1969.[25] We supplement this source with data from Matthews and Prothro (1966), who collected information about black elected officials in the early-1960s South.[26]

1.4.2 Sample: Cross-border County Pairs

An obstacle to identifying the effect of the VRA is that covered jurisdictions were not randomly singled out for additional voter protections. Rather, coverage was deliberate: the VRA targeted the "worst of the worst" in terms of political discrimination against racial and ethnic minorities. One concern, then, is that the unobservable characteristics (including social, cultural, economic political conditions) that led to coverage may also be correlated with economic outcomes, creating bias. For instance, states such as Alabama, Mississippi, and Georgia had considerably more lynchings than other states (Naidu 2012). Prejudicial views about minorities are likely correlated with both political and economic outcomes.

To mitigate concerns about potentially unobservable confounders, we analyze data for the subset of adjacent county-pairs that straddle Section 5 state and county boundaries. This approach is increasingly used in observational studies of policies such as the minimum wage, tax rate changes, and health insurance expansions (Dube, Lester, and Reich 2010; Duranton, Gobillon, and Overman 2011; Feigenbaum, Hertel-Fernandez, and Williamson 2018; Clinton and Sances 2018). The intuition is straightforward: focusing on neighboring counties allows us to compare "like" jurisdictions with "like." Many cultural, political, or economic conditions – any or all of which may affect our outcomes of interest – are likely to change smoothly rather than discretely at jurisdictional boundaries. Our approach thus mitigates concerns about smoothly varying unobservable conditions confounding causal estimates based on data from the universe of counties (or based on state-level data, where possible).[27] Two counties separated by a border either across or within a state should appear more similar than groups of counties far away, or then entire states. The underlying assumption of our approach is that after controlling for border pair-by-race-by-year fixed effects – which together net out any time-varying, pair-specific shocks to black or white wages – any changes in outcome gaps between black and white workers are attributable to the VRA, rather than to other characteristics of the two sets of counties.

[25] For 1969, the listing contains only states in the South, not the Southwest.

[26] This data in an easily usable form was generously provided to us by Jim Alt.

[27] Even state-level analyses would be complicated by the fact there is much within-state heterogeneity coverage by 1975 (Ang 2018).

How Similar are Neighboring Counties?

We create our sample by first focusing on all states where at least one county is covered by the VRA as well as that state's neighboring states. This approach accounts for both the cross-state and the within-state variation in VRA coverage.[28] Our sample includes all pairs of adjacent counties where one county is VRA-covered and the other county is not. The counties that are in our sample can be seen in Figure 3 below.

We can provide corroborating evidence that our research strategy better approximates an "apples-to-apples" comparison. Table A.1 presents summary statistics for our sample – including average county characteristics based on data from the Census as well as other sources in 1960, just before the VRA was passed. These average county characteristics provide evidence regarding both the use of our design, and also suggest that differences between counties are attenuated when we restrict the data to neighboring covered and uncovered counties. Panel A of Table A.1 displays average county-level characteristics for Southern states across all VRA-covered and non-covered counties in 1960 – pre-dating the passage of the VRA. Columns (3) and (4) present means as well as t-test results for tests of the equality of average and treated county means, where the null hypothesis is that the means of VRA and non-VRA counties are equivalent. As Panel A demonstrates, the differences between VRA and non-VRA are often always different at the 5 percent level, for an array of observable characteristics. Thus, these summary statistics suggest that economic and political conditions were different in VRA and non-VRA Southern states, and suggest that there could be fundamental unobserved differences between these states that would confound an analysis of the VRA's causal impact using all counties. Panel B corroborates our assumption of smoothly-varying changes across borders. All difference-in-means tests produce differences that are not significantly different from zero.

1.4.3 Empirical Specification

Using data for the set of adjacent VRA and non-VRA counties, we employ a generalized differences-in-differences design, comparing changes in the outcomes between VRA-covered and uncovered counties, before and after the VRA took effect. In addition, because the government targeted the political empowerment of *black* Americans, we analyze the labor market outcomes of black workers relative to white workers. In short, we estimate the causal effect of the VRA on the black-white racial wage *gap*.[29] The primary empirical specification takes the following form:

$$\log(Y_{ict}) = \beta \left[\text{VRA}_{ct} \times \mathbb{I}\left\{r(i) = \text{Black}\right\}\right] + \mathbf{x}_{ict}\boldsymbol{\gamma} + \mu_{cr(i)} + \mu_{ct} + \mu_{p(c)r(i)t} + \epsilon_{ict} \tag{1.1}$$

In this difference-in-difference-in-differences (DDD) specification, c indexes county, $r(i)$ indexes the race of person i, t indexes year, and $p(c)$ indexes a given county pair. VRA is a

[28] This includes all or parts of Alabama, Georgia, Louisiana, Mississippi, South Carolina, Virginia, Texas, North Carolina, Florida, Kentucky, Arkansas, Tennessee, Oklahoma, West Virginia, Maryland, Utah, New Mexico, and California.

[29] For an example of this approach, see Hirata and Soares (2016), who estimate the impact of trade liberalization on the minority-white wage gap in Brazil.

binary indicator variable for whether a county was VRA-covered in a given year (post-1965 or post-1975). $\mathbb{I}\{r(i) = \text{Black}\}$ is an indicator for whether a worker is black. Because we limit our data to black and white workers only, Black equal to 0 means that a person is white. Our primary dependent variable is the log hourly wage measured for person i in county c, year t (although we also analyze other outcomes).[30] The parameter of interest is β, the coefficient on the interaction $[\text{VRA}_{ct} \times \mathbb{I}\{r(i) = \text{Black}\}]$ (which we will refer to as VRA × Black for ease of exposition). This interaction term thus takes the value of 1 if an individual is a black American and lives in a VRA county after the year in which the law took effect, and 0 otherwise. We include all race, county, and year fixed effects, and two-way interactions. We also control for observable skills using experience (defined using a worker's reported age), experience-squared, and education. We allow the returns to observable skills to vary by year (γ_t) to account for changes in the wage structure during the second half of the 20th century (Katz and Autor 1999).

The parameter of interest is β, which captures the impact of the VRA on the wage gap between black and white workers (conditional on education and experience). If minority political rights improve economic welfare, one would predict minority relative wages to rise (or equivalently, minority wage *disparities* to be reduced) in areas where minority voting rights are protected under the VRA. As such, we expect $\beta_1 > 0$. Voting rights protection should be associated with reductions in the wage advantage that white workers have in relation to black workers.

Our identifying assumption is that blacks' relative labor market performance would have evolved similarly in VRA and non-VRA counties after 1965 (1975) in the absence of the VRA (VRA Amendments). A potential threat to identification is any omitted factor correlated with the passage of the VRA that affects our outcome of interest. As it is difficult to completely rule out this concern in an observational setting, we also report specifications that include interactions of county-level variables measured before the enactment of the law and that are plausibly correlated with its passage. County-year fixed effects (μ_{ct}) make our estimates robust to unobserved county labor market shocks that occur over time. County-race fixed effects ($\mu_{cr(i)}$) make the estimate robust to county-specific race-specific differences that remain constant over time. County pair-year-race fixed effects allow us to control for localized, time-varying spatial heterogeneity in relative outcome trends. Variation within county pairs with different VRA protection statuses identifies the effect of the VRA on economic outcome gaps. As previously discussed, the benefit of our regression framework is that it controls for non-institutional factors that are unlikely to vary discretely at jurisdictional borders, and which may affect the racial wage gap – such as culture or prejudicial attitudes (e.g., sources of *de facto* discrimination against black Americans). Such factors, however, may vary substantially between entire states, or across counties that are far-flung from one another (Naidu 2012). For our estimates to to be biased, there must be a trend or an event at the time that the VRA takes effect in a county that affects black and white workers differently, and this pattern must not be consistent across neighboring counties.[31] VRA coverage applied to counties within part or all of 21 states. We thus cluster at the county level for

[30] Hourly wages are constructed from DEC data on wage income earned last year, weeks worked last year, and average hours worked per week.

[31] As reviewed in Section 1.2, we are unaware of other policies that occurred only in VRA-affected states and counties that affected black and white workers differentially at the time of adoption/expansion.

inference.[32]

We can indirectly assess the identifying assumption in a few ways. First, following Hornbeck and Naidu (2014) we examine predifference between VRA and non-VRA counties. We estimate how trends in various economic, demographic, and social characteristics before the VRA relate to future coverage. The regression is as follows:

$$\mathrm{Y}_{ct} - \mathrm{Y}_{c(t-a)} = \beta \mathrm{VRA}_c + \epsilon_c \tag{1.2}$$

Table 1 demonstrates that, in terms of many different economic, social, and demographic characteristics, there is no significant difference in county trends. This is true both with and without state fixed effects (Columns 1 and 2, respectively). When we examine the interior and border counties pooled, we find that the trend differences often *are* significant. Additionally, in Section 1.5 below, we use an event-study to suggest that there was little change in the wage gap in VRA vs. non-VRA counties in the years leading up to coverage taking effect. Unfortunately, because most counties became treated in 1965, and the RDC DEC data extends only until 1950, our pre-period is short. We can provide more compelling evidence of the common trends assumption when examining the effect of the VRA on the political mobilization of black American voters, which is measured with greater frequency.

As controls, we include several county-level characteristics that may affect labor market outcomes. We generally focus on factors that are measured prior to adoption of the VRA (typically using 1960 data – prior to the VRA passage – so measures are not affected by treatment). These factors include demographics (black population, mortality rates, and literacy rates), as well as average cultural/political measures (historical presence of lynching or the fraction of the population that votes Republican).[33] County characteristics are interacted with linear and quadratic time trends.

1.5 Results

1.5.1 How Did the VRA Change Politics?

The link between the VRA and downstream improvements in minority socioeconomic outcomes likely depended on how it made government accountable to black voters' interests in economic opportunity. We thus begin by briefly assessing the impact of political incorporation on black political participation (a plausible first-stage effect the VRA).[34] To demonstrate that the VRA mobilized black voters as intended, we use voter turnout for presidential elections as our outcome, as is standard in this literature (Ang 2018).[35] The econometric specification is the following differences-in-differences (DD) analogue of our main specification:

$$\text{Political Outcome}_{ct} = \alpha \, \mathrm{VRA}_{ct} + \mu_c + \mu_{p(c)t} + \epsilon_{cp(c)t} \tag{1.3}$$

[32]For robustness, we also cluster our standard errors at the state level.

[33]Data on county characteristics comes from the Decennial Census as well as the City and County Data Books, which are themselves typically based on official Census statistics.

[34]These effects are consistent with results in political science, including from Fresh (2018).

[35]We also return to a discussion of how the VRA improved accountability in Section 1.7.

where c indexes county, t indexes year, $p(c)$ indexes county pairs. α provides an unbiased estimate of the VRA's causal effect of VRA coverage on voter turnout, under the assumptions that there are no time-varying differences across covered and uncovered counties, and that there are no geographic spillovers between counties. Spillovers are a concern if black voters chose to move to counties covered by the VRA.[36]

Table 2 presents results in Panel A. Consistent with the VRA mobilizing a new block of eligible voters, we find sizable, statistically significant increases in eligible voter turnout resulting from the extension and protection of minority voting rights under the VRA. The effects of the VRA on political participation are robust to several control variables (Column (2)), and the inclusion of state trends (Column 3). We find statistically significant increases in voter turnout ranging from 6 to 12 percentage points (p.p.) between 1950 and 1980; estimates are significant at the 1 percent level. We also find that the effect of the VRA is increasing in black population share (Column (4)). The coefficient on $VRA \times Black$ is positive and significant.[37] This specification suggests that the VRA increased participation at least in part through the mobilization of black voters, since these counties were more heavily affected by the VRA. In Appendix Table A.2, we demonstrate that results are unchanged when examining all counties within the states comprising our sample (not only the subset of county pairs).

In Figure 5, we present the results graphically, displaying estimates of event-time dummies interacted with both the fraction of the population that is black, and a dichotomous indicator for counties that became VRA-covered. The graph provides visual support of a first-stage political effect, as well as support for the parallel trends assumption. The estimates are relatively stable in the years before the VRA takes effect (turnout is slightly worse in VRA counties – although the trend is relatively flat).[38] Post-VRA, covered counties experience a large, persistent increase in voter turnout relative to uncovered counties.

The conceptual mechanisms we discuss in Section 1.3 above rely on government action by those accountable to minority voters. As such, we also consider whether elected officials responded to minority political mobilization by supporting the preferred policies of black constituents. Because historical data on local/state policymaking is limited, we examine whether the VRA changed the behavior of Congressional representatives, using the widely-known DW-Nominate score. This measure of Congressional behavior collapses a representative's legislative roll-call voting record into a time-varying measure of ideology, scaled from -1 to 1 (increasing in "conservativeness").[39] We focus on the second-dimension of the DW-Nominate score, which captures conservative ideology on issues related to race and civil rights. We estimate a district-level analogue of Table 1.3, defining VRA districts as any

[36]We demonstrate that migration is not a major concern since it is small quantitatively, and so is unlikely to account for measured effects on political and economics outcomes. See Section 1.5.3).

[37]Cascio and Washington (2014) use this specification to show how the removal of literacy tests from Southern states affected participation and redistribution.

[38]We note, though, that there does seem to also be an increase in turnout in the one period before the VRA takes effect. We believe that this is consistent with increased social activism during the peak period of the Civil Rights movement, when organizations such as the National Association for the Advancement of Colored People (NAACP), CORE, and Southern Christian Leadership Conference (SCLC) were actively engaged in voter registration drives during the early 1960s.

[39]See, for example, Poole and Rosenthal (2001) or Gentzkow, Shapiro, and Taddy (2016) for further descriptions of this data.

district that contains at least one VRA-covered county, consistent with standard practice in this literature (Ang 2018). Results are presented in Panel B Table 2. Both with and without controls (Columns 1 and 2, respectively), the VRA coefficient estimates are negative and significant at the 10 percent level, indicating that the VRA made Congressional districts more racially liberal by 6-8 percentage points (Columns 1 and 2, which include and exclude controls, respectively). We also find that the effect of the VRA on racial conservativeness is decreasing in minority population, although the interaction between VRA coverage and the fraction of a district that is black is not statistically significant.

1.5.2 Main Results

How did the VRA affect Economic Inequality?

Knowing that the VRA achieved its initial purpose of minority political incorporation, we turn to testing the main hypothesis – did political empowerment produce tangible economic gains for poor black Southerners? As Figure 1 shows, eliminating labor market discrimination was *the* most important policy issue for black Americans in the 1960s. It is therefore reasonable to hypothesize that meaningful political power in the hands of black Americans would be used to address this problem.

We begin the discussion of our main findings by examining the effect of the VRA on labor market performance using a simple DD framework. Figure 6 presents visual estimates of the impact of the VRA on wages for both black and white American workers, separately.[40] Wage trends in the decade before a county becomes covered are similar for both black and white worker, supporting the identifying assumption that outcomes between treatment and control groups would have evolved similarly in the absence of treatment. After the VRA takes effect, we observe a mean increase in the wages of black Americans (close to 5 p.p., significant at the 5 percent level), as well as a modest *reduction* in white wages of approximately 1 p.p. Given that black workers earned about 60 percent of what similarly skilled white workers earned in terms of wages, Figure 6 suggests that the VRA did indeed improve racial labor market equality. Table 4 shows that these results are stable to multiple potential specifications.

For the remainder of the paper, we focus on the main estimating equation, Equation 1.1. Table 5 presents our main results on black Americans' relative wages under several model specifications. Recall that the coefficient β on VRA $\times$ Black indicates to us the impact of the VRA on black wages relative to white wages – so an increase in black wages in this model is also indicative of a reduction in the racial wage *gap*. Across all specifications, the results suggest that the VRA caused a statistically significant improvement in black Americans' relative labor market status. Column 1 presents our baseline estimates – using only individual worker characteristics (with returns to human capital varying by year), and the full set of fixed effects. The regression estimate indicates that the VRA caused a 5.5 p.p. increase in black Americans' wages between 1950 and 1980, relative to white workers with the same characteristics and within the same geographic labor market. This impact is significant at the 5 percent level. Moreover, the increase in black relative wages is not part of an overall *decline* in wages within VRA counties, as Figure 6 shows. The effects are

[40] A limitation of using the administrative Census micro data is that we are limited to the censuses only beginning in 1950.

driven primarily by an *increase* in black wages. For American workers, VRA coverage led to 4.6% increase in wages (p<0.05), which is statistically significantly larger than the more modest negative effect on whites (p<0.03). Columns (2) through (5) show that this effect is robust across different specifications. Column (2) re-estimates the baseline model with the inclusion of several county-level controls. Because some of these controls could themselves be outcomes of the VRA (e.g., share of county population population that is non-white), we fix all controls at their pre-VRA (1960) levels, and interact the variables with linear, quadratic, and cubic time trends. The results are similar in size and significance – the VRA increased black wages by 5.8 p.p., relative to white wages (significant at the 5 percent level). Columns (3) and (4) add state and county trends respectively – and the results again confirm a statistically significant increase in relative black wages of about 5.6 p.p. Overall, these results provide evidence that the expansion and protection of black political rights had a beneficial effect in terms of promoting socioeconomic equality.

The magnitude of our estimated effect appears reasonable compared against the existing literature on drivers of racial wage and earnings gaps. Wage ratios within our sample (conditional on worker characteristics) increased from around 55 percent to just above 80 percent between 1960 and 1980. Our estimates account for around one-fifth of the decline in the adjusted wage gap. This effect is only within the South (the contribution would likely be smaller if we to considered nationwide wage convergence). By contrast, Card and Krueger (1992) find that about 15-20 percent of the nationwide reduction in the racial wage gap owes to improvements in school quality for black American schoolchildren. Donohue and Heckman (1991) find that declining labor force participation due to President Johnson's War on Poverty accounted for around 10-20 percent of black-white wage convergence during this period. Finally, another recent study by Derenoncourt and Montialoux (2018) find that the 1966 extension of the minimum wage via amendments to the Fair Labor Standards Act can explain more than 20 percent of the reduction in the racial earnings gap. Importantly, minority political power may have either contributed to or been complementary to *any* of these other channels. We find support for such complementarities, as we discuss below in Section 1.6.2.

Examining effects year by year using an event-study design allows to explore the linearity of treatment effects – useful for thinking about mechanisms (a point we will return to later) (Kose, Kuka, and Shenav 2019).[41] Figure 7 presents the visual display of these estimates. We observe little change in relative wages in the 10-year window prior to a county becoming VRA-covered – suggesting that outcomes in covered and uncovered were not following different trends prior to VRA coverage. Note, however, that the effects of the VRA emerge relatively soon after coverage takes effect, with treated counties experiencing a 5 percent

[41] In particular, we estimate:

$$\log(Y_{ict}) = \sum_{t=-2}^{2} \mu_t \times [\text{VRA}_c \times \mathbb{I}\{r(i) = \text{Black}\}] + \mathbf{x}_{ict}\boldsymbol{\gamma} + \mu_{cr(i)} + \mu_{ct} + \mu_{p(c)r(i)t} + \epsilon_{icp(c)r(i)t} \tag{1.4}$$

where c, t, Y_{ict} reference county, Census year, and the same dependent variables as before. The parameters of interest are the four μ's that we estimate. They separately test for mean shifts in individual economic outcomes post-VRA, after adjusting for pre-existing trends. Five years before the VRA takes effect in a county is the reference year.

reduction in the wage gap by five years post-coverage. The rapid improvements we observe are consistent with work by Donohue and Heckman (1991) and Card and Krueger (1992), who suggest that civil rights legislation (including the VRA) produced immediate benefits for black Americans. That the observed effect appears relatively soon after VRA coverage suggests that our results are not due to investments by government in human capital-building institutions, such as schools or other changes that would be observed in future labor market cohorts.

In Appendix Table A.7, we estimate several other modifications to the core specification that demonstrate the robustness of the core finding. First, to address the possibility that returns to education differed dramatically between Southern states, as suggested by Card and Krueger (1992), we allow for different different returns to human capital (education and experience) by state, as well as by race. In the same vein, we also allow the returns to human capital to vary by both race and geography. We also estimate the specification applying a different functional form of the control variables. Across all of these specifications, our results indicate that the VRA caused a statistically significant increase in the relative labor market performance of black men. Finally, we estimate Equation 1.1 controlling only for county-by-race fixed effects, to show the effects are not present because our empirical model is fully saturated. In this case, the effects are even stronger.

Price or Quantity Changes? Extensive-margin Effects of the VRA

Finally, we consider how civil rights policy may effect the extensive margin of employment, and how this may complicate the interpretation of our main finding. In particular, numerous studies have suggested that a variety of institutional changes (criminal justice policy, welfare policies, etc.) may have led to selective withdrawal from the labor force (especially of black workers) (Chandra 2003). One reason we are not overly concerned about this possibility is that for the time period we examine (the 1960/70s), schooling was essentially uncorrelated with men's labor force participation. Nevertheless, given the possibility of changing labor force participation confounding the interpretation of our wage results, we test for the relationship between the VRA and various extensive-margin measures of labor market activity across the entire sample of age-eligible black and white males. We reestimate our primary specification for the following outcomes: (1) whether a worker had positive earnings, (2) whether a worker was not in the labor force, (3) whether a worker had been unemployed in the previous year, (4) number of weeks worked last year, (5) usual hours worked, and (6) whether a worker was employed full-time, full-year.

Table 8 suggests that the VRA had little effect on the relative labor supply of black workers in our sample. Across several extensive-margin outcomes, we detect modest effects that are typically insignificant statistically. We observe a modest increase in the likelihood of positive earnings, and precisely-estimated zero effects on both the relative likelihood exiting the labor force and being unemployed . We also observe very modest reductions in time spent at work (around 1 week and 0.5 hours per week reductions) – both of which are statistically insignificant.

1.5.3 Robustness

In this subsection, we probe the strength of our main results.

1975 VRA Amendments & Within-NC Variation

One threat to our identification strategy is the existence of other institutional/policy changes that vary discretely at county or state borders, and that coincide with the time and geography of VRA coverage. For example, in 1964, Congress passed the Civil Rights Act (CRA), which today remains one of the major laws outlawing discrimination in hiring or pay.[42] Unlike Section 5 of the VRA, which applied to only a targeted set of jurisdictions, the CRA applied nationwide – so our empirical strategy should, in principle, account for nationwide policy shocks. Nevertheless, it is possible that the CRA affected black Americans relatively more in covered counties, given that the VRA targeted the "worst-of-the-worst" in terms of existing discrimination.

We alleviate this concern by analyzing different subsamples of the data according to the timing of VRA coverage. Amendments to the VRA in 1975 extended Section 5 coverage protections to 283 additional counties. These counties were primarily in Texas and Arizona, but also extended to counties in New Mexico and Oklahoma, among other states.[43] We use the VRA Amendments to split the sample into DEC respondents within 1965 and 1975 VRA-affected counties (and the respective neighbor counties).[44].

In Table 6, we estimate our preferred specification separately for the 1965 and 1975 coverage rounds. Column (1) provides our benchmark estimate from 5. Columns (2) and (3) present estimates for the effect of VRA coverage limiting to subsamples affected by VRA coverage in 1965 and 1975, respectively. The evidence from both panels suggest that the impact of the VRA on black relative wages is likely not a heterogeneous effect of Title VII, nor is the effect purely an artifact of different trends in the outcome of interest. The measured effect of the VRA on black relative wages for the 1965-covered counties is 7 p.p. ($p<0.05$), and for the 1965-covered counties is 4.5 p.p. ($p<0.1$) for the 1975-covered counties.

Finally, we also probe the robustness of our main finding by limiting analysis to the subsample of DEC respondents who resided in one state in which there was substantial VRA coverage heterogeneity: North Carolina. To this point, much of our main sample consists of workers within matched county-pairs that span state boundaries. State-level policy changes affect labor markets and may confound our estimated treatment effect. Using the relatively even variation in VRA coverage that existed within North Carolina (41 of its 100 counties were covered), we can allay concerns about unobserved institutional changes at the state

[42]Two provisions of the CRA were pertinent in our setting. Title II of the CRA was the provision that outlawed discrimination within places of public accommodation. Title VII of the CRA was the provision outlawing discrimination by employers.

[43]The expansion covered three of the four Census regions. Because we examine black-white economic inequality in this study, we focus on VRA coverage in the South. This focus means that we exclude from analysis Alaska and South Dakota, where the VRA was targeting voting discrimination toward Native Americans (these states had very small black populations). Moreover, we also exclude political jurisdictions in New Hampshire, Michigan, and New York, due to small black samples. Arizona and Texas alone account for over 50 percent of the additional coverage.

[44]The set of states that contain control counties beginning in 1975 consists of California, Oklahoma, New Mexico, Nevada, and Utah.

level.[45] We thus focus on North Carolina (N.C.) as a single-state case study of the impact of the VRA on black labor market outcomes.[46] Column (4) of Table 6 presents the result for this subsample. The estimates for the within–North Carolina subsample are similar to the overall results, and in fact, are even larger in magnitude. We observe that the VRA increased black relative wages by around 11 p.p. within North Carolina ($p<0.01$). In conjunction with the tests using the 1975 Amendments, these results corroborate our main finding.

Further Ruling Out Confounders

One concern for our analysis is that voter protection may change the composition of the population in covered counties, leading to changes in measured labor market performance. For example, to the extent that the ability to participate in local politics is a locational amenity, black families may have moved differentially into counties with protected voting rights (in turn changing the composition of public goods and targeted redistribution from which black households would benefit). Indeed, the *out*-migration of black Americans followed political *disenfranchisement* during the era of Jim Crow, as documented by Naidu (2012) and Margo (1980).[47] If changes in wages reflect that higher-status black families are migrating to covered counties (e.g., Banzhaf and Walsh (2008)), then we may observe changes in the underlying population characteristics of covered counties post-VRA. Changes such as these would imply that positive earnings impacts may be in part driven by changes in the types of individuals working in covered counties, rather than direct action of government due to improved political influence.

Table 7 presents results from our test of whether the VRA led to a compositional shift in the underlying population characteristics of VRA-covered counties, either due to migration or some other channel (Isen, Rossin-Slatar, and Walker 2017). Each column presents an estimate from a regression relating an indicator for county VRA status to a different dependent variable. We test for the VRA's effect on average levels of the following characteristics between 1960 and 1980: (1) education, (2) years of work experience, (3) black fraction of population, and (4) a summary earnings index that uses the predicted values from a standard Mincerian regression. The effects of the VRA on various measures of county composition are small in magnitude and statistically insignificant, suggesting that the VRA's effect on wage equality is not the product of compositional changes.

A similar concern related to our empirical strategy is the possibility of spillover effects between VRA and non-VRA counties that may bias our estimate in either direction. If labor markets are integrated across pairs of counties, labor prices may equilibrate – leading to an underestimate of any treatment effect. Alternatively, the positive selection of black

[45] Covered N.C. counties were selected for protection based on their 1964 voter turnout rates. Those with turnout below 50 percent became covered by Section 5. We view this threshold as unlikely to have been chosen with the coverage of any particular county in mind.

[46] In a study conducted concurrently with ours, Fresh (2018) documents voter turnout effects that are similar in sign and magnitude to our results on political participation.

[47] The direction of any migration-induced effect of the VRA is theoretically ambiguous. If in-migrants were substitutable with native black workers, the increase in supply would *dampen* our estimated effect of the VRA. On the contrary, if there was positive selection into migration (Boustan and Margo 2009), we may overestimate the effect of the VRA by analyzing black workers who positively select into VRA counties. Out-migration of whites ("white flight") may also exaggerate the magnitude of our finding (Boustan 2010).

workers who migrate or commute in response may lead us to overestimate the effect of treatment. This latter possibility would be consistent with an "unintegrated labor market," where commuting is possible.

We find no evidence for possible cross-border spillover bias. To demonstrate this, we examine whether the effects of the VRA are similar for matched-pair and "interior" counties. Interior counties refer to all counties within a given state that are contained (partially or fully) within our matched-pair sample, excluding the border counties – in other words, all counties that are interior to the matched-pair counties. Using both the primary and interior samples, we estimate the following spatial-differenced specification used in (Dube, Lester, and Reich 2010):

$$\log(Y_{ict}) - \log(\overline{Y_{ct}}) = \alpha + \beta(\text{VRA}_{ct} \times \text{Black}_{ict}) + (\mathbf{x}_{ict} - \overline{\mathbf{x}_{ct}})\boldsymbol{\gamma} + \mu_{cr(i)} + \mu_{ct} + \mu_{r(i)t} + \epsilon_{icr(i)t} \quad (1.5)$$

In essence, β indicates the degree to which effects for border and interior counties are different. Results based on Equation 1.5 are presented in Table 9. The coefficient of interest, VRA × Black, is presented in Column (1) and is small in magnitude (roughly 1 p.p., statistically significant at the 5 percent level). To the extent that there is amplification of our primary estimate of interest, it is relatively small. For reference, columns (2) and (3) provide estimates for the VRA wage gap effect, which is β from an analogue of Equation 1.1 without pair fixed effects; a casual glance suggests that the effects are similar for both the interior and border county samples. Robustness checks for these results are presented in Table A.8.

Finally, in Appendix Table A.9, we also test explicitly whether there is differential migration within the matched-pair sample directly using DEC data on a person's place of residence five years earlier. The regression is similar to estimating Equation 1.3, but using as an outcome an indicator variable for whether a person left a covered jurisdiction for a non-covered jurisdiction (or vice versa). As Column (1) shows, net out-migration is actually declining in treated counties (meaning the labor supply would be higher in VRA counties – likely biasing any VRA affect toward zero). In Column (2), we estimate the same specification, but include flexible controls for education and experience, in case migration is positively selected; the results are unchanged. In Columns (3) and (4), we examine whether there are heterogeneous effects by race. While the net flow of black workers appears to be negative in VRA counties relative to white workers, black workers in VRA counties are still less likely to move overall the sum of VRA and VRA × Black. Given that such movement is not driven by positive selection (Column 4), we believe it most likely that immigration of black workers would lead us to underestimate our effects.

1.6 Mechanisms: How Black Political Power Affected the Labor Market

We interpret the results in Section 1.5 as the reduced-form effect of black Americans' political representation on economic status. This intervention reduced wage inequality by nearly 6 p.p. In this section, we examine mechanisms through which minority political power operates to improve labor market outcomes.

1.6.1 Public Sector Employment

We first focus on a direct mechanism: the hiring of black workers within government. The VRA was signed into law against the backdrop of a rapid expansion of government size nationwide during the second half of the 20th century (Figure 8) (Berry, Grogger, and West 2015). This seemingly secular growth of government opened up new job vacancies that were especially valuable to minority workers. Public sector employers provided better access to high-paying managerial and professional jobs than what was available to black workers in the private sector, where employment segregation and discrimination were present (Frazier 1957; Hout 1984; Katz, Stern, and Fader 2005; Laird 2017).[48] To illustrate the value of public sector employment, in Table 10 we present estimates of the 1960 public sector wage premium for both black and white workers within our sample. These results, based on estimating a simple OLS regression of log wages on a public sector dummy, demonstrate that black workers in the public sector earned substantially more than their private-sector counterparts, conditional on worker traits. Black government workers in the South earned 20 percent more than their private sector counterparts ($p < 0.01$). The premium is drastically lower, however, for white workers.

Given these relative benefits of working in the public sector, one way that the state could respond to black economic disadvantage was through government hiring. We thus show the change in black political power facilitated with the passage of the VRA contributed to the increased presence of minority workers within the public sector. Our findings are consistent with studies by Alesina, Baqir, and Easterly (2000) and Enikolopov (2014), which demonstrate how political considerations often influence public sector hiring practices. Minority political strength (as proxied by both minority candidates and voting strength) has long been considered an important determinant of minorities' representation within the bureaucracy (Eisinger 1982; Nye, Rainer, and Strat 2015). Moreover, the secular growth of government opened up new job vacancies for minorities without the need to displace current white workers (Krislov 1967). Public sector employment contributed to improvements in black Americans' labor market outcomes directly (by offering those employees better-paying jobs) and indirectly (by improving the outside option for *other* black workers). We demonstrate both channels.[49]

[48] During the 1960s and 1970s, the proportion of black manager-level workers within government increased roughly sixty-seven percent, compared to an increase of only fifteen percent in the proportion of white managers (Collins 1983). This labor market advancement was due to both state intervention that increased recruitment of black workers as well as more opportunities in higher-ranking professional and managerial positions than existed for minorities in the private sector. State agencies were viewed as enforcers of nondiscrimination – unsurprisingly, given the ability of voters to punish discriminatory government agencies (King).

[49] In Appendix A.2, we conceptualize in more detail the process through which a positive change in minority political power increases minority public-sector hiring, as well as how this increase in labor demand affects overall black wages, including within the private sector. This conceptual and empirically-validated account of minority politics and increased public employment is consistent with historical evidence (Wright 2013). Maynard Jackson, for example, became Atlanta's first black mayor in 1972 on the promise of hiring and promoting minority workers to positions of importance within local government (as well as with the promise of government contracts based on minority hiring). However, the value of public-sector employment to black Americans in the South extends back even further. For example, one of Martin Luther King's central policy goals during the Alabama-based Birmingham Campaign of 1963 was to pressure local governments to

Impact of the VRA on Public-Sector Hiring

To examine whether minority political power achieved under the VRA increased the number of black Americans employed by government, we use the DEC "Class of Worker" variable. This variable categorizes people according to the type of ownership of the employing organization, and thus identifies workers who are employed by government.[50] To analyze whether the VRA increased the relative likelihood of a black worker being employed within the public sector, we modify our primary specification and estimate the following linear probability model:

$$\mathbb{I}(\text{Public Employee} = 1)_{ict} = \beta_0 + \beta_1(\text{VRA}_{ct} \times \text{Black}_{ict}) + \mu_{cr(i)} + \mu_{ct} + \mu_{p(c)t} + \epsilon_{icr(i)p(c)t} \quad (1.6)$$

The dependent variable is an indicator for whether an individual is employed in the public sector. β now indicates how the VRA changes the likelihood that black workers will be employed by the public sector, relative to white workers (the coefficient of interest is similar to Equation 1.1 above). Results are presented in Table 11, and suggest that a greater reallocation of public sector jobs from whites to blacks took place in VRA-protected areas. In our preferred specification (Column (1)), we find that the VRA increased the likelihood of a black worker being employed by government by 3.8 p.p. (significant at the 1 percent level). Columns (2)-(5) demonstrate that this effect is stable to the inclusion of state trends and worker controls, and also that the results hold for the cross-border and NC-only samples. Overall, our analysis suggests that the VRA increased the likelihood that blacks would receive a public sector job by between 2 and 4 percent.[51] We can also rule out that black workers' increased public sector presence is due to differential growth of government across VRA and non-VRA counties. Appendix Table A.12 uses data on total public-sector labor force (at the county level) from the U.S. Census of Governments, and shows that public sector growth by VRA coverage status is likely not a confounding factor for this result. OLS regression estimates show only a small, statistically insignificant correlation between VRA coverage and public-sector size.

Building political pressure to enforce equal opportunity in the public sector may have also led to better pay in these jobs–either through promotions or reductions in discrimination within jobs. We test for this in the Appendix, modifying our wage regression to account for heterogenous effects of the VRA on public and private-sector workers.[52] Results for this test

hire black workers (Jackson 2007).

[50]Unfortunately, we cannot distinguish between federal, state, and local public workers prior to 1970. We thus group together all workers employed by a government agency at any level.

[51]In the Appendix, we provide a number of robustness checks. For instance, Table A.11, shows that in absolute terms, black workers were also more likely to be employed in government.

[52]Specifically, we estimate the following specification:

$$\begin{aligned}\log(W_{ict}) = \beta_0 + \beta_1(\text{VRA}_{ct} \times \text{Black}_{ict}) + \beta_2(\text{VRA}_{ct} \times \text{Black}_{ict} \times \text{Public}_{ict}) + \\ \beta_3(\text{VRA}_{ct} \times \text{Public}_{ict}) + \beta_4(\text{VRA}_{ct} \times \text{Public}_{ict}) + \beta_4\text{Public}_{ict} + \\ + \mathbf{X}_{ict}\boldsymbol{\gamma} + \mu_{cr(i)} + \mu_{ct} + \mu_{p(c)r(i)t} + \epsilon_{icr(i)p(c)t} \quad (1.7)\end{aligned}$$

where $Public_{ict}$ denotes public sector worker status. We are interested in understanding the overall effect of VRA on the public wage gap which we obtain by adding the overall reduction in the wage gap plus

of heterogeneity are presented in Table A.13. Summing up the coefficients on VRA × Black is 0.02, suggesting that VRA coverage reduced the wage gap by around two percentage points, less than the private sector wage gap. These estimates are consistent with anecdotal evidence of declining racial disparities even *within* the public sector.

Minority Public Sector Hiring and Overall Minority Wages

We have thus far shown that the VRA (1) improved the overall labor market status of black workers in terms of relative wages, (2) improved the likelihood that a black worker would be employed in the public sector (which commanded a higher wage than working in the private sector), and (3) improved wages within the public sector for black workers. A given local labor market's public and private sectors do not function in isolation from one another. To the extent that the VRA created a positive "demand shock" for black workers in the public sector, one might expect some degree of upward pressure on the private wages of black workers. As such, in this subsection we establish the connection between improved performance of black workers in the public sector and improved economy-wide wages. We establish this connection in two steps. We first provide *prima facie* evidence of a relationship between the public sector and the private sector by focusing on occupations that experienced higher and lower rates of national growth from 1960 to 1980. We then calculate the general equilibrium effects that arise from an increased bargaining position of black workers in the private sector because of improvement in the outside option of working for the government.

We first provide reduced-form evidence that the VRA's effect on minority public-sector employment did put upward pressure on wages in the private sector (as one might expect when a local economy is hit with a positive sector-specific labor demand shock). Ideally, we would use exogenous changes in local public-sector labor demand across VRA and non-VRA counties to examine how the magnitudes of our main findings changes. Lacking this type of variation, we instead leverage inter-occupation heterogeneity in the exposure of private-sector employers to competition from government for the labor supplied by black workers. Specifically, we test whether black workers in occupations that experience greater public-sector growth (a proxy for increased public labor demand) over the sample time period (1960-1980) also observe differentially greater wage gains.[53] The intuition for this test of heterogeneity is that the public-sector channel of private-sector wage improvement will be strongest in occupations where there are more governmental job vacancies (that can be reallocated to minority workers). These are the jobs where private firms face the largest increase in competition for black workers.

We create the proxy for (cross-occupational) public-sector demand by sorting occupations into quartiles of national public-sector employment growth between 1960 to 1980 (the fraction of workers who work in the public sector within a given occupation).[54] Logistically,

---

the differential effect on public workers, i.e., $\beta_1 + \beta_2$. The sum of these two coefficients tells us how much the black-white wage gap went down (black relative wages increased) for public employees, in VRA-covered counties relative to uncovered counties.

[53]Not all occupational categories are populated across years. We restrict our sample to workers in occupations present in 1960, 1970, and 1980. VRA does not predict selection into this subsample.

[54]We sort occupations using the Census 1950 occupational classification system.

we first define public sector growth by occupation as follows:

$$\Delta PubSec_{o,-i} = \%PublicSector_{o80,-i} - \%PublicSector_{o60,-i} \tag{1.8}$$

which denotes the change in relative change in demand for public sector workers within occupation o. We construct these measures at the national level. We then split all occupations within our sample into quartiles, giving us $\Delta\text{PubSec}_{cq,-i}$, which we relabel as $\Delta\text{PubEmp}_{cqi}$ for simplicity. We then use these measures in a modification to our primary specification (Equation 1.1) in which we examine heterogeneous effects by exposure to increased public sector demand.[55]

If the public sector is a channel that contributes to *overall* improvement in black workers' wages, one would predict greater convergence in the top quartile of public-sector growth. As Table 12 shows, that is indeed the case. β_3 (the coefficient on $VRA_{ct} \times Black_{ict} \times \Delta PubEmp_{cq4}$) is positive, indicating that black workers in the private sector experience the greater wage gains in jobs that face the most competition from public sector agencies due to increasing demand. While we do not interpret that magnitude of coefficient, it is significant at the 5 percent level, and we interpret the sign to suggest substantial spillovers from public sector hiring gains to the private sector in those occupations that are most heavily affected. For robustness, we repeat our estimation of Equation 1.9, but interacting VRA × Black with a continuous measure of sector growth by occupation, rather than using quantiles. The effects are similar.

Having highlighted how the public sector can raise minority income in both the public and private sectors, we decompose how much of our average treatment effect on relative wages is explained by a public sector channel. In other words, what is the contribution of greater labor demand and better compensation for minority workers in the public sector on the private sector wage gap reduction? As we previewed in Section 1.3, the impact of the VRA on labor market outcomes will consist of more than the mechanical effect of having a higher-paying government job. Beaudry, Green, and Sand (2012) demonstrate that accounting only for direct effects may underestimate the true effect of changes in the public

[55]Specifically, we estimate the following specification for heterogeneous treatment effects by quartile of occupation-specific public sector growth:

$$\begin{aligned}\log(\text{Y}_{ict}) = \beta_0 + \beta_1(\text{VRA}_{ct} \times \text{Black}_{ict}) + \beta_2(\text{VRA}_{ct} \times \text{Black}_{ict} \times \Delta\text{PubEmp}_{cq1}) \\ +\beta_3(\text{VRA}_{ct} \times \text{Black}_{ict} \times \Delta\text{PubEmp}_{cq4}) + \beta_4(\text{VRA}_{ct} \times \Delta\text{PubEmp}_{cq1}) + \beta_5(\text{VRA}_{ct} \times \Delta\text{PubEmp}_{cq4}) \\ +\beta_6(\text{Black}_{ict} \times \Delta\text{PubEmp}_{cq1}) + \beta_7(\text{Black}_{ct} \times \Delta\text{PubEmp}_{cq4}) \\ + \beta_8\Delta\text{PubEmp}_{cq1} + \beta_9\Delta\text{PubEmp}_{cq4} + \mathbf{x}_{ict}\boldsymbol{\gamma} + \mu_{cr(i)} + \mu_{ct} + \mu_{p(c)r(i)t} + \epsilon_{icp(c)r(i)t}\end{aligned} \tag{1.9}$$

The identifying assumption in this test for heterogeneous effects is that factors contributing to the decrease in the wage gap in VRA counties at the border are orthogonal to growing public sector demand for certain occupations. That is, there is no factor that simultaneously: (i) differentially affects blacks relative to whites, (ii) differentially affects VRA counties at the border, (iii) has differential effects over time similar to the VRA, (iv) affects occupations with high national public-sector demand growth, and (v) operates at a scale large enough to exert pressure globally. This means, for example, that the increased national public sector demand for clerical workers was not related to decreases in the public sector wage gap in VRA counties at the border following the passage of the regulation through other channels different from the joint effects of public sector changes in occupational demand and the VRA. To the extent such factors might exist, we provide robustness estimates using different controls and fixed effects, with no significant changes in our estimates.

sector due to the VRA – due to the existence of spillover effects to the private sector. To account for these effects, we consider a labor market model with public employment à la Mortensen-Pissarides, described in detail in Appendix A.2.

Using this framework, we estimate the public sector channel in two steps: (i) we estimate the component of the private sector wage that in equilibrium arises from changes in public sector hiring practices; and (ii) we estimate the effect on wages that is due to the VRA, as we have done in previous sections. In the first step, the estimating equation we use is given by Equation (1.10), which is a rewritten version of Equation (A.3), for blacks and whites independently, after approximating the weights in the government-driven component of the wage using second-order Taylor expansions:

$$\begin{aligned}\log(Y_{ict})^{Private} &= \alpha_0 + \alpha_1 Black_{ict} + \alpha_2 \overline{PubEmp}_{ct,black} + \alpha_3 \overline{PubEmp}_{ct,white} \\ &+\alpha_4 \overline{PubEmp}^2_{ct,black} + \alpha_5 \overline{PubEmp}^2_{ct,white} + \alpha_6 \overline{PubEmp}_{ct,black} \times \overline{\log(W_{ct,black})}^{Public} \\ &+\alpha_7 \overline{PubEmp}_{ct,white} \times \overline{\log(W_{ct,white})}^{Public} + \alpha_8 \overline{PubEmp}^2_{ct,black} \times \overline{\log(W_{ct,black})}^{Public} \\ &+\alpha_9 \overline{PubEmp}^2_{ct,white} \times \overline{\log(W_{ct,white})}^{Public} + \alpha_{cont} X_{ict} + \epsilon_{i,c,p(c),t}\end{aligned} \tag{1.10}$$

where $\overline{PubEmp}$ is the proportion of public employees and $\overline{\log(W)}^{Public}$ is the average public sector wage net of Mincerian traits, both per county, year, and race. We will refer to our fitted values $\widehat{\log(W_{i,c,t})}^{Private}$ as the general equilibrium component of private sector wages. In our second step, we estimate the causal effect of the VRA on the general equilibrium component of private sector wages:

$$\widehat{\log(W_{i,c,t})}^{Private} = \beta_0' + \beta_1'(VRA_{ct} \times Black_{ict}) + \boldsymbol{\gamma'}\mathbf{X}_{ict} + (\delta_c' \times \delta_t') + (\delta_r' \times \delta_c') + (\delta_{p(c)}' \times \delta_r' \times \delta_t') + \epsilon'_{i,c,p(c),t}$$

The contribution of a change in public sector labor practices on the *private* wage gap is given by the following variance decomposition:

$$\frac{Var\big(\beta_1'(VRA_{ct} \times Black_{ict})\big)}{Var\big(\beta_1(VRA_{ct} \times Black_{ict})\big)} \tag{1.11}$$

We find that changes in public sector hiring explain between 29 and 35 percent of the reduction in the private-sector wage gap following the VRA.

1.6.2 Anti-Discrimination and Affirmative Action Regulations

Government jobs typically comprised a minority of a given area's labor force, and as such, it is unlikely that this mechanism could entirely account for our observed treatment effect. Existing research suggests ways in which political power could affect black economic outcomes through *direct* intervention in the private-sector labor market.

Perhaps the most important factors for the abatement of racial wage inequality were anti-discrimination and affirmative action policies (Donohue and Heckman 1991; Chay 1998).

Title VII of the CRA outlawed employer discrimination in pay, hiring, and promotions on the basis of race. Affirmative action policies (AA) – adopted at all levels of government – encouraged (or even required) minority hiring. For example, beginning in 1965 Executive Order 11246 required that federal government contractors maintain AA plans that explicitly outlined a contracting firm's minority and women employment goals. Firms with unacceptable plans were barred from future federal contract bidding. The federal agency responsible for enforcing legal requirements and ensuring workplace equality was (and remains today) the Equal Employment Opportunity Commission (EEOC). The EEOC in the mid-1960s possessed the authority to investigate and negotiate complaints of discrimination by private establishments.

We examine whether the VRA complemented anti-discrimination and affirmative action laws, due to the improved likelihood of enforcement. This prediction is based on research within labor history and sociology suggesting that the implementation of anti-discrimination policy was historically a "politically mediated" process, dependent on political actors (Stainback, Robinson, and Tomaskovic-Devey 2005). From 1966 through the early 1970s, for example, the EEOC investigated nearly 80,000 complaints of employment discrimination, filed in the South primarily by political activist groups such as the NAACP (Minchin 2015).[56]

There exists little granular, systematic data on either affirmative action policies or Title VII enforcement.[57] Our tests for heterogeneous effects are in the spirit of Smith and Welch (1977), who argue that the "implied threat" of government anti-discrimination activity contributed to reduced discrimination within private firms.

To test for the existence of a policy enforcement channel of the VRA, we construct a county-level measure of local workforce exposure to the mandates of federal anti-discrimination policies. We use historical data on the fraction of a county's manufacturing establishments subject to federal monitoring from the 1962 U.S. County Business Patterns. Research by Carrington, McCue, and Pierce (2000) suggests that anti-discrimination and affirmative action policies regulating the labor market were less well-enforced for small employers. This approach uses county-by-industry-by-establishment size variation. We adopt a variant of this approach, modifying it slightly to account for correlated unobservable factors (i.e., factors that are correlated with the presence of large establishments). Specifically, we exploit the change in the establishment size threshold for Title VII coverage from 25 to 15 employees. We use data on establishments both above and under 20 employees drawn from the 1962 County Business Patterns to estimate the probability of workforce exposure to CRA enforcement within a given county (details about our construction of the estimated exposure of a county's workforce to civil rights legal mandates is discussed in Appendix A.3).

Table 13 presents the results of this test for heterogeneous effects by county-level ex-

[56] We do not take a stand on the precise way through which black voting rights and improved political representation improved legal enforcement. Rather, we take at face value work in political science and sociology suggesting that even bureaucratic enforcement of the CRA depended on political factors (Wood 1990; Dávila and Bohara 1994). We readily admit, however, that this evidence is weaker than our other tests in terms of internal validity, and so urge readers to interpret the analysis in this subsection as merely suggestive.

[57] Although we were preliminarily granted access to the EEOC's establishment-level data that would have allowed us to investigate in more detail the possibility of legal enforcement within the private sector as a mechanism, the Commission's external researcher program was temporarily halted in early 2018 due to concerns about data protection.

posure to federal anti-discrimination legislation. The hypothesis we are testing is whether black political empowerment augmented the effectiveness of Title VII (as measured by the *ex-ante* enforcement likelihood). If not, we would expect to see no meaningful result for the interaction between VRA × Black and Title VII Exposure (the final row of the table). This is not the case, though. Both with and without baseline controls (Columns (1) and (2)), we find that the effects of the VRA (limiting to manufacturing workers) on relative black wages are larger in counties that are arguably more-exposed to the CRA and federal affirmative action requirements, consistent with the findings of Carrington, McCue, and Pierce (2000). Both estimates are significant at the 5% level. We do not interpret the magnitude of the estimates, as Title VII Exposure is only meant to be a proxy for the presence of anti-discrimination law in a county. However, the results do suggest that the minority electoral power may have contributed to black Americans' improved labor market standing through legal enforcement and the breaking down of labor market segregation.[58] In Column (3) of Table 13, we conduct the same test for heterogeneous effects of the VRA, but we now also control for a worker's occupation. Interestingly, the magnitude of the interaction coefficient is measurably reduced, and is no longer statistically significant. One way to interpret Column (3) relative to Columns (1) and (2), then, is that occupational upgrading may account for some part of the improved wages of black Americans. We find support for this possibility, which is discussed at length in Appendix A.4.

At the state and local levels, minority political pressure also led governments to enact their own policies to improve the labor market status of blacks (and racial minorities generally). For example, a more common form of affirmative action conditioned state contracts on the employment of women and minorities (Nay and Jones 1989; Santoro and McGuire 1997). Other policies at the state and local level that likely had indirect effects on minority economic status. For example, the Georgia legislature in the 1980s gave tax breaks to government contractors who employed black workers and subcontracted with black-owned businesses.[59]

We are unaware of any comprehensive data source on local and state AA programs. Existing studies, however, suggest that local-level AA programs are more likely to be enacted in cities where an elected mayor has primary executive authority within city government, rather than an appointed city manager. Because elected mayors are more vulnerable to electoral pressures than appointed bureaucrats, they are more likely to use AA policies (and by extension, contracts) as a form of redistribution. In the next section, we will use geographic variation in a city's form of government (mayor vs. city manager) as a source of

[58] In ongoing work, we are collecting detailed county-level data on government contracts. It is well-known that government contractors were more likely to hire blacks than were non-contractors, due in part to new federal affirmative action regulations beginning in 1966 (Leonard 1990). Moreover, a large fraction of government contracting relates to military spending. As such, we plan construct an instrument for exposure to contracting (which in turn implies greater exposure to labor market regulations related to minority hiring) using the total value of government contracts within a county in 1960). The intuition for this test is that it provides pre-VRA geographic heterogeneity in the likelihood that firms would be more or less subject to government oversight of minority hiring, which would presumably be enhanced by the effects of minority political power.

[59] Additionally, local political lobbying led to minority business incentive programs designed to increase city contracting with minority businesses, which in turn created new employment opportunities for both entrepreneurs and employees (Nay and Jones 1989)).

treatment effect heterogeneity to demonstrate that the effects of the VRA on black wages are stronger in those localities where one would expect minority electoral power to influence how politicians govern.

1.6.3 Changes in Human Capital

Finally, we analyze whether improvements in skills are a channel through which voting and political influence translated into the improved earnings prospects of black Americans. Several studies have documented how franchise expansions in the U.S., including both the VRA and state-level laws extending voting rights to women, have led politicians to increase spending on education and health (Miller 2008; Cascio and Washington 2014; Kose, Kuka, and Shenav 2019). An implication of these studies is that the relative growth in black wages may be attributable also to a rise in the supply of skills offered by an increasingly well-educated, healthier, or otherwise more productive black workforce.

Direct and indirect evidence, however, suggests that improvements in human capital caused by the VRA are not the main channel that explain our main findings. First, the timing of the effects discussed in Section 1.5.2 above is not consistent with the main channel being improved education for black workers. As Figure 7 shows, the effects of the VRA are apparent within five years of voting rights coverage taking effect. That the observed effect appears so soon after passage of the VRA suggests that our results are not due to differential changes in human capital that are due to solely to improved investments in educational attainment (or other changes that would have been observed in the labor force with a longer time lag).

We directly test this channel using DEC data on workers' educational attainment (Table A.15). First, we detect no statistically significant effect on black-white worker education gap (as measured by the highest level of education a worker achieves) (Columns (1)-(3)). Moreover, we reestimate our primary specification while accounting for the VRA's impact on education and experience. We add control variables for the interaction terms between the VRA and education/experience to Equation 1.1 (Columns (4)-(6)). If the VRA was affecting wage inequality through its effect on human capital, then one might expect the interactions of VRA and human capital traits to absorb some of our primary effect. We do not observe this to be the case, though. There is virtually no change in the parameter of interest, $VRA \times Black$. Albeit an imperfect test given that we are potentially controlling for an outcome, the results are nevertheless suggestive that the VRA did not affect black wages (at least exclusively) by improving the supply of skills provided by black American workers. Again, if anything, controlling for human capital as a mechanism *strengthens* our main results – raising our preferred estimates by 0.2 to 0.3 p.p. (main results remaining statistically significant at the 5% level). Finally, in Columns (7) and (8), we find no discernible effect of the VRA on the composition gap of minority workers that have either high school or college.

1.7 How Does Minority Political Power Operate?

The previous discussion of channels focused on the labor market mechanisms (supply and demand) that gave rise to improvements in wage equality. We have conducted relatively lit-

tle exploration, though, of how the VRA changed politics itself in a way that benefited black workers. In this final section, we examine these political channels, providing further confirmation that the VRA was an independent contributor to the labor market improvements of the civil rights period.

1.7.1 Distributive Politics

Existing research in political economy highlights two distinct political channels. Models of spatial competition suggest that policy choices reflect the preferences of the electorate – and in particular, changes in the "median voter" (Downs 1957; Meltzer and Richard 1981). These models of distributive politics suggest that politicians will target resources to identifiable and politically persuadable interest groups to earn their electoral support. Citizen-candidate models, however, suggest that politicians embody the preferences of their constituents – suggesting the electing minorities could lead to distinct policy outcomes that benefit these communities (Besley and Coate 1997).

Theories of distributive politics suggests that black enfranchisement via the VRA would have increased public expenditures for black communities (Lindbeck and Weibull 1987; Cox and Mccubbins 1986). As Cascio and Washington (2014) point out, these models of distributive politics suggest that larger post-VRA shifts in economic status should occur counties with higher black population shares in treatment areas that in control counties. The intuition is that counties in which 40% of the voting population is black, politicians will be more likely to respond to the redistributive demands of black voters than counties where only 5% of the electorate is black. If black constituents vote cohesively, then local politicians would face stronger electoral incentives to attend to the policy needs of this voting block. These needs often include the distribution of resources, including greater support in terms of employment, contracts, and other policies.

Table 14 provides evidence suggestive of this mechanism, in the spirit of Cascio and Washington (2014). The coefficient on the term VRA × Black × %PopBlack is significant and positive, suggesting that black workers benefited from political power relatively more in jurisdictions where there they could exert more pressure on elected officials. We also test for nonlinearities in the heterogeneous strength of minority voting power by dividing counties into 10 % bins by black population shares. It is possible, for example, that black constituents' ability to affect policy jumps discretely at or around 50% (when it becomes a majority within a given city – and so in principle can elect all politicians who elected are elected citywide).

We thus estimate the following regression for heterogeneous effects of the VRA on black relative wages:

$$\log(Y_{ict}) = \sum_{j=1}^{5} \alpha_j \left[\%\ \text{Pop. Black}_c = j\right] \times \left[\text{VRA}_{ct} \times \text{Black}_{ict}\right] + \mathbf{x}_{ict}\boldsymbol{\gamma} + \mu_{cr(i)} + \mu_{ct} + \mu_{p(c)r(i)t} + \epsilon_{icp(c)r(i)t} \tag{1.12}$$

where j indicates one of five different bins for county black population share. $j = 1$ indicates a county where 1960 black population share is between 10 and 20%, $j = 2$ indicates a county where 1960 black population share is between 20 and 30%, and so on; $j = 5$ indicates

a county where 1960 black population is over 50%.

The results from this estimation are presented in Figure 9. They convey a few suggestive, but nevertheless important, findings about how minority political power serves the interest of minority voters. First, as demonstrated by coefficients α_2, α_3, and α_4, it was not necessary that blacks comprise a majority of the electorate for the VRA to improve socioeconomic conditions. However, the magnitude of the coefficient on α_5 suggests that when black constituents comprise a majority of the electorate, the economic benefits are substantially larger. Although merely suggestive (due to potential correlated unobservables), this finding is consistent with the power of black Americans tipping sharply in places where they comprise a majority of the electorate. We find qualitatively similar findings for public sector employment (i.e., the effects are considerably stronger in counties where black voters constitute over 50% of the local population). We also note that these results are suggestive that local government matters.

Hetereogeneity by Local Government Structure

We also probe the distributive politics channel using locality-specific information on government structure. Specifically, we show that our measured effects are stronger in cities where the chief executive faces greater electoral incentives.

City governments in the United States come in two main forms: (1) the mayor-council form, where the mayor is elected directly by the city and functions as the chief executive of the government; and (2) the council-manager form, where the legislative and executive functions of government fall to the city council, which may appoint a city manager to administer city services and determine the composition of the bureaucracy. The former governmental setup concentrates powers in the hands of the mayor, who is elected and thus cannot removed by the city council. One may hypothesize that because mayors face reelection incentives, they are more likely to engage in targeted redistribution for political gain – while on city managers, on the other hand, are appointed bureaucrats and are thus less likely to engage in politically-motivated redistribution (perhaps due to stronger career concerns). Recent empirical evidence lends credence to this prediction, *in particular* with respect to public sector employment. Enikolopov (2014) demonstrates, for example, that a greater fraction of workers are employed by the public sector in cities with elected mayors rather than city managers.

We adopt a similar strategy, using mayor-council local government as a source of heterogeneity to test whether the benefits of minority political power are larger in those cities where the chief executive was elected. The directional effects of this test are presented in A.16 – the regression estimates are currently under embargo at the U.S. Census Bureau (awaiting disclosure review). The results suggest that both the wage improvements and public employment gains that black Americans achieved post-VRA were differentially larger in mayor-led cities. These results augment the work of Enikolopov (2014) but suggesting that government and political structure affect not just the size but the *composition* of the public sector workforce. The differentially larger effect of the VRA on wages in mayor-run cities is also consistent with existing research suggesting that local affirmative action policies that improve minority labor market outcomes are more likely to be enacted in mayor-council cities (Santoro and McGuire 1997).

1.7.2 Descriptive Representation

Identity-based or "citizen-candidate" models provide a plausible alternative (though not mutually exclusive) theoretical channel through which the VRA might have affected redistribution. Minority enfranchisement may have increased the presence of minority politicians, who in turn implemented the preferred policies of the group (such as hiring of minorities within government jobs, or the provision of government contracts). Recent work by (Beach, Jones, Twinam, and Walsh 2018), suggests that black politicians can improve the quantity and quality of public goods in black communities.[60] Anecdotal evidence lends some credence to this possibility. After 1965, black Americans ascended to political office nationwide at a pace never before seen. The number of black elected officials in local, state, and federal government rose more than six-fold from 1970 to 2000 (JCPES, 2000).

However, there is limited *causal* evidence on whether the increase in black office-holding was due to the passage of the VRA, and perhaps more importantly, whether descriptive representation improved the substantive representation, which might become manifest through improved minorities' socioeconomic outcomes. We test the plausibility of this channel by first demonstrating that the VRA increased the presence of black officials in elected office differentially in covered areas. To this end, we digitized data from various volumes published by the Joint Center for Political and Economic Studies. The JCPES published its annual "Black Elected Officials: A National Roster" each year beginning in 1969.[61] Because our constructed data begin only in 1969 (after the VRA was passed), we also supplemented our data construction using data from Alt (1984), which contains the number of black elected officials in the South in 1960.[62] Although we still cannot establish a pre-trend in minority elected officials prior to 1960, our qualitative search suggested that prior to the Civil Right era, there was virtually no black representation in the South at any level of government (JCPES, 2000).[63]

Table 15 provides results from estimating the impact of VRA coverage on the presence of black elected officials within a county (using several measures, both for the border pair and full county sample). The results clearly indicate an increase in the number of minority elected officials, as might be expected.

Given data constraints, we cannot separate the mechanisms of descriptive representation and distributive politics. We do not believe, though, that descriptive representation is the primary political mechanism at work in this setting. As previous research has pointed out, the number of black politicians holding office did not change overnight. Rather, the increase was gradual – unlike the changes in employment outcomes that we observe. Based on the

[60] A rich literature within development economics has also considered the effects of descriptive representation. In the Indian context, Pande (2003) as well as Duflo (2004), demonstrate empirically that representatives' personal ideology, proxied by gender and ethnicity, affect the distribution of public goods in a manner that benefits historically disadvantaged voters.

[61] In the process of conducted our study, we were pointed to an excellent new working paper by Bernini, Facchini, and Testa (2018), who also examine the impact of the VRA on the composition of elected officials. While the data on minority elected officials that we compiled was from the same source, we use the data for a different (albeit complementary) purpose.

[62] We are tremendously grateful to Jim Alt, who provided us with his data on minority political behavior during the pre-VRA era.

[63] See PBS (2000).

American politics literature, we believe that counties that had sufficiently large minority populations as early as 1960 were more likely to ultimately elect minority candidates, and as the previous subsection highlights, also produce benefits for their communities. We demonstrate this in Table A.17. In summary, although the results in this section are primarily suggestive (i.e., we cannot effectively rule all correlated unobservables for these tests of heterogeneity), we believe (cautiously so) that the empirical evidence is consistent with models of distributive politics as argued in Husted and Kenny (1997) and Cascio and Washington (2014).

1.8 Conclusion

Understanding the politics-economics nexus is important for understanding the effect of the VRA, as political representation is intimately related to distributional issues. Moreover, the VRA and black economic progress are intertwined historically, since racial minorities' demand for equal economic opportunity was a central feature of the Civil Rights social movement that led to passage of signature laws such as the VRA and the Civil Rights Act. As such, a complete understanding of the effects of the VRA requires understanding the accuracy of economic historian Gavin Wright's claim that, "black political power has played an important role in improving racial economic equity" (Wright 2013). If the policy demands of now-enfranchised voters include policies that improve their economic lives (i.e., desegregated labor markets, elimination of workplace discrimination, improved schools, etc.), one might expect to observe improved economic outcomes in the short-term following this large-scale enfranchisement event.

In this study, we confirm that this hypothesis is indeed true. We show that minority political empowerment has important labor market benefits for previously disenfranchised minorities. Our estimates demonstrate that counties where voting was protected experienced larger reductions in the black-white wage gap. We also thoroughly probe mechanisms, finding evidence that the VRA altered labor demand. We document that the VRA increased (relatively) the likelihood of blacks being employed in the public sector, as well potential complementarities between political power and the enforcement of private sector labor market policy.

Figures

Figure 1: Political Attitudes of Black Americans, 1963

Survey Question Asked: "Which Political Issue is Most Important to You?"
(Source: NORC)

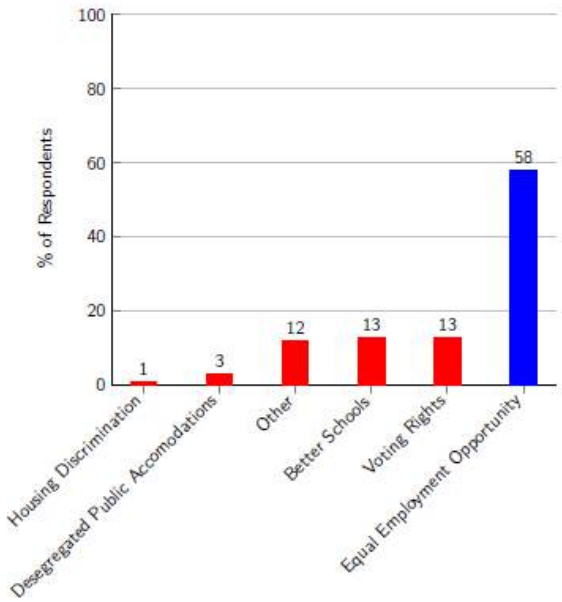
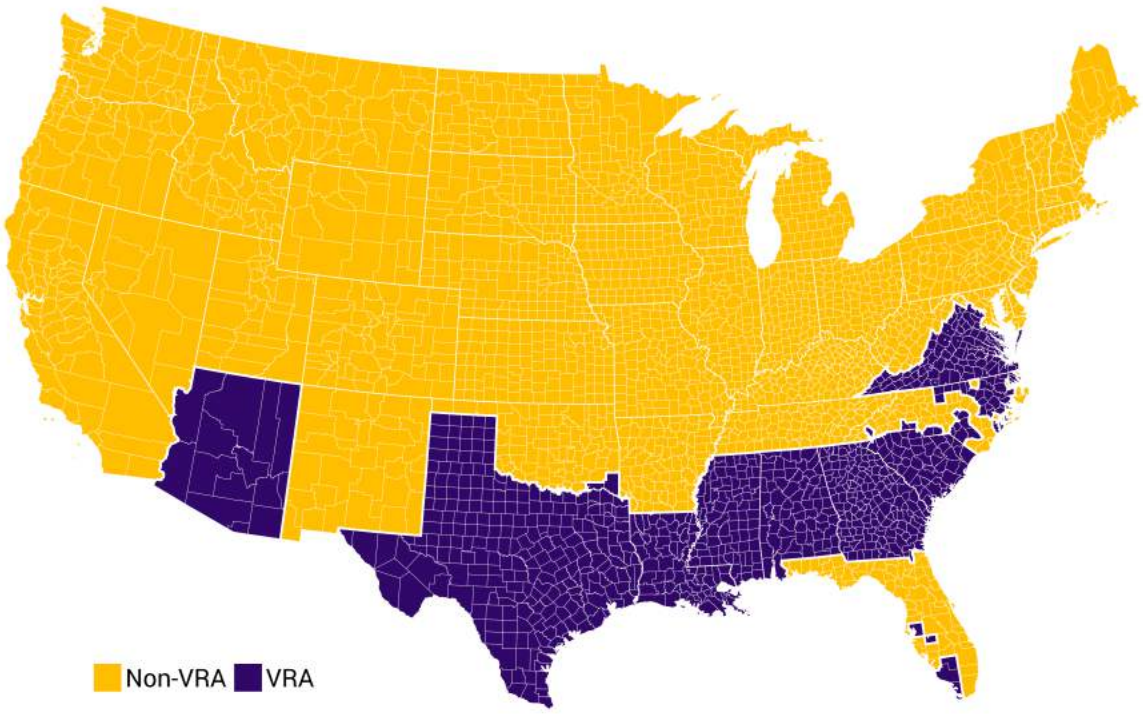
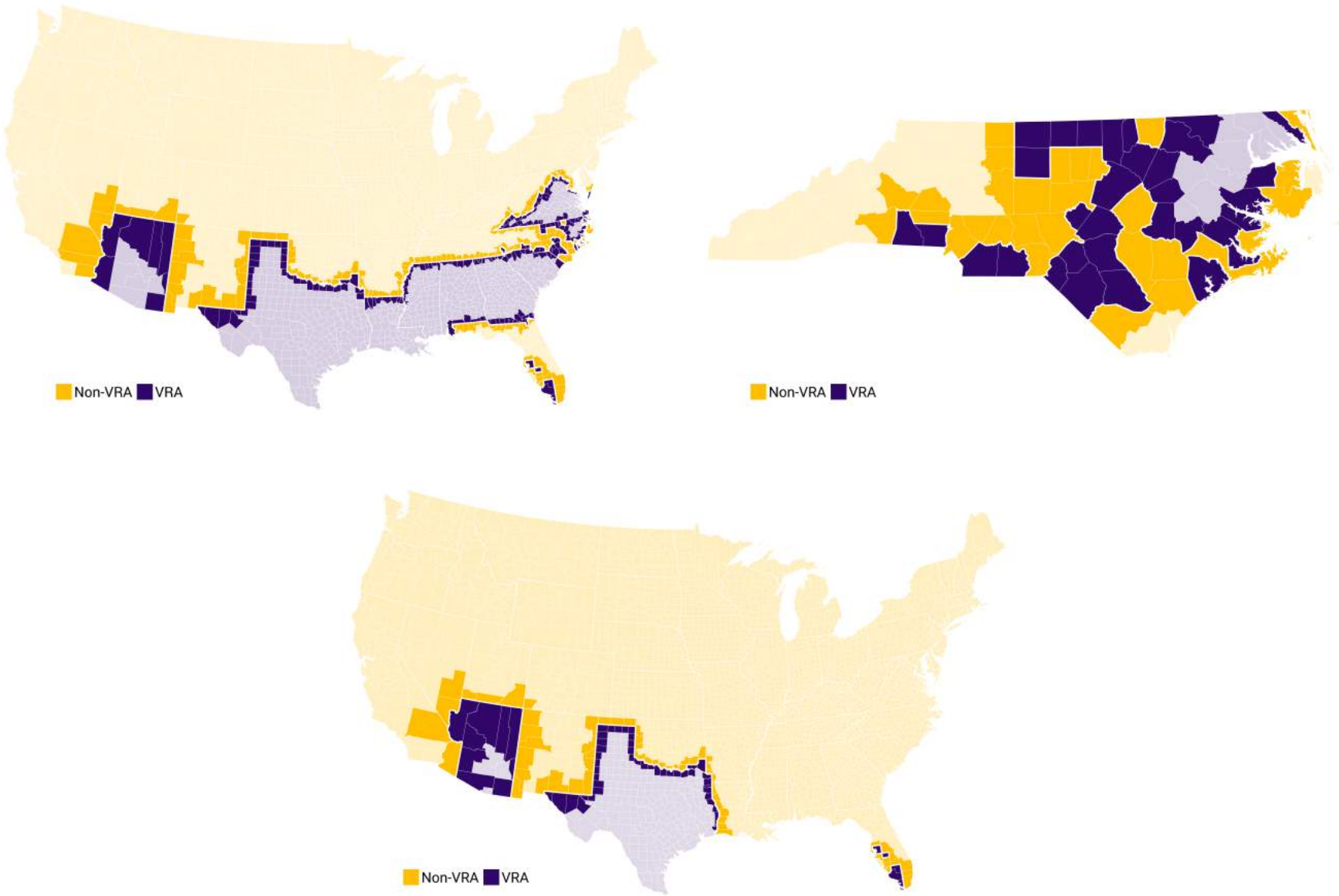


Figure 2: U.S. Counties by VRA Section 5 Coverage



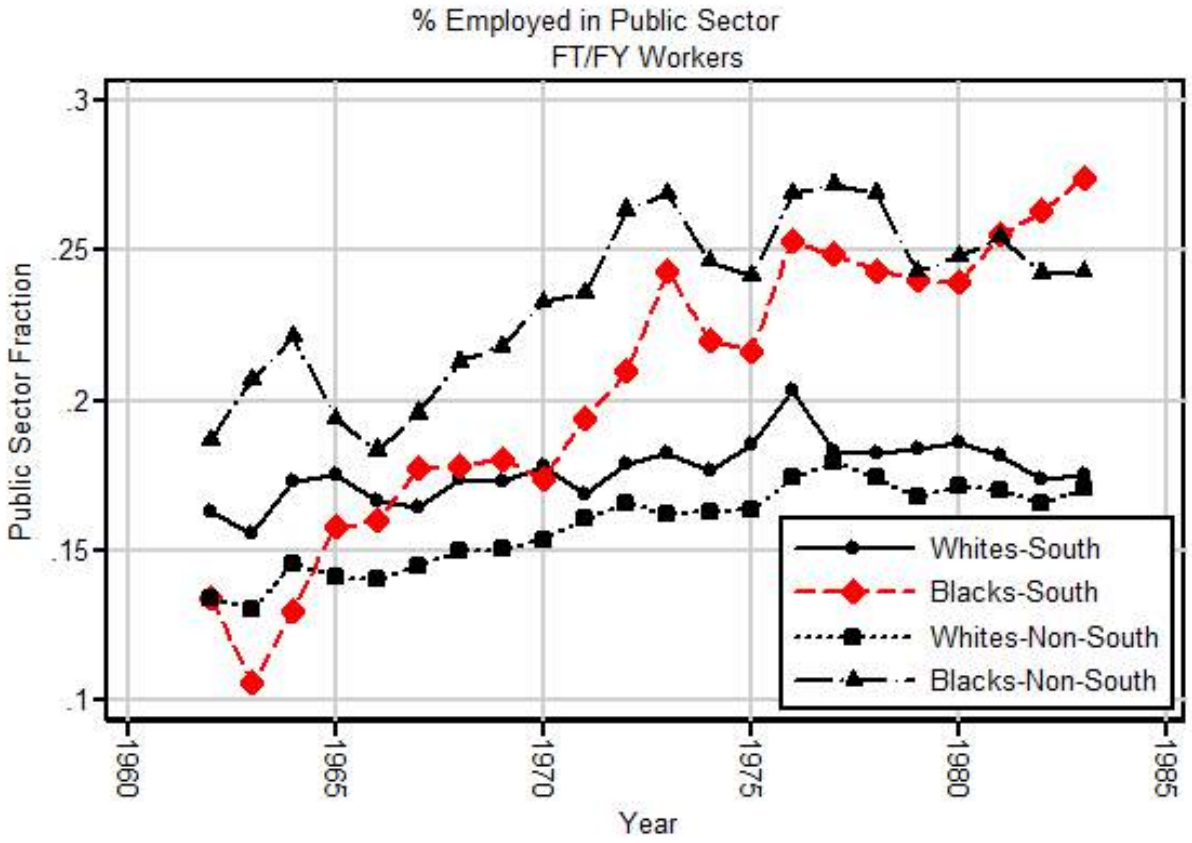
NOTES: Figure indicates all counties that were/were not covered by Section 5 of the 1965 Voting Rights Act (VRA) during the period of analysis. Source: U.S. D.O.J.

Figure 3: VRA Covered Jurisdictions



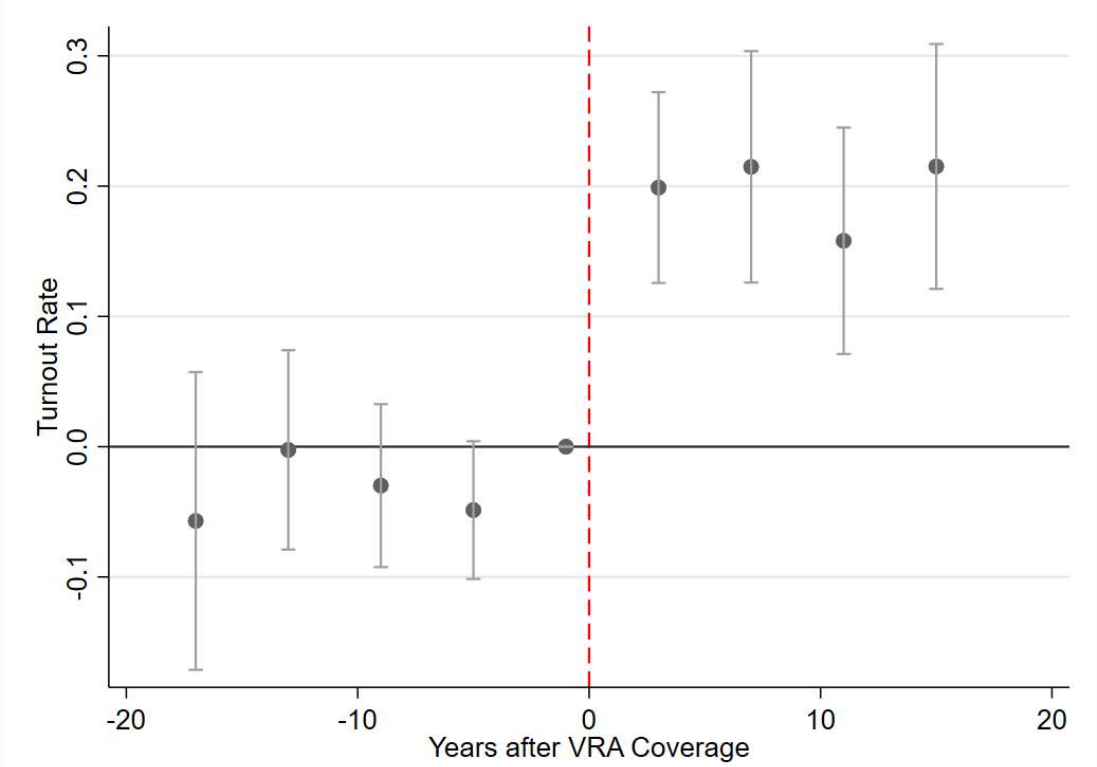
NOTES: Figure presents the main sample of counties used for analysis, as well as the primary two subsamples used to demonstrate the robustness of empirical findings. Top left panel indicates full matched county pair sample, top right panel indicates NC-only matched county pair sample, and bottom panel indicates 1975

Figure 4: Public Sector Workforce - By Race



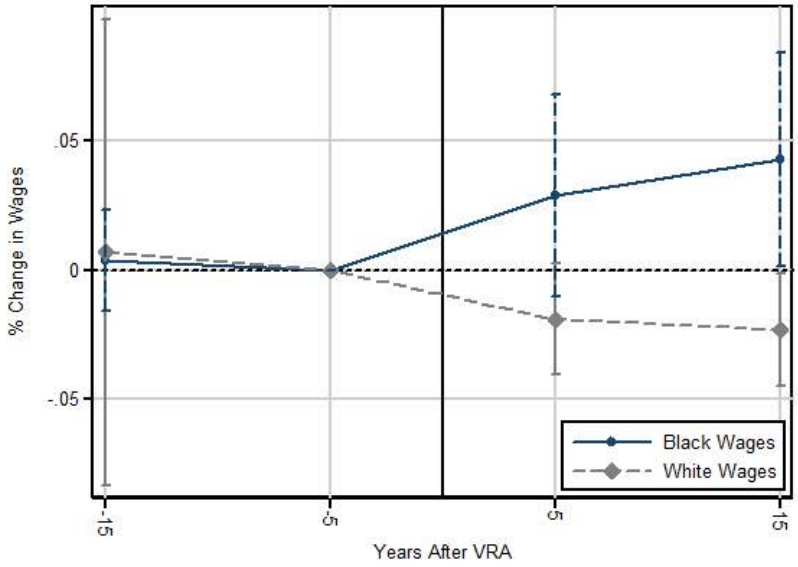
NOTES: Figure presents the fraction of full-time, fully-year workers employed as public workers, according to the Current Population Survey (CPS), by race and region. Source: CPS.

Figure 5: Impact of the VRA on Presidential Turnout
(Heterogeneous Effects by % County Black)



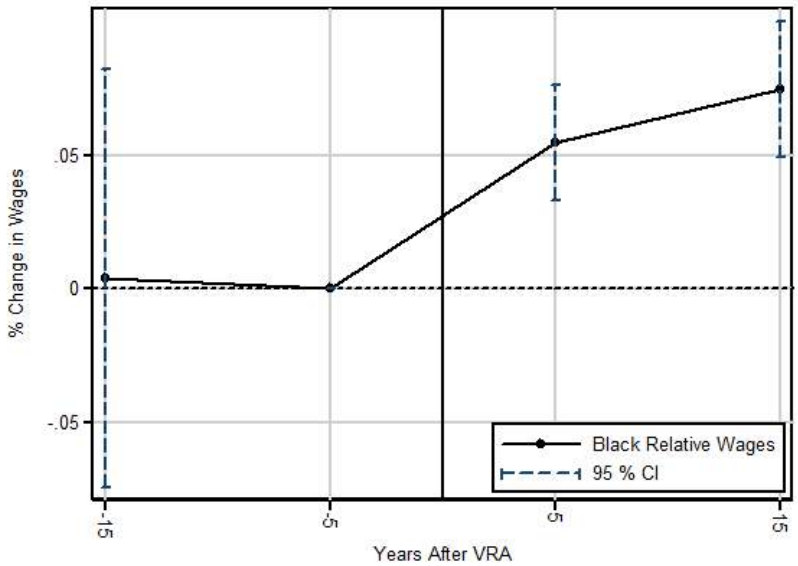
NOTES: Figure presents event-time estimates of how VRA coverage interacted with pre-VRA black population relates to voter turnout. The dependent variable (y-axis) is the voter turnout in presidential elections, and the independent variable (plotted) is the interaction between between the ever-VRA-covered indicator, a year indicator, and the pre-VRA percentage of the population that is black within a county (standardized, mean 0). Vertical bars provide 95% confidence intervals. All specifications include county and year fixed effects, as well as state-specific linear time trends. The model also includes the controls for unemployment, population density, high school graduation rate, and farm population rate, fixed at 1960 levels and interacted with linear and quadratic trends. Source: County Data Books and presidential turnout data from Gentzkow, Shapiro, & Sinkinson (2011).

Figure 6: Impact of the VRA on Wages
(by Race)



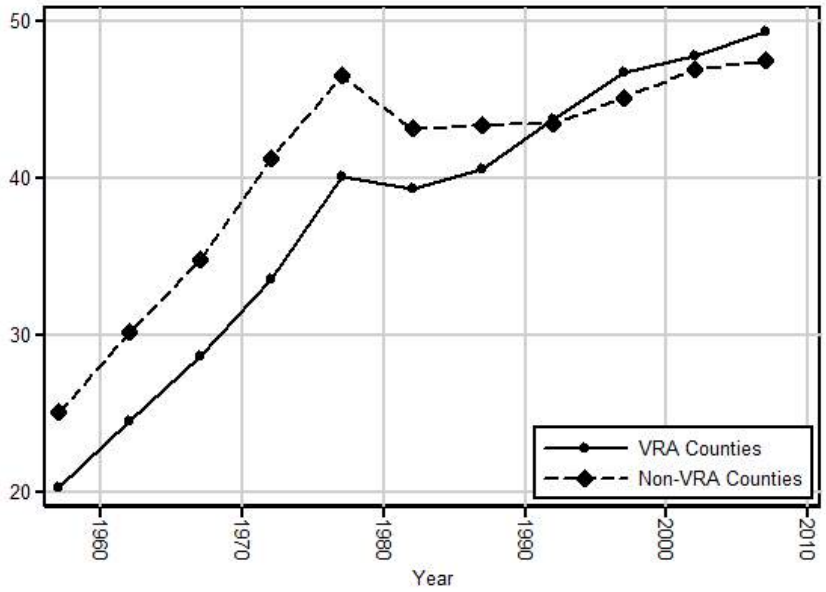
NOTES: Figure presents event-time estimates of how VRA coverage affects wages for black and white workers separately. Regressions include education and experience controls, county and county pair-year fixed effects, and baselines controls interacted with linear and quadratic trends. Estimates are normalized to five years prior to VRA coverage taking effect. Source: DEC.

Figure 7: Impact of the VRA on the Black-White Wage Gap: Event Study Estimates



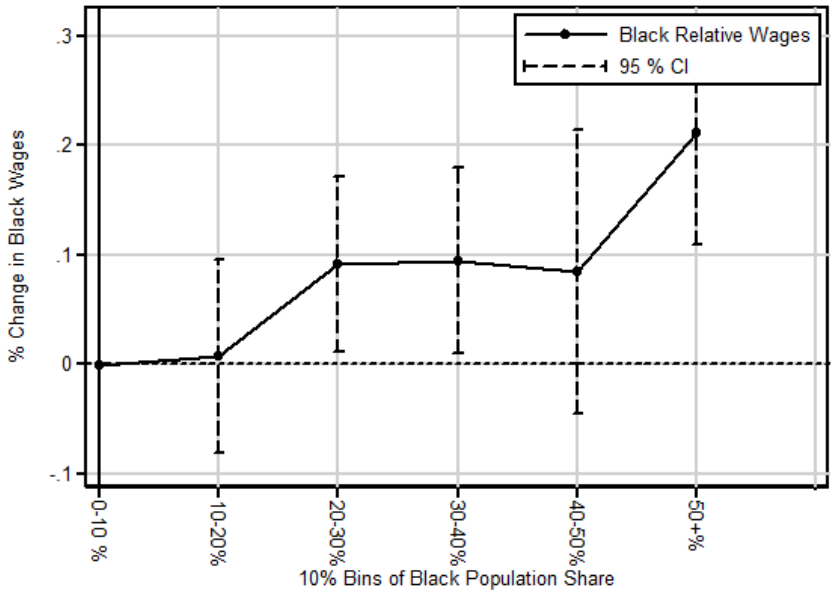
NOTES: Figure presents event-time estimates of how VRA coverage affects black relative wages. Regressions include education and experience controls, county and county pair-year fixed effects, and baseline controls interacted with linear and quadratic trends. Estimates are normalized to five years prior to VRA coverage taking effect. Source: DEC.

Figure 8: Local Government Growth, 1957-2007



Notes: Figure presents the number of employees per 1000 people in for the sample of neighboring VRA and non-VRA counties, restricting to counties with populations of larger than 10,000. Source: U.S. Census of Governments (COG).

Figure 9: Heterogeneous Effects of the VRA:
Wage Results by County Black Population Share



Notes: Figure examines the heterogeneous effects of VRA coverage on black relative wages, by black population share within a county. Each point presents the OLS regression coefficient of the interaction between the primary explanatory variable of interest (VRA $\times$ Black) and a dummy variable for whether a respondent resides in a county with a given level of black population share indicated by the X-axis. Source: U.S. DEC.

Tables

Table 1: Baseline Characteristics & Trends, by VRA Status

	Neighboring County-pair Sample			Total County Sample		
	Pre-VRA Mean	(1)	(2)	Pre-VRA Mean	(3)	(4)
Demographic Characteristics						
Population	46582.34	-0.120	0.065	82111.06	0.043	0.053
		(0.279)	(0.754)		(0.404)	(0.771)
Pop. Density	191.28	-0.012	0.058**	151.42	-0.029	0.004
		(0.111)	(0.956)		(0.575)	(0.981)
% Urban	31.87	0.183*	0.279	29.95	0.255**	0.256
		(0.110)	(0.174)		(0.000)	(0.158)
% Farmer	19.85	-0.009	0.325	21.12	-0.067	0.326**
		(0.111)	(0.097)		(0.193)	(0.047)
% Nonwhite	19.60	-0.011	-0.164	19.42	-0.136***	-0.042
		(0.111)	(0.396)		(0.009)	(0.806)
% H.S.-educ. adults	29.83	-0.121	-0.237	28.34	0.482**	-0.320*
		(0.110)	(0.240)		(0.000)	(0.061)
Economic Characteristics						
% Employed FT	69.34	-0.131	-0.128	69.91	-0.477***	-0.023
		(0.235)	(0.502)		(0.000)	(0.884)
Median Income	3732.60	0.059	-0.267	3509.47	-0.028	-0.395**
		(0.596)	(0.187)		(0.584)	(0.025)
% Construction	2.28	-0.173	-0.066	2.33	.011	0.017*
		(0.118)	(0.752)		(0.705)	(0.926)
% Manufacturing	19.54	-0.101	-0.306	19.20	-0.170***	-0.330*
		(0.363)	(0.121)		(0.001)	(0.054)
% Trades	5.38	0.115	0.077	5.30	-0.027	0.084*
		(0.299)	(0.713)		(0.595)	(0.644)
No. of Counties	329			1511		
State FE			X			X

Notes: This table presents regression coefficients from 4 separate regressions, one per column. Each column reports estimates of ordinary least squares regressions relating coverage future under the VRA to the difference in a givern county characteristic. An observation is a county-year. The dependent variable is the difference between county characteristics between 1950 and 1960, where each outcome is standardized to be mean 0, standard deviation 1 in a given year. The independent variable is an indicator for future VRA coverage. P-values are provided in parentheses. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: County Data Books (based on US Census estimates).

Table 2: The Effect of the VRA on Political Participation and Representation

	Panel A: Voter Turnout (Presidential Elections)				Panel B: Legislator Ideology (DW Nominate Score)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VRA	0.115***	0.079***	0.063***	.042***	-0.08*	-0.06*	-0.04
	(0.010)	(0.007)	(0.007)	(0.010)	(0.04)	(0.03)	(0.04)
VRA × Black Pop. Share				0.002***			-0.12
				0.000			(0.15)
Unit	County	County	County	County	District	District	District
N	2651	2651	2651	2651	1699	1699	1699
Controls		X	X	X		X	X
State Trends			X	X			

Notes: This table presents regression coefficients from 7 separate regressions, one per column. Observations in Columns (1)-(4) are county-year, and observations in Columns (5)-(7) are congressional district-year. The dependent variable is county-level turnout in presidential elections in Panel A. The dependent variable is the second dimension of the Poole-Rosenthal DW-Nominate Score, which indicate conservativeness on race-related issues. The independent variable is a dichotomous variable indicating whether a given county or Congressional district is protected under VRA-Section 5 (and where relevant, the interaction between the VRA indicator and the county population share that is black). Standard errors are in parentheses and are clustered by county. County-level controls include the employment rate, the adult population fraction with a high school education, the population fraction residing in urban areas, the adult population fraction working in agriculture, and median household income. Controls are measured at 1960 levels and interacted with linear and quadratic time trends. District-level controls include the fraction of the district population that is black. All regression include year/Congress and county/district fixed effects. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details.

Table 3: The Effect of the VRA on Turnout – Heterogeneous Effects by Examiner Status

	(1)	(2)	(3)
Outcome: Presidential Election Turnout			
VRA × Examiners Sent	.23***	.09***	.01
	(.02)	(.02)	(.02)
VRA	.11***	.09***	.05***
	(.01)	(.01)	(.01)
N	1400	1400	1400
Controls		X	X
State Trends			X

Notes: This table reports estimates of OLS regressions examining the impact of both the VRA and the use of federal election examiners on presidential turnout. All regressions include county baseline controls, county pair-year, and county fixed effects. Standard errors are in parentheses and are clustered by county. County-level controls include the employment rate, the adult population fraction with a high school education, the population fraction residing in urban areas, the adult population fraction working in agriculture, and median household income; each is measured at the 1960 level and interacted with linear and quadratic time trends. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table 4: The Effect of the VRA on Wages (by race), 1950-1980

	(1)	(2)
	Panel A: Black Workers	
VRA	0.050**	0.054**
	(.027)	(.027)
N	115000	115000
	Panel B: White Workers	
VRA	−.014**	−.007*
	(0.006)	(0.005)
N	558000	558000
	Panel C: Black-White Outcome Gap	
VRA × Black	0.055**	0.058***
	(.027)	(.027)
N	67300	67300
County-level Controls		X

Notes: This table presents regression coefficients from 6 separate regressions, 3 separate regression estimates per column, 2 regression estimates per row. Each column-row cell contains an estimate of an ordinary least squares (OLS) regression relating Voting Rights Act coverage to absolute wages by race (in Panels A and B), as well as relative wages (Panel C). An observation is an individual in a given Decennial Census year. The dependent variable is the log hourly wage, and the independent variable is either VRA (an indicator variable for whether is VRA-covered in a given Census year), or VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA in a given year). Regressions in Panels A and B include county and county pair-year fixed effects. Regressions in Panel C include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. County-level controls include the employment rate, the adult population fraction with a high school education, the population fraction residing in urban areas, the adult population fraction working in agriculture, and median household income. Controls are measured at 1960 levels and interacted with linear and quadratic time trends. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table 5: The Effect of the VRA on Black Relative Wages, 1950-1980

	Outcome Variable: Log(Wage)				
	(**1**)	(**2**)	(**3**)	(**4**)	(**5**)
VRA × Black	0.055**	0.058**	0.056**	0.056**	0.048*
	(0.027)	(0.027)	(0.28)	(0.28)	(0.35)
N	673000	673000	673000	673000	673000
County-level Controls		X	X	X	X
State Trends			X		
County Trends				X	
County-by-race Trends					X

Notes: This table presents regression coefficients from 5 separate regressions, one per column. Each column reports estimates of ordinary least squares regressions relating the VRA to (relative) wages. An observation is an individual in a given Census year. The dependent variable is the log hourly wage, and the independent variable is VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA in a given year). The (adjusted) baseline black-white hourly wage gap (in 1960) was -0.43 log points. All regressions include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. County-level controls include the employment rate, the adult population fraction with a high school education, the population fraction residing in urban areas, the adult population fraction working in agriculture, and median household income. Controls are measured at 1960 levels and interacted with linear and quadratic time trends. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table 6: The Impact of the VRA on the Black-White Wage Gap: Subsample Analysis, 1950–1980

	Outcome Variable: Log(Wage)			
	(**1**)	(**2**)	(**3**)	(**4**))
VRA × Black	0.057**	0.071**	0.046*	0.114***
	(0.027)	(0.035)	(0.032)	(0.038)
Sample	Full Sample	1965 VRA	1975 VRA	NC
Baseline-Year Controls	X	X	X	X
N	673000	530000	150000	180000

Notes: This table presents regression coefficients from 4 separate regressions, one per column – each for a different subsample (with the main sample being in Column (1)). Each column reports estimates from ordinary least squares regressions relating the VRA to (relative) wages. An observation is an individual in a given Census year. The dependent variable is the log hourly wage, and the independent variable is VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA in a given year). The (adjusted) baseline black-white hourly wage gap (in 1960) was -0.43 log points. All regressions include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. County-level controls include the employment rate, the adult population fraction with a high school education, the population fraction residing in urban areas, the adult population fraction working in agriculture, and median household income. Controls are measured at 1960 levels and interacted with linear and quadratic time trends. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table 7: The Effect of the VRA on County Compositional Changes, 1960-1980

Outcome:	(1) Education	(2) Experience	(3) % Black	(4) Earnings Index
VRA	0.40	0.22	0.01	434.58
	(0.82)	(0.80)	(0.04)	(1464.2)
N	600	600	600	600
County-level Controls	X	X	X	X

Notes: This table reports estimates of OLS regressions relating the VRA to average county charcteristics. The dependent variable in each column is a characteristic in a given year. All regressions include county baseline controls, pair-year, and county fixed effects. Standard errors are in parentheses and are clustered by county. County-level controls include the employment rate, the adult population fraction with a high school education, the population fraction residing in urban areas, the adult population fraction working in agriculture, and median household income. Controls are measured at 1960 levels and interacted with linear and quadratic time trends. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table 8: The Effect of the VRA on Additional Employment Outcomes

Outcome:	(1) 1(Have Income)	(2) 1(Not in Labor Force)	(3) 1(Unemployed)	(4) Hours Worked
VRA × Black	0.008	-0.005	-0.003	-0.402
	(0.011)	(0.014)	(0.007)	(0.603)
N	1,469,000	1,469,000	1,469,000	1,469,000
County-level Controls	X	X	X	X
Outcome:	(5) Weeks Worked	(6) 1(FTFY Worker)	(7) Log(Earnings)	
VRA × Black	-0.953	-0.038**	0.127**	
	(0.978)	(0.18)	(0.046)	
N	1,469,000	1,174,000	1,469,000	
County-level Controls	X	X	X	

Notes: This table reports estimates of OLS regressions examining the impact of the VRA on black (relative) employment outcomes, focusing on labor supply. The dependent variable in each column is an indicator for either having positive income, not being in the labor force, for being unemployed, or for being a full-time, full-year worked as defined in the text, or total hours worked, weeks worked last year, or log total earnings in Columns (4), (5), and (7), respectively. All regressions include county baseline controls, county-year, county-race, and county pair-year-race fixed effects. Standard errors are in parentheses, clustered by county. County-level controls include the employment rate, the adult population fraction with a high school education, the population fraction residing in urban areas, the adult population fraction working in agriculture, and median household income; each is measured at the 1960 level and interacted with linear and quadratic time trends. Column (7) also controls for worker education/experience and hours worked last year. Column (3) omits workers who are not in the labor force, and Column (6) omits workers who are either in the labor force or are unemployed. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table 9: The Effect of the VRA on Relative Wages: Spillover Effects

	Outcome Variable: Log(Wage)		
	(1)	(2)	(3))
VRA × Black	0.064**	0.045***	0.011**
	(0.030)	(0.018)	(0.005)
Sample	Matched Pairs	Interior	Difference
N	670000	3741000	670000
County-level Controls	X	X	X

Notes: This table presents regression coefficients from 3 separate regressions, one per column. Each coefficient is an estimate from an OLS regression relating the VRA to (relative) black wages. An observation is an individual in a given Decennial Census year. The dependent variable is the log hourly wage, and the independent variable is VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA in a given year). The (adjusted) baseline black-white hourly wage gap (in 1960) was -0.43 log points. Column (1) limits to the county pair sample, Column (2) limits analysis to the interior (counties in which all adjacent counties are either covered or uncovered), and Column (3) reports the difference. All regressions include county-race, county-year, and year-race fixed effects. Standard errors are in parentheses and are clustered by county. County-level controls include the employment rate, the adult population fraction with a high school education, the population fraction residing in urban areas, the adult population fraction working in agriculture, and median household income. Controls are measured at 1960 levels and interacted with linear and quadratic time trends. Regressions control for race-specific returns to human capital. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table 10: Public Sector Wage Premium Estimates (1960)

Outcome Variable: Log(Wage)		
	(**1**)	(**2**)
Public Worker	0.029***	0.196***
	(0.003)	(0.009)
Worker Sample	White	Black

Notes: This table presents regression coefficients from 2 separate regressions, one per column. Each coefficient is an estimate from an OLS regressions of log wages on an indicator that equals 1 if an individual is a government employee. Regressions control for individual education, years worked, and squared(years worked), and state fixed effects. Models are estimated using the 1960 Census, for all workers in counties eventually covered under the VRA. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Source: IPUMS Decennial Census

Table 11: The Effect of the VRA on Public Sector Employment, 1950-1980

	Outcome Variable: Public Sector Employment				
	(1)	(2)	(3)	(4)	(5)
VRA × Black	0.038***	0.020**	0.038***	0.027**	0.027**
	(0.009)	(0.01)	(0.009)	(0.012)	(0.012)
N	673000	673000	673000	175000	175000
County-level Controls	X	X	X	X	X
State Trends			X		
Sample	Full CB	Full CB	Full CB	NC	NC

Notes: This table presents regression coefficients from 4 separate regressions, one per column. Each coefficient is an estimate from linear probability regressions relating passage of the VRA to employment in the public sector. An observation is an individual in a given Decennial Census year. The dependent variable is an indicator that equals 1 if an individual is a government employee. The independent variable is VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA in a given year). All regressions control for individual education, years worked, and squared(years worked), and include county-race, county-year, and county pair-year-race fixed effects. Columns (2) and (4) include additional human capital controls. Standard errors are in parentheses and are clustered by county. Controls are interacted with linear and quadratic time trends. Models are estimated on either the full cross-state border (CB) sample, or the North Carolina-only (NC) sample. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table 12: Wage Effects of the VRA, by Public Sector Occupational Growth

	Outcome Variable: Log(Wage)	
	(1)	(2)
VRA × Black	0.081**	0.114**
	(.07)	(0.57)
VRA × Black × ΔPubEmp$_{60-80,Q1}$	-0.078**	-0.101**
	(0.03)	(.046)
VRA × Black × ΔPubEmp$_{60-80,Q4}$	0.088**	0.008
	(0.045)	(.043)
N	153000	54500
Worker Sample	Private	Public
County-level Controls	X	X

Notes: This table presents regression coefficients from 2 regressions, one per column. Each coefficient is an estimate from an OLS regression relating the Voting Rights Act to (relative) black wages, examining heterogeneity by public sector occupational growth. An observation is an individual in a given Decennial Census year. The dependent variable is log wage, and the independent variables are interactions between VRA, the race indicator Black, and whether a respondent works in an occuation that is in either the first or fourth quartile for public sector growth. All regressions control for individual education, years worked, and squared(years worked), and include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. Controls are interacted with linear and quadratic time trends. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table 13: Heterogeneous Effects of the VRA on Relative Black Wages: Testing Complementarity between Political Power and Civil Rights Act

	Outcome Variable: Log(Wage)		
	(1)	(2)	(3)
VRA × Black	−0.077	−0.065	−0.062
	(0.072)	(0.054)	(0.109)
TitleVIIExposure × Black	5.727**	5.302*	4.908**
	(2.34)	(3.17)	(0.2.34)
VRA × Black × TitleVIIExposure	0.361**	0.351**	0.28
	(0.142)	(0.157)	(0.365)
N	10500	10500	10500
Controls		X	X
Occupation Controls			X

Notes: This table presents regression coefficients from 3 separate regressions, one per column. An observation is an individual Census respondent in a given Census year. The dependent variable is the log wage, and the independent variable of interest is the the interaction between an indicator for a county's VRA converage status in a specific year (a dummy), an indicator for whether a worker is black, and the county-level exposure of the manufacturing workforce to federal civil rights laws (as defined in Section 7.2 and the Appendix). All regressions control for individual education, years worked, and squared(years worked), and include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table 14: Heterogeneous Effects of the VRA on Relative Black Wages:
By Black Population Share

Outcome:	Log(Wage) (1)	Pub. Emp. = 1 (2)
VRA × Black	0.036	0.036
	(0.030)	(0.030)
VRA × Black × 1960 Black Pop. Share	0.002***	0.147**
	(0.000)	(0.062)
N	673000	673000
Controls	X	X

Notes: This table presents regression coefficients from 2 separate regressions, one per column. An observation is an individual Census respondent in a given Census year. The dependent variable is the log wage, and the independent variable of interest is the the interaction between an indicator for a county's VRA converage status in a specific year (a dummy), an indicator for whether a worker is black, and the county-level exposure of the manufacturing workforce to federal civil rights laws (as defined in Section 7.2 and the Appendix). All regressions control for individual education, years worked, and squared(years worked), and include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table 15: The Impact of the VRA on the Election of Black Politicians, 1960–1980

	Outcome Variable: Black Elected Officials			
	(**1**) Log(Count)	(**2**) County-wide =1	(**3**) Mayor=1	(**4**) Mayor=1
VRA	0.148**	0.121**	0.011	0.022***
	(0.068)	(0.044)	(0.017)	(0.007)
Sample	Border	Border	Border	Full
Baseline-Year Controls	Yes	Yes	Yes	Yes
N	810	810	810	3,750

Notes: This table presents regression coefficients from 4 separate regressions, one per column. Each coefficient is an estimate from OLS regressions relating the Voting Rights Act to the presence of black elected officials. An observation is a county-year. The independent variable is the VRA indicator (whether a county was covered by the VRA in a given year). All regressions include county and year fixed effects. Standard errors are in parentheses and are clustered by county. Controls are interacted with linear and quadratic time trends. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: JCPES.

Chapter 2

Minority Representation and Protection from Targeted Violence: Evidence from Low-caste Political Parties in India

Chapter abstract: We examine whether improving the political voice of historically-marginalized minority groups can reduce human and civil rights abuse. To answer this question, we examine the impact of political parties dedicated to representing India's Scheduled Castes and Tribes (SC/STs) on caste-based violence. We address the endogenous selection of minority-favored politicians using state-level variation in aggregations of close election outcomes. We find that a 10 percentage-point increase in the fraction of SC/ST politicians reduces the incidence of SC/ST-targeted violence by three percentage points. Improved attitudes of SC/ST citizens toward state institutions suggest that our results are not the product of negative reporting bias. Further analysis suggests that the crime reductions are consistent with deterrence induced by politicians influencing the operation of the law enforcement bureaucracy in favor of SC/ST citizens.

2.1 Introduction

"[I]t is of great importance in a republic...to guard one part of the society against the injustice of the other part." - James Madison, *Federalist No. 51*

A core promise of liberal democratic rule is the protection of the basic civil rights of racial, ethnic, socioeconomic, and religious minority citizens. Many nations struggle to deliver on this promise, though. While most nations are now electoral democracies (i.e., countries where any group of citizens can compete for political power), many are considered "illiberal." These nations fail to protect minorities from civil rights abuse or the denial of equal protection under law (Zakaria 1997; Rodrik and Mukand 2016).

Violence targeted against minorities is among the most egregious of these basic civil rights violations, and is a common problem facing many democratic societies today. In the

U.S., hate crimes increased by 17% between 2016 and 2018, and 95% of these are committed on the basis of race, religion, or sexual orientation. Against a backdrop of rising nativism and political populism, outbursts of violence against minority residents are also being observed throughout Asia, Europe, and Latin America (Nyiri and English 2007). We have relatively little evidence, though, about what can effectively protect historically-disadvantaged minority groups from such abuse.

In this paper, we examine whether electoral competition and representation in government through select political parties can reduce levels of discrimination and violence perpetrated against vulnerable minority communities. We do so by examining the effects of political representation for India's historically-disadvantaged Scheduled Castes and Tribes (SC/STs) on caste-based crime. In contrast to existing research studying the political representation through mandated electoral quotas set-aside for SC/ST citizens, we examine the effects of political parties that are dedicated to the specific policy demands of socio-economically disadvantaged low caste communities (including, in large part, SC/STs) (Jaffrelot 2006). One of the main goals of these parties was to reduce violence and abuse rooted in low-caste bias.

In principle, increasing minority representation can either increase or decrease the incidence of identity-based crimes. Minority-preferred politicians may improve government responsiveness to majority abuse, which in turn may inspire confidence to report criminal incidents. Moreover, the success of such politicians may change attitudes of the majority, leading to less racial/ethnic animus. These channels should reduce crime — although deterrence induced by increased willingness to report crimes may still appear to the researcher as a crime *increase*. On the other hand, to the extent that office-holding by parties beholden to low-caste groups is perceived as a violation of long-standing social norms, minority electoral success may engender the majority's resentment, leading to *more* targeted crimes against SC/STs. Majority backlash against SC/ST political success is not unprecedented in Indian politics. During the mid-1980s, for example, the election of several minority candidates in the state of Gujarat led to rioting so severe (over 200 people were killed) that the Indian army was ultimately dispatched to restore order (Wood, 1985).[1] Whether minority electoral success reduces hate crimes is thus an empirical question.

We answer this question using unique administrative crime statistics for incidents committed by non-SC/ST perpetrators against SC/ST victims. The methodological challenge we face in estimating the causal impact of minority representation is that areas represented by SC/ST-favoring parties may differ in unobservable ways from areas that elect mainstream political parties, such as the Bharatiya Janata Party (BJP) or the Indian National Congress (INC).[2] For example, districts supporting caste-based parties may be comprised of non-SC/ST citizens that already have favorable attitudes toward their SC/ST neighbors. Such

[1]Majority resentment toward minority political success is seen in other democracies, as well. In the U.S., for example, racial animus toward minorities increased following the 2008 election of President Barack Obama (Stephens-Davidowitz 2014; Mas and Moretti 2009). In Bihar, the upward mobility of SC groups in the 1990s led to a violent backlash by the *Ranvir Sena* - a private army formed by upper caste landowning rural elites who targeted villages where the SCs were in a majority and conducted a number of massacres.

[2]The BJP is considered a center-right party, drawing much of its support from upper-caste groups. The INC is considered centrist, but has not generated consistent support from low-caste groups (Jaffrelot 2003; Jaffrelot 2011).

places are more likely to both elect candidates from caste-based parties and commit fewer targeted crimes. Alternatively, regions afflicted by SC/ST-targeted violence might turn to these parties to provide protection. We address problems related to correlated unobservables or omitted variable bias using a fixed effects instrumental variables (IV) estimator that exploits constituency-level information on close elections between low caste party and mainstream party candidates. The intuition for this strategy is that in India's first-past-the-post electoral system, a party's ability to hold office changes discontinuously when vote share goes from negative to positive. We leverage the fact that within a few percentage points of this threshold, constituencies are likely to be comparable along most dimensions. We test our identifying assumptions, confirming empirically that constituencies where the caste-party candidate barely wins are similar to constituencies where he/she barely loses, in terms of observed characteristics.[3]

Our results demonstrate that political representation for SC/STs at the state level through select low-caste parties significantly reduces caste-based murder rates. In terms of magnitudes, a one standard deviation increase in the fraction of caste-based party legislators reduces the incidence of targeted murders by 0.4 standard deviations (a decline in 1 targeted murder for every 4 legislators elected from these parties; our primary results are significant at at least the 5% level). Our primary findings are bolstered using several alternative specifications and placebo tests. For example, electoral success of mainstream parties (the BJP and INC) — traditionally dominated by upper-caste elites — have no effect on caste-targeted murders.

We also examine potential mechanisms that underlie our findings. We provide evidence to suggest that SC/ST representation changes the criminal justice administration in a manner that deters potential perpetrators of caste-based violence. In states with more representatives from low-caste parties, police arrest perpetrators of targeted crime at higher rates and pursue cases more vigorously in court — consistent with politicians influencing criminal justice policy in a way that leads to better SC/ST protection. To mitigate concerns about the underreporting of SC/ST crime, we corroborate our findings using household survey data. We show that SC/ST attitudes toward state institutions – including politicians, police officers, and court officials – improve after exogenous increases in SC/ST-favorable representation. Self-reported violent inter-caste conflict also declines. Moreover, while using state-level data comes at the cost of not being able to account for all potential time-varying unobservables at an aggregate level, it comes with the benefit that cross-border spillovers across treated units are less likely (relative to district-level data).

Our paper advances the existing literature on crime, political economy, and development in a few key ways. First, we contribute to recent work on factors that drive identity-based crime and violence against marginalized groups.[4] Existing research highlights how a range

[3]Electoral constituencies denote the areas represented by a legislator in the state (federal) legislature. At the state level, each legislator represents roughly 100,000 registered voters and states are divided into an appropriate number of electoral constituencies based on the total number of registered voters in the state.

[4]We note, however, that our examination of low-caste representation should not be conflated as providing an estimate of the effect of racial or ethnic minority representation. Race and caste are indeed distinct. Numerous scholars have pointed out that while racial discrimination reflects a colonial history and the power differences between slaves and non-slaves, caste differences reflect a system of socioeconomic stratification that predates colonialism. Nevertheless, because we do believe that there are parallels between the marginal-

of factors, such as competition over public goods, poverty, and law enforcement activity, are related to hate crime levels (Bros and Couttenier 2015; Sharma 2015; Medoff 1999). Our study is most similar, however, to two recent studies of Indian crime by Iyer, Mani, Mishra, and Topalova (2012), as well as Girard (2017). Both studies find that increases in mandated representation through quotas lead to *increases* in reported crimes against women and SC/STs, respectively. Iyer and co-authors argue that increases in measured crime indicates improved reporting, and so is socially beneficial, while Girard argues that increases in SC murders represents a form of "backlash" against minority power. These contrasting interpretations have different normative implications with respect to minority representation.

Our study, though, is quite different in terms of both research approach and findings. First, we examine a qualitatively different form of minority representation by focusing on political party effects instead of mandated minority representation (the latter of which is the focus of previous studies). Second, our findings do not raise difficulties regarding interpretation of our main results. Given that any crime-reporting bias is likely to be positive (for reasons we demonstrate), our estimated effects are likely underestimates of the true impact of SC/ST political representation.

The political channel for reduced identity-based violence that we document further highlights our contribution to the broader literature on minority representation within multi-ethnic democracies. Much of the recent evidence on this topic examines the economic impact of political representation through India's electoral quotas for SCs and STs (as well as women) within state legislatures and village councils. Our study is the first, however, to quantitatively examine the effectiveness of representation for SC/STs through political parties. Our paper thus allows us to estimate the impact of "substantive" minority representation, rather than "descriptive" representation. According to the seminal work of Pitkin (1967), "descriptive representation" is the extent to which a politician shares identity aspects (such as race) with constituents, while "substantive representation" encapsulates whether a politician actually represents their interests. While the caste-based electoral quotas examined by Pande (2003), Iyer, Mani, Mishra, and Topalova (2012), and others guarantee descriptive representation for SC/STs, they do not guarantee that politicians will act in the best interests of SC/ST constituents once elected. In fact, Jaffrelot (2003) finds that politicians elected through the quotas, who many times represent mainstream parties, are often co-opted by upper-caste party elites. Political parties formed primarily on the basis of caste, however, have policy agendas based explicitly on the needs of SC/ST citizens.

Relatedly, we also contribute to the existing literature on the importance of political parties for economic outcomes. Existing studies, which focus on the U.S. context, provide conflicting estimates on this subject (Albouy 2011; Ferreira and Gyourko 2009). However, these studies focus on mainstream political parties in a rigidly two-party system, while our paper focuses for the first time on populist, identity-based political parties in a multi-party, parliamentary democracy.[5]

---

ization within democracies of both low-caste groups and racial/ethnic minority groups, we will generically make to our results as providing an understanding of the effects of "minority" representation. We refer readers interested in the subtle differences between caste and race discrimination to Deshpande (2011).

[5]The multi-party framework in India also permits parties to form coalition governments, which in turn allows parties without a legislative majority to retain some influence on the policy process. In related ongoing

2.2 Background

2.2.1 India's Caste System and the Rise of Low-caste Political Parties

India's population has been historically segmented socially (and *de facto* economically) into endogamous groups called *jatis*, or castes, which were legitimated by centuries-old Hindu religious texts. At the bottom of the caste group ladder are the so-called "Untouchables" or *Dalits*.[6] *Dalits* have often been relegated to daily work considered "impure" by the rest of the society, such as cleaning streets and other menial jobs. Also at the bottom of India's social hierarchy are the country's indigenous populations, or *Adivasis*. These indigenous groups face large-scale exclusion from mainstream society due to geographical isolation, primitive agricultural practices, and distinctly non-mainstream customs. In addition to exclusionary socioeconomic status, Dalits and Adivasis – India's historically marginalized communities (HMCs) – have long suffered from other forms of overt discrimination at the hands of forward-caste elites. These subpopulations have at times been denied access to schools, drinking water, and other basic resources. They also suffer from various forms of civil rights abuse, including (most seriously) targeted caste-based violence. The causes for targeted violence are varied; Sharma (2015) finds that economic competition between SC/STs and upper castes may be one reason for the continued existence of targeted violent crimes against SC/STs.

Following independence from Great Britain in 1947, India sought to remedy the social and economic injustices afflicting Dalits and Adivasis through a broad program of affirmative action. The Constitution provided special protections and benefits to a list, or "schedule," of castes/tribes (first drawn up by the British); "Scheduled Castes (SCs)" and "Scheduled Tribes (ST)" are the Dalit and Adivasi groups, respectively, that are designated for support under the Constitution.[7] With regard to political representation, the Constitution provided for quotas in both the federal and state legislatures, by which a pre-determined share of electoral constituencies – proportionate to the population of SC/STs – were set aside for contestation solely amongst SC/ST candidates, irrespective of their political affiliation.[8]

The 1980s and early 1990s, however, witnessed the emergence and strengthening of new political parties with an explicit commitment to the interests of low-caste citizens. The result of this caste-based coalition was the formation of two major "caste-based parties" — the

research, we examine the impact of India's low-caste political parties on policy outcomes, both in terms of targeted spending and household welfare (Chakravarthi 2014).

[6]There are four primary caste groups. At the top of the four-tiered caste hierarchy are the *Brahmins* (priests and teachers), followed by *Kshatriyas* (warriors and members of royalty). Next are the *Vaishyas* (traders, merchants and moneylenders), and finally the *Shudras* (menial labor and typically low-end jobs).

[7]Quotas for economic and educational institutions have been extended to a third category of low-caste communities known as the "Other Backward Classes" (OBCs) since the early 1990s. This group, comprised primarily of the "Shudra" *jatis*, was not burdened with the stigma of "untouchability," but considered socially and educationally "backward" and thus worthy of government assistance.

[8]A large literature has developed over the past two decades estimating the economic and political impacts of the Indian quota policy, with generally positive, but conflicting, results regarding their efficacy (Pande 2003; Besley, Pande, Rahman, and Rao 2004; Jensenius 2015; Bhavnani 2017).

Janata Dal (JD) and the Bahujan Samaj Party (BSP).[9] Rather than restricting themselves solely to constituencies set aside for SC/STs, both parties sought electoral success across multiple states and nationwide in order to better advocate for the favored policies of low caste citizens.These parties developed in response to persistent caste-based discrimination, as well as the perceived failure of the quota system (and mainstream parties contesting reserved seats within state and local assemblies) in addressing needs of low-caste communities (Jaffrelot 2006; Jaffrelot 2011).[10]

The BSP and JD (and later regional off-shoots of the JD) have had similar political goals over the last three decades. Redressing caste-based injustice called for both redistribution of resources (such as public sector employment, educational resources, and the like), as well as equal recognition of civil rights (Sarkar and Sarkar 2016; Jaffrelot 2006).[11] One of the major policy platforms of these caste-based political coalitions was reducing targeted crimes against SC/STs. For example, the Prevention of Atrocities Against SC/STs Act of 1989 was implemented soon after the JD-led federal government was established.[12] Using historical sources to account for the regional fragmentation of the JD and the Left Parties over the past two decades, we classify a total of 46 political parties as "low-caste political parties" that focus on representing the interests of low-caste constituents, including SC/ST caste groups.[13]

Low-caste parties may improve representation of the interests of SC/ST citizens for a two reasons. First, the majority of the legislators in these parties themselves hail from low caste groups (Jaffrelot 2006). If SC/ST citizens have distinct policy preferences, which are best represented by members of SC/ST communities (Duflo 2004), then parties with a greater share of SC/ST members may better represent the interests of this community. Second, Jensenius (2015) through extensive field surveys of Indian politicians, finds that a politician's ability to influence policy (particularly on behalf of the disadvantaged) depends largely on political party membership. SC/ST politicians from mainstream parties elected through the electoral quotas to represent reserved constituencies often cite an inability to cater to SC/ST groups due to the lack of support from their respective political parties. This is not surprising, given that mainstream parties such as the BJP and INC do not make SC/ST interests a core part of their party platforms. In contrast, low-caste parties have platforms focused on the interests of low-caste citizens, and so arguably provide SC/ST constituents with a better chance of having their interests represented. In Pitkin (1967)'s

[9]SCs and the so-called "Other Backward Castes" (OBCs) were at the forefront of this movement, as they constituted larger population shares than STs.

[10]As recently as the early 1990s, researchers note that "[n]either in family relations, nor in market relations and nor in political relations has democratization of civil society" (Ilaiah 1994).

[11]In related work, the JD, BSP and their regional offshoots are referred to as "Affirmative Action parties" (or "AA" parties), given their support for affirmative action (Chakravarthi 2014).

[12]This act increased punishment for targeted SC/ST crimes, although the proposed law was initially tabled in Parliament by the INC in 1988. Its enactment was facilitated by the JD-led "National Front" government in 1989, after their victory in the following round of federal elections.

[13]Our classification of multiple parties sharing broad objectives under a single category is similar to Meyersson (2014), who studies the effect of Islamic parties in Turkey. A more detailed discussion of low-caste parties and their political goals and coalitions is provided in Appendix B.1.1. We also note that our classification of parties overlaps substantially with the classification of Besley and Burgess (2554), who categorize the "soft left" parties in India.

categorization of political representation, low caste parties provide *substantive* representation to SC/ST communities.

2.2.2 Criminal Justice Administration in India

Under the Indian Constitution, state governments have the primary responsibility for maintenance of public order. Caste-based parties representing SC/ST interests in state government can thus shape justice administration in a manner that improves protection from targeted crime (Unnithan 2013). The Code of Criminal Procedure (CCP) guides the operation of the criminal justice system in India. According to the CCP, all information given to the police must be included in a written "First Information Report" (FIR) filed by the police officer, which must then be signed by the complainant. Once the FIR has been filed, the police investigate the crime, and decide to arrest any suspect. The police must then deliver the results of its investigation to a magistrate, who decides if there is sufficient evidence to charge an accused criminal. If so, a formal "chargesheet" is prepared, providing details of an alleged offense. Public prosecutors can then bring cases to trial, depending on the quality of evidence. State politicians however have limited influence on the judiciary, which is considered insulated from political pressures.

2.2.3 Conceptual Framework: How Politics Affects Crime & Violence

Minority political power can affect caste-motivated crime by affecting the administration of justice, or by changing inter-caste attitudes. To highlight the potential channels at play, we consider a simple framework similar to those described in Donohue and Levitt (2001) and Iyer, Mani, Mishra, and Topalova (2012). An offender first decides whether to commit a crime (Stage 1). A victim then decides whether to report the incident (Stage 2). Finally, law enforcement agencies decide how to bring a perpetrator to justice (Stage 3). To show how politics can affect crime in our setting, we consider the changing incentives of the relevant actors at each stage.

Given the primary authority of state governments over public order issues, politicians from low-caste parties shape how police respond caste-based violence—leading to more rigorous investigations and more arrests for such offenses.[14] Moreover, because states are responsible for recruiting police, low-caste party legislators can increase the hiring of SC/ST police officers, or influence their promotions to positions of influence within local police departments (Sarkar and Sarkar 2016). State governments also have the authority to appoint public prosecutors and hire assistant prosecutors, who handle criminal cases once they reach court. Legislators promoting the social welfare of SC/STs can thus influence the vigor with which cases are prosecuted in court. Any of these effects would increase the cost of crime

[14]Senior police officers (Indian Police Service officers) in India are federal civil servants recruited on a nationwide basis, and are then assigned to specific states by which they are controlled. Recent research demonstrates other ways that state-level politicians exercise significant control over public officials such as federal officers (Iyer and Mani 2012). Anecdotal evidence suggests the transfer of police officers and bureaucrats are often amongst the first administrative decisions made by low-caste parties when they come to power at the state level (Jaffrelot 2011).

for a potential offender. Collectively, we refer to these changes in various aspects of justice administration as an "enforcement effect."

Changes in how state actors respond at the prosecution stage of a criminal incident can affect SC/ST victims' decisions at Stage 2, the reporting phase. Increased attention of law enforcement to SC/ST communities increases the returns to reporting future incidents of abuse perpetrated by upper castes, since reported criminal cases are now more likely to be resolved in SC/ST victims' favor. We call this the "reporting effect" of minority representation. This channel is well-documented in seminal work by Iyer, Mani, Mishra, and Topalova (2012), who find that female legislators in local assemblies lead to increased reporting of gender-biased crimes. Alternatively, the presence of elected representatives for SC/ST communities may also increase reporting by inspiring greater self-confidence and reduced tolerance for being mistreated—a "role model effect."

Finally, at the first stage of a criminal episode, when an offense takes place, the aggregate effects of greater minority political representation (on either police or victim behavior) may deter potential offenders. Deterrence would lead to a decline in the true number of crimes against SC/ST individuals. However, greater political power for groups that have been historically discriminated against may also produce an increase in targeted violence for other reasons. If potential high-caste offenders experience resentment due to either increased power or assertiveness among low-caste communities, high-caste individuals may commit *more* caste-motivated crimes (a "retaliation" effect). Such retaliation may be part of a strategy to intimidate leaders and voters.[15]

As the discussion highlights, the net effect of minority political representation on documented violence against SC/ST populations is directionally ambiguous. If the deterrence effect dominates, one would expect to observe a decline in crimes against these subpopulations. However, this crime decline may be masked if the law enforcement, reporting and retaliation effects outweigh the deterrence effect, and we would actually observe an increase in the total number of documented crimes against low-caste minorities.

Finally, the overall ability of minority-favored politicians to deliver public goods or legislative policy can affect targeted crimes. Limited research suggests that the competence (human capital) of politicians from low-caste parties has occasionally come into question (Acharya, Roemer, and Somanathan 2015). To the extent that legislators from caste-based parties are less effective, either due to an intrinsic lack of quality, or overall inexperience as these are relatively new parties, one might think that they are *less* likely to be able to offer protection to underrepresented minorities.

2.3 Data and Empirical Strategy

To identify the effects of increased legislative representation from low-caste political parties on caste-based violence against SC/STs, we combine official crime statistics with exogenous variation in party representation arising from close elections between low-caste and mainstream non-low-caste party candidates. We construct a state-by-year panel for 16 states from 1994 to 2012, merging data from three main sources: government crime reports, self-

[15]This response was observed in the state of Bihar during the 1990s, when private armies of upper castes clashed against an increasingly assertive Dalit populations following JD electoral success (Ankit 2018).

reported survey data on trust and inter-caste conflict, and constituency-level election returns. Additional data is also sourced from the decennial Indian Censuses.

2.3.1 Data

Crime & Criminal Justice Data: We collect targeted caste-based violent crimes data (our main outcome) by digitizing the "Crime in India" publications issued annually by India's National Crime Records Bureau (NCRB). These data are aggregates based on FIRs submitted by police stations across the state. Since the early 1990s, the NCRB has released statistics on the number of violent crimes perpetrated specifically against SC/STs by non-SC/STs.[16] As discussed by Sharma (2015), these data are strictly for crimes where the victim belongs to an SC/ST caste group while the perpetrator belongs to a non-SC/ST caste group.

The use of administrative crime statistics in our setting raises a few common concerns related to data quality. In particular, underreporting by victims and poor record-keeping by police make problematic the use of police-recorded crime statistics. For example, disadvantaged populations may have a lower propensity to report crimes if they perceive the police and government to be unresponsive to their grievances. The issue of separating the number of crimes committed from the improved reporting of static crime levels is a common concern raised in the empirical literature about crime in India. To the extent that we observe crime *decline*, we believe that our estimates understate the true crime-reducing effects of improved government representation. Positive shocks in minority representation should induce *more* reporting of targeted crime, rather than less. We provide evidence to this effect later in the analysis. Nevertheless, to increase confidence in the validity of our point estimates, we follow conventions in similar studies by focusing on our analysis on SC/ST-targeted murder; Girard (2017), Iyer, Mani, Mishra, and Topalova (2012), Prasad (2012) point out that the reporting of murder is much less likely to suffer from measurement error related to underreporting than other identity-based crimes, such as rapes, due to the presence of evidence — a physical body.[17]

For robustness and mechanisms-testing, we also collect data on other caste-targeted crimes (rape, assault, robbery, and arson), total murders (irrespective of caste), and crimes reported under India's two main hate crime laws — the 1955 Protection of Civil Rights Act (PCR) and the 1989 Scheduled Castes and Scheduled Tribes Prevention of Atrocities Act (POAA). These latter two laws were special social enactments to safeguard the interests of SC/ST groups, and defined specific crimes against SC/STs as "atrocities" with specific punishments attached to deter such crimes.[18] Finally, we collect additional data on other criminal justice variables from the NCRB, such as crime-specific arrest rates and prose-

[16]To our knowledge, these statistics do not exist for targeted crimes committed against OBCs. We include the following states: Andhra Pradesh, Assam, Bihar, Chhattisgarh, Gujarat, Haryana, Himachal Pradesh, Jharkhand, Karnataka, Kerala, Madhya Pradesh, Maharashtra, Orissa, Punjab, Rajasthan, Tamil Nadu, Uttarakhand and Uttar Pradesh. These states cover 85 percent of India's population and account for the vast majority of its economic activity. Out of these states, Chhattisgarh, Jharkhand and Uttarakhand were formed in the year 2000.

[17]Moreover, Dalit-targeted murders are frequently discussed in the literature about caste-related abuse (Mangubhai and Lee 2012; Hanchinamani 2001).

[18]Examples of banned activities include forcing members of an SC/ST to eat an offensive substance (such as feces), parading SC/ST persons naked, or compelling them to engage in bonded labor or scavenging jobs.

cutor/court activity, which allow us to test whether politicians affect crime reductions by improving justice administration (and by extension, increasing deterrence).[19]

Election Data: Our explanatory variable of interest is the level of SC/ST representation within a state assembly (the Vidhan Sabha) from low-caste parties. We contend that this provides a reliable measure of substantive representation for SC/ST citizens. We construct state-by-year measures of SC/ST political representation using data from the Election Commission of India, which maintains detailed results for each electoral constituency within a given election cycle.[20] The electoral data allows us to determine the candidates contesting each election within a given state and year, their party affiliations, and votes received by each candidate. For all races, we categorize candidates as being from a low-caste or mainstream political party, based on the classification of parties described previously (as well as in Appendix B.1.1).

We use the vote counts to calculate the winning and losing candidates, their parties, and the winning vote margin. We subsequently aggregate to the state level the fraction of elections won by each political party in an electoral cycle. As described in greater detail in the subsequent subsection, we also use information on vote margins to calculate the fraction of *close* wins for each party, at multiple bandwidths of narrow victory margins to create our measure of quasi-random variation in low caste representation through low-caste parties. State elections occur at five-year intervals, so SC/ST representation (as measured by low-caste party success) is fixed within an election cycle. Election years vary across states; in a given year, elections for state legislatures are held in 4-5 states nationwide.

Self-Reported Data on Caste Conflict & Trust in Public Institutions: As discussed, misreporting is a concern with administrative crime data that we attempt to mitigate by focusing on SC/ST murders. We however also explore an alternative approach by examining data on self-reported inter-caste conflicts. While there exists no representative victimization survey in India, the Indian Human Development Survey (IHDS) collects limited information regarding local violent caste-conflicts and trust in public institutions. The IHDS is a nationally-representative household survey covering 40,000 households across all Indian states.[21] The survey tracks households over time, with data collected in 2004-05 and 2011-12. While the primary focus of the IHDS is on household income, expenditures and health indicators, they also inquire about village-level inter-caste conflicts and confidence in government institutions. We construct dummy variables for whether an individual within a household experiences a high degree of violent inter-caste conflict, as well as dummy variables for "high confidence" in the following government institutions: police, the judiciary, state government and elected politicians. Details of how this measure is constructed are contained in Appendix B.2.1.

[19] We exclude the state of West Bengal from our primary analysis, as the data has little variation, and often records no caste-targeted crimes. As this is highly unlikely in the light of other data such as self-reported caste-conflict data in the IHDS, we interpret the large number of 0s in the West Bengal data as measurement error and omit the state from our sample. However, as Appendix B.4 shows, our findings are not affected by this choice.

[20] A constituency is the administrative area that a state legislator represents in the United States. One might consider a constituency analogous to, for example, a district represented within the state general assembly.

[21] The data is sampled across 1,503 villages and 971 urban neighborhoods.

Using this data, we first examine whether the impact of low-caste party legislators on violent inter-caste conflicts is consistent with their impact on targeted murders against SC/STs, and second, whether the presence of legislators from caste-based parties leads to increased trust in public institutions, particularly among lower-caste households. This data thereby allows us to both probe mechanisms (i.e., how SC/ST victims may respond to crime) and indirectly analyze the likelihood of crime underreporting of targeted crimes.

Additional Data Sources: To both demonstrate the exogeneity of the low-caste representation instrument, and generate state-level covariates for our regressions, we use data from decennial Censuses of India conducted in 1991, 2001 and 2011. Data for intervening years are constructed using linear interpolation. We create six state-level measures — gender ratio, literacy rate, population density, fraction of urban population, fraction of workers in the population, and the fraction of population who are SCs or STs — that are used by Dreze and Khera (2000) to predict violent crimes in India.

Descriptive Statistics: In Table 2.8, we provide summary information on state-level electoral characteristics. Geographically, we observe wide variation in party representation and political competition. On average, there are 200 state-level elections in a given election year, each of which is contested by multiple parties. Some legislatures are quite small. Jharkhand, for example, had two sets of elections for the state assembly over our data period for 81 constituencies. Uttar Pradesh on the other hand, elected over 400 legislators in each of five elections. Our data covers 19,034 elections between 1990 and 2012 with at least one low-caste party contesting an MLA electoral contest. Electoral outcomes are decided by the first-past-the-post principle, with the winner on average receiving 45 percent of the votes.[22] Of these, 4,830 elections (around 25 percent) are "close" (decided by less than a 4-percent margin of victory between the winner and runner-up, based on an optimal bandwidth calculations implemented in line with Cattaneo, Jansson, and Ma (2018)). 1,570 of these involve a low caste party and a non-low caste party, out of which the former win 48.5 percent of the time.[23]

There is substantial cross-sectional and temporal variation in the electoral success of low-caste parties and the number of close elections at the 4% threshold involving low-caste party candidates (see Figures B.1 and B.2 in Appendix B.1.1). On average, states witness 31 close elections at the 4% margin per cycle featuring a low-caste party against a non-low-caste party. Looking at some of the states individually: of Uttar Pradesh's (average) 418 elections, 109 were close and pitted a low-caste party against a traditional party candidate. We also observe variation in the strength of the traditional political powers – the BJP and INC.

On average, there are 32 targeted murders per ten million SC/ST persons, across all states. This figure is driven largely by targeted murders against SCs. The state of Uttar

[22]Typically, each election involves multiple independent candidates who are able to garner a very small number of votes. We control for the effective number of political parties contesting elections to account for political competition, in line with Laasko and Taagepera (1979).

[23]We exclude 430 close elections involving 2 low caste parties Conceptually, this can be thought of in the LATE framework of an instrumental variable. Close elections involving 2 low caste parties are the "always compliers" with the randomness in the outcome of close elections not altering the identity of the legislators. Likewise, close elections involving 2 mainstream parties are the "non-compliers." The paper's focus is on the "switchers" – where given a coin-flip, we have a low caste party legislator elected, rather than a non-low caste party legislator.

Pradesh (UP) observes the highest levels of inter-caste violence, with an average yearly SC/ST-targeted murder rate above 70 per ten million SC/ST persons (1 standard deviation above the mean). There has been little change in the average annual targeted murder rate during the period of our study. This is in contrast to the aggregate state-level murder rate (irrespective of the caste of the victim or the perpetrator) which declined from 366 to 283 (per million persons). On average, then, caste-targeted murders comprise about 10 percent of aggregate murders recorded at the state-level.

2.3.2 Empirical Strategy

We motivate our empirical strategy with a simple ordinary least squares (OLS) framework that relates SC/ST-targeted crime to low caste representation achieved through the electoral success of low-caste parties:

$$CrimeRate_{st} = \alpha_s + \delta_t + \phi_{t-y} + \beta ShLowCasteWin_{st} + \gamma \mathbf{X}_{st} + \epsilon_{st} \tag{2.1}$$

The outcome variable, $CrimeRate$ is the incidence of targeted crimes against SC/ST victims in state s and year t, normalized by the state's SC/ST population (i.e., the state crime rate).[24] $ShLowCasteWin$ is the explanatory variable of interest, and denotes the fraction of elections won by low-caste parties in a state. β estimates the impact of a 1 percentage point increase in the fraction of elections won by low-caste parties on the crime rate. A negative β coefficient corresponds to low-caste party legislators reducing targeted crimes against SC/STs. $\mathbf{X}$ is a vector of state-specific factors that may also affect caste-targeted crimes. α and δ are state and year fixed effects, while ϕ indicates the year-within-electoral-cycle fixed effect to control for political factors affecting crimes.[25] As elections are typically held every 5 years, $ShLowCasteWin$ is constant within an electoral cycle.[26] If an election occurs in state s in year y and the subsequent election occurs in year $y+5$, the explanatory variable is constant for all years t in which $y \leq t \leq$y+5.

It is possible that unobservable factors may be correlated with SC/ST-targeted crime, as well as with the success of low-caste party candidates within a given state. Such factors would bias our estimate of β. For example, states that elect more SC/ST-favorable legislators from low-caste parties may already be favorably disposed towards SC/ST communities. If such attitudes are also shared by law enforcement agencies, they may already be vigorous in their protection of SC/STs from targeted crimes. An estimate of the effect of political representation, β, on SC/ST-targeted murder would be negatively biased. Another potential problem for identification is that in areas where crimes against minorities are policed more carefully and prosecuted more vigorously, SC/STs might feel more empowered to join low-

[24]For robustness we also use crime rates expressed in natural log terms, although we do not focus on this approach because of zero-crime observations. Log-transformed crime measures can be useful to account for nonlinearities in the relationship between violent crime and its determinants (Fajnzylber, Lederman, and Loayza 2002). As Table 4 shows, the results are similar.

[25]For instance, incumbent legislators might attempt to influence law enforcement agencies to artificially deflate *reported* crimes for electoral gains.

[26]While the usual term of a state legislative assembly is 5 years, legislative assemblies can be dissolved prior to this if the existing government no longer has a legislative majority and no other party or political coalition has a legislative majority.

caste parties and run for office, leading to greater electoral success of low-caste parties. This would again induce a downward bias in our coefficient of interest due to reverse causality. Alternatively, though, SC/STs may be more likely to turn to low-caste favoring politicians *because* SC/ST-targeted crimes are trending upwards, positively biasing the estimated coefficient.

To resolve concerns about non-random selection of SC/ST-preferred politicians, we use the variation in low-caste party success arising from close contests between low caste and traditional party candidates. This "close election" regression discontinuity (RD) approach is common in economics and political science to examine the effect of politician traits on economic outcomes (Ferreira and Gyourko 2009; Meyersson 2014). In our setting, politicians represent constituencies (sub-state administrative units), while the outcome data — SC/ST-targeted crime—are reported at the state level. We thus adopt the fuzzy regression discontinuity design used by Clots-Figueras (2011) & Bhalotra and Clots-figueras (2014), instrumenting for overall low-caste party presence with fraction of *close* elections in which low-caste parties won against mainstream (non-low caste) parties. We also control for the total number of low-caste party-vs.-non-low-caste party close contests.

The premise underlying our approach is that the identity (party) of the winner of a close election is quasi-random. To see this, note that in a first-past-the-post electoral system, the likelihood of being elected is a function of the vote difference between the winner and runner-up, which changes discontinuously at 0, where the outcome of the election changes from defeat to victory. As the difference in vote margin approaches that discontinuity, constituencies in which an low-caste party wins by a small vote margin are increasingly similar in political preferences to constituencies in which traditional parties (the BJP or INC) win by a small margin (Lee 2001; Pettersson-Lidbom 2001). If such constituencies are also comparable on other dimensions, the only factor which varies across such constituencies is the identity of the winning party.

We then aggregate these constituency-level discontinuities to the state level at which the outcome variable is measured. We construct our instrument – *ShLowCasteCloseWin* – by scaling this aggregation of close wins by the total number of close contests. For *ShLowCasteCloseWin* to be a valid instrument, it should be a strong exogenous predictor of *ShLowCasteWin* (first stage), and affect the crime rate solely through its impact on *ShLowCasteWin* (exclusion restriction). We provide a visual intuition of the first-stage distribution in Figures 1 and 2. Figure 1 presents a density plot of the fraction of close elections won by low-caste parties in the state, and shows that the distribution is centered around 0.5. This is what we would expect – if each candidate in a close election had an equal chance of winning, low-caste parties would win half in expectation. Our strategy essentially exploits deviations from this expectation, which comprise the tails around 0.5 in this distribution.

Figure 2 further supports our contention by exhibiting that the actual number of close elections won by low-caste parties in the state is well-approximated by a linear function equal to half the number of close elections contested by low-caste parties in the state. The y-axis depicts actual number of close low-caste party wins at the 4% margin while the x-axis depicts the expected number of close low-caste party wins at the 4% margin, based on half the number of close elections contested by low-caste parties. The red dashed line provides a linear fit between the two variables while the green line is the 45-degree line where actual close wins equal expected close wins. We see that most of the points in Figure 2 lie around

the 45-degree line and the linear fit is also very close to the 45-degree line, signifying a strong positive correlation between the two variables. With regard to the exclusion restriction, we note that as electoral success in a close election only results in the election of one additional legislator, the only channel in which *ShLowCasteCloseWin* can affect the outcome of interest is through *ShLowCasteWin*.

Using the variation in aggregate close low-caste parties wins, we implement an instrumental variables (IV) strategy similar to Clots-Figueras (2011) with the following two-stage least-squares (2SLS) approach:

$$CrimeRate_{st} = \alpha_s + \delta_t + \phi_{t-y} + \beta ShLowCasteWin_{st} + \eta ShLowCasteClose_{st} + \gamma \mathbf{X}_{st} + \epsilon_{st} \tag{2.2}$$

$$ShLowCasteWin_{st} = \alpha_s + \delta_t + \phi_{t-y} + \pi ShLowCasteCloseWin_{st} + \eta ShLowCasteClose_{st} + \gamma \mathbf{X}_{st} + \epsilon_{st} \tag{2.3}$$

Equation 2.3.2 is the first-stage of our empirical set-up, where we use the fraction of close elections won by low-caste political parties to predict the overall fraction of elections won by these parties. Equation 2.2 is the second-stage, where β is the coefficient of interest. The identifying assumptions to interpret β as causal are that (a) the outcome of a close election is "as good as random", and (b) conditional on the number of close elections contested by low-caste parties in the state, total close elections won by low-caste parties is uncorrelated with state-level crime, except through its impact on the fraction of legislators elected from low caste parties. As the incidence of close elections is non-random, and determined by the underlying political environment (such as candidate ability and intensity of political campaigns), we control for the share of close elections (*ShLowCasteClose*) contested by low-caste parties in the state in all our specifications.

We determine the threshold at which an election is considered to be "close" using the bandwidth selection method proposed by Cattaneo, Jansson, and Ma (2018). Based on this approach, elections are classified as "close" if the margin of victory between the winning low-caste (non-low-caste) party and losing non-low-caste (low-caste) party is less than or equal to 4 percent of the votes cast.[27] We verify that our results are not sensitive to the choice of election margin threshold, using alternate definitions of close contests defined at the 2, 3, and 5 percent thresholds.

2.3.3 Validity Checks: Verifying the Exogeneity of the Representation Instrument

Before moving to our findings, we provide evidence to suggest that our measure of SC/ST substantive representation is quasi-exogenous. First, we use the test proposed by McCrary (2007) to examine whether the density of the forcing variable — low-caste party win margin

[27] Close elections where both the winner and the loser come from a low-caste party are excluded from the sample. Based on the distribution of constituency level electoral outcomes, the density tests occasionally suggest a close election bandwidth of 5-7 percent but we choose a more conservative threshold of 4%, and test the sensitivity of our results to alternate close election bandwidths.

— is discontinuous at the threshold of 0 where the outcome of the election switches from a low-caste party defeat to a low-caste party victory. The absence of a discontinuity decreases the likelihood of non-random sorting of low-caste party candidates into treatment (victory) and control (loss) status. The results are presented in Figure 1. The running variable is partitioned into 0.2 percent bins for the interval between -0.5 and 0.5, and we plot the density of the number of observations in each bin (500 bins). Visually there is no discernible discontinuity in Figure 1, and the formal discontinuity estimate from the McCrary (2007) test also fails to reject the null of no discontinuity at the cutoff. We thus cannot reject discontinuity in the running variable at the win/loss threshold. We further test for the smoothness of constituency-level observables across the low-caste party victory margin at the cutoff 0, similar to Meyersson (2014). We plot unconditional means of several constituency-level covariates corresponding to 8 percent low-caste party victory margin bins, and smoothness at the win-loss threshold is evidence of the absence of winner-sorting.[28] The results are shown in Figure 2. The vertical broken line indicates the cutoff point of 0, and a local linear polynomial with a 95 percent confidence interval is used to plot the relationship between the covariate and low-caste party victory margin on either side of the cutoff. We observe from Figure 2 that the plots are smooth for all the constituency-level characteristics. The test statistics suggest the same.[29]

Our empirical strategy hinges on aggregating close low-caste party wins to the state level, and for this to be a valid identification strategy, the fraction of close elections won by low caste parties in a given state and year should not be predicted by other state characteristic (Clots-Figueras 2011). We thus also provide evidence that our instrument for the state-level electoral success of low-caste parties is also uncorrelated with state-level covariates. To this end, we regress the fraction of state-level close wins by low-caste parties (conditional on total close contests) on state-level demographic and political covariates. If our instrument is indeed quasi-exogenous, we would expect no covariate to be significantly predictive of the fraction of low-caste party close wins.

Results for this check of exogeneity are presented in Tables 2a and 2b. Each column presents the coefficient from regressing the fraction of low-caste party close wins (at the 4% threshold) on a state-level covariate, with state and election year fixed effects.[30] Table 2a shows that no demographic covariate (literacy rate, fraction of SC/ST population share, adult literacy rate, fraction of children, fraction of workers or fraction of urban population) with the exception of gender ratio (significant at the 10 percent level) predicts the fraction of close elections won by low-caste parties in the state. Similarly, state-level political characteristics in Table 2b also do not explain the quasi-random variation in low-caste representation. Importantly, we demonstrate that the fraction of close elections won by low-caste

[28]We show results for the following covariates: low-caste party vote share, registered voters (logged), voter turnout, the number of parties contesting an election, constituencies reserved for SC/ST candidates, fraction of male winners, fraction of SC/ST, and age. For the final two covariates, we only have data in the 2005-2011 period.

[29]Test statistics for each covariate discontinuity estimate available upon request.

[30]Tables B.1 and B.2 in Appendix (B.4 show the results with the exclusion of the state and election year fixed effects, which as expected, explain a substantial fraction of variation in the data. With the exception of the state literacy rate, no other political or demographic covariate significantly predicts the fraction of close low-caste party victories at the 4 percent margin.

parties in the current electoral cycle remains unaffected by a) prior political power of low-caste parties (column (1)); b) prior electoral support for low-caste parties (columns (2)-(4)) and contemporaneous electoral support to low-caste parties (column (5)); c) fraction of close elections contested by low-caste parties (columns (6) and (7)); and d) contemporary political participation and electoral competition (columns (8)-(10)).

Collectively, these tests suggest that our instrument for state-level electoral success of low-caste parties is orthogonal to state-level demographic and political characteristics. As such, we can interpret our estimate of β from Equation 2.2 as the causal effect of minority representation.

2.4 Main Results

2.4.1 Baseline Results: SC/ST-Preferred Representatives Reduce Targeted Crimes

Our baseline results are presented in Table 3. The estimates are weighted by state population and standard errors are clustered by state. We begin with the OLS specification in Equation 2.1, regressing targeted SC/ST murder rates on the fraction of elections won by caste-based parties. Column (1) reports the coefficients obtained from estimating a parsinomious version of Equation 2.1 including only the fixed effects, as well the fraction of close elections in the state involving a low-caste party; column (2) includes covariates.[31] The relationship between low-caste party representation and crime is negative but imprecisely estimated, and also declines in magnitude with the addition of covariates, indicating that state-level characteristics affecting targeted murder rates are negatively correlated with the share of elections won by low-caste parties in the state, biasing our coefficient downwards (away from 0). Columns (3)-(6) present the results based on the IV specification outlined in Section 2.3.2, where we instrument the fraction of elections won by caste-based low-caste parties in the state with the fraction of close elections won by low-caste parties. Column (3) again excludes state controls, and column (4) includes them, although the covariates have little impact on either the magnitude or precision of our estimates.

Using the IV specification, we consistently identify a statistically-significant crime-reducing effect of SC/ST political representation via caste-based parties. Column (4) presents the estimate for our preferred specification. In terms of magnitude, our analysis suggests that a 24 percentage-point increase (one standard deviation) in the fraction of elections won by low-caste parties in the state results in a causal decline of 11 SC/ST-targeted murders per ten million SC/ST persons – equivalent to 0.4 standard deviation. In terms of the impact of the marginal low-caste party legislator, as the average state legislature comprises of 190 legislators, an additional low-caste party legislator reflects a 0.53 (1/190) percentage point increase in low-caste party representation, and by extension, a 0.24 (0.53*0.31) reduction in targeted SC/ST murders. Scaling up the effects, our results suggest that approximately four additional low-caste party legislators would result in one less targeted SC/ST murder.

[31] We include the following covariates in our main specification: percent of workers; literacy rate; rate of urbanization; percent of SC/ST population; percent of constituencies reserved for SC/ST; state voter turnout and a cubic in low-caste party vote share.

Consistent with the correlation evdient in Figure 5, the IV estimates are significantly larger in magnitude than the OLS estimates, suggesting the OLS results are biased toward zero. Our results in this regard are consistent with work by Rehavi (2007), Clots-Figueras (2011), and Bhalotra and Clots-figueras (2014), all of whom use a comparable empirical strategy to study the economic effects of female representation, and document that the OLS results are biased toward zero. We can think of at least two potential explanations for this directional bias. First, there may exist a positive correlation between unobservable factors affecting low-caste parties' electoral success and the incidence of targeted crimes against SC/STs. This may occur if states experiencing an increasing trend of targeted crimes also witness a higher fraction of low-caste party victories, as suggested in Figure 5. Second, the sample of elections used to generate IV estimates may be affected by positive selection. For instance, if parties can anticipate that an election will be closely-contested, then even though they cannot influence the ultimate electoral outcome, parties may nominate higher-quality candidates. When these candidates win, they may exert more effort to respond to constituent needs (in our case, protection from crime) upon being elected (Bhalotra and Clots-figueras 2014).

In Columns (5) and (6), we examine the heterogeneity of the baseline results across the specific historically-marginalized group. We observe that our effects are driven primarily through a decline in targeted murders of SCs. This finding is unsurprising for two reasons. First, as described in Section 2.3, the majority of targeted low caste murders involve victims from the SC community; as such, the margin for potential benefits from political intervention is higher among SC communities. Second, qualitative research suggests that the party mobilization of low-caste groups was significantly more concentrated among SCs than STs. As such, one might expect low-caste representation to primarily benefit SCs if we believe that elected representatives prioritize the interests of their controlling electorates.

One concern regarding statistical inference in Table 3 is the small number clusters for our sample. That we only have 18 states in the sample raises concerns regarding the potential for detecting false positives (Angrist and Pischke 2008). To alleviate this concern, we recalculate our results using the wild cluster bootstrap with 1,000 replications. Reassuringly, the z-statistic obtained with the bootstrapped standard errors is significant at the 5 percent level in column (3), and the 1 percent level in columns (4) and (5).[32]

Table B.4 (see Appendix B.4) presents the reduced-form and first-stage specifications corresponding to the baseline results in Table 3. Consistent with the IV estimates, the reduced-form coefficients in columns (1) and (2) are negative and statistically significant at the 1% level. Columns (3) and (4) confirm that we have a strong first stage: the fraction of close elections won by low-caste parties in the state exhibits a positive and statistically-significant relationship with the fraction of total elections won by low-caste parties in the state; the first-stage F-statistic is well above the conventional threshold of 10. The coefficient reflects that a four percentage point increase in the fraction of close elections won by low-caste parties (equivalent to 1 additional close election won by low-caste parties in the state) results in a 0.48 percentage point increase in the fraction of elections won by low-caste parties in the state – equivalent to approximately 1 additional low-caste party legislator being elected

[32]The exact z-statistics corresponding to columns (3)-(6) are 1.98, 2.92, 3.37 and 2.66, respectively, as we show in the appendix.

in the mean state legislature comprising of 190 legislators.

The first-stage coefficient also confirms that our IV strategy satisfies the exclusion restriction: if the outcome of close elections involving low-caste parties is truly orthogonal to all other factors, we would expect one additional close electoral race won by low-caste parties to mechanically result in the election of only 1 additional low-caste party legislator. This also rules out that the positive correlation between the fraction of low-caste party victories in close electoral races and the fraction of overall low caste party wins in the state is generated through "wave" elections in favor of low-caste parties, which could increase the vote share of low-caste parties across all races. If the outcome of close elections was indeed driven by such "wave" elections, we would have expected a low-caste party win in one close race to have a positive spillover on multiple other (non-close) electoral races involving low-caste parties.

2.4.2 Robustness

Alternative Specifications and Samples

Our primary finding that SC/ST representation reduces targeted criminal violence is robust to several alternative specifications, as shown in Table 4. Columns (1) and (2) show that the finding of reduced SC/ST-targeted murder is unchanged when we use a log transformation of the outcome variable–either in terms of rates or levels.[33] Column (1) suggests that a 10 percentage point increase in the fraction of low-caste party legislators leads to a 33 percent decline in the targeted murder rate against SC/STs; column (2) suggests that a 10 percentage point increase in the fraction of low-caste party legislators causes a 2 percent decline in the incidence of targeted murders against SC/STs.

Our results are also stable to using alternative winning vote margins that define "close" elections. Columns (3)-(5) of Table 4 show the sensitivity of our main findings to victory margins of 2, 3, and 5 percent, respectively. The results are very similar when restricting our explanatory variation to elections won at either the 3 or 5 % thresholds. In both cases, we detect a negative and statistically significant relationship between the fraction of state-level low-caste party wins and targeted SC/ST-targeted murders–similar in magnitude to the results from our preferred specification. At the more-conservative 2% threshold, the coefficient magnitude is similar, but the estimate is less precise (p-value of 0.19). We suspect that the lack of precision may be due to the limited variation in caste party electoral success in such a narrow bandwidth, reducing our ability to precisely detect a significant effect; at the 4% margin, the average state has 30 close elections, but only 15 at the 2% margin. The coefficient, however, remains similar in magnitude: a 1 standard deviation increase in the fraction of low-caste party legislators reduces the incidence of targeted murders against SC/STs by 0.33 standard deviations.

Table B.5 examines the impact of low-caste party representation on aggregate targeted crimes against SC/STs and crimes in addition to murder. The general finding of reduced targeted violence holds for total violent crime, as well as for several other specific crime categories. The top panel shows the results for targeted crimes against both SC/STs, while the bottom panel estimates the results only for SCs. As there are concerns regarding re-

[33]We add 1 the total crime count prior to the log transformation where the incidence of targeted murders in the state was 0.

porting and the severity of offenses across crime categories, we avoid a direct summation across various crime categories and instead create a standardized mean-effects index across crime categories, similar to the approach used in Kling, Liebman, and Katz (2007).[34] In column (1) of both panels A and B, we find that an increase in the fraction of low-caste party legislators in the state results in a significant decline in overall targeted crimes against SC/STs.

We also identify the impact of SC/ST-preferred legislators on other major targeted crimes. First, we examine the number of offenses which are singled out by the federal government as "special crimes" related to the persistence of caste-based practices that are outlawed today, as well as the intentional humiliation of lower castes (Sharma 2015). As we described in Section 3.3, these officially-designated crime categories are subject to specific procedures under the 1955 PCR Act and the 1989 Prevention of Atrocities Against SC/STs (POAA) legislation. Both laws aim to protect the lower castes by providing stronger punishment for some symbolically sensitive offenses than what would be the penal code ruling. For simplicity, we will refer to both types of offenses as officially-designated "hate crimes." Column (2) of Table B.5 confirms that the electoral success of low caste party legislators results in a reduction in registered official hate crimes. We find that a 1 standard deviation increase in the fraction of low-caste party legislators (24 percentage points) results in a statistically-significant decline of around 0.6 standard deviations (335 incidents) of crimes registered under these two categories. As before, the results are driven by the impact of legislators on SC-targeted crimes.

Column (3) also documents a negative causal relationship between the fraction of low-caste party legislators and targeted robberies, suggesting that low-caste party legislators are successful in reducing both the incidence of targeted murders and violent property crimes against SC/ST citizens.[35] We detect little impact of caste-based parties on rates of kidnapping, assault, or rape; the coefficient estimates suggest null effects of varying precision. However, as noted in Section 3.3, these crime categories are more likely to suffer differentially positive reporting bias in states with positive SC/ST representation shocks, and thus results should be interpreted with caution.[36] We restrict our focus for the remainder of the paper to the relationship between low-caste party representation and targeted murders against SC/STs.

In Table B.6 (Appendix B.4), we conduct several more robustness checks to verify that the results are not driven by sample or specification choices. We first show in Column (1) that the results are unchanged if we restrict the sample to states in which there has been at least 1 close election between low-caste and non-low-caste parties in every electoral cycle. While most state-electoral cycles witnessed at least 1 close election between low-caste and non-low-caste parties, there are 6 state-electoral cycles where there were no elections

[34]The targeted crime categories considered are: a) murders; b) robberies; c) dacoities; d) rapes; e) kidnappings; f) assault; g) arson; and h) crimes registered under federal hate crime statutes.

[35]We include "dacoity" incidents also under the category of robberies. A robbery is classified as a dacoity if the robbery is committed by a gang of robbers.

[36]It is worth noting that while the coefficient is imprecisely measured, the qualitative impact of low-caste party legislators on targeted rapes is equivalent to that of targeted murders: a one standard deviation increase in the fraction of low-caste party legislators reduces the incidence of targeted rapes by 0.67 standard deviations.

between low-caste and non-low-caste parties. In such states, our instrument – the fraction of close elections won by low-caste parties – takes a value of 0. We verify in column (1) that our baseline results are not driven by this choice. Column (2) displays the point estimate for the *full* sample, which includes West Bengal (which we exclude fully due to reporting concerns); we see the results are quite similar to those presented in Table 2.4.1. In column (3), we show that our results are not an artefact of including the "Left" parties within the definition of caste-based low-caste parties.[37] As these parties were not formed strictly through the political mobilization of low-caste groups, we verify that the exclusion of the state of Kerala, where the Left parties have historically been the most successful, does not affect the magnitude or precision of our results. Columns (4) and (5) verify that our results are stable to the exclusion of population weights, as well as an alternate level of clustering at the state-electoral cycle level.[38] We also verify in Figure B.3 (Appendix B.4) that our baseline results are not determined by any single state. This could be a concern as most of the parties in our sample at present are regional parties with considerable clout within their respective states but having limited influence beyond their state boundaries. We thereby re-estimate our baseline specification dropping one state at a time and present the results as coefficient plots in Figure B.3 (Appendix B.4), where we see that the exclusion of any one state do not significantly alter our results.

District-Level Caste Study: Low-caste Politics in Uttar Pradesh

As we described in Section 2.2 on the context of our study, we categorize multiple regional political parties under a single political party umbrella. We thus are implicitly assuming that every regional party that we classify as a "low case party" has similar common interests – in this case, reducing targeted violence against SC/STs. Several of these caste-based parties are breakaway factions of the JD, and made broad appeals to low-caste voters cutting across various sub-caste groups. However, their core base on constituents typically remained select lower sub-caste groups.[39] This raises a potential concern that our broad classification of low-caste parties might not be accurate in terms of homogeneously representing the interests of SC/STs. It is therefore possible that despite the political rhetoric, select low-caste parties cater only to select sub-castes and would have little interest in representing the interests of SC/STs. This draws into question the results obtained in the previous sections where we identified an average negative effect of low-caste parties on targeted murders against SC/STs.

To show that the effectiveness of caste-based political party mobilization of specifically SC/STs, and at a lower level of disaggregation, we examine the effects of caste-based political parties in a state where the focus on SC welfare was particularly pronounced, Uttar Pradesh (UP). We focus on UP because it is India's largest state, and it witnessed an extensive mobilization of low-caste citizens through the late 1980s and early 1990s. This was undertaken by two major political parties - the Samajwadi Party (SP) and the Bahujan Samaj Party (BSP).

[37] As discussed in previously Section 2.2.3, we classify Left parties within the group of caste-based since they have similar political objectives and also frequently formed electoral alliances formed with other low-caste parties.

[38] We consider this level of clustering since the variable of interest varies across state-electoral cycles.

[39] For instance, the core support group of Rashtriya Janata Dal (RJD) and the Samajwadi Party (SP) – regional powerhouses in Bihar and Uttar Pradesh respectively which split from the JD – remain the *Yadav* sub-community which is a dominant OBC group.

While both parties in political rhetoric claimed to represent the interest of underprivileged low caste citizens (OBCs and SC/STs), the SP's core support group was the *Yadav* OBC community while the BSP's support group was amongst the SCs. As the key leaders of the BSP also hailed from the SC community, we test whether the electoral success of the BSP also resulted in a reduction in targeted crimes against SC/STs.

As we focus now on a single state, our unit of observation is the district, which forms the third tier of administration in India, comparable to the US county. While the district-level data on targeted crimes against SC/STs exist only since 2001, the presence of 40 districts in UP provides us in excess of 400 annual observations across 3 electoral cycles. We now define both our independent variable of interest and the instrument at the district level, as the fraction of elections won by the BSP in the district, instrumented by the fraction of close elections won by the BSP in the district. Close elections involving the BSP thereby are elections where the difference in victory margin between the BSP winner (BSP loser) and the non-BSP runners-up (non-BSP winner) is less than 4% of the votes cast. The outcome of interest is district-level targeted murders against SC/STs, scaled by the SC/ST population in the district.

The results in Table 7 identify a negative impact of an increase in the fraction of elections won by the BSP on targeted murders against SC/STs. While the coefficients in column (1) for all SC/STs is signficant only at the 20 percent level (p-values of 0.19), it is sizeable in terms of economic magnitude: at the respective means of the independent variable and the outcome of interest, a one standard deviation increase in the fraction of elections (20 percentage points) won by the BSP in the district reduces targeted crimes by 13 murders (per ten million persons) or 0.18 standard deviations. Columns (2) and (3) yield findings similar to those in Table 3: while the coefficient in column (2) is not precisely estimated, we see that the effect of BSP legislators on targeted crimes against SC/STs is driven through its impact on targeted crimes against SCs (the coefficient is significant at the 20 % level). This is consistent with our expectations as the BSP caters primarily to the SC community.

2.4.3 Ruling Out Alternative Hypotheses

In addition to the robustness checks, we also undertake 5 placebo tests to confirm that our results are capturing the impact of low-caste parties alone on targeted murders against SC/STs. These results are are presented in Table 5. We first test whether the fraction of low-caste party legislators elected in the current electoral cycle affects the targeted murder rate corresponding to the previous electoral cycle.[40] If our observed results were generated by a spurious negative trend in the targeted murder rate and the electoral success of low-caste parties, we would expect this specification to yield a significant coefficient. Reassuringly, column (1) reports a precise 0 coefficient.

In column (2), we consider the alternative explanation that the negative relationship between low-caste party legislators and targeted murders is driven by a broader relationship between low-caste party legislators and aggregate crimes. This would be true if low-caste party legislators place a higher premium on law and order and the negative relationship identified till now is but a spillover from a secular decline in aggregate murders. The esti-

[40] As each electoral cycle comprises of 5 years, we lag the targeted murder rate by 5 years.

mated coefficient again reflects a precise 0 effect. In column (3), we examine whether our instrument is predicted by state-level observables using the Altonji-Elder-Taber (2005) test. We now instrument the fraction of low-caste party wins in the state with the *best prediction* of the fraction of close low-caste party wins in the state, based on the state-level covariates and fixed effects. We would expect a null effect from this specification if state-level observables are unable to predict the fraction of close elections won by low-caste parties in the state. The coefficient in column (3) confirms this to be the case: the estimated coefficient is positive and imprecisely estimated. As expected, the coefficient on *predicted* fraction of close low-caste party wins in the first stage is negative and statistically insignificant.

Finally, columns (4)-(5) test the impact of electoral victories accruing to the two major national parties – the right-wing Bharatiya Janata Party (*BJP*) and the centrist Indian National Congress (*INC*) and identify a null effect in both cases.[41] The coefficients are an order of the magnitude smaller than those associated with the fraction of elections won by low-caste parties. In summary, Table 5 confirms that our results are not generated by a secular decline in targeted murders against SC/STs or overall state-level murders.

2.4.4 Sources of Heterogeneity: The Role of Political Power

The results in Section 2.4.1 established that a higher fraction of legislators elected from caste-based parties result in a causal decline in the incidence of targeted murders against low caste SC/ST citizens. In this section, we examine sources of heterogeneity in our main results. We focus in particular on how politics operate in our setting and estimate heterogeneous effects of caste-based party legislators using the following specification:

$$CrimeRate_{st} = \alpha_s + \delta_t + \gamma_{t-y} + \beta_1 ShLowCasteWin_{st} + \beta_2 D^k_{st} * ShLowCasteWin_{st} + \beta_3 D^k_{st} + \phi X_{st} + \epsilon_{st} \quad (2.4)$$

In (2.4), D^k is a dummy indicating a political characteristic k on which we examine heterogeneity in our results. β_2 tests for the differential effect of an increase in low-caste party legislators in states by political characteristic k. The remaining variables are defined as as before and we instrument *ShLowCasteWin* with *ShLowCasteCloseWin*. As *ShLowCasteWin* appears twice in (2.4), we have two instruments for the two endogenous variables in (2.4).

Column (1) of Table 6 tests for the differential impact of low-caste party legislators when caste-based parties control the state government. As discussed in Section 2.2.3, belonging to the party which controls or influences the actions of the state government bestows upon legislators an additional quantum of legislative and executive power. We test whether low-caste party legislators are more effective in reducing targeted crimes against SC/ST when empowered such.[42]

We see that the direct effect of the fraction of low-caste party wins in the state remains negative and statistically significant (even when low-caste parties are not the ruling party in

[41] In both the specifications, the fraction of elections won by the BJP (INC) in the state is instrumented with the fraction of close elections won by the BJP (INC) in the state.

[42] We also consider instances when multiple low-caste parties have combined to form a government. This has occured in Kerala and Uttar Pradesh. We however exclude instances when low-caste parties have entered into coalitions with non-low-caste parties and formed the government.

the state), while the interaction is negative, albeit not statistically significant. This suggests that low-caste party legislators are more effective in reducing targeted murders when low-caste parties are the ruling parties in the state. In terms of magnitudes, the results imply that moving from a state where low-caste parties are not in power to one in which low-caste parties alone are in power results in a decline of 120 targeted murders against SC/STs (per ten million persons).[43]

Columns (2) and (3) of Table 6 present results identifying the interaction between political representation for low caste citizens through caste-based parties, versus representation through the institution of electoral quotas. Iyer et al. (2012) show that states' adoption of local reservations for SC/ST politicians resulted in a reduction in crimes against women.[44] We test whether the impact of low-caste party legislators on targeted murders against SC/STs is generated through the empowerment of local SC/ST politicians. The interaction dummy equals 1 for all the years subsequent to the state's introduction of electoral quotas in local government. The coefficient in column (2) is positive but statistically indistinguishable from 0, suggesting that the implementation of electoral quotas in local government did not affect the negative impact of low-caste party legislators on targeted murders in any direction. In column (3), we test whether low-caste parties have a greater impact on crime outcomes in states with a high fraction of reserved candidates, implying a higher fraction of legislators hailing from the SC/ST communities. The interaction dummy equals 1 if a state's share of reserved constituencies exceed the median share of reserved constituencies across states. The interaction term in column (3) is negative and almost significant at the 10 percent level (p-value is .105). The coefficients thereby suggest a complementary role of quotas within state legislatures: low-caste party legislators have a significantly higher effect on targeted murders in states where there is a higher fraction of SC/ST legislators elected through the quotas.

Column (4) in Table 6 examine the impact of low-caste party legislators by the strength of relection concerns and test whether the negative impact of low-caste party legislators on targeted murders is concentrated in the last two years of the legislature's tenure by interacting the fraction of low-caste party legislators in the state with a dummy equaling 1 in the last two years of the state legislature. The latter test also incorporates a test of whether our results are generated through a misreporting of murders against SC/STs. A potential concern regarding the baseline results is that low-caste party legislators may strategically manipulate the murder data to cause a reduction in *reported* murders (as opposed to an actual reduction) to boost their re-election prospects. The coefficient on the interaction terms is not statistically significant while the direct effect of the fraction of low-caste party legislators on targeted murders remain negative and significant, affirming that the reduction in targeted murders is not concentrated in the last two years of the tenure of the state legislators.

[43]This is calculated as the following: in the absence of low-caste parties being in power, the average fraction of low-caste party legislators in the state is 16 percent while the corresponding coefficient when low-caste parties are the sole parties in power is 65 percent. The impact on the outcome variable from moving to a state when low-caste parties are not in power to one where they are in power equals 2.59*(65-16) = 127.

[44]These reservations were implemented through a constitutional amendment in 1993 which mandated states to reserve one third of the positions in local governments for women and SC/ST candidates.

2.5 Mechanisms

We interpret our results as the reduced form effect of increased SC/ST representation through caste-based political parties, which could affect ethnic violence through improvements in criminal deterrence as well as improved attitudes toward SC/ST citizens. In this section we explore which of these mechanisms contribute to the impact of caste-based parties' representation on SC/ST-targeted murder.

2.5.1 Changes in the Behavior of Police, Prosecutors, and Judges

Political competition and SC/ST-preferred politicians may reduce targeted violence by leading politicians to influence criminal justice institutions (police agencies or prosecutors) respond to caste violence. Importantly, law and order is under the purview of state (not local) governments in India (Iyer, Mani, Mishra, and Topalova 2012). As such, it is reasonable to examine whether exogenous changes in low-caste representation at the state level affect the administration of criminal justice. We thus test whether low caste party legislators affect the actions of police agencies, prosecutors, and judges and law courts with respect to the targeted murder of SC/STs. We use data on how each of these criminal justice actors responds to SC/ST reported offenses. We note, however, that these data are only available beginning in 2001; as such, these results should perhaps be treated with more caution.

We first examine whether quasi-exogenous increases in caste-based party representation affect how the police pursue and apprehend the perpetrators of targeted caste-based murder. We do so using the NCRB arrest data for SC/ST-targeted murders. We reestimate the IV specification (Equations 2.2 and 2.3.2) using the SC/ST-murder arrest rate as the outcome of interest, instead of crime.[45] Table 9 supports suggests that SC/ST-preferred legislators do, in fact, affect police effort, consistent with crimes being reduced through deterrence channel. While we lose one-third of our original sample, we nevertheless detect a positive, statistically-significant impact of low caste party legislators on arrests for SC/ST-targeted murders. The magnitude is also non-trivial: at the mean of the dependent variable, a one standard deviation (28 percentage points) increase in the fraction of elections won by low-caste parties increases the arrest rate for targeted murders against SC/STs by 2 standard deviations.[46]

We next examine whether the increased representation of SC/ST interests affects the criminal investigation process. In particular, politics may shape how/whether chargesheets are filed in court, as well the public prosecutor's case selection and effort (Iyer, Mani, Mishra, and Topalova 2012). Since judges are assumed to be impartial and apolitical, we would not expect legislators to necessarily affect case *outcomes*. The data on case filings or court

[45]The NCRB reports this data only since 2001, we lose almost half our original sample. We define arrest rate as the total arrests divided by the number of SC/ST-targeted murders. Scaling arrests in this way accounts for the possibility that arrests may be mechanically related to targeted murders. An alternate specification would be to regress the number of arrests for targeted murders on the fraction of elections won by low-caste parties, while controlling for the murder rate against targeted crimes. The results from this specification are shown in Table B.7, (Appendix B.4) and are very similar to those obtained when the dependent variable is arrests against targeted murders, scaled by the incidence of targeted murders.

[46]This is calculated as 28*0.0978 = 2.73, which is 2 times the standard deviations of the arrest rate for targeted murders against SC/STs (1.29).

outcomes are not disaggregated by specific crime category (i.e., murder-specific arrest), but are aggregated across crimes. We thus present the outcomes in log terms, and control for the SC/ST-targeted murder rate (contemporaneous and lagged).[47] This would also control for the mechanical reduction in the filing of cases due to a reduction in targeted murders caused by the electoral success of caste-based parties.

The results in Table 10 provide further evidence that low-caste parties improve the prosecution of cases of targeted caste violence. We first examine whether low-caste party legislators reduces the likelihood that the government conducts no investigation after the filing of a police report. This outcome is a measure of prosecutorial effort to complete cases. Column (1) detects a negative effect of low-caste party legislators on the number of cases pertaining to targeted crimes against SC/STs in which no investigation was undertaken. This is accompanied by a significant increase in columns (2) and (3) in the number of cases in which a final report was filed, and the number of chargesheets which are filed in cases pertaining to targeted crimes against SC/STs. The coefficients again are economically significant – a 1 standard deviation increase in the fraction of elections won by low-caste parties (28 percentage points) leads to a 1 percent increase in the number of chargesheets filed in cases on targeted crimes against SC/STs. As the successful investigation to any criminal incidence under the Indian Penal Code culminates in the filing of a chargesheet, following which the case is taken up in the courts of law, an increase in the number of chargesheets filed shows that low-caste party legislators are able to influence the law enforcement agencies to investigate targeted crime incidences against SC/STs to a stage where the charges can be pressed in courts.

While we detect in the investigation phase of a case, we detect little effect of politics on the judiciary. Columns (4)-(7) show that low-caste party legislators have little impact on the outcome of cases in courts. All the estimated coefficients are small in magnitude and not precisely estimated. In this regard, we conclude from the results in Table 10 that low-caste party legislators are able to use their legislative influence to direct the police to vigorously investigate cases of targeted crimes against SC/STs to the extent of filing chargesheets which is the first step to prosecution in the law courts.

2.5.2 Changes in the Likelihood of Reporting

While we think that our results are unlikely to be biased by differential underreporting after positive representation shocks, we can provide evidence that this is indeed the case. As Section 2.2.3 highlights, criminal behavior against racial/ethnic minorities will also be shaped by the likelihood that victims respond by reporting crimes to the police. Marginalized communities may be less willing to report crime if either: (1) they feel that the state is not likely to respond, (2) they are unaware of what constitutes certain crimes (such as rape or assault), or (3) if they fear retaliation by forward-caste counterparts. Government representation can alter any of these conditions. Improving trust in state institutions (and the police in particular) has been one of the chief goals of caste-based political parties in India (Sarkar and Sarkar 2016).

[47] We include the lagged murder rate as the filing of chargesheets and court outcomes are often time-consuming. Moreover, Indian courts are known for their lack of expediency.

We test whether SC/ST political representation increases confidence in government institutions, including the police, courts, and politicians. To the extent that low-caste party representation improves their constituents' faith in public institutions, we believe that these tests provide indirect evidence that SC/STs' responses to potential offenders may also contribute to reductions in SC/ST-targeted crime. We use data the India Human Development Survey (IHDS), which is a nationally representative sample of Indian households that enquires about individuals' perceptions of government as well as about inter-caste conflict. We estimate the following linear probability model:

$$1((GovTrust/Conflict))_{hst} = \alpha_h + \delta_t + \beta ShLowCasteCloseWin_{st} + \eta ShLowCasteClose_{st} + \gamma \mathbf{X}_{hst} + \epsilon_{hst} \tag{2.5}$$

As the IHDS identifies the specific community identity of the household h, we estimate the results separately for SC/ST and upper-caste households, after using household fixed effects. We also present results for OBCs in Table B.8.

The results are shown in Table 8. Columns (1)-(2) restricts the sample to SC/ST households; columns (3)-(4) OBC households; and columns (5)-(6) upper caste households. For each community, the first column shows the impact of low-caste parties on violent caste conflicts and the second column shows the impact for trust in public institutions. Column (1) records a negative effect of low-caste party legislators on self-reported violent caste conflicts for SC/ST households. The coefficient estimated in column (1) using a linear probability model indicates that a 1 standard deviation increase in the fraction of low-caste party legislators elected (28 percentage points) reduces the likelihood of a violent inter-caste conflict (as reported by SC/STs) by 0.33 standard deviations. Column (2) documents that an increase in the fraction of low-caste party legislators leads to significantly higher trust in public institutions. The decline in violent inter-caste conflicts in response to caste-based parties' electoral success is also reported by upper caste households (column (5)). As both SC/ST and upper caste households report a decline in violent inter-caste conflicts, this rules out concerns regarding reporting biases and suggest an actual decline in the incidence of violent inter-caste conflicts. Moreover, it also indicates the lack of higher targeted caste violence through the retaliation effect.

Collectively, the results in Table 8 corroborate the baseline results obtained using the administrative data. Moreoever, as we identify higher trust amongst SC/ST households in public institutions, it alleviates concerns that the results obtained using the administrative data are driven by a reduction in *reported* murders as opposed to a reduction in actual targeted murders against SC/STs. If the latter was in effect, we would have expected to identify a decline in SC/ST households' trust in public institutions due to poorer recording of criminal incidents.

2.6 Conclusion

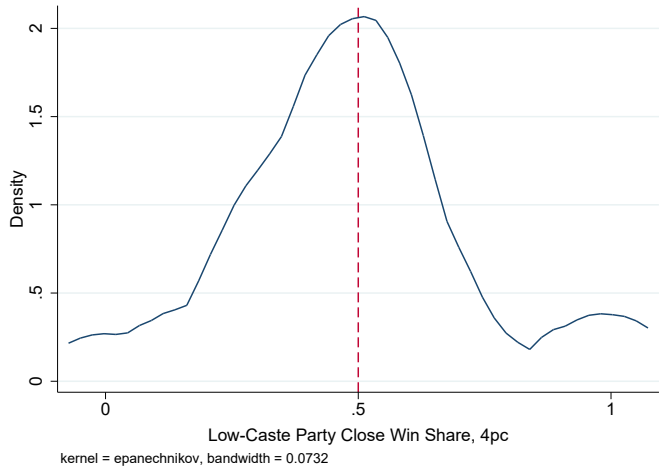
This paper provides evidence on the social effects of achieving a political voice for historically disadvantaged minorities. We estimate the impact of increased political representation through active party mobilization for India's SC/STs on murders committed against members of these groups. Using variation arising from collections of close elections, we find that

substantive political representation in state government has the effect of reducing the incidence of murder targeted against low-caste groups. Taken collectively, our results suggest that minority politicians can better protect the rights and dignity of minority citizens. The analysis of mechanisms is broadly consistent with the deterrence of potential caste-crime perpetrators due to increased minority political representation. Minority politicians can affect the enforcement priorities of government, as well as change attitudes of citizens in a manner consistent with reductions in racially-motivated crime.

Finally, we note that while measurement error is a concern for all crime data, but in particular for data such as rape and assault, where there is abundant qualitative evidence that rape is underreported, particularly by low-caste survivors (Deshpande 2011). In the presence of classical measurement error, this would likely bias our estimate toward zero. The fact that we still observe an economically significant negative coefficient for several crime categories may suggest to us that low-caste party legislators are indeed having a significant negative effect on the incidence of some violent crimes. Future work may be useful to test the validity of the results presented here using longitudinal self-reported data, such as those from victimization surveys.

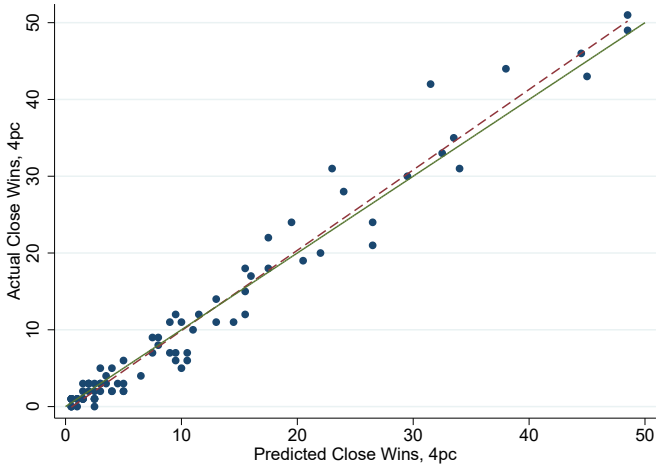
2.7 Figures

Figure 1: Distribution of Fraction of Close Elections Won by Low-caste Parties in States



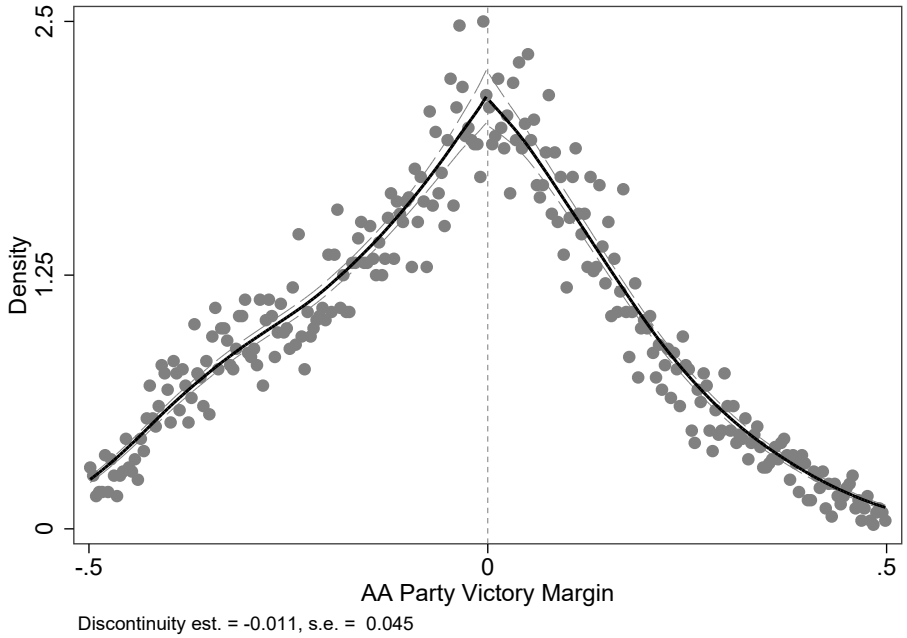
This figure presents the kernel density estimate of the fraction of close elections won by low-caste parties in the state. Close elections refer to elections where the difference in victory margin between the winner and the runners-up is less than or equal to 4 percent of the valid votes cast.

Figure 2: Expected and Actual Wins for Low-caste Parties in Close Elections



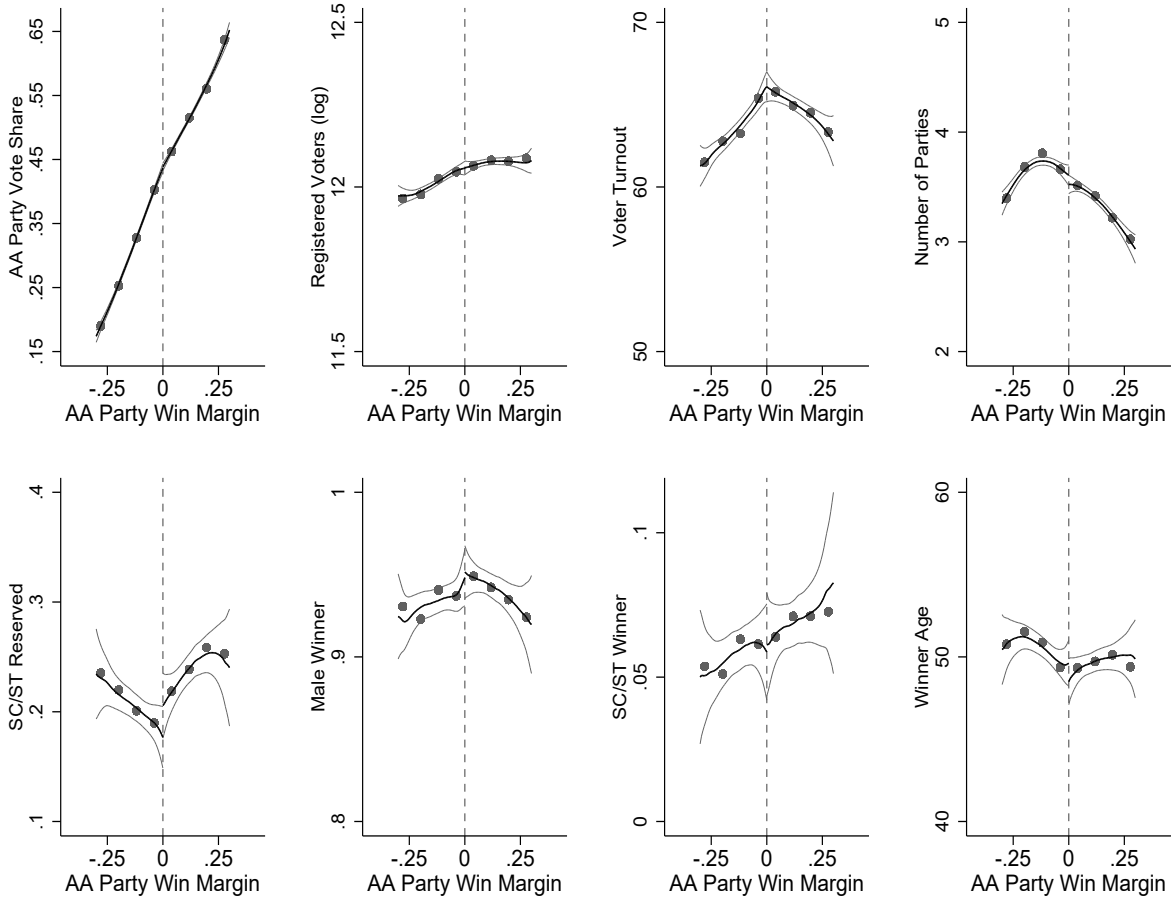
The y-axis represents actual close wins by low-caste parties in a state-electoral cycle. The x-axis represents expected close wins by low-caste parties for that state-electoral cycle. Expected close wins equals half the number of close elections contested by low-caste parties in a state-electoral cycle. The green line represents the 45 degree line where expected close wins equal actual close wins. The red dashed line is the predicted linear relationship between actual and expected close low-caste party wins. The deviation from this line provide the "unexpected" variation in SC/ST representation through low caste parties that we exploit for our analysis. Close elections refer to elections where the difference in victory margin between the winner and the runners-up is less than or equal to 4 percent of the valid votes cast.

Figure 3: McCrary Test for Discontinuity of Low-caste Party Victory Margin Around 0



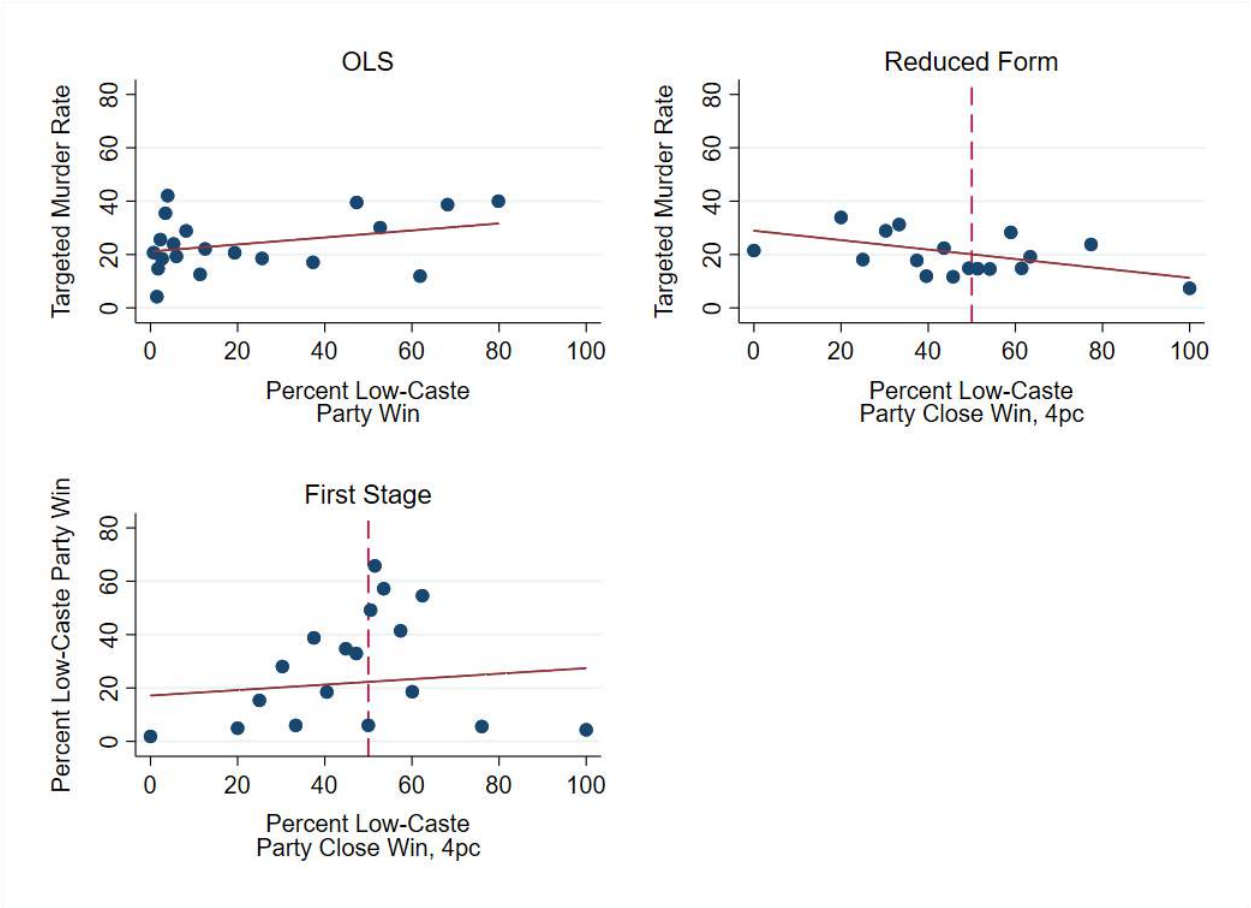
Notes: The figure plots the density of elections across 500 bins of low-caste party victory margin between -0.5 and 0.5. Each point on the plot represents the density of low-caste party victory margin in each of the 500 bins of low-caste party vote share. The solid black line is a local polynomial fit on either side of cutoff threshold 0 (represented by dashed line), where the outcome of a close election changes discontinuously from low-caste party defeat to low-caste party victory. The discontinuity estimate is -0.012, with a standard error of 0.049, suggesting there is no selective sorting around the "win" discontinuity.

Figure 4: Check of Covariate Balance Across Low-caste Party Win Margin



Covariate balance checks at the constituency level. The following covariates are tested: low-caste party vote share; number of eligible voters; voter turnout; number of parties; share of constituencies reserved for SC/ST candidates; share of male winners; share of SC/ST winners and winner age. Each point corresponds to unconditional means of each covariate in 8 percent bins of low-caste win margin between -0.3 and 0.3. The solid lines represent 95 percent confidence intervals. The dashed line represents the cutoff threshold of 0 where the outcome of an election changes discontinuously from low-caste party defeat to low-caste party victory.

Figure 5: Low-caste Party Representation and Targeted Murders Against SC/STs - OLS and Reduced Form Relationships



The y-axis in the first two figures represent the targeted murder rate against SC/STs; in the last figures, the fraction of elections won by low caste parties in the state. The x-axis in the first figure is the fraction of elections won by low-caste parties in the state; in the last 2 figures, the fraction of close elections won by low-caste parties in the state. The sample is restricted to states where low-caste parties contested in at least 1 close election. Close elections refer to elections where the difference in victory margin between the winner and the runners-up is less than or equal to 4 percent of the valid votes cast. The x-axis in each case is divided into 18 equally spaced bins and the dots represent the unconditional mean of the targeted murder rate against SC/STs corresponding to each bin.

2.8 Tables

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	N
SC/ST Murder Rate (per '00000)	32.04	27.14	297
SC Murder Rate (per '00000)	27.64	27.41	297
ST Murder Rate (per '00000)	4.4	5.98	297
Aggregate Murder Rate (per '00000)	53.81	18.27	297
Arrest Rate, SC/ST Murder	2.71	1.25	217
Arrest Rate, SC Murder	2.81	1.4	217
Arrest Rate, ST Murder	1.73	1.94	236
Police Cases, No Investigation	6.17	12.51	207
Police Cases, Final Report Filed	228.62	317.54	207
Police Cases, Chargesheet	2307.49	1994.99	207
Court Cases Completed	0.19	0.08	207
Convictions	820.34	1218.05	207
Acquitals	1449.45	1039.44	207
Cases Pending Trial	9747.96	8667.5	207
Low-Caste Party Win Share	28.07	28.05	297
Pct. AA Close Wins, 4pc	42.72	20.89	297
Low-Caste Party Close Elections Share	0.1	0.09	297
Effective No. of Parties	3.23	0.61	297
No. of Candidates	11.01	4.39	297
Voter Turnout	0.62	0.08	297
Low-Caste Party Vote Share	0.27	0.21	297
BJP Vote Share	0.2	0.14	297
INC Vote Share	0.25	0.13	297
Share Reserved Seats	0.23	0.08	297
Percent Literate	65.49	10.77	297
Percent Workers	39.6	5.14	297
Percent Urban	27.92	10.71	297
Gender Ratio	1.07	0.04	297
Percentage SC/ST	0.24	0.07	297

Note: The above table presents the summary statistics associated with the key variables from the murder, arrests, court, demographic and electoral data. The unit of observation is the state. The data is obtainedf rom the National Crime Record Bureau (NCRB), the Census of India (1991, 2001 and 2011) and the Election Commission of India.

Table 2a: Verifying Demographic Covariates Do Not Predict Low-caste Party Close Wins

	(1) Pct Workers	(2) Pct Urban	(3) Pct Literate	(4) Pct SC/ST	(5) Pct Rsvd	(6) Population Density	(7) Gender Ratio
Close Low-caste Party Win Share	-.0010	-.0019	-.0176	.0111	2.9532	-.0002	-7.9085*
	(.0228)	(.0067)	(.0129)	(.0336)	(2.4351)	(.0005)	(4.5412)
Observations	67	67	67	67	66	67	67
R^2	.80	.80	.81	.80	.81	.80	.81

Notes: This panel presents the results from regressing the fraction of close elections won by low-caste parties in the state on state-level demographic covariates. The unit of observation is the state. The outcome of interest is the fraction of close elections won by low-caste parties in the state. Close elections are elections where the difference in margin of victory is less than or equal to 4 percent of votes cast. The demographic covariates of interest correspond to the previous electoral period and are averaged across 5 years. The demographic covariates are the percentage of workers, percent of urban population, percentage literate, percentage SC/ST population, percentage of elections reserved for SC/ST candidates, population density and gender ratio. Specifications include state and election year fixed effects. Standard errors are in parentheses, clustered by state.

Table 2b: Exogeneity Check: Verifying Political Covariates Do Not Predict Low-caste Party Close Wins

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Low-caste Party Power Lag	Low-caste Party Win Lag	Low-caste Party Close Win Lag	Low-caste Party Vote Share Lag	Low-caste Party Vote Share	Close Election Lag	Close Elections	Voter Turnout	No. of Candidates	No. of Parties
Low-caste Party Close Win Share	-.0420	-.0232	-.1833	-.0224	.4817	.1523	.1909	-.2866	-.0168	-.0433
	(.1137)	(.2317)	(.2095)	(.5558)	(.6195)	(.4201)	(.6033)	(.7270)	(.0151)	(.1736)
Observations	67	66	64	66	85	66	85	85	85	85
R^2	.80	.80	.78	.80	.49	.80	.48	.48	.50	.48

Notes: This table presents the results from regressing the fraction of close elections won by low-caste parties in the state on state-level political covariates. The unit of observation is the state. The outcome of interest is the fraction of close elections won by low-caste parties in the state. Close elections are elections where the difference in margin of victory is less than or equal to 5 percent of votes cast. The political covariates are 1) a dummy equal to 1 if low-caste parties were in power the previous electoral period; 2) lagged fraction of elections won by low-caste parties; 3) lagged fraction of close elections won by low-caste parties; 4) lagged low-caste party vote share; 5) lagged fraction of low-caste party contests which were close; 6) contemporaneous low-caste party vote share; 7) contemporaneous fraction of elections contested by low-caste parties which were close; 8) contemporaneous voter turnout; 9) contemporaneous number of candidates; 10) contemporaneous effective share of parties. Specifications include state and election year fixed effects. Standard errors are in parentheses, clustered by state.

Table 3: Low-caste Party Representation and Murders Against SC/STs: Baseline OLS and IV Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	SC/ST Murder Rate				SC Murder Rate	ST Murder Rate
Low-caste Party Win Share	-.172	-.100	-.361**	-.450***	-.516***	.067
	(.159)	(.119)	(.182)	(.154)	(.153)	(.100)
Observations	297	297	297	297	297	297
R^2	.89	.90	.88	.90	.93	.66
Dep. Var. Mean	32.3	32.3	32.3	32.3	28.0	4.3
Covariates	No	Yes	No	Yes	Yes	Yes
Sample	SC & ST	SC & ST	SC & ST	SC & ST	SC	ST
Method	OLS	OLS	IV	IV	IV	IV

Notes: This table presents the results estimating the impact of an increase in low-caste parties' electoral success on targeted murders against SC/STs. The unit of observation is the state. The outcome variable in columns (1)-(4) is total targeted murders against SC/STs, normalized by the SC/ST population. The outcome variable in column (5) is the targeted murder rate rate against SCs; in column (6), the targeted murder rate against STs. The independent variable of interest is the fraction of elections won by low-caste parties in the state, corresponding to that state-electoral cycle. Columns (1) and (2) present the OLS estimates; columns (3)-(6) presents the IV estimates. In columns (3)-(6), the fraction of elections won by low-caste parties is instrumented by the fraction of close elections won by low-caste parties. An election is deemed to be close if the difference in victory margin between the winner and the runners up is less than 4% of the total votes cast. Each specification controls for the share of close elections contested by low-caste parties in the state, state, year and electoral cycle fixed effects. Columns (2) and (4)-(6) also control state-level time-varying demographic characteristics. All specifications are weighted by the state population and standard errors are in parentheses, clustered by state.

Table 4: Targeted Crimes Against SC/STs: Robustnessness Checks

	(1)	(2)	(3)	(4)	(5)
	Log Transformations		Alternate Defintion of Close Elections		
	Logged Murder Rate	Logged Murder	Close Election Threshold 2pc	Close Election Threshold 3pc	Close Election Threshold 5pc
Low-caste Party Win Share	-.033***	-.026***	-.402	-.553**	-.489*
	(.013)	(.009)	(.308)	(.244)	(.272)
Observations	297	297	297	297	297
R^2	.75	.90	.90	.89	.89

Notes: This table presents results from robustness checks of specifications identifying the impact of low-caste parties on targeted murders against SC/STs. The unit of observation is the state. The outcome variable in column (1) is the logged rate of targeted murders against SC/STs. The outcome variable in column (2) is the log number of targeted murders against SC/STs. The outcome variable in columns (3)-(5) is the rate of targeted murders against SC/STs. The independent variable of interest is the fraction of elections won by low-caste parties in the state. The fraction of elections won by low-caste parties in the state is instrumented by the fraction of close elections won by low-caste parties in the state. An election is deemed to be close if the difference in victory margin between the winner and the runners up is less than 4% of total total votes cast in columns (1)-(2); 2% in column (3), 3 % in column (4) and 5% in column (6). Each specification controls for the share of close elections contested by low-caste parties in the state, state, year and electoral cycle fixed effects, along with state-level time-varying demographic characteristics. All specifications include population weights. Standard errors are in parentheses, clustered by state.

Table 5: Targeted Crimes Against SC/STs: Placebo Tests

	(1)	(2)	(3)	(4)	(5)
	Lagged Murder Rate	Non-Targeted Murder Rate	Altonji Elder Taber Test	BJP Representation	INC Representation
Low-caste Party Win Share	.218	.452	1.653		
	(.291)	(1.365)	(9.453)		
Pct. BJP Wins				-.156	
				(.373)	
Pct. INC Wins					.112
					(.238)
Observations	225	297	297	297	297
R^2	.92	.90	.72	.92	.91

Notes: This table presents results from robustness checks of specifications identifying the impact of low-caste parties on targeted murders against SC/STs. The unit of observation is the state. The outcome variable in column (1) is the lagged murder rate corresponding to the previous electoral cycle; in column (2), the rate of non-targeted murders; and in columns (3)-(5), the targeted murder rate against SC/STs. The independent variable of interest in columns (1)-(3) is the fraction of elections won by low-caste parties in the state, instrumented by the fraction of close elections won by party in the state. In column (3), we instrument the fraction of elections won by low-caste parties with the best prediction of the fraction of close elections won by low-caste parties. An election is deemed to be close if the difference in victory margin between the winner and the runners up is less than 4% of total votes. Each specification controls for the share of close elections contested by parties in the state, state, year and electoral cycle fixed effects, along with state-level time-varying demographic characteristics. All specifications include population weights. Standard errors are in parentheses, clustered by state.

Table 6: Targeted Crimes Against SC/STs: Differential Effects for Low-caste Parties Across Political Power and Political Institutions

	(1)	(2)	(3)	(4)
	SC/ST Murder Rate			
Low-caste Party Win Share	-.582*	-.669	-.010	-.433***
	(.305)	(.539)	(.314)	(.160)
Low-caste Party Govt.×Low-caste Party Win Share	-2.116			
	(2.057)			
Local Reservation Policy×Low-caste Party Win Share		.252		
		(.487)		
Fraction Rsvd. Seats - High×Low-caste Party Win Share			-1.268	
			(.782)	
Final Two Years×Low-caste Party Win Share				-.124
				(.235)
Observations	297	265	297	297
R^2	.88	.89	.86	.89
Dep. Var. Mean	32.28	32.28	32.28	32.28

Notes: This table presents the heterogeneous effects of low-caste parties' electoral success on targeted murders against SC/STs, across political factors. The unit of observation is the state. The outcome variable is total targeted murders against SC/STs, normalized by the SC/ST population. The independent variable of interest is the fraction of elections won by low-caste parties in the state and its interatction with the political factor of interest. All results are estimated using an IV specification where the the fraction of elections won by low-caste parties is instrumented by the fraction of close elections won by low-caste parties. An election is deemed to be close if the difference in victory margin between the winner and the runners up is less than 4 percent of the total votes cast. Each specification controls for the share of close elections contested by low-caste parties in the state, state, year and electoral cycle fixed effects, along with state-level time-varying demographic characteristics. Column (1) tests for heterogeneous effects in periods when low-caste parties are the sole parties enjoying political power in the state; column (2) tests for heterogeneous effects across states which has implemented reservations for SC/STs in local governments; column (3) tests for heterogenous effects across states with a high fraction of seats reserved for SC/STs in the state legislature; and column (4) tests for heterogeneous effects in the last 2 years of the state legislature's tenure. All specifications are weighted by the state population and standard errors are in parentheses, clustered by state.

Table 7: BSP Representation and Targeted Crimes Against SC/STs: District-Level Crimes for Uttar Pradesh

	(1) Murder SC/ST	(2) Murder SC	(3) Murder ST
Share of BSP Win	-.632	-.637	2.534
	(.490)	(.492)	(7.248)
Observations	417	417	417
R^2	.64	.63	.17
Dep. Var. Mean	88.90	90.19	85.62

Notes: This table presents the results estimating the impact of BSP legislators on targeted murders against SC/STs. The unit of observation is the district. The outcome variable is total targeted murders against SC/STs in the district, normalized by the district SC/ST population. Columns (2) and (3) estimates the specification separately for SCs and STs. The independent variable of interest is the fraction of elections won by the BSP, instrumented by the fraction of close elections won by the BSP in the state. An election is deemed to be close if the difference in victory margin between the winner and the runners up is less than 4% of the total votes cast. Each specification controls for the share of close elections contested by the BSP in the state, district and year fixed effects, along with district-level time-varying demographic and political characteristics. The sample is restricted to districts in the state of Uttar Pradesh. Standard errors are in parentheses, clustered by district.

Table 8: Low-caste Party Success and Self-Reported Caste Conflicts and Trust in Public Institutions

	(1)	(2)	(3)	(4)
	SC/ST Households		Upper Caste Households	
	Caste Conflict	Trust Institutions	Caste Conflict	Trust Institutions
Low-case Party Win Share	.0617***	-.0022	.1029	-.0052
	(.0223)	(.0016)	(.0725)	(.0049)
Observations	17966	17966	13689	13689
R^2	.01	.00	.00	.00
Dep. Var. Mean	.00	.07	.00	.06

Notes: The above table presents results from specifications identifying the impact of low-caste parties on self-reported violent caste conflicts and trust in public institutions. The unit of observation is the household. The outcome variable in columns (1) and (3) is a binary which equals 1 if the individual reports a high occurrence of violent caste conflicts in the village. The outcome variable in columns (2) and (4) is a standardized index measuring trust in public institutions: namely politicians, police, state government and courts. The specifications in columns (1), (3) and (5) are estimated using a linear probability model. The independent variable of interest is the fraction of elections won by low-caste parties, instrumented by the fraction of close elections won by low-caste parties. An election is deemed to be close if the difference in victory margin between the winner and the runners up is less than 4% of the total votes cast. All specifications include household and survey year fixed effects, in addition to political and household-level covariates. Standard errors are in parentheses, clustered at level of the primary sampling unit (village or town).

Table 9: Low-caste Party Representation and Arrest Rate for Targeted Murders Against SC/STs

	(1)	(2)	(3)
	Murder Arrests All	Murder Arrests SC	Murder Arrests ST
Low-caste Party Win Share	.098*	.084*	.218**
	(.054)	(.052)	(.091)
Observations	189	189	189
R^2	.51	.51	.43

Notes: This table presents the results estimating the impact of low-caste parties on arrests against targeted murders against SC/STs. The unit of observation is the state. The outcome variable is the number of arrests effected in cases pertaining to targeted murders against SC/STs, scaled by the total number of targeted murders against SC/STs in that year. The independent variable of interest is the fraction of elections won by low-caste parties in the state, instrumented by the fraction of close elections won by low-caste parties. An election is deemed to be close if the difference in victory margin between the winner and the runners up is less than 4% of the total votes cast. Each specification controls for the share of close elections contested by low-caste parties in the state, state, year, electoral cycle fixed effects and state-level time-varying demographic and political characteristics. All specifications are weighted by the state population and standard errors are in parentheses, clustered by state.

Table 10: Low-caste Party Representation & Prosecutorial/Judicial Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Police Cases			Court Cases			
	No Investigation	Final Report Filed	Chargesheet Filed	Completed	Convictions	Acquittals	Pending Trial
Low-caste Party Win Share	-.152**	.154***	.029*	.000	-.014	.006	.003
	(.065)	(.039)	(.016)	(.003)	(.034)	(.031)	(.013)
Observations	189	189	189	189	189	189	189
R^2	.58	.88	.98	.74	.97	.97	.98

Notes: This table presents results from identifying the impact of low-caste parties on police investigation and court outcomes. The unit of observation is the state. The independent variable of interest is the fraction of elections won by low-caste parties in the state, instrumented by the fraction of close elections won by low-caste parties. All specifications control for the fraction of close elections won by a party in the state, state, year and electoral cycle fixed effects, along with state-level time-varying demographic and political characteristics and contemporaneous and lagged rates of targeted murders against SC/STs. The outcome variable in column (1) is the number of police cases in which no investigation has been undertaken; in column (2), the number of cases in which a final report has been filed; in column (3), the number of cases in which a chargesheet has been filed; in column (4), the number of cases which has been completed in court; in column (5), the number of convictions; in column (6), the number of acquitals; in column (7), the number of cases pending trial. All outcome variables are logged. All specifications include population weights. Standard errors are in parentheses, clustered by state.

Chapter 3

Can Minority Political Representation Through Political Parties Improve Minority Welfare: Evidence from India's Silent Revolution

Chapter abstract: This paper provides new evidence on the socioeconomic effects of minority political representation. While previous research focuses on the effect of electoral quotas on minority welfare (finding mixed evidence), we instead study the impact of *political parties* explicitly representing minority interests. Specifically, we study how caste-based political parties affect the welfare of low caste citizens in India – including Scheduled Castes and Tribes (SC/ST groups). These caste-based parties emerged in response to the perceived inadequacy of electoral quotas, and were unique in their singular focus on improved low-caste welfare. We use the outcome of close elections between caste-based and mainstream parties for exogenous variation, and document two novel findings. We first show the impact of low caste party legislators on low caste households' consumption from the PDS along the extensive and intensive margins. We complement these findings by showing the impact of low caste party legislators on exclusion errors and leakages from the PDS for low caste households.

3.1 Introduction

The question of how to best represent the policy interests of minority groups (typically defined by race or ethnicity) is a question that has generated debate for centuries.[1] Liberal democracies have in turn devised various constitutional, legal, and political institutions over the past century to strengthen the voice of racial, ethnic and religious minorities in the

[1] According to the famed French political philosopher Alexis de Tocqueville, "as democracy is conceived today, the minority's rights must be protected no matter how singular or alienated that minority is from the majority society; otherwise, the majority's rights lose their meaning."

political process.[2] Proponents of affirmative steps toward providing minority political representation argue that minority citizens have preferences which are distinctly different from non-minorities, and these interests are poorly represented by politicians not hailing from these communities (see for instance, Duflo 2003; Lublin and Bolwer, 2018).[3] As minority citizens often comprise socially and economically disadvantaged populations, minority political representation can also increase the trust and participation of these groups in public institutions (Aneja and Ritadhi, 2018; Lublin and Bolwer, 2018).[4] This has led societies to explore varied policy instruments to enhance the political representation of minorities, the most widespread being electoral quotas for minority politicians.[5]

This paper, however, studies the economic imcts emanating from an alternate mode of minority political representation - the electoral success of political parties exclusively representing the interests of l caste citizens. We exploit the large scale political mobilization of low caste citizens in Indi originating in the 1980s, and resulting in the formation of political parties on the basis of caste identities. The stated goal of these caste-based parties was to capture political power through electoral competition and subsequently, redistribute state resources towards low caste populations and increase their representation in public institutions (Jaffrelot, 2003; Jaffrelot, 2011).

For causal identification, the paper extends a regression discontinuity design based on the outcome of "close elections" to state legislative assemblies, contested between caste-based and traditional mainstream parties. The critical identifying assumption is that the outcome of an electoral contest can be considered to be "as good as random" once the margin of victory becomes arbitrarily small (Eggers et al., 2014). As welfare spending for low caste citizens is measured at the state-level and elections are conducted at the lower administrative level of electoral constituencies, we aggregate the results of constituency-level "close elections" contested by caste-based parties in the state to generate quasi-random variation in the number of state-level legislators elected from caste-based parties in each electoral cycle. This provides us with quasi-random variation in political representation for low caste citizens from caste-based parties. The paper exploits this variation to causally identify the welfare impacts for low caste citizens arising from the marginal legislator elected from caste based parties, exclusively representing the interests of low caste citizens.

The chapter first shows that the impact of low caste party legislators on low caste households' consumption from the PDS along the extensive and intensive margins. We then show that the impact of low caste party legislators on exclusion errors and leakages from the PDS for low caste households.

[2]This has been accompanied by concurrent attempts to expand the participation of women in national polity. See Duflo (2012) for an extensive discussion of the same.

[3]For example, Besley et al. (2004) notes that as minorities are often economically disadvantaged, they would have a higher demand for targeted low spillover public goods which non-minority politicians, catering to a wider constituency of voters may be unwilling to provide.

[4]Researchers have also argued that the continued exclusion of minority or marginalized citizens can foment major political upheavals such as ethnic conflicts and civil wars.

[5]Other strategies considered include financial support to minority political parties, redistricting of minority populations and over-representation of minority regions. See Lublin and Bolwer (2018) for a detailed review of the various modes of minority political representation.

3.2 Background

We begin with a background on the public distribution system, low caste parties, and the rise of low caste parties.

3.2.1 Public Distribution System

The public distribution system in India was established during the Second World War to implement the rationing of food grains during the war effort. It was expanded post-independence for the procurement, stocking and distribution of food grains at subsidized prices during the initial period of food shortages, and made universal in the 1970s. In 1997, the federal government introduced the Targeted PDS (TPDS) to target the PDS towards the poor. Under the TPDS, two groups of beneficiaries were established: below and above poverty line (BPL and APL) households. The latter were entitled to 20 kilograms of food grains per month while the former could also purchase food grains from the PDS subject to availability, but were ineligible for the subsidy, which equaled the difference between the federal government's price for procuring food grains and the price at which it was sold in the PDS (the issue price). While BPL households received a BPL "ration card" which enabled them to access the PDS, the APL households had to apply for a ration card in order to access the PDS. The PDS constitutes a major chunk of the public expenditure towards social safety nets: the Bhattacharya et al. 2017 estimates that the food subsidy accounted for 58 percent of social protection spending in 2004 and 41 percent in 2009.

The PDS is jointly administered by the federal and state governments. The former is responsible for the procurement and transfer of food grains to states and also finances the subsidy; the latter is responsible for selecting the beneficiaries eligible to receive the subsidized food grains and ensure their delivery to households through designated fair price shops. States are also empowered to increase the subsidy and expand the coverage of the PDS.[6] The identification of BPL households was undertaken through the Ministry of Rural Development in each state while the federal government targeted the volume of food grain subsidy to the headcount ratio of 1993 and left this unchanged through 2013 (Balani, 2013). Since 2000, a new group of PDS beneficiaries were created through the Antodaya Anna Yojana (AAY - translating to "food to the poorest") targeting the most vulnerable households such as landless households, marginal farmers, and urban slum dwellers, who guaranteed 20 kilograms of grains per month from the PDS (Balani, 2013).

Since its inception though, the PDS has remained plagued by errors of inclusion and exclusion, along with concerns regarding diversion of subsidized food grains to the open market by owners of the fair price shops. For instance, the Planning Commission in an evaluation of the PDS in 2005 estimated exclusion errors up to 43% and only 43% of the aggregate food subsidy reaching the intended households (Planning Commission, 2005). This observation is supported from household survey data from the NSS. In 1999-00, for instance, only 36 percent of households below the poverty line reported purchasing any food grains from the PDS in the past 30 days. Along, the intensive margin, for households reporting any purchase of food grains from the PDS, the average purchase is 15 kg of food grains

[6]For instance, Tamil Nadu in 2000 made the PDS universal. This model was adopted in late 2000s by a number of other states.

in the past 30 days – significantly lower than the monthly entitlement of 20 kg. In light of these leakages, the Planning Commission's (2005) evaluation of the PDS recommends the enhanced participation from elected local representatives for improved accuracy in the identification of eligible households and reduce exclusion errors, and also to check for the pilferage of subsidized food grains from fair price shops.[7]

3.2.2 Low Caste Citizens and Low Caste Parties

A large literature has evolved over the past three decades on the hierarchical caste system operating in India (see for instance Munshi, 2017). The agents of interest in this paper are three broad communities – namely the Scheduled Castes, the Scheduled Tribes and the Other Backward Classes – who are collectively referred as "low caste" groups. These three groups were discriminated against, both economically and socially, over centuries through denial of education, health care and basic public services such as drinking water and public roads. Moreover, the caste system was both endogamous and rigidly upheld occupational heredity, which severely restricted the occupational mobility of individuals hailing from these low caste groups.

Since independence, the Indian state took cognisance of the extreme socio-economic disavantages faced by low caste communities and embarked on an aggressive program of affirmative action to provide proportional representation to low caste groups in public institutions. In addition to affirmative action quotas in publicly aided higher educational institutes and public sector jobs, the Constitution also provided for affirmative action quotas in terms of political representation whereby a pre-determined share of electoral constituencies at both the federal and state levels were "reserved" solely for citizens belonging to SC/ST backgrounds. Thus, only SC/ST candidates could contest elections from these constituencies, guaranteeing the election of a fixed number of SC/ST politicians to the state and federal legislatures, proportional to the population of SC/ST citizens in the state (across India).[8] The socio-economic impact of affirmative action quotas have been widely studied over the past two decades, pioneered by the initial work of Pande (2003) and Chattopadhyay and Duflo (2004), both of whom found positive effect of the quotas on redistribution of public resources towards low caste citizens. However, more recent work by Dunning and Nilekani (2014), Jensenius (2015) and Bhavnani (2017) have found more muted impacts of these quotas.

In addition to the quotas, an alternate form of political representation for low caste citizens emerged in the 1980s through the political mobilization of these underprivileged groups. This was powered primarily by two nascent political parties, the Janata Dal (JD) and the Bahujan Samaj Party (BSP). The latter catered mostly to the Scheduled Castes (SC) or *Dalits* while the former appealed mostly to the OBC community.[9] Both parties had very similar political objectives: to capture political power through electoral competi-

[7]The Planning Commission specifically calls for a greater involvement of *Gram Sabhas* and *Panchayati Raj Institutions* which forms the lower tier of locally elected representatives in India.

[8]Since 1993, the 73rd and 74th amendments to the Constitution extended these quotas local self-government bodies.

[9]For a more detailed discussion of the caste system in India and its bearing on politics, see Jaffrelot (2003) and Jaffrelot (2011).

tion and consequently, increase the allocation of public resources to low caste groups and expand their communities' access to administrative power by enhancing their representation in public institutions. These caste-based parties had two common features distinguishing them from other mainstream parties: first, the majority of the party leadership were comprised of individuals hailing from the lower castes; and second, a high proportion of electoral candidates from these parties also hailed from lower caste backgrounds.[10] Using historical sources on party origins and electoral alliances, we classify a total of 31 political parties as caste-based parties and collectively term them as "low caste parties".[11]

Collectively, the electoral success of low caste parties between 1990 and 2010 resulted in a shift in political power from upper caste elites to low caste citizens, in what Jaffrelot (2003) terms as "India's silent revolution."[12] As the low caste parties registered significant electoral gains across 7 major states, they increased the representation of low caste citizens in state legislatures, well beyond that achieved through the electoral quotas which set aside on average only 20 percent of the positions for SC/ST communities.

Extending Pitkin's (1967) classification, our paper contends that the electoral success of low caste parties provided low caste citizens with "substantive representation", above and beyond the "descriptive representation" achieved through the electoral quotas. As shown by Jaffrelot (2003), the electoral success of low caste parties also altered the distribution of political power from upper caste elites to low caste citizens, making this process akin to a change in *de facto* political power in favour of low caste citizens as per Acemoglu and Robinson (2008). This paper seeks to empirically identify the economic effects emanating from this substantive representation (*de facto* political power) for low caste citizens.[13]

3.2.3 Politicians and the PDS: Channels of Influence

The ability of state governments to select eligible households and determine the quantum of subsidy implies that state-level legislators can impact households' consumption of subsidized food grains through PDS transfers. In addition to influencing legislative measures such as an expansion of the PDS to include new beneficiaries or an increase in the existing subsidy, state legislators can use their local influence and networks to improve the efficacy of the existing PDS structure. This achieves salience in light of the inefficiencies plaguing PDS operations described in the previous paragraphs. Based on the evaluations of the PDS

[10]For instance, the JD in 1989 stated that 60 percent of its candidates in the federal elections would hail from lower caste backgrounds (Jaffrelot, 2003).

[11]The large number of low caste parties is due to the multiple splits which have occurred in the Janata Dal, particularly in the 1990s. Most of these splits have resulted due to contests for party leadership between major party leaders and have resulted in the formation of regional parties which typically wield political power in one or two major states. Most of the parties which have split from the Janata Dal have attempted to retain the original party name in some form – for instance, the two major off-shoots of the Janata Dal in the northern state of Bihar are called "Rashtriya Janata Dal" and the "Janata Dal (United)". Similarly, the Janata Dal off-shoots in Orissa and Karnataka are called "Biju Janata Dal" and "Janata Dal (Secular)."

[12]Jaffrelot (2003) borrows this phrase from V.P. Singh – a key leader of the JD who was the Prime Minister in the JD led federal government in 1989-90.

[13]By studying the economic impacts arising from the electoral success of multiple political parties sharing a common political goal, our paper adopts an approach similar to Meyersson (2015) who studies the impact of Islamic parties – a collection of distinct political parties who share a conservative Islamic world view – on female education in Turkey.

undertaken by the Planning Commission and the findings of Dreze and Khera (2015), we consider three channels – namely bureaucratic, enforcement and legislative – through which low caste party states legislators can impact low caste households' consumption of subsidized food grains from the PDS.

In the bureaucratic channel, legislators can direct the inclusion of eligible households through the issuance of ration cards, without which households are incapable of accessing the PDS. Recall from our discussion that while BPL households are entitled to such a ration card, APL households receive a ration card only if they apply for one. Thus, low caste legislators can improve the access of low caste households to the PDS along the extensive margin by ensuring that low caste households below the poverty line receive the ration card to which they are entitled to. While the actual issuance of ration cards is handled by local bureaucrats, state legislators can influence their actions as they exercise significant control over the career prospects of bureaucrats by recommending them for promotions and transfers (Iyer and Mani, 2012).[14] Low caste party legislators t affecting consumption along the extensive margin.

Second, as legislators possess a sufficient degree of social and political influence within their constituencies (Jensenius, 2015; Gulzar and Pasquale, 2017, they can arrest leakages from the PDS by checking on the pilferage of subsidized food grains from local ration shops and ensuring that these shops remain open for the stipulated duration to enable households to purchase food grains. Legislators can also ensure that BPL and AAY households in particular are receiving their full monthly entitlement of 20 kilograms of food grains as prescribed by law. We refer to this as the "enforcement" channel and envisage its impact on low caste households' consumption from the PDS to be primarily through the intensive margin.[15]

The third channel considered is the "legislative" channel which explicitly deals with the framing of legislation through which states can expand outlays to the PDS program. This would be reflected in public expenditures and can result in a higher level of subsidy per unit of foodgrain issued, or improvements in the delivery of foodgrains through investments in transportation and storage technologies.[16] The primary impact along this channel is again envisaged to be through the intensive margin.[17] It is worth noting that only the legislative channel would require low caste parties to have a legislative majority – impact of low caste legislators on low caste households' consumption from the PDS along the bureaucratic and enforcement channels is primarily a function of legislator effort and networks.

[14]The transfer of bureaucrats and police officers is an instrument which is frequently used by low caste party politicians, primarily to facilitate the promotion of bureaucrats belonging to low caste backgrounds (Jaffrelot, 2003).

[15]There can also be an impact along the extensive margin if households who were initially disincentivized from purchasing from the PDS due to the high rates of leakages start consuming from the PDS due to lower opportunity costs of time and the prospect of higher returns on their cost of traveling to the PDS shop and waiting in the queue.

[16]This could include contracting with high quality transporters to ensure timely delivery; improved storage facilities to prevent the infesation of rodents; and investments in human capital to improve the quality of workers in PDS shops and ensure the efficient dissemination of food grains to beneficiaries.

[17]Again, one can envisage an impact along the extensive margin if higher subsidies or improved delivery mechanisms reduce the opportunity cost of recipients traveling to purchase food grains from PDS shops and expand the set of included beneficiaries.

Based on the three channels described above, we identify the impact of low caste legislators on two key outcomes of interest: first, the extensive margin effect, which is a dummy equaling 1 if the household purchased any food grains from the PDS in the past 1 month. Second, the intensive margin effect, measured as the quantity of food grains (in kilograms) purchased by the household from the PDS in the past month. In addition to these, we also assess the impact of low caste legislators on shortfalls and exclusion errors. As BPL households are entitled to 20 kilograms of subsidized food grains from the PDS per month, we identify the impact of low caste legislators on the shortfall in PDS consumption. Finally, we construct two measures of exclusion errors: the first equals 1 if a BPL household undertakes no consumption from the PDS; the second equals 1 if the household is either BPL or landless (and thereby eligible for the AAY scheme) undertakes no consumption from the PDS.

3.3 Data

This paper combines data from two sources: the electoral data is obtained from the Election Commission of India (ECI) and the data for household consumption is obtained from consumption surveys conducted by the NSS.[18] Additional variables are sourced from the Handbook of Statistics on the Indian Economy (maintained by the Reserve Bank of India (RBI)) and the decennial Census of India.

3.3.1 Electoral Data

The responsibility for conducting elections in India lies with the ECI, which is a constitutional body, unaffiliated to any political party and independent of the incumbent state (federal) government. The outcome of each election is decided by the first-past-the-post principle with the party securing the maximum number of votes in an electoral constituency being declared the winner.[19] The legislator subsequently represents that constituency for a fixed term of 5 years.

The ECI provides data on turnout and candidate vote shares for all state legislative assembly elections in every state. We use a rich sample covering 62 state-electoral cycles across 16 major Indian states between 1994 and 2011. With elections being held periodically every 5 years across states, this implies at least 3 electoral cycles for each state and an universe exceeding 14,000 elections, out of which 90 percent – or nearly 13,000 elections – involving at least 1 low caste party. Low caste parties in this period captured on average 22 percent of the votes cast in a district, and won 24 percent of the elections contested in the district.

[18] The NSS is part of the Indian government's Ministry of Programme Implementation and Statistics and conducts regular surveys to provide official statistics on individual employment and household consumption.

[19] The pluralist framework of Indian democracy allows for multiple political parties contesting the election in each electoral constituency. Individuals unaffiliated to any political party may also contest the elections as independent candidates.

3.3.2 Household Consumption Data

We test the impact of low caste parties on household consumption of subsidized food grains using household survey data collected by the National Sample Survey (NSS). We restrict ourselves to the quenquennial surveys, or "thick rounds", covering over 80,000 households across all Indian districts.[20] The NSS data is a repeated cross-section, precluding us from tracking households over time. Additionally, as the NSS did not identify OBC households till 1999, we limit the sample to four "thick" rounds – the 55th, 61st, 66th and 68th rounds – between 1999 and 2012.[21]

As the NSS conducts its surveys at periodic intervals, we match the electoral outcomes for the closest election year preceding the NSS survey year.[22] The NSS consumption surveys enquire from sample households their consumption of major food and non-food commodities in the past 30 days. For food items, both the physical quantity and the rupee amount spent on the item is reported. We exploit this information to determine the amount of subsidized food grains - wheat and rice - consumed by households from the PDS.[23]

The NSS data reports a significant rise in households' consumption from the PDS between 1999 and 2012. While only 31 percent of households reported any consumption from the PDS in 1999, this had risen to over 47 percent in 2012.[24] Amongst poor households too, there was a large increase in the share of households reporting any consumption from the PDS between 1999 and 2012 from 35 to 63 percent. Similar trends were seen along the intensive margin: in 1999-00, conditional on making any purchases from the PDS, the household purchased 14 kilos of grain, which subsequently increased to 20 kilos in 2011-12.

Looking at low and non-low caste households, we see that low caste households had a higher propensity to consume from the PDS: while 35 percent of low caste households reported any consumption from the PDS in 1999-00, the same was reported by only 23 percent of non-low caste households. However, upon restricting the sample to BPL households, there was no difference in participation in the PDS across low and non-low caste households in 1999-00, with 35 percent of households across both groups reporting some purchase of susbidized food grains from the PDS in the past month. In 2011-12 however, BPL low caste households had a higher rate of participation in the PDS with 64 percent households reporting some purchase of food grains, as opposed to 54 percent non-low caste BPL households. Moreoever, the subsidized food grains from the PDS constituted a major source of consumption for BPL households. Food grains consumed from the PDS accounted for 25 percent of

[20] We restrict ourselves to the quenquennial surveys as the sample size of the annual surveys is significantly smaller.

[21] The years corresponding to the survey rounds are: 1999-2000, 2004-05, 2009-10 and 2011-12. Due to the large sample size, the surveys are conducted through the year, between July 1st and June 30th, and the NSS provides seasonal identifiers, allowing us to control for seasonal trends.

[22] As there is often a lag between the election of politicians and policy outcomes, we allow for a 1 year lag between the year of elections and household outcomes are measured. Thus, we map the electoral outcomes to the previous electoral cycles for the states for which held elections during or just prior to the NSS survey period.

[23] The PDS also provides subsidized sugar and kerosene in addition to food grains. However, as food grains form the largest compononent of the PDS, we restrict our focus only to food grains.

[24] It is worth noting that the 1999-2012 period also witnessed a sharply decline in headcount ratios, suggesting that higher participation in the PDS is due to systemic improvements and better targeting, as opposed to a mechanical increase in PDS consumption due to higher incidence of poverty.

all food grains consumed by BPL households in this period.

Finally, the household data shows that in this period, low caste households had significantly lower per capita expenditures relative to non-low caste households, a higher propensity to be landless, and be classified as BPL. During the period of consideration, the per capita expenditures of low caste households were 25-40% less than non-low caste households. For rural households, while 29 percent of non-low caste households were landless in this period, this was true for 40 percent of low caste households.[25] Finally, while the headcount ratio amongst low caste households was 30 percent in this period, the headcount ratio amongst non-low caste households is 12 percent.

The summary statistics from the NSS provide us with four key insights: first, subsidized food grain supplied from the PDS comprise a non-trivial share of poor households' PDS consumption. Second, low caste households have a disproportionately higher likelihood of being poor and landless, making them eligible for receiving subsidized food grains from the PDS. Third, at the beginning of the sample period, the majority of BPL and landless low caste households received no benefit from the PDS, which subsequently declined over time, signifying a reduction in the exclusion error in PDS targeting. Fourth, along the intensive margin also, the consumption of low caste BPL households has increased significantly in the 12 year period of the study, indicating a decline in leakage from the PDS. The following section lays out our empirical strategy to test whether this improvement in PDS targeting and reduction in leakage for low caste households can be causally attributed to the electoral success of low caste parties, through the channels described in Section 3.2.3.

3.4 Empirical Strategy

3.4.1 Instrumental Variables Framework

Our paper aims to identify the impact of legislators representing low caste parties on low caste households' consumption from the PDS. Our primary estimating equation can be expressed as:

$$Y_{idt} = \alpha_d + \delta_t + \eta_{t-y} + \beta ShLowCasteWin_{dt} + \mathbf{X}_{idt}\psi + \epsilon_{idt} \tag{3.1}$$

The unit of observation in (3.1) is household i, residing in district d, and surveyed in survey round t. We have two major outcomes of interest. The first is a dummy equaling 1 if the household has made any purchase of food grains (rice or wheat) from the PDS in the past 30 days. This measures a household's participation in the PDS along the extensive margin. Our second outcome is the logged per capita quantity (in kilograms) of food grains purchased by the household from the PDS in the past 30 days. This measures the household's participation in the PDS along the intensive margin. The outcome of interest is *ShLowCasteWin*, denoting the fraction of legislators representing low caste parties in the district in election year y. This is defined as:

$$ShLowCasteWin_{dy} = \frac{LowCasteWin_{dy}}{TotalElections_{dy}} \tag{3.2}$$

[25] Households are considered landless if they have less than 0.1 acres of land holdings.

where *LowCasteWin* is the total number of elections won by low caste parties in district d and electoral cycle y, scaled by the total number of elections held in district d and electoral cycle y. Along the extensive margin, β measures the impact of a 1 percentage point increase in the fraction of legislators elected from low caste parties on the change in likelihood of a household to purchase food grains from the PDS in the past 30 days. Along the intensive margin, β measures the impact of a 1 percentage point increase in the fraction of legislators elected from low caste parties on the percentage change in the monthly per capita quantity of food grains purchased by the household from the PDS.

$\mathbf{X}$ is a vector of household and district-level covariates while α and δ denote district and survey round fixed effects, controlling for time and region-invariant characteristics affecting households' consumption from the PDS. η a fixed effect for legislature tenure, controlling for any secular changes in low caste households' PDS consumption attributable to the duration of the legislature.[26]

Expectedly, we would expect an OLS estimation of (3.1) to yield a biased estimate of β due to unobservables correlated with both the electoral success of low caste parties in the district and low caste households' consumption from the PDS. For instance, as low caste households are on average poorer, they have a higher likelihood of consuming food grains from the PDS. If these households are also more likely to vote for low caste parties, it would bias the estimated β upwards. Alternately, if low caste households are concentrated in regions with poor physical infrastructure affecting the timely delivery of food grains and low caste parties also enjoy higher electoral success in such areas, an OLS estimate of β using (3.1) would be biased downwards.

We counter this classic endogeneity problem with an instrumental variables approach based on the outcome of "close elections". This approach is motivated by Rehavi (2008), Bhalotra () and Clots-Figueras (2012), all of whom use a similar approach to study the impact of legislator identity on economic outcomes. The distinguishing feature of "close elections" is that the difference in vote margin between the two principal contestants, the winner and the runners-up, is arbitrarily small. As political parties have imperfect control over their respective vote shares (Eggers et al. 2014), the outcome of such elections can be determined by small shifts in vote share, such as those attributed to voter turnout. Consequently, the outcome of such elections can be considered "as good as random".

We exploit the "randomness" in the outcome of close elections and define our instrument, *ShLowCasteCloseWin* for district d and electoral cycle y, as:

$$ShLowCasteCloseWin_{dy} = \frac{LowCasteCloseWin_{dy}}{TotalCloseElections_{dy}} \tag{3.3}$$

where the numerator is the total number of close elections won by low caste parties, scaled by the total number of close elections contested by low caste parties. For *ShLowCasteCloseWin* to be a valid instrument, it has to be a strong exogenous predictor of the endogenous variable of interest (a strong first stage), and also uncorrelated to the outcome variable, except through its effect on the endogenous variable (exclusion restriction).

[26]Thus, if low caste legislator's are more prone to increasing allocations to the PDS as they approach re-election, and this causes an increase in low caste households' consumption from the PDS, η would capture the impact of such secular increases.

With regard to the first stage, if the outcome of close elections is "as good as random", we would expect each of the two contestants close election to have an an equal chance of winning. Consequently, if there are n close elections in a district involving low caste and non-low caste parties, we would expect low caste parties to win $\frac{n}{2}$ close elections, and this would be independent of district-level factors as long as the outcome of close elections is "as good as random". Conditional on the number of close elections occurring in a district and the total elections contested in the district, an additional close election won by low caste parties mechanically increases the number of elections won by low caste parties in the district by 1 additional legislator. Thus, the instrument is positively correlated with the endogenous variable of interest, while being centred around $0.5 * TotalCloseElections$ by construction, satisfying the first criteria for being a valid instrument. With respect to the exclusion restriction, we note that an additional close election won by low caste parties only implies that an additional legislator representing low caste parties is elected to the state legislative assembly. It has no bearing on other factors which might be affecting low caste households' consumption from the PDS (such as education or physical infrastructure) except through its impact via the additional legislator elected from low caste parties due to its success in an additional close election.

We use our IV strategy in a two-stage least squares framework to identify the impact of legislators representing low caste parties on low caste households' consumption from the PDS. The first stage of the IV specification is:

The independent variable of interest, the fraction of elections won by low caste parties in the district, is instrumented by the fraction of close elections won by low caste parties in the district. As the incidence of close elections in a district is possibly endogenous to various district characteristics, we control for the fraction of close elections contested by low caste parties in all our specifications. For the purposes of this paper, an election is considered to be "close" if the difference in victory margin between the winner and the runners up is less than or equal to 5 percent of the votes cast in the constituency. Section 3.6 verifies that our results are not sensitive to this particular threshold for close elections. The identification is derived from variations in the fraction of elections won by low caste parties in a district resulting from a change in the fraction of close elections won by low caste parties in the district.[27]

3.4.2 Empirically Verifying the Exogeneity of the Instrument

Prior to presenting our results based on the IV specification outlined in 3.4.1, we first empirically verify the exogeneity of our instrument. Since the key assumption is that the outcome of close elections between low caste and non-low caste parties at the level of constituencies is "as good as random", we first show that at the constituency level, the outcome of close elections involving these two sets of parties is orthogonal to observable characteristics. Subsequently, we show that our instrument – the fraction of close elections won by low caste parties in the district – is also orthogonal to district-level characteristics.

[27]Within the IV framework, these districts form the "switcher" groups. Thus, districts which witness no close elections between low caste and non-low caste parties do not contribute to our empirical strategy.

3.4.3 Constituency Level Covariate Balance

At the level of electoral constituencies, we first test for the selective sorting of low caste party candidates into winning (treatment) and losing (control) status. The running variable of interest is the victory margin of low caste parties, ($LCVM$), defined as the difference in vote share between a low caste and a non-low caste party. A positive (negative) victory margin signifies a low caste party victory (defeat).[28] Figure 1 presents the McCrary density test (2008), testing for a discontinuity in the running variable at the cutoff point 0, where the electoral outcome changes discontinuously from a low caste party defeat to a low caste party victory. The horizontal axis is divided into 500 bins and the grey circles represent the density of observations in each bin. Visually, we are unable to discern any discontinuity at the cutoff 0 and the small statistically insignificant discontinuity estimate from the McCrary test (2008) does not permit us to reject the null hypothesis of no discontinuity at the cutoff.

Next, we perform covariate balance checks in the spirit of Meyersson (2015) for 8 constituency-level observables[29] across the support of low caste party victory margin. For each covariate, we calculate its unconditional mean in 8 percent bins of the running variable between -0.3 and 0.3; the solid black line represents a local linear polynomial plotting the relationship on either side of the cutoff while the dashed lines represent the 95 percent confidence intervals. Figure 2 presents the graphical plots from this exercise. Visually, we are unable to discern any discontinuity in the running variable across any of the constituency-level covariates and the confidence intervals also overlap, confirming covariate balance at the cutoff for low caste party victory margin.

3.4.4 District Level Covariate Balance

Section 3.4.3 established that the outcome of close elections involving low caste and non-low caste parties is orthogonal to constituency-level observables. We now show that our instrument – the fraction of close elections won by low caste parties in the district is also orthogonal to district-level observables. We begin by verifying our hypothesis that the fraction of close elections won by low caste parties in the district approaches 0.5. Note first that average district in our sample witnesses 3 close elections in an electoral cycle and conditional on the incidence of at least 1 close election in the district, the unconditional mean of the fraction of

[28] In the simplest case where an election has two candidates, one belonging to a low caste party and the other to a non-low caste party, the victory margin is the difference in vote share between the low caste and the non-low caste party. With multiple candidates from low caste and non-low caste parties, we define the low caste victory margin in constituency c and election year y - $LCVM$ - as the following: in the event of a low caste party victory;

$$LCVM_{cy} = WLCVS_{cy} - \max\left(NLCVScy\right) \tag{3.4}$$

The vote share of the winning low caste party is represented by $WLCVS$ and we subtract from it the maximum vote share received amongst all non-low caste parties contesting the election. The victory margin is defined analogously, in the event of a non-low caste party winning the election.

[29] These are low caste party vote share; registered voters (logged); voter turnout; effective number of parties contesting; share of constituencies reserved for SC/ST candidates through the electoral quotas; share of male winners; share of SC/ST winners; and the age of winner. The data for the last two covariates are only available since 2005.

close elections won by low caste parties is 0.49 and statistically indistinguishable in a t-test from 0.5.

3.5 Results

This section presents the paper's findings. We first show the impact of low caste party legislators on low caste households' consumption from the PDS along the extensive and intensive margins. We complement these findings by showing the impact of low caste party legislators on exclusion errors and leakages from the PDS for low caste households.

3.5.1 Average Effect of Low Caste Party Legislators on Household PDS Consumption

Table 3 presents our baseline results identifying the impact of legislators representing low caste parties on low caste households' consumption from the PDS. Columns (1)-(4) show the results along the extensive margin where the dependent variable is a dummy equaling 1 if the household had purchased any rice or wheat from the PDS in the past 30 days; columns (5)-(8) contain the intensive margin results where the outcome of interest is the logged per capita quantity (in kilograms) of food grains purchased from the PDS in the past 30 days. For each specification, the first column shows the results without covariates (except for the fixed effects) and the second column includes covariates. The coefficients are estimated using an OLS specification in columns (1), (2), (5) and (6), and using an IV specification in columns (3), (4), (7) and (8). Standard errors are clustered by district.

The OLS specification in columns (1) and (2) detect a positive correlation between the fraction of elections won by low caste parties and the likelihood of low caste households consuming from the PDS. The coefficient is small and significant at the 10 percent level upon the addition of covariates. Along the intensive margin though, the correlation is positive only upon the addition of covariates but imprecisely estimated. Columns (3) and (7) show the results using the IV specification without covariates while columns (4) and (8) include covariates. The addition of covariates have little impact on the results and henceforth, we focus solely on columns (4) and (8) which includes the covariates. Recall that in the IV specification, the fraction of close elections won by low caste parties in the district is instrumented by the fraction of close elections won by low caste parties in the district.

Column (4) identifies a positive causal impact of legislators from low caste parties on the likelihood of low caste households' food grain consumption from the PDS. Similarly, the coefficient in column (8) is also positive and significant, signifying that a higher fraction of elections won by low caste parties increases the quantity of food grains purchased from the PDS. Both the coefficients are significant at the 5 percent level.

The coefficients are economically significant: a 10 percentage point increase in the fraction of elections won by low caste parties increases the likelihood of low caste households consuming from the PDS by 3 percentage points (relative to a mean of 37 percent in this period), with the corresponding increase for the quantity of food grains being 7 percent. Along the extensive margin, as the mean level of food grains consumed by a low caste household from the PDS in this period is 1.8 kilograms, the coefficient implies that a 10 percentage

point increase in the fraction of elections won by low caste parties increases the per capita monthly consumption of food grains from the PDS by approximately 120 grams every month, or 1.5 kilograms per year.[30]

To consider the impact of the marginal legislator representing low caste parties, we note that each district on average witnesses 19 elections, implying that one additional legislator elected due to an additional close election won by low caste parties equals a 0.05 (1/19) percent increase in the fraction of elections won by low caste parties in the district. This amounts to a 2 percentage point increase in the likelihood of low caste households consuming from the PDS and a 4 percent increase in the quantity of food grains purchased by low caste households from the PDS.

Comparing the IV and OLS coefficients in Table 3, we see that relative to the IV coefficients, the OLS coefficients are biased downwards. This is consistent with the results obtained by Rehavi (2008) and Clots-Figueras (2012), who use a comparable empirical strategy to identify the impact of female legislators on economic outcomes. There are two potential explanations for this: fir,st, as described in Section 3.4.1, omitted factors correlated with the success of low caste parties are negatively correlated with the outcome of interest, biasing downwards the relationship between low caste parties and PDS consumption for low caste households. Second, there could be positive selection amongst legislators who contest close elections on behalf of low caste parties. Thus, if political parties can anticipate the incidence of a close electoral contest (without being able to influence its outcome due to imperfect control over its vote share), they may nominate candidates with higher ability to such constituencies. These candidates upon winning can also be expected to exert greater effort, resulting in improved outcomes for low caste households.

In published work, show the reduced form and first stage results corresponding to the IV coefficients All reduced form are positive and significant, irrespective of the inclusion of covariates. The first stage coefficients are also positive and highly significant while the associated F-statistic is significantly higher than the lower bound of 10. To assess the impact of 1 additional close election won by low caste parties in the district, note that conditional on total close elections contested by low caste parties and total elections in the district, 1 additional win by a low caste party would result in $\beta * \frac{TotalCloseElections}{TotalElections}$.[31]. As the average district witnesses 19 close elections and low caste parties contest 2.6 close elections, an estimated $\beta = 0.11$ implies that 1 additional close win for low caste parties in the district implies 0.8 or approximately, 1 additional electoral win for low caste parties in the district.

The first stage coefficient effectively validates our exclusion restriction which states that the fraction of close elections won by low caste parties should affect outcomes for low caste households soley through its impact on the fraction of total elections won by low caste parties. The first-stage coefficient points exactly to that - an additional close election won by low caste parties in the district results only in 1 additional election by low caste parties in the district. The lack of a multiplier effect also rules out that our identification is driven by

[30]In terms of nutritional value, under the assumption that 100 grams of cooked rice provides 300 calories, the coefficient implies that a 10 percentage point increase in the fraction of low caste legislators in the district increases the daily subsidized caloric consumption for a low caste individual by 12 calories or approximately 0.6 percent of daily nutritional intake for an adult male.

[31]This is derived by totally differentiating the equation $\frac{LowCasteWin}{TotalElections} = \alpha + \beta \frac{LowCasteCloseWin}{TotalCloseElections}$, holding constant *TotalCloseElections* and *TotalElections* constant.

"wave" elections where there is a local positive "wave" in favour of low caste parties. If so, we would have expected an additional close election won by low caste parties to have a positive spillover on the success of low caste parties across other constituencies in the district.

3.5.2 Political Parties or Electoral Quotas? What Drives Low Caste Households' Consumption from PDS

We now turn to the paper's key hypothesis: are political parties with a specific policy commitment towards low caste citizens more effective than the institution of electoral quotas in transferring public resources towards low caste citizens. We exploit the institution of electoral quotas by virtue of which a pre-determined share of electoral constituencies are "reserved" in the district from which only SC/ST candidates can contest the elections. Thus, regardless of the party which wins the election, the legislator elected from these constituencies would always hail from a SC/ST background. This allows us to isolate the impact of legislators across political parties on our outcomes of interest, while holding the caste identity of the legislator constant.

We use this strategy to compare the impact of legislators from low caste parties and two other mainstream parties – namely the BJP, and the INC – on low caste households' consumption from the PDS. The results are shown in Table 4: the outcome of interest in Panel A is consumption from the PDS along the extensive margin; in Panel B, along the intensive margin. For each party, the independent variable of interest in columns (1), (3) and (5) is the fraction of elections won by the party of interest in reserved constituencies; in columns (2), (4) and (6), the fraction of legislators won by the party of interest in non-reserved constituencies. In each instance, the fraction of elections won by the party from (non) reserved constituencies in a district is instrumented by the fraction of close elections won by the party from (non) reserved constituencies.

Across both Panels A and B, columns (1) and (2) of Table 4 show that regardless of the type of constituency, legislators from low caste parties have a positive and significant impact on low caste households' consumption from the PDS. All the coefficients are significant at the 10 percent level with the extensive margin impact of low caste party legislators from reserve constituencies being significant at 1 percent level and the intensive margin impact of low caste party legislators from non-reserved constituencies significant at the 5 percent level.[32] On the contrary, irrespective of the type of constituency, neiher the two mainstream parties have any impact of low caste households' consumption from the PDS, either along the extensive, or the intensive margins. The estimated coefficients for both these parties are small and imprecisely estimated.

In terms of statistical significance, we are unable to reject that the coefficient for low caste party legislators elected from reserved constituencies is equal to that for low caste party legislators elected from non-reserved constituencies. Thus, irrespective of the type of constituency from which they are elected, legislators representing low caste parties have a positive and significant impact on low caste households' consumption from the PDS along both the extensive and intensive margins. Comparing the coefficients across political parties,

[32]The corresponding p-values in columns (1) and (2) of Panels A and B are, 0.002, 0.063, 0.052 and 0.023 respectively.

we note that within reserved constituencies, legislators from low caste parties have a higher impact than legislators representing the two mainstream parties, with the coefficients being significantly different at the 5 percent level. Morever, when we compare the impact of legislators representing low caste parties elected from non-reserved constituencies (thereby the legislator not necessarily hailing from a low caste background), against those elected from reserved constituencies (whereby the legislator by design hails from a low caste background), but representing one of the two mainstream parties, we see that the former also has a significantly higher impact on PDS consumption by low caste households (significant at the 5 percent level for both sets of parties and outcomes).

In summary, the results in this section highlight the critical role played by the policy preferences of political parties and provide a potential explanation to the recent studies by Dunning and Nilekani (2012), Jensenius (2015) and Bhavnani (2017) who find muted effects of the electoral quotas. This is in contrast to earlier works identifying a positive impact of the electoral quotas by Pande (2005) and Chin and Prakash (2010). Our results show that legislators hailing from low caste backgrounds and elected through the quotas do affect outcomes for low caste households, but only if they also represent political parties with an ex-ante policy commitment towards low caste citizens. The findings in this regard provide empirical evidence documenting the role played by substantive representation, as opposed to descriptive representation, for socio-economically underprivileged citizens (Pitkin 1967). The results also offer causal evidence in favour of Acemoglu et al.'s (2016) theoretical prediction that institutional changes affecting *de jure* political power will continue to have a limited effect on redistribution, unless accompanied by broader changes in *de facto* political power, which alters the overall structure of political power.

3.5.3 Differential Effects of Low Caste Party Legislators

This section tests for heterogeneity in the impact of low caste party legislators across household demographics, political factors and low caste party political power. In the process, we rule out alternative channels which might be explaining our results and isolate the role of these specific political parties. The results are shown in Table 5 with the outcome of interest being along the extensive margin in Panel A and intensive margin in Panel B. In each case, we interact the fraction of elections won by low caste parties in the district with a dummy representing the characateristic of interest.[33]

Column (1) tests for differential effects across the specific caste-group of households. As mentioned before, we collectively categorize three broad citizen groups – SCs, STs and OBCs – as "low caste". On the other hand, the core support group of the majority of the low caste political parties studied in this paper are the OBCs. We thereby test whether the positive impact of low caste party legislators is limited to select groups of low caste citizens. The coefficients in column (1) of Panels A and B rule out this possibility: the base category is the OBC group and the coefficient is positive and significant along both the extensive and intensive margins. The interaction term for SC and ST households is small and not

[33]As we are interacting the endogenous variable of interest with a dummy, each specification now has 2 (3 in column (1)) endogenous variables, which are instrumented with 2 (3 in column (1)) instruments: namely the fraction of close elections won by low caste parties in the district and the interaction between the dummy and the fraction of close elections won by low caste parties in the district.

precisely estimated. Thus, we are unable to reject the hypothesis that low caste parties have comparable effects across the three broad low caste groups. The sum of the coefficients remain positive and significant in each case, confirming that an increase in the fraction of legislators elected from these parties positively affect their consumption from the PDS.

Column (2) tests for differential effects across districts with a high share of low caste citizens. This is to verify if low caste party legislators are targeting resources towards select districts with a high concentration of low caste party legislators. We consider a district to have a "high" share of low caste citizens if the fraction of low caste citizens residing in a district exceeds the median share of low caste citizens across all districts in the specific survey round. The interaction term is negative in both Panels A and B (albeit insignificant) and rules out that low caste parties are targeting PDS resources to select districts with a high fraction of low caste citizens.

Column (3) tests for complementarity between low caste parties and the institution of electoral quotas by testing for differential effects across districts with a high fraction of reserved constituencies. Again, we consider districts to have a high share of reserved constituencies if the fraction of electoral constituencies reserved for SC/ST candidates exceeds the median fraction of reserved constituencies across all districts. The interaction terms are not statistically significant, ruling out that the impact of low caste parties are concentrated solely in districts with a high fraction of constituencies reserved for SC/ST candidates. This once again underlines the "party effect": if the results were driven solely by legislator identity, we would have expected the positive impact of low caste parties to be concentrated in districts where political representation was higher for low caste citizens by design through the electoral quotas.

We test for differential effects of the impact of low caste party legislators across political power in column (4). The dummy *Low Caste Party Govt* equals 1 if low caste parties are also in power in the state government, either through a legislative majority, or in coalition with other parties. The interaction terms are small and statistically not significant while the uninteracted term estimating the impact of low caste party legislators when low caste parties are not in power is positive and significant in each instance, while the sum of the coefficients is also positive and significant. Thus, while legislators representing low caste parties impact low caste households' consumption from the PDS when low caste parties are the ruling party in the state, they continue to retain their positive influence even when these parties are not the ruling party in the state. Based on our discussion in Section 3.2.3, these results suggest that low caste party legislators impact PDS consumption from low caste households along both the bureaucratic and the enforcement channels. This is consistent with the fieldwork of Jensenius (2016) who documents that state legislators are locally influential within their constituencies and can affect outcomes solely through local networks, even without broader political power. These results also indicate that effort exerted by local politicians can improve the access of underprivileged households to public welfare programs.

Finally, columns (5) and (6) rule out alternate explanations that our results are being generated through citizen participation in the political process or vote-buying incentives. As access to PDS is an entitlement for poor households, it is possible that citizens need first to be aware of their rights in order to influence politicians to deliver public resources. We measure citizen participation in the political process using district-level voter turnout. Districts with a voter turnout in excess of the voter turnout in the median district are considered districts

to have a relatively "high" level of engagement with the political process. Column (5) however finds no differential effect across such districts - while the coefficient is positive, it is statistically not significant and the direct impact of low caste party legislators remains positive and significant across Panels A and B, implying that even in district with low voter turnout (and thereby, low citizen participation and awareness), low caste parties have a positive impact on low caste households' consumption from the PDS.

Column (6) tests the alternate explanation that our baseline results are being generated through vote-buying incentives. Thus, it is plausible that legislators representing low caste parties engage in cheap talk during the lead up to elections by claiming to represent low caste interests, and deliver on their agendas only prior to elections to boost their prospects of re-election. In such a situation, we would be incorrectly interpreting targeted vote buying strategies as low caste parties' policy commitment towards low caste citizens. If this explanation is correct, we would expect the positive impact on low caste households to be concentrated in the years prior to re-election. We test this by interacting the fraction of low caste legislators elected in the district with a dummy equaling 1 if the survey round is conducted in the last two year's of the state legislature's tenure. The results however rule out this channel: the coefficients on the interaction term in Panels A and B are in fact negative and significant – opposite of what we would have expected if low caste party legislators were using PDS transfers as a form of vote buying. This negates concerns that the positive effect of low caste party legislators on low caste households is explained purely through re-election concerns.

3.5.4 Who are Being Targeted? Low Caste Households or Poor Households?

Earlier sections of the paper noted that the PDS is one of the largest anti-poverty programs operated by the government and low caste households have a significantly higher likelihood of being poor.[34] This however mechanically increases their propensity of being a beneficiary of the PDS. This however raises concerns as to whether low caste parties are targeting low caste households or poor households in general. For instance, our results would be consistent with the explanation that low caste parties are essentially left-of-centre pro-poor parties who increase aggregate welfare spending, and as low caste households have a higher likelihood of participating in such programs, they also benefit most from a general expansion of such schemes. This would imply that we are erroneously classifying pro-poor parties as low caste parties. If this above explanation is correct however, we would first expect the effects to be concentrated within the poorer low caste households; and second, expect poor non-low caste households who might also be dependent on the PDS to also benefit in the presence of these legislators.

To examine this hypothesis, we test for differential effects across households falling in the bottom decile (quartile) of the consumption distribution. For each survey round, we compute the 10th and 25th percentile of the monthly per capita consumption distribution and assign households to each of these groups. As consumption patterns vary across rural and urban

[34]For instance, based on the NSS surveys 87 and 90 percent of the BPL households in 1999-00 and 2011-12 were low caste.

households, we compute the percentile breaks separately for urban and rural households. Subsequently, we define a dummy equaling 1 if the household falls in the bottom decile (quartile) and interact this dummy with the fraction of legislators elected from low caste parties in the district. We would expect a positive coefficient on the interaction term for both low and non-low caste households if low caste parties are essentially working towards poverty alleviation and not targeted public resource allocation towards low caste households.

The results in Table 6 confirm that low caste parties are exclusively targeting low caste households and not only poor households. The interaction term in columns (1)-(4) where the sample is restricted to low caste households is small and statistically insignificant, ruling out any differential effects for this sub-sample of households. The uninteracted effect of low caste party legislators however remains positive and statistically significant at the 5 percent level in each case, and the sum of coefficients is also jointly significant at the 5 percent level. Thus, an increase in the fraction of low caste legislators elected from low caste parties results in an increase in low caste households' consumption from the PDS, irrespective of whether the households are located towards the bottom of the consumption distribution.[35]

On the contrary, when the sample is restricted to non-low caste households (columns (4)-(8)), we see that while the coefficient on the interaction term remains small and imprecisely estimated, the direct effect of low caste legislators is also significantly smaller than in columns (1)-(4) and fails to achieve statistical significance with the exception of column (8). There too, the coefficient is weakly significant at the 10 percent level (p-value of 0.08) and a third in magnitude to the corresponding coefficient for low caste households estimated in column (4). Thus, legislators representing low caste parties continue to have no impact on non-low caste households' consumption from the PDS, irrespective of whether they fall in the bottom decile (quartile) of the consumption distribution. These findings support our contention that low caste parties are exclusively targeting public resources towards low caste households and rule out concerns that these are possible parties engaging in pro-poor politics and the impact on low caste households is but a positive spillover from the impact of these parties on poor households' consumption from the PDS.

3.5.5 Low Caste Party Legislators and Exclusion Errors from PDS

Section 3.2.1 noted that the PDS has been afflicted by errors of exclusion while Section 3.2.3 discussed how legislators can improve households' access to the PDS along the extensive margin through the bureaucratic and enforcement channels. We now test whether low caste party legislators indeed reduce the exclusion error from the PDS associated with low caste households.[36] We use two measures of exclusion errors: the first is a dummy which equals 1

[35]These results however do not indicate elite capture of the PDS benefits by the relatively less poor low caste households. Note first that as the sum of the coefficients are statistically significant, low caste housheolds in the bottom quartile (decile) of the consumption distribution also continue to access the PDS, both along the intensive, and the extensive margins. Moreover, while the PDS is an entitlement to BPL households, APL households are also eligible to purchase from PDS stores subject to availability. A case for elite capture could have been made if the relatively poor amongst low caste households were excluded from the PDS, while the relatively non-poor continued to benefit from it, which we do not observe in our results.

[36]While inclusion errors have also been a concern for the PDS, we refrain from examining inclusion errors as states have the flexibility to expand on the list of PDS beneficiaries making it harder to identify errors of inclusion.

if the household is a BPL household but did not undertake any consumption from the PDS in the past 30 days. The second is a broader definition where the dummy equals 1 if the household is a BPL household, or a rural landless household, and did not undertake any consumption from the PDS in the past 30 days. The first outcome variable essentially views exclusion errors as BPL households who did not consume from the PDS; the second measure is aimed to capture the AAY group, which includes BPL or landless households.

Columns (1) and (2) of Table 7 shows that the low caste party legislators have a negative impact on exclusion errors impacting low caste households. In column (1), where the sample is restricted to BPL low caste households, a 10 percentage point increase in the fraction of low caste legislators in the district causes a 4 percentage reduction in a low caste BPL household not consuming from the PDS. As 56 percent of low caste BPL households report no consumption from the PDS in our sample, this reflects a 7 percent decline in exclusion errors in response to a 10 percentage point increase in the fraction of low caste party legislators in the district. Column (2) restricts the sample to low caste households who are either BPL or are rural and landless and finds a similar effect of low caste legislators on exclusion errors – at the mean of the dependent variable, a 10 percentage point increase in the fraction of low caste party legislators in the district reduces the likelihood of exclusion errors by 11 percent.[37] On the contrary, columns (3) and (4) undertake the same exercise but restricting the sample to upper caste households. The coefficients, while negative, are smaller in magnitude (particularly column (2) vs column (4)) and imprecisely estimated. Thus, while legislators representing low caste parties reduce the likelihood of exclusion errors for eligible low caste households, they have no corresponding impact for upper caste households. In unpublished analysis, we show on on the other hand that none of the other mainstream parties have a comparable effect on exclusion errors from the PDS for underprivileged low caste households. The coefficients in fact are positive, although imprecisely estimate in all but one instance. The results in 7 thereby provide empirical support for the presence of the bureaucratic and enforcement channels, while documenting yet again that low caste party legislators primarily target their effort towards low caste households, as opposed to all poor households.

3.5.6 Low Caste Legislators and Public Expenditures on Food

In our discussion of how politicians can affect the functioning of the PDS, we mentioned the legislative channel whereby politicians can use their voting powers in the state legislature to influence state governments to add to the existing subsidy provided by the federal government, expand upon the existing set of beneficiaries, and invest in the overall improvement and upgradation of the physical infrastructure necessary for the effective implementation of the subsidized food grains scheme. In this section, we test whether legislators from low caste parties exercise their legislative powers to vote on budgetary allocations and increase public expenditures allocated to the PDS. While publicly available budgetary documents do not provide the exact public allocation towards the PDS, they do inform us on the expenditures undertaken for food and nutrition by the state government, under which PDS expenditures

[37] The mean of the dependent variable is 35 percent so the coefficient reflects a reduction by 0.039/0.352 = 11 percent.

are classified.[38] In this regard, we aggregate our electoral results to the state-level and test whether a higher fraction of legislators elected from low caste parties to the state legislative assembly results in higher public spending towards food.

Ther results from this exercise are reported in Table 8. The independent variable of interest in columns (1) and (2) is logged annual per capita state expenditures on food; in columns (3) and (4), food expenditures, scaled by total state expenditures. In each specification, the fraction of elections won by low caste parties in the state is instrumented by the fraction of close elections won by low caste parties in the state. We conduct this analysis between 1994 and 2012, corresponding to the period covered by the NSS survey rounds. Standard errors are clustered by state. The data on state expenditures is compiled by the Reserve Bank of India.

The results confirm that legislators representing low caste parties increase public expenditures allocated towards food. The results in column (2), while significant only at the 10 percent level, signify that a 10 percentage point increase in the fraction of legislators representing low caste parties is associated with a 27 percent increase in per capita spending on food. At the mean of the dependent variable, this represents an annual per capita increase in food expenditures equivalent to 39 rupees, or a little over 3 rupees per person per month. Column (4) also detects a positive impact of low caste party legislators on the fraction of public expenditures allocated towards food spending (p-value of 0.106). This confirms an active redistribution by legislators representing low caste parties to increase public spending on programs which would have a higher likelihood of benefiting low caste citizens. Importantly, this confirms that the increase in per capita food expenditures documented in column (2) is not due to a secular increase in aggregate spending effected by low caste parties.

3.6 Robustness Checks

We now verify the robustness of our baseline results to alternate defintions of close elections, changes in sample size and alternate specification choices. We conclude with a set of placebo tests.

3.6.1 Alternate Thresholds of Close Elections

All the results discussed till now rest upon an election being considered to be close if the difference between the winning (losing) low-caste party and the losing (winning) non-low caste party is less than or equal to 5 percent of the votes caste. We thereby begin our robustness checks by verifying that our results are not sensitive to this specific 5 percent threshold used to classify close elections. Columns (1)-(6) of Table 9 show that both the extensive and the intensive margin results remain stable to alterations of the close election threshold to 3, 4 and 6 percent differences in victory margin. All the coefficients are statistically significant at the 5 percent level, with the exception of the extensive margin coefficient at the 3 percent threshold of close elections (column (1)), which is significant at the 10 percent level.

[38]For the handful of states for which we can access this information through statements submitted by states to the federal auditor, we see that PDS expenditures form the overwhelming majority of state expenditures classified under food and nutrition.

One potential concern with our specifications is the lack of close elections in 40 percent of the district-electoral cycle pairs in our sample. For each of these district-cycle pairs, the value of our instrument is set to zero as the denominator is 0. While all our specifications control for the fraction of close elections contested by low caste parties in the district, it is possible that our strategy to force the instrument to take a value of 0 when there are no close elections might be biasing our results. We verify this is not the case by re-estimating the baseline results in columns (7) and (8) of Table 9 after excluding districts with no close elections between low caste and non-low caste parties from the sample. Reassuringly, we are still able to detect a positive and statistically significant impact of low caste party legislators on household consumption from the PDS. Both the coefficients are statistically significant at the 1 percent level and are comparable in magnitude to those obtained when all the districts are included in the sample.

3.6.2 Restricting the Set of Low Caste Parties

We next check the robustness of the results to alternate samples. First we test the robustness of the results to the exclusion of the Left parties. Recall that we argued in favour of the inclusion of the Left parties within the umbrella of low caste parties as they had repeatedly formed coalitions with low caste parties and also shared very similar policy objectives. However, as the Left parties have typically viewed policy debates through the prism of class, as opposed to caste, they strictly cannot be considered as low caste parties. In this regard, columns (1) and (2) drop the states of West Bengal and Kerala where the Left parties have been the strongest and achieved the majority of their electoral success. The coefficients remain positive and statistically significant at the 5 percent level when we exclude these states.[39]

In ongoing (unpublished) analysis, we also verify the robustness of the baseline results after a further restriction of the sample to only major northern Indian states. This is motivated by Jaffrelot's (2003) study of the historical evolution of low caste movements in India where the origin of low caste movements in southern and western India are traced to the pre-independence period, often as a result of intervention by local princely states or British officials.[40] This could possibly make legislators representing low caste movements in southern India more effective if experience in politics is positively correlated with legislator efficacy. Here, we thereby restrict our sample to 6 major north Indian states – namely Bihar, Jharkhand, Madhya Pradesh, Chhattisgarh, Rajasthan and Uttar Pradesh – which experienced aggressive caste-based mobilization only since the late 1980s, under the auspices of the JD or the BSP. The results confirm that legislators from low caste parties continue to have a positive impact on low caste households' consumption from the PDS in these states. While we lose half of our sample size, the coefficients are significant at the 15 percent level. In terms of magnitude, a 10 percentage point increase in the fraction of elections won by low caste parties in a district increases the likelihood of low caste households to consume for the PDS by 2 percent; along the intensive margin, the corresponding impact is a 5 percent

[39]When the same specification is re-estimated for non-low caste households, the coefficients remain small and imprecisely estimated, confirming yet again a null effect.

[40]For instance, the low caste movement in Tamil Nadu was linked to the Dravidian self-respect movement started by Periyar prior to independence.

increase in the per capita monthly quantity of food grains purchased by low caste households from the PDS.

3.6.3 Robustness to Dropping Individual States

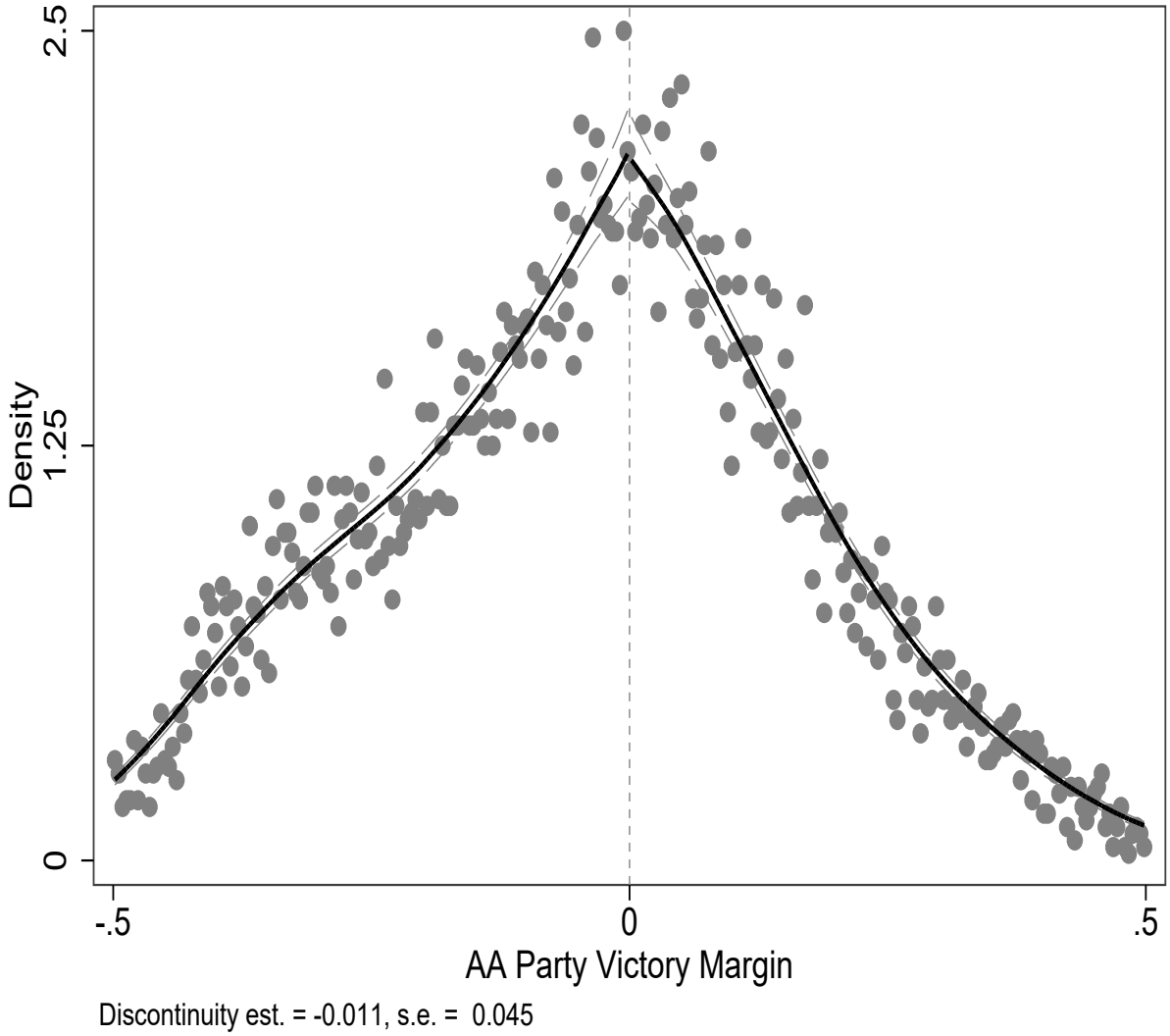
Our discussion of low caste parties in Section 3.2.2 noted that a number of these parties are regional parties, powerful within their state but having limited presence outside the state. It is thereby possible that our results are being driven by a single party, operating out of a single state, and is not due to a common policy goal shared by these parties of targeting public resources to low caste citizens. We check this after dropping individual states from our sample and re-estimating our specification. The results are presented as coefficient plots in Figure 4 (Appendix). The vertical lines reflect the 90 percent confidence intervals and we see that along both the extensive and the intensive margins, we continue to identify a positive and statistically significant impact of low caste parties after dropping individual states from our sample. This assuages concerns that the results are being driven by a single low caste party or state.

3.6.4 Placebo Tests

Finally, we validate our results using two placebo tests. The first shows that a 1 period lead of the fraction of elections won by low caste parties has no impact on low caste households' consumption from the PDS. The first two columns of Table 10 confirm that the associated coefficients with a single period lead of the fraction of elections won by low caste parties are negative and not statistically significant. The remaining columns in Table 10 show that the fraction of elections won by the two major mainstream parties – namely the BJP and the INC – do not impact low caste households' consumption from the PDS. In each case, the fraction of elections won in the district by the BJP (INC) is instrumented by the fraction of close elections won in the district by the BJP (INC). None of the coefficients are statistically significant and are smaller by an order of magnitude.

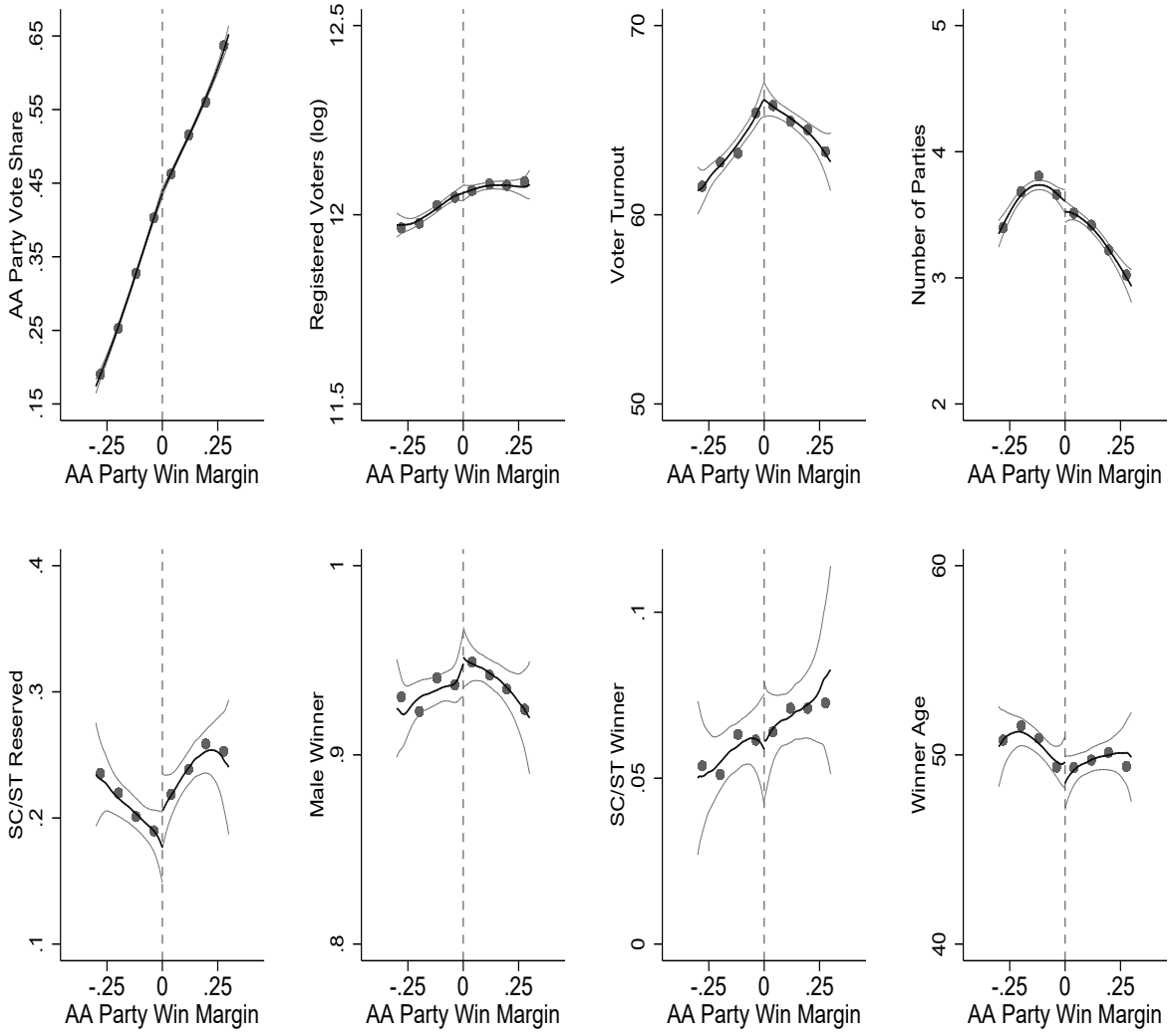
3.7 Figures

Figure 1: McCrary Test for Discontinuity of Low Caste Party Win Margin Around 0



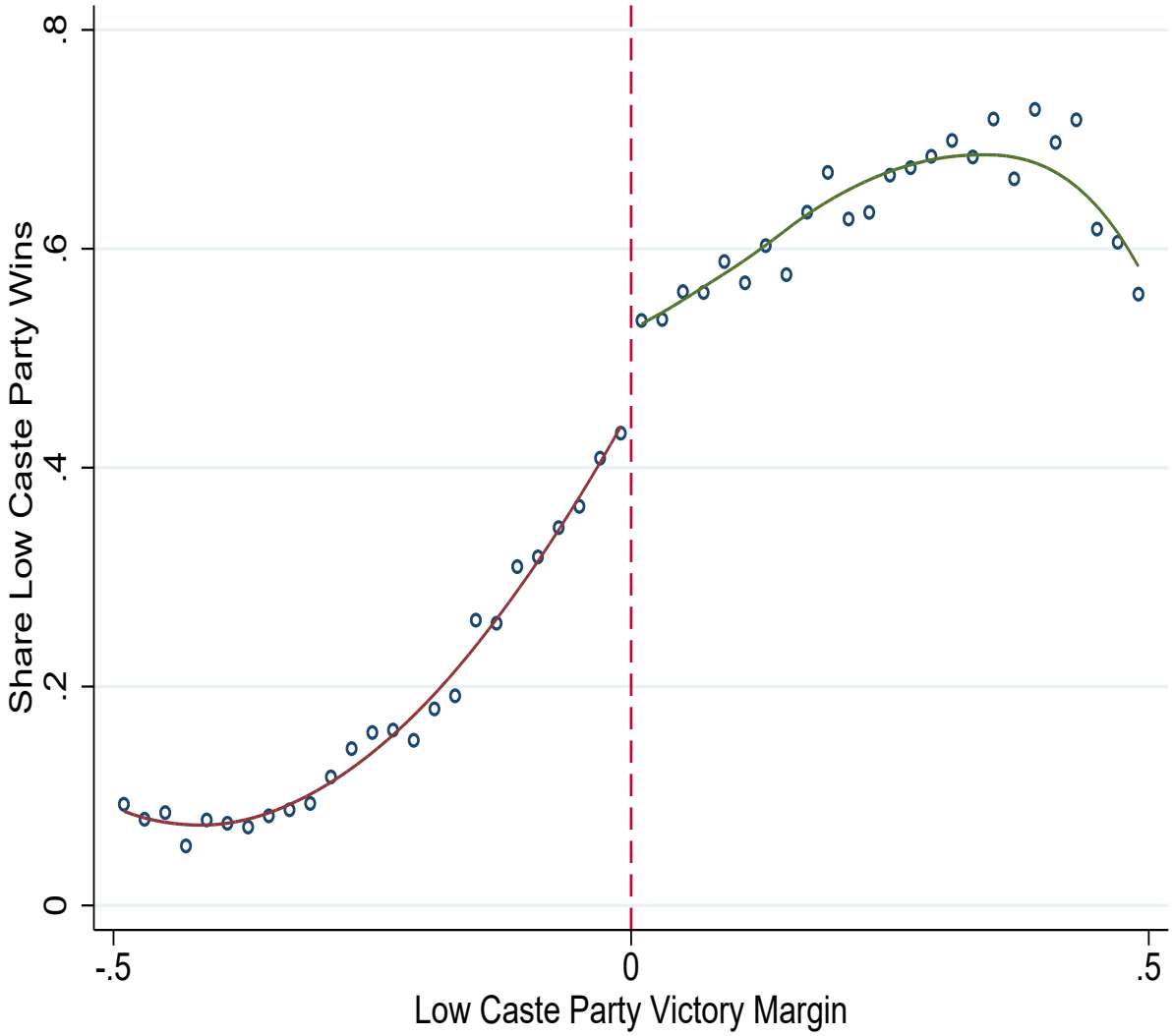
Notes: The figure plots the density of elections by win margin. A victory margin in excess of 0 represents an AA party victory; a victory margin less than 0 represent an AA party loss. Each point on the plot represents the density of AA party victory margin in each of the 500 bins of AA party vote share. The line segments indicate the test for a discontinuity in the AA party victory margin at the threshold of 0, as proposed by McCrary (2007). The associated robust t-statistic and p-value is -1.35 and 0.176 - suggesting there is no sorting around the "win" discontinuity.

Figure 2: Check of Covariate Balance Across Low Caste Party Win Margin



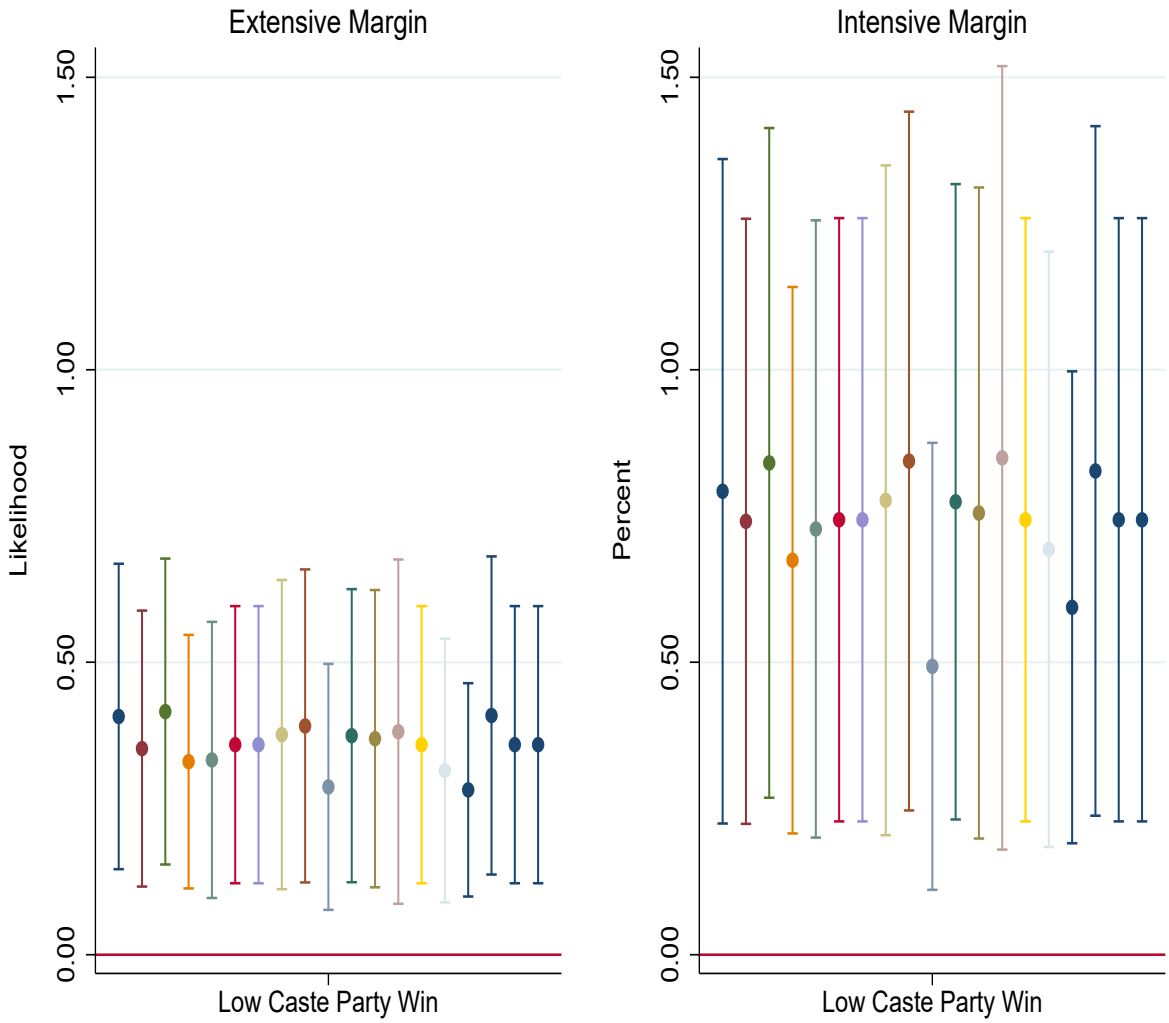
Covariate balance checks at the constituency level. The following covariates are tested: AA party vote share; number of eligible voters; voter turnout; share of constituencies reserved for SC/ST candidates; number of candidates contesting; share of male winners; share of winners from SC/ST community; share of male winners. Registered voters Each point corresponds to unconditional means of each covariate in each of the 500 bins of AA party vote share. The solid lines represent 95 percent confidence intervals.

Figure 3: Low Caste Party Win Margin and Fraction of Elections Won by Low Caste Parties in the District



The above figure presents a graphical representation of the first stage of empirical specification. The horizontal axis is divided into 100 1 percentage point bins between -0.5 and 0.5 of low caste party win margin. Each point depicts the unconditional mean of the fraction of close elections won by low caste parties in the district corresponding to that bin. The red dashed line represents the win margin of 0, where the outcome of the election switches discontinuously from a low caste party defeat to a low caste party victory. The horizontal line is a local second order polynomial fit on either side of the cutoff of 0.

Figure 4: Robustness of Results to Exclusion of Individual States



The above figures plot the robustness of our baseline results to the exclusion of individual states. The outcome of interest in the left hand panel is a dummy equaling 1 if the household had made any purchases from the PDS in the past 30 days; the outcome of interest in the right hand panel is the logged quantity of food grains purchased from the PDS by households in the past 30 days. The independent variable of interest is the fraction of elections won by low caste parties in the district, instrumented by the fraction of close elections won by low caste parties in the district. All specifications include district and survey round fixed effects, along with household and district covariates. Standard errors are clustered by district. The vertical lines represent the 90 percent confidence intervals.

3.8 Tables

3.8.1 Main Results

Table 2a: Verifying District Political Factors Don't Predict the Fraction of Close Elections Won by Low Caste Parties

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Past Support, Lag 1			Contemporaneous Factors			
	State Power	Share Win	Vote Share	Voter Turnout	Effective No. of Parties	Average Candidates	SC/ST Reserved
Low Caste Party Close Win 5pc	-.0017	-.0299	.0735	-.0748	.0162	-.0064*	-.1448
	(.0695)	(.0686)	(.1431)	(.1660)	(.0213)	(.0036)	(.1673)
Observations	1233	1469	1469	1724	1724	1724	1724
R^2	.47	.43	.43	.41	.41	.42	.41

Notes: This table shows that the fraction of close elections won by low caste parties in a district is orthogonal to district-level political observables. The unit of observation is the district. The outcome of interest in each column is the fraction of close elections won by low caste parties in the district in an electoral cycle. All specifications include district and election year fixed effects. The independent variable of interest in column (1) is whether a low caste party was in power during the previous electoral cycle; in column (2), the fraction of elections won by low caste parties in the previous electoral cycle; in column (3), the district-vote share of low caste parties in the past electoral cycle; in column (4), the contemporaneous voter turnout; in column (5), the average effective number of parties contesting elections; in column (6), the average number of candidates contesting elections; in column (7), the fraction of constituencies in the district reserved for SC/ST candidates. Standard errors in parentheses are clustered by district.

Table 2b: Verifying District Demographic Factors Don't Predict the Fraction of Close Elections Won by Low Caste Parties

	(1) Share Rural	(2) Share LowCaste	(3) Share Female	(4) Share Female Head	(5) Avg Household Size	(6) Share Literate	(7) Share Secondary+
Fraction Close Win 5pc	-.0994	.0297	.3982	-.4682	-.0346	.1632	.3256
	(.2438)	(.2076)	(.8478)	(.4653)	(.0430)	(.2391)	(.2966)
Observations	711	711	711	711	711	711	711
R^2	.58	.58	.58	.58	.58	.58	.58

Notes: This table shows that the fraction of close elections won by low caste parties in a district is orthogonal to district-level demographic observables. The unit of observation is the district. The outcome of interest in each column is the fraction of close elections won by low caste parties in the district in an electoral cycle. All specifications include district and survey round fixed effects. The independent variable of interest in column (1) is the share of rural households; in column (2), the share of low caste households; in column (3), the share of females; in column (4), the share of female headed households; in column (5), the average household size; in column (6), the share of literate workers; in column (7), the share of workers with secondary education or higher. Standard errors in parentheses are clustered by district.

Table 2c: Verifying District Economic Factors Don't Predict the Fraction of Close Elections Won by Low Caste Parties

	(1) Consumption Per Capita	(2) Land Holdings	(3) LFP	(4) Share Self Employed	(5) Share Salaried Worker	(6) Share Formal Sector	(7) Share Public Sector
Fraction Close Win 5pc	-.1029	-.0013	-.0760	.1656	.1391	.5017	.2131
	(.1263)	(.0151)	(.2957)	(.3002)	(.3215)	(.3894)	(.3399)
Observations	711	711	711	711	711	711	711
R^2	.58	.58	.58	.58	.58	.58	.58

Notes: This table shows that the fraction of close elections won by low caste parties in a district is orthogonal to district-level economic observables. The unit of observation is the district. The outcome of interest in each column is the fraction of close elections won by low caste parties in the district in an electoral cycle. All specifications include district and survey round fixed effects. The independent variable of interest in column (1) is monthly per capita consumption for households; in column (2), the average land holdings for rural households; in column (3), labour force participation rate; in column (4), the share of self-employed workers; in column (5), the share of salaried workers; in column (6), the share of formal sector workers; in column (7), the share of workers employed in the public sector. Standard errors in parentheses are clustered by district.

Table 3: Low Caste Party Legislators and Household Consumption from PDS: OLS and IV

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pr(Any PDS = 1)				Monthly Per Capita PDS (Logged)			
Low Caste Party Win	.0373	.0487*	.3692**	.3591**	-.0049	.0077	.8016**	.7436**
	(.0271)	(.0287)	(.1872)	(.1440)	(.0410)	(.0433)	(.4006)	(.3135)
Observations	222452	222452	222452	222452	222452	222452	222452	222452
R^2	.26	.31	.25	.30	.24	.30	.23	.29
Dep Var Mean	.37	.37	.37	.37	1.80	1.80	1.80	1.80
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Specification	OLS	OLS	IV	IV	OLS	OLS	IV	IV

Notes: This table presents the baseline results estimating the impact of low caste party legislators on low caste households' purchase of food grains from the PDS. The unit of observation is the household. The outcome of interest in columns (1)-(4) is a dummy equaling 1 if the household purchased any food grains from the PDS in the past 30 days; in columns (5)-(8), the outcome is the logged quantity of food grains purchased from the PDS over the past 30 days. The sample is restricted to low caste households. The independent variable of interest is the fraction of elections won by low caste parties in the district. The coefficients in columns (1)-(2) and (5)-(6) are estimated using an OLS specification; in columns (3)-(4) and (7)-(8) are estimated using an IV specification. In the IV specification, the fraction of elections won by low caste parties in the district is instrumented by the fraction of close elections won by low caste parties in the district. All specifications include the fraction of close elections contested by low caste parties, along with district, survey round, survey subround, and electoral cycle fixed effects. Columns (2), (4), (6) and (8) also include household and district covariates. Standard errors in parentheses are clustered by district.

Table 4: AA Party Representation Shock and Consumption of Food Grains from the Public Distribution System for High Caste Households: 1999-2012

			Panel A: Extensive Margin			
	(1)	(2)	(3)	(4)	(5)	(6)
			Pr(Any PDS = 1)			
Reserved Constituencies	.1346***		-.0215		-.0262	
	(.0509)		(.0240)		(.0325)	
Non-Reserved Constituencies		.2181*		-.0269		.0043
		(.1145)		(.0367)		(.0747)
Observations	219411	219695	219411	219695	219411	219695
R^2	.31	.31	.31	.31	.31	.31
Dep Var Mean	.37	.37	.37	.37	.37	.37
			Panel B: Intensive Margin			
	(1)	(2)	(3)	(4)	(5)	(6)
			Per Capita PDS (Logged)			
Reserved Constituencies	.2071*		-.0076		-.0498	
	(.1076)		(.0441)		(.0619)	
Non-Reserved Constituencies		.4902**		-.0269		-.0152
		(.2344)		(.0679)		(.1158)
Observations	219411	219695	219411	219695	219411	219695
R^2	.30	.29	.30	.30	.30	.30
Dep Var Mean	1.78	1.80	1.80	1.80	1.80	1.80

Notes: The above table shows the impact of legislators from low caste and mainstream parties on low caste households' consumption from the PDS across reserved and non-reserved constituencies. The unit of observation is the household. The dependent variable in Panel A is a dummy equaling 1 if the household has purchased any food grains from the PDS in the past 30 days; the dependent variable in Panel B is the logged quantity of food grains purchased by the household from the PDS in the past 30 days. Reserved constituencies refer to constituencies from which only candidates from low caste SC/ST backgrounds can contest elections; non-reserved constituencies are constituencies from which any candidate can contest elections. The independent variable of interest in columns (1) and (2) is the fraction of elections won by low caste parties in reserved/non-reserved constituencies in the district; in columns (3) and (4), fraction of elections won by the BJP; in columns (5) and (6), the fraction of elections won by the INC. In each specification, the fraction of elections won by low caste parties/BJP/INC from reserved (non-reserved) constituencies is instrumented by the fraction of close elections won by low caste parties/BJP/INC from reserved (non-reserved) constituencies. All specifications control for the fraction of close elections contested by the respective party from reserved and non-reserved constituencies, along with district, survey round, survey subround, and electoral cycle fixed effects. Household and district-level covariates are also included. Standard errors in parentheses are clustered by district.

Table 5: Low Caste Party Legislators and Household Consumption from the PDS: Differential Effects by Political Factors

	Panel A: Extensive Margin					
	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Any PDS = 1)					
Low Caste Party Win	.3586**	.4419***	.3441	.4410**	.3222**	.4512**
	(.1454)	(.1502)	(.2195)	(.2157)	(.1614)	(.1826)
SC*Low Caste Party Win	-.0119					
	(.0271)					
ST*Low Caste Party Win	.0203					
	(.0842)					
High Low Caste*Low Caste Party Win		-.1233				
		(.0978)				
High Reserved*Low Caste Party Win			-.0004			
			(.2155)			
Low Caste Party Win*Low Caste Party Govt				-.1095		
				(.2765)		
High Turnout*Low Caste Party Win					.0939	
					(.1300)	
Last 2 Years*Low Caste Party Win						-.1637*
						(.0904)
Observations	222452	222452	222452	222452	222452	222452
R^2	.30	.30	.30	.30	.30	.30
Dep Var Mean	.39	.39	.39	.39	.39	.36
	Panel B: Intensive Margin					
	(1)	(2)	(3)	(4)	(5)	(6)
	Per Capita PDS (Logged)					
	(1)	(2)	(3)	(4)	(5)	(6)
Low Caste Party Win	.7507**	.9424***	.9209	.7607*	.7102**	.9963**
	(.3225)	(.3458)	(.5716)	(.3933)	(.3560)	(.4168)
SC*Low Caste Party Win	-.0445					
	(.0581)					
ST*Low Caste Party Win	.0186					
	(.1630)					
High Low Caste*Low Caste Party Win		-.2645				
		(.2183)				
High Reserved*Low Caste Party Win			-.3439			
			(.5441)			
Low Caste Party Win*Low Caste Party Govt				.0575		
				(.5876)		
High Turnout*Low Caste Party Win					.0755	
					(.2610)	
Last 2 Years*Low Caste Party Win						-.4492**
						(.1857)
Observations	222452	222452	222452	222452	222452	222452
R^2	.29	.29	.29	.29	.29	.28
Dep Var Mean	1.91	1.91	1.91	1.83	1.91	1.80

Notes: The above table shows the differential effect of low caste party legislators across specific low caste groups, and other political factors. The unit of observation is the household. The dependent variable in Panel A is a dummy equaling 1 if the household has purchased any food grains from the PDS in the past 30 days; the dependent variable in Panel B is the logged quantity of food grains purchased by the household from the PDS in the past 30 days. The interaction term in column (1) is a dummy equaling 1 if the household is a SC or ST; in column (2), a dummy equaling 1 if the district has a high share of low caste

Table 6: Low Caste Party Representation Shock and Consumption from PDS: Differential Effects Across Poor Households

	Low Caste				Non-Low Caste			
	Bottom Decile		Bottom Quartile		Bottom Decile		Bottom Quartile	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Any PDS	PDS Per Capita	Any PDS	PDS Per Capita	Any PDS	PDS Per Capita	Any PDS	PDS Per Capita
Low Caste Party Win	.3596**	.7502**	.3738**	.7783**	.1289	.2460	.1422	.2685*
	(.1455)	(.3164)	(.1455)	(.3165)	(.0909)	(.1531)	(.0904)	(.1535)
Low Caste Party Win*Bottom 10pc	-.0155	-.0715			.0644	.1007		
	(.0437)	(.0843)			(.0619)	(.1207)		
Low Caste Party Win*Bottom 25pc			-.0335	-.0891			-.0121	-.0274
			(.0341)	(.0699)			(.0424)	(.0751)
Observations	222452	222452	222452	222452	84020	84020	84020	84020
R^2	.31	.30	.32	.30	.23	.21	.24	.22
Dep Var Mean	.48	2.60	.46	2.33	.36	1.73	.33	1.47

Notes: The above table shows the differential effect of low caste party legislators across households in the bottom 10 (25) percent of the consumption distribution. The unit of observation is the household. The dependent variable in columns (1), (3), (5) and (7) is a dummy equaling 1 if the household has purchased any food grains from the PDS in the past 30 days; the dependent variable in columns (2), (4), (6) and (8) is the logged quantity of food grains purchased by the household from the PDS in the past 30 days. The interaction term in columns (1), (2), (5) and (7) is a dummy equaling 1 if the household falls in the bottom 10 percent of the monthly per capita expenditures distribution; in columns (2), (4), (6) and (8), a dummy equaling 1 if the household falls in the bottom 25 percent of the monthly per capita expenditures distribution. In each instance, the fraction of elections won by low caste parties in the district is instrumented by the fraction of close election won by low caste parties in the district. All specifications control for the fraction of close elections contested by low caste parties, along with district, survey round, survey subround, and electoral cycle fixed effects. Household and district-level covariates are also included. Columns (1)-(4) restrict the sample to low caste households; columns (4)-(8) restrict the sample to non-low caste households. Standard errors in parentheses are clustered by district.

Table 7: Low Caste Party Wins and Exclusion Errors from PDS

	(1)	(2)	(3)	(4)
	Pr(No PDS = 1)			
	Low Caste		Non-Low Caste	
	BPL	BPL or Rural and Landless	BPL	BPL or Rural and Landless
Low Caste Party Win	-.4111**	-.3926**	-.2593	-.1139
	(.1923)	(.1525)	(.2333)	(.1527)
Observations	61584	97753	9729	17715
R^2	.31	.30	.27	.24
Dep Var Mean	.56	.55	.68	.69

Notes: This table shows the impact of low caste parties on the likelihood of eligible low and non-low caste households being excluded from the PDS. The unit of observation is the household. The dependent variable in each specification is a dummy equaling 1 if the household did not undertake any consumption of food grains from the PDS in the past 30 days. Columns (1) and (3) restrict the sample to BPL households; columns (2) and (4) restrict the sample to households who are either BPL, or rural and landless. The households considered in columns (1) and (2) are low caste households; in columns (3) and (4), non-low caste households. The independent variable of interest is the fraction of elections won by low caste parties in the district, instrumented by the fraction of close elections won by low caste parties in the district. All specifications control for the fraction of close elections contested by low caste parties, along with district, survey round, survey subround, and electoral cycle fixed effects, and household and district covariates. Standard errors in parentheses are clustered by district.

Table 8: Low Caste Party Representation and Public Expenditures on Food

	(1)	(2)	(3)	(4)
	Food Expenditures Per Capita		Food Expenditures Share	
Low Caste Party Wins	2.9722*	2.6971*	.0719*	.0735
	(1.6675)	(1.5933)	(.0429)	(.0491)
Observations	262	262	262	262
R^2	.67	.74	.40	.44
Dep.	143.48	143.48	.02	.02

Notes: The results in this table show the impact of low caste party legislators on state-level food expenditures. The unit of observation is the state. The outcome of interest in columns (1) and (2) is the logged per capita state expenditures allocated towards food and nutritions; in columns (3) and (4), expenditures allocated towards food and expenditures as a share of total expenditures. The independent variable in each case is the fraction of elections won by low caste parties in the state, instrumented by the fraction of close elections won by low caste parties in the state. All specifications include the fraction of close elections contested by low caste parties in the state, along with state, year and electoral cycle fixed effects. Columns (2) and (4) also include state-specific covariates. Standard errors are in parentheses, clustered by state.

Table 9: Low Caste Party Legislators and Consumption from PDS: Robustness to Alternate Thresholds of Close Elections and Excluding Districts Without Close Elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Alternate Threshold For Close Elections							
	3 Percent		4 Percent		6 Percent		Exclude Districts Without Close Elections	
	Any PDS	PDS Per Capita	Any PDS	PDS Per Capita	Any PDS	PDS Per Capita	Any PDS	PDS Per Capita
Low Caste Party Win	.3461*	.7984**	.4071***	.7660**	.1735**	.3551**	.2859***	.6108***
	(.1835)	(.3849)	(.1561)	(.3335)	(.0819)	(.1537)	(.0967)	(.2255)
Observations	222452	222452	222452	222452	222452	222452	140903	140903
R^2	.30	.29	.30	.29	.31	.30	.34	.32

Notes: The above table shows the robustness of the baseline results to alternate definitions of close elections and restricting the sample to districts with at least 1 close election. The unit of observation is the household. The dependent variable in columns (1), (3), (5) and (7) is a dummy equaling 1 if the household has purchased any food grains from the PDS in the past 30 days; in columns (2), (4), (6) and (8), the logged quantity of food grains purchased by the household from the PDS in the past 30 days. The independent variable of interest is the fraction of close elections won by low caste parties in the district, instrumented by the fraction of close election won by low caste parties in the district. The threshold for close elections in columns (1) and (2) is a victory margin of 3 percent; in columns (3) and (4), 4 percent; in columns (5) and (6), 6 percent. Columns (7) and (8) exclude districts which have no close elections at the 5 percent margin of victory involving low caste parties. All specifications control for the fraction of close elections contested by low caste parties, along with district, survey round, survey subround, and electoral cycle fixed effects. Household and district-level covariates are also included. Standard errors in parentheses are clustered by district.

Table 10: Low Caste Party Representation Shock and Consumption from PDS: Placebos

	(1)	(2)	(3)	(4)	(5)	(6)
	Low Caste Party Wins, Lead 1		Mainstream Party Wins			
	Any PDS	PDS Per Capita	Any PDS	PDS Per Capita	Any PDS	PDS Per Capita
Low Caste Party Win, Lead 1	-.0277	-.1987				
	(.1322)	(.3143)				
BJP Win			.0147	.0164		
			(.0829)	(.1327)		
INC Win					-.0474	-.0640
					(.0487)	(.0914)
Observations	183766	183766	222452	222452	222452	222452
R^2	.33	.32	.31	.30	.31	.30

Notes:The above table shows the results from 3 placebo tests. The unit of observation is the household. The dependent variable in columns (1), (3) and (5) is a dummy equaling 1 if the household has purchased any food grains from the PDS in the past 30 days; in columns (2), (4) and (6), the logged quantity of food grains purchased by the household from the PDS in the past 30 days. The independent variable of interest in columns (1) and (2) is a single period lead of the fraction of elections won by low caste parties in the district; in columns (3) and (4), the fraction of elections won by the BJP; in columns (5) and (6), the fraction of elections won by the INC. In each instance, the fraction of close elections won by a party is instrumented by the fraction of close elections won by the party. All specifications control for the fraction of close elections contested by the parties, along with district, survey round, survey subround, and electoral cycle fixed effects. Household and district-level covariates are also included. Standard errors in parentheses are clustered by district.

References

Acharya, A., J. E. Roemer, and R. Somanathan (2015). Caste, Corruption and Political Competition in India. *Research in Economics 69*(3), 336–352.

Albouy, D. (2011). Do voters affect or elect policies? A new perspective, with evidence from the U.S. Senate. *Electoral Studies 30*(1), 162–173.

Alesina, A., R. Baqir, and W. Easterly (2000). Redistributive Public Employment. *Journal of Urban Economics 48*(2).

Anderson, G. M. and D. Halcoussis (1996). The Political Economy of Legal Segregation: Jim Crow and Racial Employment Patterns. *Economics & Politics 8*(1), 1–15.

Ang, D. (2018). Do 40-Year-Old Facts Still Matter? Long-Run Effects of Federal Oversight under the Voting Rights Act. (703).

Angrist, J. and J.-S. Pischke (2008). *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton University Press.

Ankit, R. (2018). Caste Politics in Bihar. *History and Sociology of South Asia 12*(2), 115–136.

Banzhaf, H. S. and R. P. Walsh (2008). Do People Vote with Their Feet? An Empirical Test of Tiebout's Mechanism. *American Economic Review 98*(3), 843–863.

Beach, B., D. B. Jones, T. Twinam, and R. Walsh (2018). Minority Representation in Local Government.

Beaudry, P., D. A. Green, and B. Sand (2012). Does Industrial Composition Matter for Wages? A Test of Search and Bargaining Theory. *Econometrica 80*(3), 1063–1104.

Bernini, A., G. Facchini, and C. Testa (2018). Race, Representation, and Local Governments in the U.S. South: The Effect of the Voting Rights Act.

Berry, C., J. Grogger, and M. West (2015). The Growth of Local Government.

Besley, T. and R. Burgess (2554). Land Reform, Poverty Reduction, and Growth: Evidence from India. *Quarterly Journal of Economics 115*(2), 389–430.

Besley, T. and S. Coate (1997). An Economic Model of Representative Democracy. *Quarterly Journal of Economics 112*(1), 85–114.

Besley, T., R. Pande, L. Rahman, and V. Rao (2004). The politics of public good provision: Evidence from Indian local governments. *Journal of the European Economic Association 2*(2-3), 416–426.

Bhalotra, S. and I. Clots-figueras (2014). Health and the Political Agency of Women. *American Economic Journal: Economic Policy 6*(739), 164–197.

Bhavnani, R. R. (2017). Do the effects of temporary ethnic group quotas persist? Evidence from India. *American Economic Journal: Applied Economics 9*(3), 105–123.

Blank, R. M. (1994). *Public Sector Growth and Labor Market Flexibility: The United States versus the United Kingdom*. Number January. University of Chicago Press.

Bound, J. and R. B. Freeman (1992). What Went Wrong? The Erosion of Relative Earnings and Employment Among Young Black Men in the 1980s. *The Quarterly Journal of Economics 107*(1), 201–232.

Boustan, L. P. (2010). Was Postwar Suburbanization White Flight? *Quarterly Journal of Economics* (February).

Boustan, L. P. and R. A. Margo (2009). Race, Segregation, and Postal Employment: New Evidence on Spatial Mismatch. *Journal of Urban Economics 65*(1), 1–10.

Bros, C. and M. Couttenier (2015). Untouchability, homicides and water access. *Journal of Comparative Economics 43*(3), 549–558.

Button, J. (1989). *Blacks and Social Change*. Princeton: Princeton University Press.

Card, D. and A. B. Krueger (1992). School Quality and Black-White Relative Earnings: A Direct Assessment. *Quarterly Journal of Economics 107*(1), 151–200.

Carrington, W. J., K. McCue, and B. Pierce (2000). Using Establishment Size to Measure the Impact of Title VII and Affirmative Action. *Journal of Human Resources 35*(3), 503.

Carruthers, C. and M. H. Wanamaker (2017). Separate and Unequal in the Labor Market: Human Capital and the Jim Crow Wage Gap. *Journal of Labor Economics 35*(3), 655–696.

Cascio, E. U. and E. Washington (2014). Valuing the Vote: The Redistribution of Voting Rights and State Funds Following the Voting Rights Act of 1965. *The Quarterly Journal of Economics 129*(1), 1–55.

Cattaneo, M. D., M. Jansson, and X. Ma (2018). Manipulation testing based on density discontinuity. *Stata Journal 18*(1), 234–261.

Chakravarthi, R. (2014). Political Favoritism and Public Good Provision : Evidence from Indian States.

Chandra, K. (2004). *Why Ethnic Parties Succeed: Patronage and Ethnic Head Counts in India*. New York: Cambridge University.

Chay, K. (1998). The Impact of Federal Civil Rights Policy on Black Economic Progress: Evidence from the Equal Employment Opportunity Act of 1972. *Industrial and Labor Relations Review 51*(4), 608–632.

Chen, Y., J. V. Henderson, and W. Cai (2017). Political favoritism in China's capital markets and its effect on city sizes. *Journal of Urban Economics 98*, 69–87.

Clinton, J. D. and M. W. Sances (2018). The Politics of Policy: The Initial Mass Political Effects of Medicaid Expansion in the States. *American Political Science Review 112*(1), 167–185.

Clots-Figueras, I. (2011). Women in politics. Evidence from the Indian States. *Journal of Public Economics 95*(7-8), 664–690.

Collins, S. M. (1983). The Making of the Black Middle Class. *Social Problems 30*(4), 369–382.

Collins, W. J. (2003). The Labor Market Impact of State-Level Anti-Discrimination Laws, 1940-1960. *Industrial and Labor Relations Review 56*(2), 244–272.

Cox, G. W. and M. D. Mccubbins (1986). Electoral Politics as a Redistributive Game. *Journal of Politics 48*(2), 370–389.

Davidson, C. and B. Grofman (1992). *Controversies in Minority Voting: The Voting Rights Act in Perspective*. Washington: Brookings Institution Press.

Dávila, A. and A. Bohara (1994). Equal Employment Opportunity Across States: the EEOC, 1979-1989. *Public Choice 80*(3), 223–243.

Derenoncourt, E. and C. Montialoux (2018). Minimum Wages and Racial Inequality.

Deshpande, A. (2011). *The Grammar of Caste: Economic Discrimination in Contemporary India*. New Delhi: Oxford University Press.

DeSilver, D. (2014). Who's Poor in America? 50 Years Into the "War on Poverty". Technical report, Pew Research Center.

Dixit, A. and J. Londregan (1996). The Determinants of Success of Special Interests in Redistributive Politics. *Journal of Politics 58*(4), 1132–1155.

Donohue, J. J. and J. Heckman (1991). Continuous Versus Episodic Change: The Impact of Civil Rights Policy on the Economic Status of Blacks. *Journal of Economic Literature 29*(4), 1603–1643.

Donohue, J. J. and S. D. Levitt (2001). The Impact of Race on Policing and Arrests. *The Journal of Law and Economics 44*(2), 233–261.

Downs, A. (1957). An Economic Theory of Political Action in a Democracy. *Journal of Political Economy 89*(2), 392–406.

Dreze, J. and R. Khera (2000). Crime Gender and Society in India. *Population and Development Review 26*(2), 335–352.

Dube, A., T. W. Lester, and M. Reich (2010). Minimum Wage Effects Across State Borders: Estimates Using Contiguous Counties. *The Review of Economics and Statistics 92*(4), 945–964.

Duflo, E. (2004). Women as Policy Makers : Evidence from a Randomized Policy Experiment in India Author (s): Raghabendra Chattopadhyay and Esther Duflo Published by : The Econometric Society Stable URL : http://www.jstor.org/stable/3598894 . *Econometrica 72*(5), 1409–1443.

Duranton, G., L. Gobillon, and H. G. Overman (2011). Assessing the Effects of Local Taxation using Microgeographic Data. *Economic Journal 121*, 1017–1046.

Eccles, M. (1975). Race, Sex, and Government Jobs: A Study of Affirmative Action Programs in Federal Agencies.

Eisinger, P. K. (1982). Black Employment in Municipal Jobs: The Impact of Black Political Power. *The American Political Science Review 76*(2), 380–392.

Enikolopov, R. (2014). Politicians, bureaucrats and targeted redistribution. *Journal of Public Economics 120*, 74–83.

Fajnzylber, P., D. Lederman, and N. Loayza (2002). Inequality and Violent Crime. *Journal of Law and Economics 45*(1), 1–39.

Feigenbaum, J., A. Hertel-Fernandez, and V. Williamson (2018, jan). From the Bargaining Table to the Ballot Box: Political Effects of Right to Work Laws. Working Paper 24259, National Bureau of Economic Research.

Ferreira, F. and J. Gyourko (2009). Do Political Parties Matter? Evidence from US Cities. *Quarterly Journal of Economics 24*(1), 399–422.

Frazier, E. F. (1957). *Black Bourgeoisie*. Chicago.

Freeman, R. (1973). Changes in the Labor Market for Black Americans. *Brookings Papers on Economic Activity* (1), 67–123.

Freeman, R. B. (1976). *The Black Elite*. New York: McGraw-Hill.

Fresh, A. (2018). The Effect of the Voting Rights Act on Enfranchisement: Evidence from North Carolina. *Journal of Politics 80*(2), 1–46.

Fujiwara, T. (2015). Voting Technology, Political Responsiveness, and Infant Health: Evidence From Brazil. *Econometrica 83*(2), 423–464.

Gentzkow, M., J. M. Shapiro, and M. Taddy (2016). Measuring Group Differences in High-Dimensional Choices : Method and Application to Congressional Speech.

Girard, V. (2017). Stabbed in the back . Is sabotage taking place after mandated political representation ?

Glaeser, E. L. and A. Shleifer (2005). The Curley Effect: The Economics of Shaping the Electorate. *Journal of Law, Economics, and Organization 21*(1), 1–19.

Hanchinamani, B. (2001). Human Rights Abuses of Dalits in India. *Human Rights Brief 8*(2), 15–19.

Hirata, G. and R. Soares (2016). Competition and the Racial Wage Gap: Testing Becker's Model of Employer Discrimination.

Ho, D. E. (2018). Building an Umbrella in a Rainstorm: The New Vote Denial Litigation Since Shelby County. *Yale Law Journal 10304*(2012), 799–824.

Hornbeck, B. R. and S. Naidu (2014). When the Levee Breaks : Black Migration and Economic Development in the American South †. *104*(3), 963–990.

Hout, M. (1984). Occupational Mobility of Black Men: 1962 to 1973. *American Sociological Review 49*(3), 308–322.

Husted, T. A. and L. W. Kenny (1997). The Effect of the Expansion of the Voting Franchise on the Size of Government. *Journal of Political Economy 105*(1), 54–82.

Ilaiah, K. (1994). BSP and Caste as Ideology. *29*(12), 668–669.

Isen, A., M. Rossin-Slatar, and W. R. Walker (2017). Every Breath You Take - Every Dollar You'll Make: The Long-Term Consequences of the Clean Air Act of 1970.

Iyer, L. and A. Mani (2012). Traveling Agents: Political Change and Bureaucratic Turnover in India. *Review of Economics and Statistics 94*(August), 723–739.

Iyer, L., A. Mani, P. Mishra, and P. Topalova (2012). The Power of Political Voice : Women's Political Representation and Crime in India The Power of Political Voice : Women ' s Political Representation and Crime in India. *American Economic Journal: Applied Economics 4*(4), 165–193.

Jaffrelot, C. (2003). *India's Silent Revolution: The Rise of the Lower Castes in North India*. New York: Columbia University Press.

Jaffrelot, C. (2006). The Impact of Affirmative Action in India: More Political than Socioeconomic. *India Review 5*(2), 173–189.

Jaffrelot, C. (2011). *Religion, Caste and Politics in India*.

Jensenius, F. R. (2015). Development from representation? A study of quotas for the scheduled castes in India. *American Economic Journal: Applied Economics 7*(3), 196–220.

Karlan, P. S. (1989). Maps and Misreadings: The Role of Geographic Compactness in Racial Vote Dilution Litigation. *Harvard Civil Rights and Civil Liberties Review 24*, 173.

Katz, L. F. and D. H. Autor (1999). Changes in the Wage Structure and Earnings Inequality. *Handbook of Labor Economics 3A*, 1463–1555.

Katz, M. B., M. J. Stern, and J. Fader (2005). The New African American Inequality. *Journal of American History 92*(1), 75–108.

Keech, W. (1968). *The Impact of Negro Voting: The Role of the Vote in the Quest for Equality*. Chicago: Rand McNally.

King, D. The Persistence of Material Racial Inequality: The Role of the American State's Public Sector.

Kline, P. and E. Moretti (2013). Place Based Policies with Unemployment. *American Economic Review 103*(3), 238–243.

Kling, J., J. Liebman, and L. Katz (2007). Experimental Analysis of Neighborhood Effects. *Econometrica 75*(1), 83–119.

Kose, E., E. Kuka, and N. Shenav (2019). Who Benefited from Women's Suffrage?

Kousser, J. M. (1980). Progressivism-For Middle-Class Whites Only: North Carolina Education. *The Journal of Southern History 46*(2), 169–194.

Krislov, S. (1967). *Negro in Federal Employment: The Quest for Equal Opportunity*. Minneapolis: University of Minnesota Press.

Kuziemko, I. and E. Washington (2018). Why Did Democrats Lose the South? Bringing New Evidence to an Old Debate. *American Economic Review 108*(10), 2830–2867.

Laasko, M. and R. Taagepera (1979). "Effective" Number of Parties. *Comparative Political Studies 12*(1), 3–27.

Laird, J. (2017). Public Sector Employment Inequality in the United States and the Great Recession. *Demography 54*, 391–411.

Leonard, J. S. (1990). The Impact of Affirmative Action Regulation and Equal Employment Law on Black Employment. *Journal on Economic Perspectives 4*(4), 47–63.

Lindbeck, A. and J. W. Weibull (1987). Balanced-Budget Redistribution as the Outcome of Political Competition Author (s): Assar Lindbeck and Jörgen W . Weibull Published by : Springer Stable URL : https://www.jstor.org/stable/30024725. *Public Choice 52*(3), 273–297.

Logan, T. D. (2018). Do Black Politicians Matter?

Maccoon, J. P. (1980). The Enforcement of the Preclearance Requirement of Section 5 of the Voting Rights Act of 1965. *Catholic University Law Review 29*(1), 107–127.

Mangubhai, J. P. and J. G. Lee (2012). *Dalit women speak out: Caste, class and gender violence in India.* Zubaan: Zubaan.

Margo, R. (1982). Race Differences in Public School Expenditures: Disenfranchisement and School Finance in Louisiana, 1890-1910. *Social Science History 6*(1), 9–34.

Mas, A. and E. Moretti (2009). Racial Bias in the 2008 Presidential Election. *American Economic Review 99*(2), 323–329.

Matthews, D. and J. W. Prothro (1966). *Negroes and the New Southern Politics*. New York: Harcourt, Brace and World.

McCrary, J. (2007). Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test.

Medoff, M. (1999). Allocation of Time and Hateful Behavior. *International journal of language & communication disorders / Royal College of Speech & Language Therapists 42*(2), 130–153.

Meltzer, A. and S. Richard (1981). A Rational Theory of the Size of Government. *1Journal of Political Economy 89*(5), 914–927.

Meyersson, E. (2014). Islamic Rule and the Empowerment of the Poor and Pious. *Econometrica 82*(1), 229–269.

Miller, G. (2008). Women's Suffrage, Political Responsiveness, and Child Survival in American History. *The Quarterly Journal of Economics 123*(3), 1287–1327.

Minchin, T. J. (2015). Making Best Use of the New Laws: The NAACP and the Fight for Civil Rights in the South, 1965-1975. *Journal of Southern History 74*(3), 669–702.

Motomura, H. (1983). Preclearance under Section Five of the Voting Rights Act. *North Carolina Law Review 61*(2), 189–246.

Naidu, S. (2012). SUFFRAGE, SCHOOLING, AND SORTING IN THE POST-BELLUM U.S. SOUTH.

Nay, L. A. and J. E. J. Jones (1989). Equal Employment and Affirmative Action in Local Governments: A Profile. *Law and Inequality 8*(1), 103–149.

Nye, J. V. C., I. Rainer, and Strat (2015). Do Black Mayors Improve Black Relative to White Employment Outcomes: Evidence from Large US Cities. *Journal of Law, Economics, and Organization 31*(2), 383–430.

Nyiri, Z. and C. English (2007). Is Europe a Good Place for Racial and Ethnic Minorities?

Pande, R. (2003). Can Mandated Political Representation Increase Policy Influence for DisadvantagedMinorities? Theory and Evidence from India. *American Economic Review 93*(4), 1132–1151.

PBS (2000). The First Measured Century: Black Elected Officials.

Perman, M. (2001). *Struggle for Mastery: Disenfranchisement in the South, 1888-1908*. Chapel Hill: University of North Carolina Press.

Pitkin, H. (1967). *The Concept of Representation*.

Pitts, S. (2011). Black Workers and the Public Sector.

Poole, K. T. and H. Rosenthal (2001). D-Nominate after 10 Years : A Comparative Update to Congress: A Political-Economic History of Roll-Call Voting. *Legislative Studies Quarterly 26*(1), 5–29.

Prasad, K. (2012). Economic Liberalization and Violent Crime. *Journal of Law & Economics 55*(4), 925–948.

Pritchett, J. B. (1989). The Burden of Negro Schooling: Tax Incidence and Racial Redistribution in Postbellum North Carolina. *Journal of Economic History 49*(4), 966–973.

Roback, J. (1984). Southern Labor Law in the Jim Crow Era: Exploitative or Competitive. *University of Chicago Law Review 51*(4), 1161–1192.

Roberts, K. W. S. (1977). Voting Over Income Tax Schedules. *Journal of Public Economics 8*, 329–340.

Rodrik, D. and S. Mukand (2016). The Political Economy of Liberal Democracy.

Romer, T. (1975). Individual Welfare, Majority Voting, and the Properties of a Linear Income Tax. *Journal of Public Economics 4*, 163–185.

Santoro, W. A. and G. M. McGuire (1997). Social Movement Insiders: The Impact of Institutional Activists on Affirmative Action and Comparable Worth Policies. *Social Problems 44*(4), 503–519.

Sarkar, R. and A. Sarkar (2016). Dalit Politics in India: Recognition without Redistribution. *Economic & Political Weekly LI*(20), 14–16.

Schuit, S. and J. C. Rogowski (2017). Race, Representation, and the Voting Rights Act. *61*(3), 513–526.

Schwartz, M. A. (1967). Trends in White Attitudes Toward Negroes. Technical report, National Opinion Research Center, Chicago.

Sharma, S. (2015). Caste-based crimes and economic status: Evidence from India. *Journal of Comparative Economics 43*(1), 204–226.

Shulman, S. (1984). Competition and Racial Discrimination: The Employment Effects of Reagan's Labor Market Policies. *Review of Radical Political Economics 16*(4).

Smith, J. and F. Welch (1989). Black Progress After Myrdal. *Journal of Economic Literature 27*(2), 519–564.

Smith, J. P. and F. R. Welch (1977). Black-White Male Wage Ratios: 1960-70. *American Economic Review 67*(3), 323–338.

Stainback, K., C. L. Robinson, and D. Tomaskovic-Devey (2005). Race and Workplace Integration A Politically Mediated Process? *American Behavioral Scientist 48*(9), 1200–1228.

Stephens-Davidowitz, S. (2014). The cost of racial animus on a black candidate: Evidence using Google search data. *Journal of Public Economics 118*, 26–40.

Sundstrom, W. A. (2007). The Geography of Wage Discrimination in the Pre-Civil Rights South. *Journal of Economic History 67*(2), 410–444.

Unnithan, N. P. (2013). *Crime and Justice in India*. Fort Collins: SAGE Law.

Wanamaker, M. H. (2017). 150 Years of Economic Progress for African American Men: Measuring Outcomes and Sizing Up Roadblocks. *Economic History of Developing Regions 32*(3), 211–220.

Wood, B. D. (1990). Does Politics Make a Difference at the EEOC? *American Journal of Political Science 34*(2), 503–530.

Wright, G. (2013). *Sharing the Prize*. Cambridge: Harvard University Press.

Zakaria, F. (1997). The Rise of Illiberal Democracy Saturday. *Foreign Affairs November/D*.

Additional References:

Acemoglu, D. and J. Robinson (2008). Persistence of Power, Elites and Institutions. *American Economic Review 98(1)*: 267-293.

Acemoglu, D. and J. Robinson (2008). The Role of Institutions in Growth and Development. *Working Paper No. 10, Commission on Growth and Development, The World Bank.*

Acemoglu, D., S. Naidu, P. Restrepo and J. Robinson (2013). Democracy, Redistribution and Inequality. Unpublished Working Paper.

Adena, M., R. Enikopolov, M. Petrova, V. Santarosa and E. Zhuravskaya (2016). Radio and the Rise of the Nazis in Pre-War Germany. *The Quarterly Journal of Economics*: 1885-1939.

Asher, S. and P. Novosad (2017). Politics and Local Economic Growth: Evidence from India. *American Economic Journal: Applied Economics 9(1)* 229-273.

Balani, S. (2013). Functioning of the Public Distribution System: An Analytical Report. *PRS Legislative Research*.

Bardhan, P. (2016). State and Development: The Need for a Reappraisal of the Current Literature. *Journal of Economic Literature, 54(3)*: 862-892.

Bardhan, P., D. Mookherjee and P. T. Monica (2010). Impact of Political Reservations in West Bengal Local Governments on Anti-Poverty Targeting. *Journal of Globalization and Development, 1(1)*: 1-38.
Beland, L. (2015). Political Parties and Labour Market Outcomes: Evidence from U.S. States. *American Economic Journal: Applied Economics 7(4)*: 198-220.
Beland, L. (2016) and S. Oolomi. Party Affiliation and Public Spending: Evidence from U.S. Governors. *Economic Inquiry 55(2)*: 982-995.
Besley, T. and R. Burgess (2000). Land Reform, Poverty Reduction and Growth: Evidence from India. *The Quarterly Journal of Economics 115(2)*: 389-430.
Besley, T., R. Pande, L. Rahman and V. Rao (2004). The Politics of Public Goods Provision: Evidence from Indian Local Governments. *Journal of the European Economic Association 2(3)*: 416-426.
Bhagwati, J. and T. Srinivasan (2002). Trade and Poverty in Poor Countries. *American Economic Review: Papers and Proceedings*, May 2002.
Bhalhotra, S., G. Cassan, I. Clots-Figueras, and L. Iyer (2013). Religion, Politician Identity and Development Outcomes: Evidence from India. *Working Paper 13-102, Harvard Business School*.
Bhattacharya, S., V.L. Falcao and R. Puri "The Public Distribution System in India: Policy Evolution and Program Delivery Trends" in The 1.5 Billion People Question: Food, Vouchers, or Cash Transfers?. The World Bank. Washington D.C., 2017.
Bhavnani, R. (2017). Do the Effects of Temporary Ethnic Group Quotas Persist? Evidence from India. *American Economic Journal: Applied Economics 9(3)*: 105-123.
Bursztyn, L. (2016). Poverty and Political Economy of Public Education Spending: Evidence from Brazil. *Journal of the European Economic Association 14(5)*: 1101-1128.
Calonico, S., M. D. Cattaneo and R. Titiunik. Robust Non-Parametric Confidence Intervals for Regression Discontinuity Designs. *Econometrica 82(6)*: 2295-2326.
Chattopadhyay, R. and E. Duflo (2004). Evidence from a Nationwide Randomized Experiment. *Economic and Political Weekly, 39(9)*: 979-986.
Chattopadhyay, R. and E. Duflo (2004). Women as Policy Makers: Evidence from a Randomized Policy Experiment in India.*Econometrica, 72(5)*: 1409-1443.
Chin, A. and N. Prakash (2011). The Redistributive Effects of Political Reservations for Minorities: Evidence from India. *Journal of Development Economics 96(1)*: 265-277.
Clots-Figueras, I. (2011). Women in Politics: Evidence from Indian States. *Journal of Public Economics 95*: 664-690.
Cole, S. (2009). Fixing Market Failures or Fixing Elections? Agricultural Credit in India. *American Economic Journal: Applied Economics 1(1)*: 219-250.
Dreze, J. and R. Khera (2013). Rural Poverty and the Public Distribution System. *Economic and Political Weekly XLVIII(45 and 46)*: 55-60.
Dreze, J. and R. Khera (2015). Understanding Leakages in the Public Distribution System. *Economic and Political Weekly L(7)*: 39-42.
Duflo, E. (2012). Women Empowerment and Economic Development. *Journal of Economic Literature, 50(4)*: 1051-1079.
Dunning, T. and J. Nilekani (2013). Ethnic Quotas and Political Mobilization: Caste, Parties and Distribution in Indian Village Councils. *American Political Science Review 107(1)*: 35-56.

Enikolopov, R. (2014). Politicians, Bureaucrats and Targeted Redistribution. *Journal of Public Economics 120*: 74-83.

Eggers, A. C., A. Fowler, J. Hainmueller, A. B. Hall and J. M. Snyder, Jr. (2015). On the Validity of the Regression Discontinuity Design for Estimating Electoral Effects: New Evidence from Over 40,000 Close Races. *American Journal of Political Science 59(1)*: 259-274.

Ferreira, F. and J. Gyourko (2009). Do Political Parties Matter? Evidence from U.S. Cities. *Quarterly Journal of Economics, February 2009*: 399-422.

Hodler, R. and P. Raschky (2014). Regional Favouritism. *The Quarterly Journal of Economics*: 995-1033.

Iyer, L., A. Mani (2012). Traveling Agents: Political Change and Bureaucratic Turnover in India. *The Review of Economics and Statistics 94(3)*: 723-739.

Iyer, L., A. Mani, P. Mishra, and P. Topalova (2011). The Power of Political Voice: Women's Political Representation and Crime in India. *Working Paper 11-092, Harvard Business School*.

Iyer, L., T. Khanna, and A. Varshney (2011). Caste and Entrepreneurship in India. *Working Paper 12-028, Harvard Business School*.

Jensenius, F. R. (2015). Development from Representation? A Study of Quotas for the Scheduled Castes in India. *American Economic Journal: Applied Economics 7(3)*: 196-220.

Jaffrelot, C. (2003). *India's Silent Revolution: The Rise of the Lower Castes in North India*. New York: Columbia University Press.

Jaffrelot, C. (2011). *Religion, Caste and Politics in India*. London: Hurst Publishers.

Kijima, Y. (2006). Caste and Tribe Inequality: Evidence from India, 1983-1999. *Economic Development and Cultural Change 54(2)*: 369-404.

Krishnan, N. (2007). Political Reservations and Rural Public Good Provision in India. Working Paper.

Lee, D. (2001). The Electoral Advantage to Incumbency and Voters' Valuation of Politicians' Experience: A Regression Discontinuity Analysis of Elections to the U.S. House. *Working Paper 8441, National Bureau of Economic Research*.

Lee, D., E. Moretti, and M.J. Butler (2004). Do Voters Affect or Elect Policies? Evidence from the U.S. House. *The Quarterly Journal of Economics, 119(3)*: 807-859.

Lizzeri, A. and N. Persico (2001). The Provision of Public Goods Under Alternative Electoral Incentives. *American Economic Review 91(1)*: 225-239.

Lublin, D. and S. Bowler "Electoral Systems and Ethnic Minority Representation." The Oxford Handbook of Electoral Systems. Ed. Erik S. Herron, Robert J. Pekkanen and Matthew S. Shugart. New York: Oxford University Press, 2018.

Levitt, S. (1996). How Do Senators Vote? Disentangling the Role of Voter Preferences, Party Affiliation and Senate Ideology. *American Economic Review 86(3)*: 425-441.

Meltzer, A.H. and S.F. Richard (1981). A Rational Theory of the Size of Government. *The Journal of Political Economy 89(5)*: 914-927.

Meyersson, E. (2014). Islamic Rule and the Empowerment of the Poor and Pious. *Econometrica 82(1)*: 229-269.

Munshi, K. (2017). Caste and the Indian Economy. *Unpublished Working Paper*.

Nagavarapu, S. and S. Sekhri (2014). Informal Monitoring Mechanisms in Public Service Delivery: Evidence from the Public Distribution System in India. Working Paper

Pande, R. (2003), Can Mandated Political Representation Provide Disadvantaged Minorities Policy Influence? Theory and Evidence from India. *American Economic Review, 93 (4)*: pp. 1132-1151.
Persson, T. and G. Tabellini (2000). *Political Economics: Explaining Economic Policy.* Cambridge: The MIT Press.
Pitkin, H. (1967). *The Concept of Representation.* Berkeley: University of California Press.
Planning Commission of India (2005). *Performance Evaluation of the Targeted Public Distribution System.* Programme Evaluation Organisation, Planning Commission, Government of India.
Rehavi, M. (2007). Sex and Politics: Do Female Legislators Affect State Spending? *Unpublished Working Paper.*
Topalova, P. (2010). Factor Immobility and Regional Effects of Trade Liberalization: Evidence on Poverty and Inequality from Indian Districts. *American Economic Journal: Applied Economics 2(4)*: 1-41.
Trebbi, F., P. Aghion and A. Alesina (2008). Electoral Rules and Minority Representation in U.S. Cities. *Quarterly Journal of Economics 123(1)*: 325-357.
Varshney, A. "Mass Politics or Elite Politics? India's Economic Reforms in Comparative Perspective" India in the Era of Economic Reforms. Ed. Jefferey D. Sachs, Ashutosh Varshney and Nirupam Bajpai. New Delhi: Oxford University Press, 1999.

Appendix A

The Effect of Political Power on Labor Market Inequality: Evidence from the 1965 Voting Rights Act

A.1 Additional Background: Evidence of Post-VRA Government Responsiveness to Black Voters' Interests

In this section, we provide further evidence that the VRA affected policy outcomes in favor of black voters – in this case, at the local levels. Table A.5 presents evidence of how the VRA changed the allocation government spending in a manner that benefited black communities. Panel A examines how VRA coverage affected the distribution of public assistance benefits (such as welfare or UI benefits).[1] Prior to the mid-1960s, black Americans were often denied access to social programs. historical accounts of the early 1960s, for example, suggest that during the era in which President John F. Kennedy started to expand anti-poverty programs, the provision of services to black American families was limited. During Congressional debates that led to the VRA's passage, advocates for a strong voter protection bill believed that minority political power would ensure that President Johnson's newly-initiated War on Poverty through social spending would not become a war waged "for white people only." As our estimates suggest, VRA coverage is positively associated with the per capita public assistance recipients. In Columns (2)-(4) we estimate a triple-differences framework by further interacting the VRA indicator with the pre-VRA black population share. This regressions provides even stronger evidence that the VRA increased access to social spending. We observe differentially higher levels of public assistance support in counties with higher black population shares in 1960 (Columns (2)-(4) of Table A.5). Each percentage point increase in black population share increased the percentage of county residents receiving public as-

[1] We use data from the Census County Data Books, which has data in 1964 and 1980 on the number of public assistance recipients in a given county.

sistance by between 0.07 and 0.1 percentage point. Given that four-in-ten (41.8%) of black Americans were poor in the mid-1960s (DeSilver 2014), facilitating access to welfare support was likely a key role for government actors who cared about the social wellbeing of black Americans. These estimates are consistent with a shift in the distribution of state transfers toward areas with higher minority population shares – which we would expect to matter after black Americans could exercise their voting rights. In Columns (5)-(7), we re-estimate the core specification of Cascio and Washington (2014), to show that the VRA also increased the within-state distribution of state transfers.

A.2 Conceptual Framework

The goal of building this model is to provide analytical clarity regarding the impact political power has on racial wage and unemployment disparities. In short, political power directly affects redistribution by changing the allocation of public employment, public wages and benefits. However, how these effects translate to the private sector is less clear. To guide our thoughts on how political empowerment can affect labor market outcomes, we develop a labor equilibrium model with search frictions in the vein of Mortensen-Pissarides. But in order to account for redistributive policies amongst groups of voters we augment our model by incorporating hiring in the public sector.

The public sector and unemployment transfers are controlled by a politician who maximizes a welfare function weighted by the relative political strength of each group (in our case, black and white voters). Our model follows a line of research exploring the role of government intervention in the labor market. For example, in one recent paper, Kline and Moretti (2013) use similar tools to explore the interaction between migration, standard spatial equilibrium models, and the impact of place-based policies on the labor market.

To incorporate racial disparities within the labor market into our setting, we allow private sector employers to allocate vacancies across different groups of workers. This is sensible as long as there are incentives for the employer to hire differently as, for example, when one group has a lower bargaining power. As hiring in one group entails increasing search costs, in equilibrium, both groups are employed.

The rationale behind our model is that by raising the demand for one group of workers in the public sector, the government impacts the labor supply of this group in the private sector, thus increasing the group's private sector wage. We show formally that this increase in wage is greater than offsetting changes in both the demand for the affected and unaffected groups of workers. Expectedly, employment rates in the private sector will decrease for the affected group relative to the unaffected one. As we will show, this will have the consequence of affecting the redistribution policies in the public sector.

Labor Market with Public Employment

We consider an economy where workers differ only along a non-productivity dimension, $i \in \{a, b\}$ under which they can be identified (e.g., race). Each dimension contains an identical continuum of infinitely lived workers of measure one. The private sector employer interviews candidates with full information of their type, or equivalently, posts vacancies (v_i)

for each group. Each match generates productivity p. The matching function, $m(u, v)$ is increasing and concave in both unemployed workers (u) and vacancies (v), and has constant returns to scale. The arrival rate for workers is defined as $\frac{m(u,v)}{u} \equiv m(\theta)$, where $\theta = \frac{v}{u}$ is the labor market tightness. The hiring rate per vacancy is defined as $\frac{m(u,v)}{v} = \frac{m(\theta)}{\theta} \equiv q(\theta)$. The arrival rate of job offers for workers is increasing in labor market tightness, $m_\theta(\theta) > 0$, while the hiring rate decreases with labor market tightness, $q_\theta(\theta) < 0$. The wage for each group is determined by bargaining between the employer and each employee of all groups. While the bargaining position and labor market tightness might differ across groups, the marginal product of labor is the same for each worker. γ is the cost to the firm of posting a job. δ is the exogenous separation rate, which we take to be constant across groups and types of employers. Search on the job is not allowed. To simplify notation, we postpone the use of superscripts to next subsection.

The value of a unfilled vacancy, V, obeys:

$$rV = -\gamma + q(\theta)(J - V)$$

while the value of a filled vacancy, J, follows:

$$rJ = -w + \delta(V - J)$$

where w are the wage flow paid to the worker. Competitive entry of firms to the market requires that the value of an unfilled vacancy goes to zero:

$$rV = 0$$

We depart from the standard model by adding public sector employment. The public sector wages, w_g, and the public hiring matching rate, are decided by the politician and taken exogenously by the market. The value of public sector employment follows:

$$rW_g = w_g + \delta(U - W_g)$$

The value of private sector employment, W, and unemployment, U, are given by:

$$rW = w + \delta(U - W)$$

$$rU = b + m_g(W_g - U) + m(\theta)(W - U)$$

The wage for each group is determined by Nash Bargaining principles:

$$\beta J = (1 - \beta)(W - U).$$

The equilibrium dynamics of unemployment, public sector employment, and private sector employment are governed by the flows in and out of unemployment. In the steady state, flows from unemployment to employment must match separations:

$$u = \frac{\delta}{\delta + m(\theta) + m_g}$$

$$e_g = \frac{m_g}{\delta}u, \qquad e = \frac{m(\theta)}{\delta}u$$

$$e + e_g + u = 1$$

The model can be reduced to the following two relationships for each group:

$$\frac{\gamma}{q(\theta)} = \frac{p - w}{r + \delta} \tag{A.1}$$

$$w = \frac{\gamma\beta}{1-\beta}\frac{r + \delta + m(\theta)}{q(\theta)} - m_g\frac{b - w_g + \theta\frac{\gamma\beta}{1-\beta}}{r + \delta + m_g} + b \tag{A.2}$$

Equation (A.1) is the familiar job creation condition. As expected, labor market tightness decreases with wage and increases with the productivity level. Equation (A.2) is the wage equation and can be rewritten as:

$$w = \underbrace{\frac{\gamma\beta}{1-\beta}\frac{r+\delta}{q(\theta)}}_{\text{Surplus Distribution}} + \underbrace{\frac{r+\delta}{r+\delta+m_g}\Big(\overbrace{b}^{\text{Reservation Utility}} + \overbrace{\theta\frac{\gamma\beta}{1-\beta}}^{\text{Private Sector Gains}}\Big) + \frac{m_g}{r+\delta+m_g}\overbrace{w_g}^{\text{Public Wage}}}_{\text{Social Planner Component}} \tag{A.3}$$

This relationship illustrates the various components determining the wage level in the private sector. The wage is given by a surplus distribution component plus a component arising, at least partly, through the intervention of the public sector, which we label social planner component. The latter component can be thought of as a weighted average of the public wage on the one hand, and reservation utility and private sector gains in proportion to the tightness of employment conditions (demand premium) on the other, where the weights are determined through public hiring.

A.2.1 Politician

The politician's goal is to maximize a weighted average of the welfare of each group of voters, where the weights are a function of each group's political strength. The welfare of each group is given by:

$$\Phi^i = e^i(w^i - \tau) + e^i_g w_g + u^a b$$

where τ is the tax rate, e^i_g is race-group government employment, and e^i is a group's private employment. Since workers have linear utility, and public transfers as well as public wages are financed through the tax proceedings, it follows that a given race group's welfare equals the total private wages:

$$\Phi^i = e^i w^i$$

Denoting the political strength of blacks by ω, the politician's problem is to solve:

$$\max_{m^a_g, w^a_g} (1-\omega)\Phi^a + \omega\Phi^b$$

subject to the budget constraint:

$$e^a_g w^a_g + e^b_g w^b_g + u^a b^a + u^b b^b = (e^a + e^b)\tau$$

The following derivatives are useful:

$$\frac{d\Phi^i}{db^i} = e^i\left(\frac{r+\delta}{r+\delta+m_g^i}\right),\ \frac{d\Phi^i}{dw_g^i} = e^i\left(\frac{m_g^i}{r+\delta+m_g^i}\right),\ \frac{d\Phi^i}{dm_g^i} = \frac{d\Phi^i}{db^i}\left(\frac{b^i - w_g^i + \gamma\theta^i\frac{\beta^i}{1-\beta^i}}{r+\delta+m_g^i}\right).$$

The first order conditions with respect to public wages, w_g, and public hiring, m_g, provide the following equilibrium conditions:

$$\begin{cases} (i: Public\ Wage) & \omega\left[1 + \frac{e^b}{e^a}\frac{u^a}{u^b}\frac{r+\delta+m_g^a}{r+\delta+m_g^b}\right] = 1 \\ (ii: Public\ Hiring) & \frac{w_g^b - b - \gamma\theta^b\frac{\beta^b}{1-\beta^b}}{r+\delta+m_g^b} = \frac{w_g^a - b - \gamma\theta^a\frac{\beta^a}{1-\beta^a}}{r+\delta+m_g^a} \end{cases}$$

From conditions *(i)* if there is an increase in black workers' political strength, the politician needs to increase black public hiring, m_g^b, relative to white public hiring, m_g^a, in order to stay in equilibrium:

Remark 1 (Public Hiring): *Given an increase in political strength of group b, ω, public hiring for group b must increase relative to group a.*

Furthermore, since from condition *(i)* public hiring for blacks increases, condition *(ii)* implies that the public wage of black workers increases relative to the public wage of white workers, i.e. the public wage gap narrows:

Remark 2 (Public Wage Gap): *Given an increase in political strength of group b, ω, the public sector wage disparity between group a and group b narrows.*

From the wage equation (A.1), condition *(i)*, and *(ii)* it follows that the wage gap in the private sector narrows. To see this, consider the job creation conditions for each group:

$$\frac{\gamma}{q(\theta^a)} = \frac{p - w^a}{r+\delta}, \qquad \frac{\gamma}{q(\theta^b)} = \frac{p - w^b}{r+\delta}.$$

Subtracting both equations, and taking derivatives on both sides, we obtain a relationship characterizing the change in racial wage gap:

$$\frac{d(w^a - w^b)}{d\omega} = (r+\delta)\gamma\frac{d}{d\omega}\left(\frac{1}{q(\theta^b)} - \frac{1}{q(\theta^a)}\right)$$

The equilibrium unemployment for each group is:

$$u^i = \frac{\delta}{\delta + m(\theta^i) + m_g^i}$$

rearranging yields an expression for the private sector equilibrium match rate:

$$m(\theta^i) = \frac{(1-u^i)\delta - m_g^i u^i}{u^i} = \frac{\delta}{u^i} - \delta - m_g^i$$

Since $m(\theta^i)$ is an increasing function of θ^i, an increase in the public sector match rate implies θ^i must decrease. Similarly, since the hiring rate per vacancy, $q(\theta)$, is decreasing on labor market tightness, a decrease in θ^i implies $\frac{1}{q(\theta)}$ decreases. Since following an increase in political strength for blacks, the match rate for blacks increases relative to white's, the wage

gap in the private sector is also reduced:

Remark 3 (Private Wage Gap): *Given an increase in political strength of group b, ω, the private sector wage disparity between group a and group b narrows.*

Importantly, condition *(ii)* also describes the consequences of increasing unemployment transfers and decreasing overall public sector wages. Although the narrowing in the public sector wage gap is necessary, changes in unemployment transfers can be used to attenuate the extent to which the wage gap narrows. This is to say, unemployment benefits can increase after an increase in the political strength of one group vis-à-vis the other. But that change is to attenuate the gains accrued by the disadvantaged group in the private sector. To see this, recall the wage equation:

$$w = \frac{\gamma\beta}{1-\beta}\frac{r+\delta}{q(\theta)} + \frac{r+\delta}{r+\delta+m_g}\Big(b + \theta\frac{\gamma\beta}{1-\beta}\Big) + \frac{m_g}{r+\delta+m_g}w_g$$

Since the private sector wage is affected by the weighted average of benefits and public sector wages, the group with the highest public sector matching rate will benefit the least. Public sector revenue neutrality implies:

$$\frac{d(w_g^b - w_g^a)}{d\omega}e_g^b + \frac{db}{d\omega}u + \frac{dw_g^a}{d\omega}(e_g^b + e_g^a) = 0$$

Condition *(ii)* tells us that public revenue can be reallocated (1) from benefits to reduce the wage gap; and (2) from level wages to either increase benefits or reduce the wage gap. The optimal transfer is determined by the matching rate for each group, and by the size of the unemployed sector relative to the size of black government employment. Formally:

Remark 4 (Unemployment Transfers & Public Wages): *If, $m_g^a > m_g^b$, revenue will be reallocated from unemployment transfers to reduce the public sector wage gap. If, $m_g^a < m_g^b$, revenue will be reallocated from level wages to unemployment transfers, if $\frac{m_g^b - m_g^a}{u(r+\delta+m_g^a)} > \frac{1}{e_g^b}$, and to reduce the public sector wage gap, if $\frac{m_g^b - m_g^a}{u(r+\delta+m_g^a)} < \frac{1}{e_g^b}$.*

Remark 4 provides an unexpected perspective on why transfers increase in places with more minority political participation. Transfers do not necessarily operate to optimize the welfare of black constituents, but instead could operate to compensate losses of whites in the private sector. To see this, we can again to examine the wage equation. When $m_g^a > m_g^b$, black workers benefit the most from transfers, but since these funds are used to finance the reduction in the wage gap, the reduction is partly offset. Conversely, when $m_g^a < m_g^b$, unemployment transfers benefit white constituents more, while decreasing wage levels in the public sector affects black workers more. By financing extra transfers through reductions in wage levels, transfers are used for the purpose of offsetting private wage gains generated by increased public sector hiring and reductions in the public sector wage gap.

A.3 Constructing County-level Measure of Exposure to Civil Rights Act-Title VII & Federal Affirmative Action Requirements

As we discuss in Section1.5, there is currently no readily-available data that allows us to examine how political influence due the VRA improved the enforcement of anti-discrimination policies within the private labor market, such as Title VII of the Civil Rights Act. To indirectly test this hypothesis, we leverage previous research documenting that federal anti-discrimination and affirmative policies were better-enforced within establishments that were large enough to fall under the more stringent oversight of the EEOC. In particular, the Equal Employment Opportunity Act (EEOA) of 1972 extended civil rights coverage to employers with 15-24 employees. We use data on establishments both above and under 20 employees drawn from the U.S. County Data Books to estimate the probability of workforce exposure to CRA enforcement within a given county. Because we cannot exactly observe the number of establishments subject and not subject to the amended Title VII, we estimate the likelihood of exposure using the following 3-step methodology.

(1) First, we create estimates of the probability of a worker being in a small-to-medium establishment (SME - less than 100 employees). Assuming an uniform distribution for establishment size for SMEs and setting total employment equals the expected value of employment from that distribution, we can retrieve the proportion of small and medium establishments subject to EEOA. For estimating the parameters of the distribution we use a sample with no large establishments.

(2) Second, using this distribution, we estimate the number of medium establishments subject ot the 1972 EEOA (between 20 and 25 workers), and similarly the number of small establishments subject to the EEOA (between 15 and 20 workers). From (1), we obtain that the probability of being a small firm conditional being an SME (less than 100 employees) is 12.3%. The probability of being a medium firm conditional on being an SME is 15.2% . Also from (1), small firms constitute 82% of all SMEs, and medium firms the remaining 18%. This means that 15% of all small firms and 84% are affected by the EEOA.

(3) Finally, we define our CRA penetration measure (TitleVIIExposure in Table 13) as:

$$TitleVIIExposure = \frac{\text{estimated number of wokers in firms affected}}{\text{total number of workers}}.$$

A.4 Occupational Upgrading

The impact of minority political empowerment on employment outcomes in both the private and public sector also likely affected the occupational redistribution of workers. Prior research on black economic progress in the North, for example, documents positive effects of government action through fair employment agencies on black workers' occupational upgrading in the 1950s (Collins, 2003; Liggett 1969).

Understanding the VRA's effect on occupational upgrading (distinct from wages and employment) is important for a few reasons. To a large extent, discrimination in labor market opportunities (within both the public and private sectors) involved barriers to entry for certain occupations. For example, most black workers within the public sector in 1960 worked as janitors. Thus, to the extent that the VRA improved black wages, one would reasonably expect this impact is at least partially understood as positive occupational upgrading.

The occupational redistribution and upgrading of black American workers likely reflect the mechanisms we test. Ample research shows that the public sector, for example, provided more opportunities for upward job mobility to managerial positions (Hout 1984). Similarly, through the desegregation of labor markets that Jim Crow politics sustained (Roback, 1986), the VRA helped break down the segregated labor markets through which wage discrimination operated.

However, the VRA may have also indirectly created opportunities for black American workers to move up the economic ladder. The movement of black Americans to the public sector likely created new opportunities for other black workers within the private sector (assuming private labor demand stayed fixed). As we discussed above, the public sector was the entryway for an emergent black middle class. The proportion of black Americans working as managerial and professional workers was *62* percent greater within the public sector than for white workers. By 1970, 27 percent of black managers and 11 percent of white managers and administrators worked in government (Collins 1983).

We test for occupational upgrading in a similar spirit to Collins (2003). We compute a similar measure, *OccScore*, as follows: using the median income earned in 1960 by for each three-digit occupational category, we create an ordinal ranking of all occupations in our sample. This ranking we define as our *OccScore* variable. Using this variable, we reestimate Equation 1.1, with the natural log of the occupational score instead of income.[2]

Results are presented in Table A.14. We can also probe these results more to understand the mechanism of upgrading better. In particular, increased opportunities to reach the professional and managerial ranks within government would most directly affect highly-educated black workers. We confirm that this is indeed the case by showing that the VRA's positive effect on the likelihood of being employed within the public sector is substantially larger for black workers who are college graduates or higher (results available upon request). Moreover, as we have just discussed, if government hiring was reducing the supply of college-educated blacks within the private labor force, we might expect more occupational upgrading

[2]The estimating equation is thus:

$$\log(OccScore_{ict}) = \beta_0 + \beta_1(VRA_{ct} \times White_{ict}) + \mathbf{x}_{ict}\boldsymbol{\gamma} + (\delta_c \times \delta_t) + (\delta_r \times \delta_c) + (\delta_{p(c)} \times \delta_r \times \delta_t) + \epsilon_{i,c,p(c),t} \quad \text{(A.4)}$$

withing the *private* sector for black workers with less education. This is indeed what we find.

The results in this subsection help paint a more complete picture of how the VRA (and the civil rights era more broadly) may have contributed to black economic advancement. Although black workers in the South occupied the lower rungs of the economic ladder prior to mid-century, the combination of increased public sector hiring as well as private sector intervention – both facilitated by the VRA – allowed black Americans to achieve success in new occupations and professions.

A.5 Additional Tables

Table A.1: Summary Statistics - County Characteristics in 1960

	(1)	(2)	(3)	(4)
Variable	**Non-VRA Counties**	**VRA Counties**	**Mean Difference**	**P-value**
Interior Counties				
Median Income	3799.23	3429.09	370.14	0.02
% Pop. Black	.08	0.26	-0.18	0.01
% Ag. Workers	0.230	0.199	0.03	0.19
% FT Employed	0.71	0.68	0.03	0.01
% 25 y.o.-HS Educated	0.31	0.28	0.03	0.00
Rep. Party Voteshare	0.62	0.67	0.05	0.00
Border Counties				
Median Income	3818.27	3649.84	168.34	0.39
% Pop. Black	.18	0.17	-0.01	0.61
% Pop. Urban	.29	0.32	-0.03	0.38
Farm Share	0.21	0.21	0.00	0.82
% FT Employed	0.71	0.68	0.02	0.17
% 25 y.o.-HS Educated	0.29	0.29	0.00	0.97
Rep. Party Voteshare	0.61	0.63	0.02	0.08

NOTES: This table reports average characteristics across both Section 5 and non-Section 5 counties, for both the border county sample as well as the interior county sample.

Table A.2: The Effect of the VRA on Political Participation (All Counties in Sample States)

	(1)	(2)	(3)	(4)
VRA	.148***	.098***	.066***	.019***
	(0.005)	(0.004)	(0.003)	(0.005)
VRA × Black Pop. Share				0.002***
				(0.000)
N	12848	12848	12848	12848
Controls		X	X	X
State Trends			X	X

Notes: This table presents regression coefficients from 4 separate regressions, one per column. An observation is a county-year. The dependent variable is county-level turnout in presidential elections. The independent variable is a dichotomous variable indicating whether a given county is protected under VRA-Section 5 (and where relevant, the interaction between the VRA indicator and the county population share that is black). Standard errors are in parentheses and are clustered by county. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively.

Table A.3: The Effect of the VRA on Political Participation
(Turnout for Congressional Races)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VRA	0.120***	0.092***	0.058***	0.153***	0.101***	0.098***	0.065***
	(0.010)	(0.009)	(0.009)	(0.005)	(0.005)	(0.012)	(0.018)
VRA × Black Pop. Share						0.001***	0.001
						(0.000)	(0.001)
N	2651	2651	2651	12848	12848	2651	2651
Controls		X	X	X	X	X	X
State Trends			X		X		X

Notes: This table presents regression coefficients from 7 separate regressions, one per column. An observation is a county-year. The dependent variable is county-level turnout in congressional elections elections. The independent variable is a dichotomous variable indicating whether a given county is protected under VRA-Section 5 (and where relevant, the interaction between the VRA indicator and the county population share that is black, "Black Pop. Share"). Standard errors are in parentheses and are clustered by county. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details.

Table A.4: Impact of the VRA on Legislator Ideology (Republican vs. Democratic)

	(1)	(2)	(3)
VRA	-0.05*	-0.02	0.02
	(0.03)	(0.02)	(0.03)
VRA × Black Pop. Share			-0.21*
			(0.11)
N	1699	1699	1699
DW-Nom. Dimension	1	1	1
Controls		X	X

Notes: This table presents regression coefficients from 3 separate regressions, one per column. An observation is a congressional district-year. The dependent variable is Dimention 1 of the Poole-Rosenthal DW-Nominate Score (while Dimension 2 indicates conservativeness on race-related issues, Dimension 1 indicates overall partisan conservativeness), and the independent variable is an indicator variable for whether a district is covered under Section 5 of the VRA. All regression include Congress (year) and district (geography) fixed effects. Standard errors are in parentheses and are clustered by district. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively.

Table A.5: Impact of the VRA on Policy Responsiveness (Expenditures), 1957 - 1982

	Panel A: Per Cap. Public Assistance				Panel B: Per Cap. State-Local Transfers		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VRA	0.01***				-0.05***	-0.05***	.01
	(0.001)				(0.01)	(0.01)	(0.01)
VRA × Black Pop. Share		0.07***	0.07***	0.10***	0.12***	0.11***	0.06***
		(0.02)	(0.02)	(0.02)	(0.02)	(.02)	(0.02)
N	690	690	690	690	2176	2176	2176
County Controls	X		X	X		X	X
State Trends	X			X			X

Notes: This table presents regression coefficients from 4 separate regressions, one per column. An observation is a county-year. The dependent variable in Columns (1)-(4) is the per capita number of public assistance recipients in a given county (measured twice - in 1964 and 1980). The dependent variable in Columns (5)-(7) is the per capita levels of state-to-local intergovernmental transfers a given county receives (measured every five years between 1957 and 1983). The independent variables are an indicator variable for whether a district is covered under the VRA, as well as (where relevant) the interaction between the VRA indicator and the 1960 county population share that is black. Standard errors are in parentheses and are clustered by county. County-level controls include the employment rate, the adult population fraction with a high school education, the population fraction residing in urban areas, the adult population fraction working in agriculture, and median household income. Controls are measured at 1960 levels and interacted with linear and quadratic time trends. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Sources: County Data Books, 1944-1977; U.S. Census of Governments

Table A.6: The Effect of the VRA on Black Relative Wages, 1950-1980 - Robustness

	(1)	(2)	(3)
VRA × Black	0.56**	0.057**	0.058**
	(0.27)	(0.28)	(0.27)
N			
County-level Controls	X	X	X
State-by-human capital FE	X		
Race-by-human capital FE		X	
County-by-race-by-human capital FE			X

Notes: This table presents regression coefficients from 3 separate regressions, one per column. Each estimate is based on an OLS regression relating the VRA to black (relative) wages. An observation is an individual in a given Census year. The dependent variable is the log hourly wage, and the independent variable is VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA in a given year). All regressions include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. Controls are county characteristics in 1960 interacted with linear and quadratic time trends. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table A.7: The Effect of the VRA on Black Relative Wages, 1950-1980 - Subsample Analysis

	(1)	(2)	(3)
	Panel A: 1965 Sample		
VRA × Black	0.071**	0.071**	0.073**
	(0.035)	(0.034)	(0.035)
N	524000	524000	524000
	Panel B: 1975 Sample		
VRA × Black	0.043	0.048*	0.047**
	(0.045)	(0.027)	(0.026)
N	149000	149000	149000
	Panel C: North Carolina		
VRA × Black	0.116**	0.158***	0.158***
	(0.054)	(0.048)	(0.048)
N	175000	175000	175000
Controls		X	X
County Trends			X

Notes: This table presents regression coefficients from 9 separate regressions, 3 per panel and 1 per column. Each coefficient is an estimate from OLS regressions relating VRA to wages. An observation is an individual in a given Decennial Census year. The dependent variable is the log hourly wage, and the independent variable is either a VRA dummy, or VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA in a given year). Panel A presents presents estimates using only the VRA border county pairs for which the VRA became active in 1965. Panel B presents presents estimates using only the VRA border county pairs for which the VRA became active in 1975. Panel C presents presents estimates using only the VRA border county pairs within North Carolina. All regressions include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. Controls are interacted with linear and quadratic time trends. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table A.8: Comparing Border and Interior Estimates
(Testing for Cross-border Spillovers) - Robustness

	(1)	(2)	(3)
	Panel A: Border		
VRA × Black	0.0055**	0.064**	.0.064**
	(0.027)	(0.03)	(0.028)
N	670000	670000	670000
	Panel B: Interior		
VRA × Black	0.043**	0.044**	.0.044**
	(0.017)	(0.018)	(0.018)
N	3741000	3741000	3741000
	Panel C: Difference		
VRA × Black	0.009*	0.010**	0.018*
	(.005)	(.004)	(.010)
N	670000	670000	670000
County-level Controls		X	X
Race-Education Controls			X

Notes: This table presents regression coefficients from 9 separate regressions - three panels with three columns per panel, and each panel-column cell providing results from one regression. This table reports estimates of ordinary least squares regressions relating the VRA to (relative) black wages. An observation is an individual in a given Decennial Census year. The dependent variable is the log hourly wage, and the independent variable is VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA in a given year). All regressions include county-race, county-year, and year-race fixed effects. Standard errors are in parentheses and are clustered by county. Controls are interacted with linear and quadratic time trends for column (3) (our preferred specification), while in column (2) are interacted with only linear trends to show robustness. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Source: DEC.

Table A.9: Effect of the VRA on Cross-Border Migration, 1960-1970

	(1)	(2)	(3)	(4)
VRA	-0.109*	-0.118*	-0.123**	-0.124**
	(0.061)	(0.062)	(0.62)	(0.62)
VRA × Black			0.077	0.082
			(0.048)	(0.052)
N	198000	198000	198000	198000
County Controls	X	X	X	X
Individual Controls		X		X

Notes: This table presents regression coefficients from 4 separate regressions, one per column. The sample used is the "migration sample" (i.e., those individuals who changed residence from five years earlier). Each coefficient is an estimate from OLS regressions relating the Voting Rights Act to cross-border migration, using Census data on a person's place of residence five years ago. An observation is an individual in a given Decennial Census year. The dependent variable is an indicator for whether a person moved across VRA lines, and the independent variables are VRA and VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA in a given year). All regressions include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. Controls are interacted with linear and quadratic time trends. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table A.10: The Effect of the VRA on Public Sector Employment, 1950-1980 (Robustness)

	(1)	(2)	(3)	(4)
VRA × Black	0.028**	0.03***	0.024**	0.035***
	(0.11)	(0.01)	(0.011)	(0.009)
N	673000	673000	673000	673000
Human Capital Controls		X	X	X
County-level Controls			X	X
Returns to Ed. by Race				X

Notes: This table presents regression coefficients from 4 separate regressions, one per column. Each coefficient is an estimate from linear probability regressions relating passage of the VRA to employment in the public sector. An observation is an individual in a given Decennial Census year. The dependent variable an indicator that equals 1 if an individual is a government employee. The independent variable is VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA in a given year). All regressions include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. Controls are interacted with linear and quadratic time trends. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table A.11: The Effect of the VRA on Public Sector Employment, 1950-1980 (Absolute Levels)

	(1)	(2)
VRA	0.082***	0.055***
	(0.01)	(0.01)
N	34000	34000
County-level Controls		X

Notes: This table presents regression coefficients from 2 separate regressions, one per column. Each coefficient is an estimate from linear probability regressions relating passage of the VRA to employment in the public sector. An observation is an individual in a given Decennial Census year. The dependent variable an indicator that equals 1 if an individual is a government employee. The independent variable is the VRA indicator variable, for whether the VRA was in place in a given county and year. Standard errors are in parentheses and are clustered by county. County controls are measured at 1960 levels, and interacted with linear and quadratic time trends. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Source: DEC.

Table A.12: Impact of the VRA on County-level Public Employment, 1957-1982

	(1)	(2)	(3)
VRA	0.001	0.001	0.001
	(0.030)	(0.001)	(0.002)
N	1780	1780	1780
County-level Controls		X	X
County Trends			X

Notes: This table presents regression coefficients from 3 separate regressions, one per column. Each coefficient is an estimate from an OLS regression relating the VRA to the overall size of the public sector. The dependent variable is the size of the government workforce, normalized by total population. All regressions include county pair-year and county fixed effects. Standard errors are in parentheses and are clustered by county. Controls are measured at 1960 levels, and interacted with linear and quadratic time trends. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table A.13: Heterogeneous Wage Effects of the VRA, by Sector (Public or Private)

	Outcome Variable: Log(Wage)		
	(1)	(2)	(3)
Public	0.02	0.02	0.02
	(0.022)	(0.022)	(0.022)
VRA × Black	0.139***	0.144***	0.149***
	(0.019)	(0.019)	(0.019)
Black × Public	0.052***	0.053***	0.053***
	(0.015)	(0.016)	(0.016)
VRA × Public	0.011	0.011	0.011
	(0.036)	(0.036)	(0.036)
VRA × Black × Public	-0.069**	-0.069**	-0.070**
	(0.027)	(0.027)	(0.027)
N	673000	673000	673000
County-level Controls		X	X
Race-by-education Controls			X

Notes: This table presents regression coefficients from 3 separate regressions, one per column. Each coefficient is an estimate from an OLS regression relating the Voting Rights Act to (relative) black wages. Public is an indicator variable for whether a worker is employed in the public sector. An observation is an individual worker in a given Decennial Census year. The dependent variable is log wage, and the independent variables are interactions for: VRA × Black × Public (the interaction between a worker's race, public sector status, and whether the worker's county of residence was covered by the VRA in a given year), as well as all lower-order interactions. The (adjusted) baseline black-white hourly wage gap (in 1960) was -0.43 log points. All regressions control for individual education, years worked, and squared(years worked), and include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. Controls are interacted with linear and quadratic time trends. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table A.14: The Effect of the VRA on Occupational Upgrading

	Outcome Variable: Occupational Income Rank		
	(1)	(2)	(3))
VRA × Black	0.053*	0.053*	0.055**
	(0.033)	(0.033)	(0.032)
N	673000	673000	673000
County-level Controls	X	X	X
Educ./Exp. Controls		X	X
VRA × Educ./Exp. Controls			X

Notes: This table presents regression coefficients from 3 separate regressions, one per column. Each coefficient is an estimate from an OLS regression relating the VRA to (relative) black wages. An observation is an individual in a given Decennial Census year. The dependent variable is the log occupational income score, as calculated in Collins (2003). The independent variable is VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA in a given year). All regressions include county-race, county-year, and year-race fixed effects. Standard errors are in parentheses and are clustered by county. County-level controls include the employment rate, the adult population fraction with a high school education, the population fraction residing in urban areas, the adult population fraction working in agriculture, and median household income. Controls are measured at 1960 levels and interacted with linear and quadratic time trends. Regressions control for race-specific returns to human capital. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table A.15: Testing the Human Capital as a Mechanism

	Outcome: Education			Outcome: Log(Wage)			Outcome: Higher Ed. Achieved?	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VRA ×Black	-0.111	-0.132	-0.105	0.061**	0.061**	0.061**	-0.01	0.012
	(0.133)	(0.136)	(0.139)	(0.03)	(0.03)	(0.03)	(0.022)	(0.15)
N	673000	673000	673000	673000	673000	673000	673000	673000
Controls - 1		X	X		X	X	X	X
Controls - 2			X			X	X	X

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents regression coefficients from 8 separate regressions, one per column. Each regression result reports estimates of ordinary least squares regressions relating the VRA to either: black (relative) wages (Columns (1)-(3), log wages (Columns (4)-(6), or an indicator variable for whether a respondent completed high school or college (Columns (7) and (8), respectively). An observation is an individual in a given Census year. The independent variable is VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA ins a given year). All regressions include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. Controls are county characteristics in 1960 interacted with linear ("Controls-1") and quadratic time trends ("Controls-2"). ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table A.16: Effects of the VRA on Relative Wages, by Local Government Structure

	(1)	(2)
Black × VRA × Mayor-Council Govt.	+0.xxx***	+0.xxx***
	(0.xxx)	
Black × VRA	+0.xxx***	+0.xxx***
	(0.xxx)	(0.xxx)
Outcome	Log(Wage)	Public Employment

Notes: This table presents regression coefficients from 2 regressions. An observation is an individual Census respondent in a given Census year. The dependent variable is the log wage, and the independent variable of interest is the the interaction between an indicator for a county's VRA coverage status in a specific year (a dummy), an indicator for whether a worker is black, and a dummy variable for whether the county-seat in a given county has a mayor-council executive structure. All regressions control for individual education, years worked, and squared(years worked), and include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC and ICMA.

Table A.17: Effects of the VRA on Black Mayorship, by Black Population Share, 1960-1980

	(1)	(2)
VRA	0.001	0.009
	(0.016)	(0.007)
VRA × %Black$_{over50\%}$	0.074	0.056***
	(0.054)	(0.017)
Sample	Border	Interior
N	810	3750
County-level Controls	X	X

Notes: This table presents regression coefficients from 2 separate regressions, one per column. Each coefficient is an estimate from linear probability regressions relating passage of the VRA to the elction of a black mayor within a given county. The dependent variable an indicator that equals 1 if there is a black mayor in a given county in years 1960, 1971, and 1980 (pooling all cities within a county). The independent variable of interest is the interaction between the county-level black population share and the VRA indicator. Standard errors are in parentheses and are clustered by county. County controls are measured at 1960 levels, and interacted with linear and quadratic time trends. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Source: DEC and JCPES.

Appendix B

Minority Representation and Protection from Targeted Violence: Evidence from Low-caste Political Parties in India

B.1 Additional Background

B.1.1 Description and Categorization of Low-Caste Political Parties

This section describes the history of caste-based parties in greater detail. The upsurge in political mobilization of low caste citizens started under two major political parties around 1985 – the Janata Dal (JD) and the Bahujan Samaj Party (BSP). The former effected a broader coalition of low caste groups - principally the Other Backward Classes (OBC) and the *Dalits* (in addition to the Muslims) while the latter explicitly caterered to the *Dalits* (Scheduled Castes). The rise of these parties were concentrated particularly in the populous north Indian states of Uttar Pradesh, Bihar, Madhya Pradesh and Haryana. The JD was also politically successful in eastern India – Orissa – and southern India (Karnataka). Following the initial electoral successes in federal and state elections between 1988 and 1991, the JD coalition splintered through the 1990s into powerful regional parties which continued to wield considerable clout at the state level. Thus in Bihar, the erstwhile JD split into the Rashtriya Janata Dal (RJD), the Janata Dal-United (JD(U)) and the Lok Janshakti Party (LJP). The former's base was entrenched amongst the *Dalits*, *Yadavs* (a dominant OBC community) and the Muslims; the LJP catered to a sub-section of *Dalit* communities while the JD(U) appealed to non-*Yadav* OBC communities and *Dalit* groups who were not part of the RJD's core voter base. Similarly, in Uttar Pradesh, the JD was rechristened as the Samajwadi Party (SP); in Orissa, it became the Biju Janata Dal (BJD), and in Karnataka, the Janata Dal-Secular (JD(S)). Each of these parties have been successful in forming a powerful legislative voting bloc and stable governing coalitions these four Indian states between 1990 and 2013, covering nearly a third of the country's population.

As the OBCs, SCs and STs collectively account for 70 percent of the nation's population and an overwhelming share of the households under the poverty line, most political parties claim to represent their interests and work towards improving their socio-economic conditions. The numerical majority and geographic dispersion of the low-castes makes it difficult for any political party aspiring for national or state power to be openly hostile, or even indifferent to their interests. This is particularly true for the Indian National Congress (INC) which spearheaded the independence struggle in the colonial period and became the dominant political formation in the first three decades, often representing a rainbow coalition of upper castes, Muslims and SC/STs. The mandated reservation of electoral constituencies for SC/ST politicians also imply that all political parties need to reach out to these communities to select candidates which can contest from these constituencies.

In this light, based on the pioneering work by Jaffrelot (2003), we distinguish caste-based parties from the remaining parties in the following two manners: a) these parties provided political representation to low caste citizens by nominating a significantly higher proportion of low caste candidates in elections; and b) the leadership positions in low-caste parties were typically held by politicians hailing from low-caste communities. Thus, as documented by Jaffrelot (2003), the JD committed in 1989 to nominate 60 percent of its candidates from lower caste backgrounds and there has been a steady decline in the share of upper caste legislators in the legislative assemblies in the states of Bihar, Uttar Pradesh and Madhya Pradesh between 1980 and 1999.

While the upsurge in political representation accorded to low caste citizens was initiated by the JD and the BSP, it was soon matched by mainstream parties such as the INC and the BJP which attempted to electorally counter these nascent caste-based parties through the 1990s. This was acknowledged by V.P. Singh – one of the key leaders in the JD and the Prime Minister in the JD-led federal government between 1989 and 1990 – who mentioned that the increased political representation of low caste citizens across political parties had led to a "silent revolution", particularly across north India.

This brings us to the second distinguishing factor on political leadership of the low-caste parties. Thus, while the political leadership in almost all of these caste-based political parties remained with individuals hailing from low-caste backgrounds, this was not the case for mainstream parties such as the INC and the BJP. Thus, while the INC's political leadership was controlled during this period entirely by a single upper-caste family (barring 6 years between 1991 and 1997) – namely the Nehru-Gandhi family – the BJP remained under the grip of its parent institution, the Rashtriya Swayamsevak Sangh (RSS) which represented a conservative Hindu social order, ordained by Hindu scriptures. Additionally, both the RSS and key BJP leaders on occassions had openly questioned the relevance of affirmative action policies or "social engineering", referring to the increased political representation of low caste groups. In combination with Jensenius' (2017) recent research documenting the fundamental role played by political parties in shaping the legislative agenda of politicians, we contend (and empirically verify) that politicians hailing from low-caste backgrounds would be more effective in representing the interests of low caste citizens while operating in low-caste parties than mainstream parties such as the INC and the BJP which lack both an explicit mandate for furthering the interests of low caste citizens, and political leadership from low caste individuals.

From the perspective of public policy, low-caste parties were in the forefront to advocate

for an aggressive expansion of the affirmative action program as a form of social justice. Chandra (2004) notes that in India, the platform of "social justice" is in fact coded language meant to signal support for all forms of affirmative action sought by the parties representing HMCs. For instance, a key administrative decision undertaken by the short-lived Janata Dal (JD) led federal government in 1990 was to implement the recommendations of the Mandal Commission report which reserved 27 percent of public sector jobs to OBCs. While the report itself along with its recommendations had been completed and submitted to the federal government in 1979, the two successive Congress governments between 1980 and 1989 did not act on the recommendations. Subsequently, while no political party actively opposed the affirmative action program, they were often found lacking in ensuring that the vacant positions under the reserved category were filled by applicants from lower caste groups.

B.1.2 The Politics and Policies of Low-caste Parties

The JD as mentioned before was the first political group to aggressively put forth a political mobilization of low-castes involving low-caste leaders, appealing to their low-caste constituents directly on the basis of their identity, and asking low-caste voters to reject upper-caste dominated parties like the BJP and Congress. Throughout the 1980s and early 1990s, the JD party coalition was able to win several state assembly races, forming governments in key states such as Bihar, Uttar Pradesh, Gujarat, Orissa and Karnataka. They were also successful in several constituencies in the federal parliament. The BSP on the other hand had was a party which catered almost exclusively to the *Dalits* and had a much slower rise through the 1980s in contrast to the JD which was able to form a federal government. The BSP first tasted power in its stronghold of Uttar Pradesh in 1996, when it formed a coalition government with the BJP but the government barely lasted a year. A similar experiment was repeated in 2002 but it too did not last beyond a year. Finally, in 2007, the BSP was successful in obtaining a majority by itself in the Uttar Pradesh legislative assembly and formed the government for 5 years.

In addition to the regional fragments of the JD and the BSP – we also consider the Dravida Munnetra Kazhagam (DMK) (based in Tamil Nadu) and the Left parties within the ambit of caste-based parties. We include them because the policy agendas were often aligned in favor of SC/ST communities. The DMK was the first political formation in India to adopt a militant anti-upper caste posture in the 1960s and actively implemented state-level reservations in public service jobs after coming to power in 1967. The Left parties are a coalition that, while dominated by leaders hailing from privileged social classes, have actively represented the poor and the underprivileged. Both the DMK and the Left parties have repeatedly aligned with the caste-based parties to collectively contest elections, and vote strategically on legislation in the federal Parliament and state legislative assemblies. For instance, the JD led federal government formed in 1989 and the United Front federal government in 1996 and 1997 led by splinter JD groups included active legislative support from both the DMK and Left parties. These parties combined once again to form two successive federal governments between 1996 and 1998.

B.1.3 List of Low-caste Parties

Presented here is the full list of caste-based low caste political parties considered in the paper.

Caste Identity Based Low-Caste Parties and their Abbreviations[1]

1. All India Rashtriya Janata Party - AIRJP
2. Apna Dal - AD
3. Bahujan Samaj Party - BSP
4. Bharipa Bahujan Manasangha - BBM
5. Biju Janata Dal - BJD
6. Indian People's Front - IPL
7. Janata Dal - JD
8. Janata Dal (Secular) - JD(S)
9. Janata Dal (United) - JD(U)
10. Janata Party - JP
11. Lok Dal - LKD
12. Lok Janshakti Party - (LJP)
13. Peasants and Workers Party of India - PWP
14. Rashtriya Janata Dal - RJD
15. Rashtriya Lok Dal - RLD
16. Republican Party of India - RPI
17. Samajwadi Janata Dal - SJD
18. Samajwadi Party - SP
19. United Minorities Front, Assam - UMFA

Left Parties and their Abbreviations

1. All India Forward Bloc - AIFB
2. Communist Marxist Party Kerala State Committee - CMPKSC
3. Communist Marxist Party of India - CPM(K)
4. Communist Party of India - CPI
5. Communist Party of India (Marxist) - CPI(M)
6. Communist Party of India (Marxist-Lenninist) - CPI(ML)
7. Democratic Socialist Party - DSP
8. Forward Block (Socialist) - FB(S)
9. Kerala Revolutionary Socialist Party (Baby John) - KRSP
10. Marxist Coordination - MCOR
11. Revolutionary Socialist Party - RSP
12. Revolutionary Socialist Party of Kerala (Bolshevik) - RSPK(B)

South Indian Low-caste Parties and their Abbreviations

1. Dravida Munnetra Kazhagam - DMK
2. Pattali Makkal Katchi - PMK

[1]These are provided by the Election Commission of India for the purpose of contesting elections

B.2 Details of Data Construction

B.2.1 Indian Household Development Survey (IHDS)

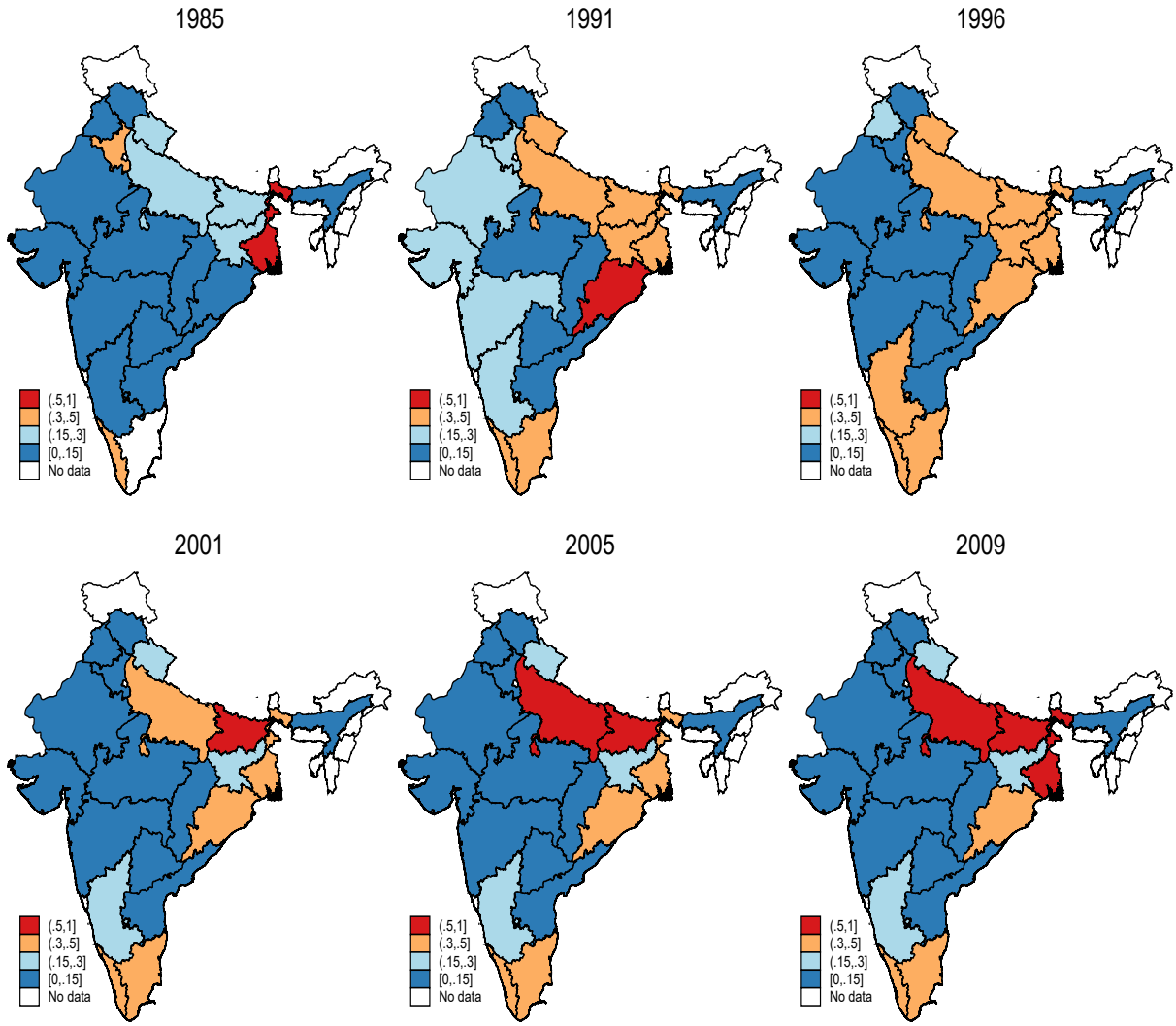
For the survey responses we use to construct our measure of inter-caste conflict and public trust, individuals' responses are ordinarily ranked by response intensity – for instance, for inter-caste conflicts, 1 represents the lowest level of conflict and 3 represents high conflict; while for public institutions, 1 represents the least amount of confidence and 3 represents the highest level of confidence. We transform these variables into binary categories and assign a score of 1 if a respondent reports high inter-caste conflict (ordinal score of 3) and 0 elsewise. Likewise, we assign a score of 1 to the public institution dummies if the respondent reports the highest degree of confidence in public institutions (ordinal score of 3).[2]

We construct two outcomes of interest for each household. First, a dummy equaling 1 if the household reports a high degree of violent inter-caste conflict in the village. While the IHDS does not identify the perpetrators of caste violence, it does identify households by broad caste status, permitting us to observe changes in household perceptions regarding caste violence across different caste groups as a function of the variation in low-caste parties' electoral success. Second, an aggregate standardized index of households' trust across 4 public institutions: namely, politicians, police, state governments and courts. For each of these four categories, we construct a dummy which equals 1 if the household expresses trust in these institutions, and subsequently standardize each variables and compute the sum of the four standardized indices. This increases our chances of detecting an effect, while negating the likelihood of detecting a false positive through multiple hypothesis testing.

[2]This conservative mode of classification is chosen to bias ourselves against finding a result.

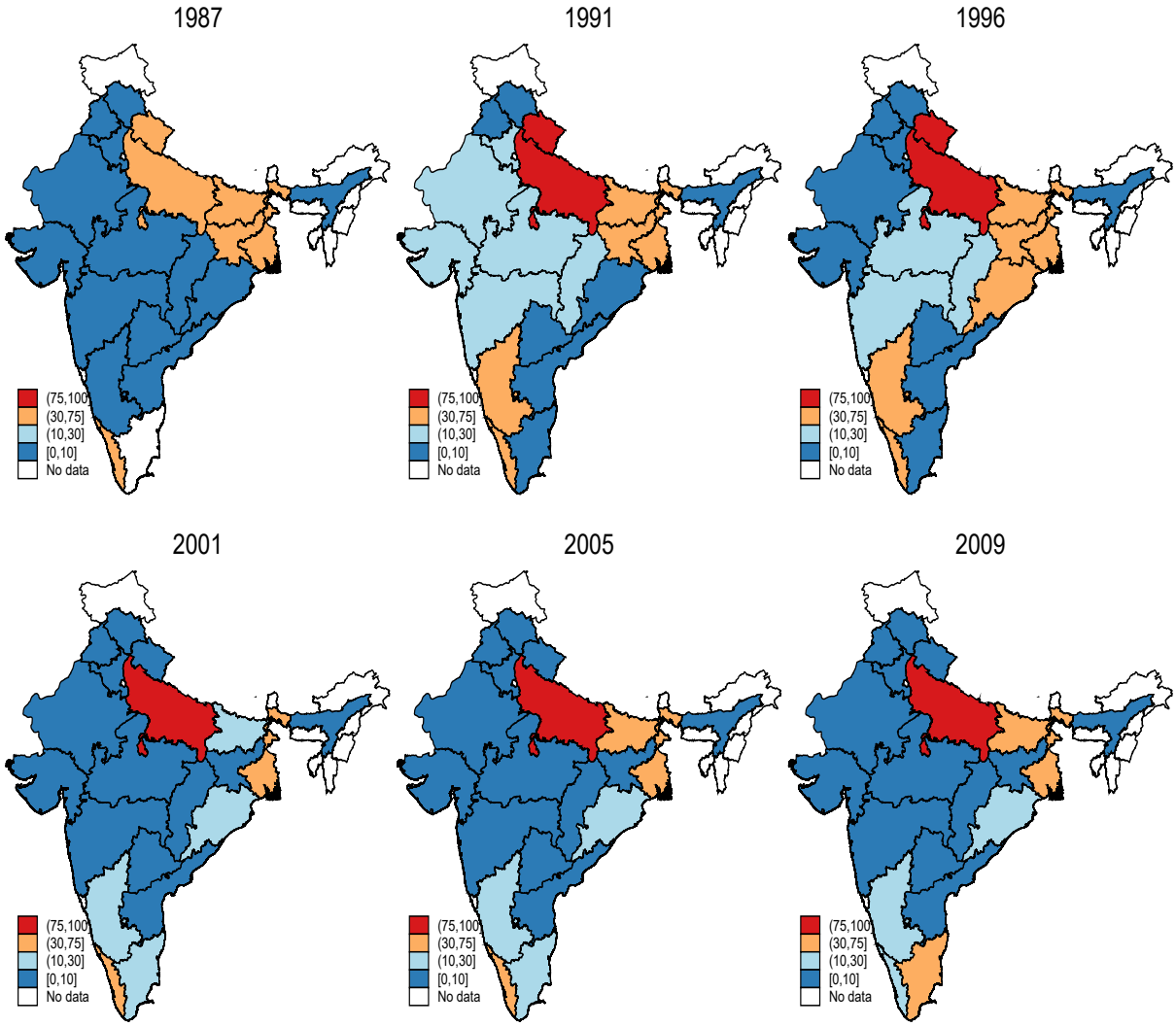
B.3 Additional Figures

Figure B.1: State-level Low-Caste Party Vote Shares: 1987-2009



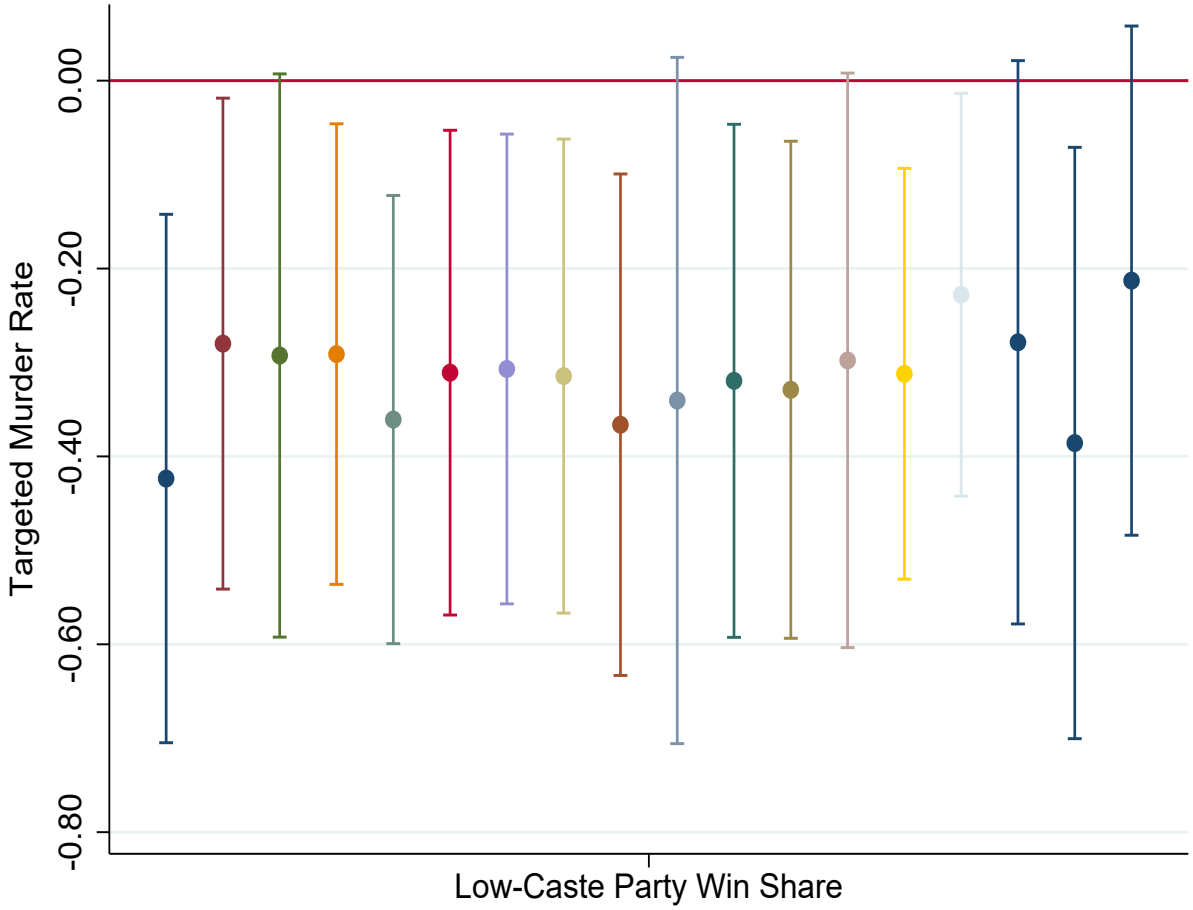
Notes: The figure depicts the vote share of low-caste parties across major Indian states between 1987 and 2009. Low-caste party vote share is calculated as total votes received by low-caste parties in the state during an electoral cycle, normalized by total votes cast in the state.

Figure B.2: State-level Low-caste Party Close Elections: 1987-2009



Notes: The figure depicts the total close elections contested by low-caste parties against non-low-caste parties across major Indian states between 1987 and 2009. Close elections are defined as elections where the difference in victory margin between the winner and the runners up is less than 4 percent of the votes cast.

Figure B.3: Robustness of Main (IV) Results to the Dropping of Individual States



The above coefficient plots presents coefficients estimating the effect of low-caste party legislators on targeted murders against SC/STs. The regressions sequentially drop 1 state at a time. The unit of observation is the state. The outcome variable is total targeted murders against SC/STs, normalized by the SC/ST population. The independent variable of interest is the fraction of elections won by low-caste parties in the state, instrumented by the fraction of close elections won by low-caste parties. All specification control for the share of close elections contested by low-caste parties in the state, state, year and electoral cycle fixed effects and state-level time-varying demographic characteristics.

B.4 Additional Tables

Table B.1: Exogeneity Check: Verifying Demographic Covariates Do Not Predict Low-caste Party Close Wins - No Fixed Effects

	(1) Pct Workers	(2) Pct Urban	(3) Pct Literate	(4) Pct SC/ST	(5) Pct Rsvd	(6) Population Density	(7) Gender Ratio
Low-caste Party Win Share	.0043	-.0028	.0034*	-.0008	-.0623	.0000	-.6571
	(.0082)	(.0030)	(.0019)	(.0036)	(.2928)	(.0002)	(.4744)
Observations	67	67	67	67	66	67	67
R^2	.01	.02	.03	.00	.00	.00	.02

Notes: This table presents the results from regressing the fraction of close elections won by low-caste parties in the state on state-level demographic covariates. The unit of observation is the state. The outcome of interest is the fraction of close elections won by low-caste parties in the state. Close elections are elections where the difference in margin of victory is less than or equal to 4 percent of votes cast. The demographic covariates of interest correspond to the previous electoral period and are averaged across 5 years. The demographic covariates are the percentage of workers, percent of urban population, percentage literate, percentage SC/ST population, percentage of elections reserved for SC/ST candidates, population density and gender ratio. Standard errors are in parentheses, clustered by state.

Table B.2: Exogeneity Check: Verifying Political Covariates Do Not Predict Low-Caste Party Close Wins

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Low Caste Party									
	Lag Power	Lag Win	Lag Close Win	Lag Vote Share	Vote Share	Lag Close Election	Close Elections	Voter Turnout	No. of Candidates	No. of Parties
Low-caste Party Win Share	-.040	.014	-.110	.025	.122	.121	.077	.361	-.007	-.053
	(.0662)	(.113)	(.124)	(.220)	(.365)	(.161)	(.232)	(.212)	(.005)	(.035)
Observations	67	66	64	66	66	85	85	85	85	85
R^2	.01	.00	.01	.00	.00	.01	.00	.02	.02	.02

Notes: This table presents the results from regressing the fraction of close elections won by low-caste parties in the state on state-level political covariates. The unit of observation is the state. The outcome of interest is the fraction of close elections won by low-caste parties in the state. Close elections are elections where the difference in margin of victory is less than or equal to 5 percent of votes cast. The political covariates are 1) a dummy equal to 1 if low-caste parties were in power the previous electoral period; 2) lagged fraction of elections won by low-caste parties; 3) lagged fraction of close elections won by low-caste parties; 4) lagged low-caste party vote share; 5) lagged fraction of low-caste party contests which were close; 6) contemporaneous low-caste party vote share; 7) contemporaneous fraction of elections contested by low-caste parties which were close; 8) contemporaneous voter turnout; 9) contemporaneous number of candidates; 10) contemporaneous effective share of parties. Standard errors are in parentheses, clustered by state.

Table B.3: Comparison of Confidence Intervals with Wild Cluster Bootstrapped Standard Errors Versus Confidence Intervals with Standard Errors Clustered by State

	SC/ST Murder Rate No Covariates	SC/ST Murder Rate With Covariates	SC Murder Rate With Covariates
Standard Errors - State Cluster	[-.718, -.004]	[-.751, -.148]	[-.815, -.217]
Standard Errors - Wild Cluster Bootstrap	[-.714, -.048]	[-.822, -.179]	[-.894, -.260]

Note: The above table compares the 95% confidence intervals obtained using standard errors clustered by state, versus those obtained using the wild-cluster bootstrap to adjust for the small number of state clusters (19). The standard errors correspond to the coefficients obtained in columns (3), (4) and (5) of Table 4. The wild-cluster bootstrap is based on 1,000 repititions.

Table B.4: Low Caste Party Representation and Murders Against SC/STs: Reduced Form and First Stage

	(1)	(2)	(3)	(4)
	Reduced Form		First Stage	
	Murder Rate	Murder Rate	Low Caste Party Win Share	Low Caste Party Win Share
Low Caste Party Close Win Share	-.102**	-.058**	.196**	.119**
	(.047)	(.026)	(.091)	(.052)
Observations	297	297	297	297
R^2	.89	.90	.87	.95
Dep. Var. Mean	32.28	32.28	28.07	28.07
Covariates	No	Yes	No	Yes

Notes: This table presents the reduced form and first stage results corresponding to the baseline specification estimating the impact of an increase in low caste parties' electoral success on targeted murders against SC/STs. The unit of observation is the state. The outcome variable in columns (1) and (2) is total targeted murders against SC/STs, normalized by the SC/ST population. The outcome variable in columns (3) and (4) is the fraction of elections won by low caste parties in the state. The independent variable of interest is the fraction of close elections won by low caste parties in the state, corresponding to that state-electoral cycle. An election is deemed to be close if the difference in victory margin between the winner and the runners up is less than 4% of the total votes cast. Each specification controls for the share of close elections contested by low caste parties in the state, state, year and electoral cycle fixed effects. Columns (2) and (4) also control for state-level time-varying demographic characteristics. All specifications are weighted by the state population and standard errors are in parentheses, clustered by state.

Table B.5: Low-Caste Party Legislators and Targeted Crimes Against SC/STs

			Panel A: SC/ST			
	(1) Crime Index	(2) SC/ST Prevent Rate	(3) Robbery Rate	(4) Kidnapping Rate	(5) Assault Rate	(6) Rape Rate
Low-caste Party Win Share	-.133**	-13.952*	-.461**	.186	.707	-1.335
	(.059)	(7.258)	(.184)	(.249)	(.932)	(2.911)
Observations	297	297	297	297	297	297
R^2	.71	.50	.53	.76	.78	.72
Dep Var Mean	0.00	548.09	8.55	18.45	72.29	209.76
			Panel B: SC			
	(1) Crime Index	(2) SC/ST Prevent Rate	(3) Robbery Rate	(4) Kidnapping Rate	(5) Assault Rate	(6) Rape Rate
Low-caste Party Win Share	-.137**	-12.267*	-.384**	.127	.668	-.907
	(.068)	(6.575)	(.174)	(.230)	(.940)	(2.580)
Observations	297	297	297	297	297	297
R^2	.78	.55	.60	.83	.69	.70
Dep Var Mean	0.00	503.54	7.12	15.88	53.22	181.69

Notes: This table presents the results estimating the impact of an increase in low caste parties' electoral success on targeted crimes against SC/STs. The unit of observation is the state. The outcome variable in column (1) is a composite index of all targeted crimes against SC/STs; column (2), crimes registered under the Civil Rights Act and the Prevention of Atrocities Against SC/ST Act; in column (3), robberies; in column (4) kidnapping; in column (5) violent assaults; and in column (6), rape. The crime incidences are scaled by the SC/ST population in the state. The independent variable of interest is the fraction of elections won by low caste parties in the state, instrumented by the fraction of close elections won by low caste parties. An election is deemed to be close if the difference in victory margin between the winner and the runners up is less than 4% of the total votes cast. Each specification controls for the share of close elections contested by low caste parties in the state, state, year, electoral cycle fixed effects and state-level time-varying demographic characteristics. All specifications are weighted by the state population and standard errors are in parentheses, clustered by state.

Table B.6: Targeted Crimes Against SC/STs: Robustness Checks

	(1)	(2)	(3)	(4)	(5)
	Sample Restrictions			Clustering and Weighting	
	At Least 1 Close Election	Exclude Kerala	All Major States	Exclude Population Weights	State Year Clusters
Low-caste Party Win Share	-.323**	-.318**	-.510***	-.931**	-.450**
	(.160)	(.147)	(.197)	(.395)	(.229)
Observations	272	317	279	297	297
R^2	.90	.90	.90	.74	.90

Notes: This table presents results from robustness checks of specifications identifying the impact of low-caste parties on targeted murders against SC/STs. The unit of observation is the state. The outcome variable is total targeted murders against SC/STs, normalized by the SC/ST population. The independent variable of interest is the fraction of elections won by low-caste parties in the state. The fraction of elections won by low-caste parties in the state is instrumented by the fraction of close elections won by low-caste parties in the state. An election is deemed to be close if the difference in victory margin between the winner and the runners up is less than 4% of total total votes cast. Each specification controls for the share of close elections contested by low-caste parties in the state, state, year and electoral cycle fixed effects, along with state-level time-varying demographic characteristics. All specifications with the exception of column (4) includes population weights. Column (1) restricts the sample to states which have at least 1 close election between low-caste and non-low-caste parties. Column (2) excludes the state of Kerala from the sample where the Left parties are the strongest. Column (3) includes all states including West Bengal in the sample. Column (4) excludes population weights while column (5) clusters the errors at the state-electoral cycle.

Table B.7: Low-Caste Party Representation and Arrest Rate for Targeted Murders Against SC/STs

	(1)	(2)	(3)
	Murder Arrests All	Murder Arrests SC	Murder Arrests ST
Low-caste Party Win Share	.075*	.042*	.017
	(.045)	(.022)	(.011)
Observations	189	189	189
R^2	.94	.86	.87

Notes: This table presents the results estimating the impact of low-caste parties on arrests against targeted murders against SC/STs. The unit of observation is the state. The outcome variable is the logged number of arrests effected in cases pertaining to targeted murders against SC/STs. The independent variable of interest is the fraction of elections won by low-caste parties in the state, instrumented by the fraction of close elections won by low-caste parties. An election is deemed to be close if the difference in victory margin between the winner and the runners up is less than 4% of the total votes cast. Each specification controls for the share of close elections contested by low-caste parties in the state, state, year, electoral cycle fixed effects and state-level time-varying demographic and political characteristics, along with the targeted murder rate against SC/STs. All specifications are weighted by the state population and standard errors are in parentheses, clustered by state.

Table B.8: Low-caste Party Success and Self-Reported Caste Conflicts and Trust in Public Institutions for OBC Households

	(1) Trust in State Institutions	(2) Caste Conflict
Low-case Party Win Share	.0962	-.0067
	(.0617)	(.0041)
Observations	23650	23650
R^2	.00	.00
Dep. Var. Mean	-.01	.07

Notes: The above table presents results from specifications identifying the impact of low-caste parties on self-reported violent caste conflicts and trust in public institutions for OBC households. The unit of observation is the household. The outcome variable in column (1) is a binary which equals 1 if the individual reports a high occurrence of violent caste conflicts in the village. The outcome variable in column (2) is a standardized index measuring trust in public institutions: namely politicians, police, state government and courts. The independent variable of interest is the fraction of elections won by low-caste parties, instrumented by the fraction of close elections won by low-caste parties. An election is deemed to be close if the difference in victory margin between the winner and the runners up is less than 4% of the total votes cast. All specifications include household and survey year fixed effects, in addition to political and household-level covariates. Standard errors are in parentheses, clustered at level of the primary sampling unit (village or town).