More COPS, Higher Turnout?

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Abstract
What is the effect of police on the political lives of Americans? Extant studies suggest that scaling back police presence may increase political participation among Black and Latino Americans by reducing police surveillance and harassment. I theorize that police indirectly affect voter turnout through their effects on crime, and I argue that Black Americans are uniquely positioned to benefit from reductions in crime. Using data from a natural experiment induced by the disbursement of a federal hiring grant to municipal police agencies, I find evidence that an additional police officer increases Black turnout by two percentage points. This increase can be attributed, at least in part, to reductions in criminal violence. Importantly, I find that mobilization primarily occurs among Black men — the group most at risk of criminal victimization. This project speaks to the importance of incorporating criminal violence into theories of the political consequences of criminal-justice institutions.

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1 Introduction

After the police killing of George Floyd on May 25, 2020, nationwide protests called for the defunding of police agencies across the country. Importantly, some existing theory and evidence within political science suggests that defunding police agencies and shrinking police presence may have the benefit of bolstering political participation among those affected by practices of racialized policing and surveillance, namely Black Americans (Weaver and Lerman 2010; Soss and Weaver 2017; Burch 2014). Despite few municipalities adhering to their 2020 promises of defunding their police departments and shrinking their footprints (Goodman 2021; Hoang and Benjamin 2023), a recent natural experiment provides us with an opportunity to estimate how police numbers affect political participation.

Prior research provides us with good reasons to expect that the size of police forces will have meaningful consequences for the political lives of Americans. We know interactions with the criminal-justice system reduce citizen trust and involvement with politics (Weaver and Lerman 2010; White 2019b; McDonough et al. 2022) and that these political effects reverberate across familial networks, neighborhoods, and broader communities (Anoll and Israel-Trummel 2019; Burch 2013; Lee, Porter, and Comfort 2014; Walker 2020; White 2019a). Importantly, there is reason to expect that the socio-political costs of policing are disproportionately borne by racial minorities, as police stop, search, ticket, and brutalize non-Whites at higher rates than their White counterparts (Baumgartner, Epp, and Shoub 2018; West 2018; Fryer 2019; Gonçalves and Mello 2021; Aggarwal et al. 2022; Luh 2022). Thus, a growing or shrinking police force may have the starkest effects on the political lives of non-White civilians.

At the same time, an alternative mechanism may mediate the causal relationship between police and voting behavior. Crime is a destructive force in the lives of Americans today. There is evidence that exposure to crime reduces life satisfaction and labor market attachment, harms physical and mental health, and even impairs cognitive functioning (Sharkey 2010; Cornaglia et al. 2014; Dustmann and Fasani 2016; Johnston et al. 2018; Bindler and Ketel 2022). Moreover, the costs of crime fall disproportionately on Black Americans (Thompson and Tapp 2022; Langley and Sugarmann 2023). To the extent that police reduce crime, police may indirectly shape opportunities for political engagement, especially among those most affected by crime. Yet no study to date has theorized or tested whether policing's effects on crime have downstream consequences for voter turnout.

I argue that policing reduces crime, which in turn affects turnout. Notably, police-mediated changes in crime may shape voter participation in ways that are distinct from adversarial encounters with police. Although negative interactions with police may engender distrust in government and push citizens out of politics (Weaver, Prowse, and Piston 2020), reducing crime also may enable citizens to draw upon financial, physical, psychological, and cognitive resources that would otherwise have been lost to crime to engage in the political process.

Taking into consideration both mechanisms that might shape the relationship between police and voter turnout — adverse police contact and exposure to crime — it becomes clear that, *a priori*, the effect of police on community turnout is ambiguous. That is, police may lower voter turnout if additional police alienate voters through aggressive policing, or police increase turnout by lowering crime. Another possibility is that both statements are true but for different groups of people: police lower turnout among those affected by surveillance practices while other groups who benefit from protection are mobilized.

What, then, is the effect of police on turnout? This paper's main contribution is to provide an estimate of the causal effect of hiring an additional officer on voter turnout across racial groups at the municipal level. Leveraging data from a natural experiment induced by the disbursement of a federal hiring grant to municipal police agencies, I show that in areas where these agencies received the grant, the Black-White turnout gap decreased by approximately 2 percentage points — driven by increased Black turnout. After showing that police numbers affect voter turnout, I turn my attention to violent crime's effect on turnout. Using grant disbursement as an instrument for the homicide rate, I find that an additional homicide significantly widens the Black-White turnout gap because homicides reduce Black turnout. Finally, additional analyses reveal that the narrowing of the Black-White turnout gap in response to police is a result of higher turnout among Black men — the group most likely to be affected by crime.

2 Background: Racial Disparities in Victimization and Demand for Police

Despite popular support for police reforms in the United States, there remains widespread demand for policing services. Across the country, majorities of Americans from all ethno-racial backgrounds express demand for maintaining policing services within their community (Saad 2020).[1] Even among Black Americans, whose confidence in police has consistently lagged behind other groups (Jones 2021), 61% report wanting police to spend the same amount of time in their neighborhood, with roughly equal proportions (approximately 20%) wanting police to spend more or less time in their area. The demand for policing is notable given national discussions surrounding police reform inspired by the 2020 protests against police violence.

Importantly, demand for policing services by Black Americans is not an aberration. Scholars of policing remind us that historically, demand for police transcends racial lines

[1] National polling conducted by Gallup finds that majorities of Black (61%), White (71%), and Hispanic (59%) Americans want to maintain current levels of policing in their neighborhoods; on average, 19% want to see more police (Saad 2020). National opinion is consistent with survey findings from municipal samples. Public opinion polls across five American cities find a majority of respondents from all backgrounds expressed they would feel safer with more, not fewer, police. https://www.suffolk.edu/academics/research-at-suffolk/political-research-center/polls/cityview-polls/2021.

(Fortner 2015; Clegg and Usmani 2017; Forman 2017). Even as Black Americans are more likely to embrace alternatives to traditional policing than White Americans (Forman 2017; Clegg and Usmani 2017), they also express more anxiety about crime (Clegg and Usmani 2017). Indeed, in its diagnosis of the root causes of the 1967 race riots, the Kerner Commission observed that Black civilians interpreted the withholding of policing services and enforcement from their neighborhoods as a form of racial discrimination, even as they simultaneously recognized and denounced discriminatory police violence against Black civilians and suspects (Kerner 1968).

Perhaps the clearest contemporaneous example of this appetite for policing is the 2021 election in Minneapolis, MN. Faced with a ballot proposal that sought to replace the Minneapolis Police Department with a public safety agency that would rely on a public health approach to order maintenance, Minneapolis voters rejected reform. Local reports suggest that opposition to reform was motivated, at least in part, by concerns that a reformed agency would employ fewer police officers. Notably, a larger share of Black residents opposed a smaller police force compared to White residents (Collins 2021). In an interview with local news media, Ea Porter, a Black Minneapolis resident, expressed concerns about the racially disparate effects that a shrinking police force would have on Black residents, saying "I think Black voters are more likely to feel the effects of the lack of effective, and inadequate policing," Underlining her point, she added, "Again, don't experiment on us, because we're the ones that are going to be hit hardest first" (Collins 2021).

Minneapolis is by no means an outlier. In 2020, several U.S. cities cut funding to their police agencies as a step towards police reform; however, this change was short-lived as agencies saw their budgets restored the following year (Goodman 2021; Hoang and Benjamin

2023). This about-face was motivated by increasing crime in cities across the United States. Given the salience of the Floyd protests at the time, local leaders cited the racialized dimension of criminal victimization, noting that Black Americans are at higher risk of suffering from criminal violence (Goodman 2021).

Indeed, Black Americans do suffer from higher rates of serious violent crime than their White counterparts (Thompson and Tapp 2022). To give an example, by the age of 40, 7% of Black and Hispanic Chicagoans have been shot compared to just 3% of White residents (Lanfear, Bucci, and Kirk 2023). Similarly, more than half of Black and Hispanic Chicagoans have seen someone shot by age 40, compared to 25% of Whites. In Chicago and Philadelphia, military-aged men living in zip codes with the highest rates of fatal shootings faced a greater risk of firearm death than US military personnel deployed to Afghanistan during the war (Del Pozo et al. 2022). These risks are largely borne by Black and Hispanic male residents. Nationally, Black Americans are also over-represented among homicide victims according to data collected by the FBI's Uniform Crime Reports (UCR), with 53% of race-identified homicide victims being Black in 2019 (United States Federal Bureau Of Investigation). Similarly, data from the CDC's National Vital Statistics System indicate that Black men were more than 6 times as likely to be murdered than White men.[2] These inequalities in protection from violence are compounded by the fact that even non-fatal violent encounters can have profound consequences for victims.

The Behavioral Consequences of Crime

In addition to the costs directly associated with the crime (e.g., physical injury, damaged or lost property, loss of life, etc.), exposure to criminal violence carries a host of adverse effects.

[2] It is worth noting that although the homicide victimization rate is higher among Hispanic men than White men, the male Hispanic victimization rate is substantially lower than the victimization rate among Black men. For example, in 2016, the age-adjusted White homicide rate among men was 5.2 per 100,000. The rates for Hispanic men and Black men were 8.6 and 38, respectively (National Center for Health Statistics 2018).

Bindler and Ketel (20222) show that victimization reduces earnings and increase healthcare expenditures. Crime increases mental distress among victims and non-victims (Cornaglia, Feldman, and Leigh 2014; Dustmann and Fasani 2016; Bindler and Ketel 2022) and reduces life satisfaction (Cohen 2008; Johnston, Shields, and Suziedelyte 2018). To fully appreciate the extent to which crime distorts everyday life, it is useful to consider that even children are not immune to these adverse effects. School-aged children exposed to episodes of criminal violence perform markedly worse on measures of academic achievement (Sharkey 2010; Koppensteiner and Menezes 2021).

These ancillary costs of crime, coupled with the direct costs of crime (Bindler, Ketel, and Hjalmarsson 2020), are likely to have substantial consequences for the political participation and attitudes of entire communities. After all, inadequate protection from crime constrains citizenship, as citizens are unable to fully engage in democratic activities when their physical security is threatened or uncertain (González 2017; Moffett-Bateau 2023). Just like time, effort, or money, physical safety may be a fundamental determinant of who participates in politics (Verba et al. 1993; Brady, Verba, and Schlozman 1995). Thus, it is plausible that disparities in political participation between Black and White Americans are due, at least in part, to differential exposure to crime.

There is evidence that experiences with crime or violence are associated with changes in political engagement, though evidence of the direction of the effect is mixed. In some contexts, respondents who report prior episodes of criminal victimization are more likely to report engaging in non-electoral forms of political participation (Bateson 2012). Similarly, individuals exposed to wartime violence are more likely to engage with the political process through voting (Blattman 2009). Additional results supporting this conclusion are reported by Bellows and

Miguel (2008) using data from Sierra Leone. In contrast, other studies report that victimization is negatively associated with voter turnout. Data from Mexico suggests that criminal violence is negatively associated with turnout at both the municipal level (Trelles and Carreras 2012) and individual level (Malone 2013; Ley 2018). To overcome challenges to inference and measurement error associated with survey-based measures, Sønderskov et al. (2022) leverage administrative data from Denmark to identify the causal effect of criminal victimization. These authors find that victimization increases turnout, but this holds only for victims of violent crime.

Using data from the U.S., both Hassell, Holbein, and Baldwin (2020) and Garcia-Montoya, Arjona and Lacombe (2022) investigate the effects of school shootings on political participation in the U.S. Neither study finds an effect on voter turnout, and the former does not find any effect on voter registration. Moffett-Bateau (2023) uses ethnographic data to show that experiences of violence can diminish political engagement among Black women living in poverty. In contrast, in his study of traumatic events, Marsh (2022) finds that racial violence raises Black turnout. Overall, then, some studies find a positive, null, or negative effect of crime or violence on measures of political participation.

Despite the mixed evidence regarding crime's (de-)mobilizing effects, the potential for crime to be politically meaningful remains clear. How then should this affect our current conceptualizations of the policy feedback of policing in the U.S. (Pierson 1993; Soss and Weaver 2017)? Extant studies of policing carefully consider how the socio-political costs that emerge from police enforcement shape policy feedback. Mainly, scholars in this field have argued that policing as an authoritarian institution degrades civic trust and capacity by imparting lessons of social, class, and racial hierarchies and reducing access to important social and economic resources (Soss and Weaver 2017; Michener 2019; Garcia-Rios et al. 2021). However, when one considers the potential benefits of police numbers (e.g., lower crime or crime expectations, reduced fear of crime, reduced private investments in security, etc.), different policy feedback dynamics become possible.

Of course, this by no means suggests that authoritarian policing practices have no bearing on the civic capacity of individuals and communities. A large literature within political science has taught us that interactions with the criminal-justice system have significant social and economic consequences that curtail voting participation (Weaver and Lerman 2010; Burch 2013; Soss and Weaver 2017; White 2019b; McDonough et al. 2022; Ben-Menachem and Morris 2022). Fatal encounters with police, on the other hand, counter-mobilize citizens (Ang and Tebes 2024; Burch 2022; Morris and Shoub 2023) and reduce citizen cooperation with police (Ang et al. 2021). Thus, extreme forms of police violence and other moments of perceived police injustice may galvanize voters (Walker 2014), while day-to-day interactions with police diminish voting participation.

The growing recognition of the regressive tools available to police — in the form of extractive traffic fines and fees (Garrett and Wagner 2009; Harris, Ash, and Fagan 2020) and unnecessary stops, arrests, and searches (Cho, Gonçalves, and Weisburst 2021) — and their disproportionate deployment against non-White civilians (Gelman, Fagan, and Kiss 2007; Luh 2022; Aggarwal et al. 2022) has led many scholars of American politics to understand policing as "as an institution of social control, rather than providing safety for people vulnerable to crime" (Harris, Walker, and Eckhouse 2020). Still, as concerns about crime among the public and evidence of the downstream costs of crime make clear, there are good reasons to incorporate violence into our theories and analyses of policing and its policy feedback.

3 Policing and Voter Turnout: Can policing reduce Black-White disparities in voting?

Current conditions of racial disproportionality with respect to both policing and crime should push us to consider how policy feedback effects may be conditional on race (Michener 2019; Garcia-Rios 2021). In particular, the "security gap" (Miller 2013) faced by Black Americans — that is, the over- and under-policing of this group — may make these voters sensitive to changes in the police footprint in their communities in ways that are distinct from White, Asian, and even Hispanic voters. Because Black Americans are disproportionately exposed to criminal violence, then changes in rates of violent crime are more likely to be discerned by this group. At the same time, the costs associated with policing in the forms of surveillance and enforcement may also be more readily discerned by Blacks. In other words, the theoretical approach proposed here does not entirely revise the prior approach to the study of policing, but rather considers both the costs and benefits of policing.

To see how incorporating crime into the analysis affects our theoretical expectations, let us consider two theoretical mechanisms through which public policy affects mass publics: resource and interpretive effects. The former refers to the fact that policies provide resources and incentives that facilitate participation and pull citizens into politics. Second, the interpretive effects channel suggests that policies affect how citizens come to understand their government and their standing as citizens. Citizens' perceptions of government and themselves are shaped through repeated interactions with bureaucrats, as these interactions teach them how government implements and enforces its goals. The combination of resource and interpretive effects can draw citizens into or out of politics.

With respect to policing and other law enforcement institutions, current understandings highlight how criminal-justice involvement undermines access to resources and serves as a crucial moment of socialization out of formal politics (Soss and Weaver 2017). However, contact with civilians is not the only means through which police and other criminal justice institutions may structure the decisions and choices available to citizens.

More cops, less crime, higher turnout?

The primary responsibility of police is to address and deter crime in their jurisdiction. Indeed, there is substantial evidence that police presence lowers crime by increasing the probability of apprehension (Becker 1968). Extant studies point to a substantial crime-reduction benefit to police presence (Cohen and Ludwig 2003; Klick and Tabarrok 2005; Draca et al. 2011; MacDonald et al. 2016; Heaton et al. 2016; Weisburd 2021). There is also strong evidence that an additional police hire causes reductions in both violent and property crimes, though violent crimes are more sensitive to police (Evans and Owens 2007; Worrall and Kovandzic 2010; Chalfin and McCrary 2018; Mello 2019; Weisburst 2019; Chalfin et al. 2022).[3] Among these, Chalfin et al. (2022) consider the differential effects of police numbers on Black-White victimization. This study uses historical data from 1981–2018 to estimate the effect of police on rates of homicide victimization by race. They find that police reduce homicides, and importantly, the benefits of these abated murders flow disproportionately to Blacks. That is, when it comes to the costliest crimes, police benefit Blacks more than Whites due to the former's over-representation among crime victims. This provides important empirical support for the idea that the policy feedback effects of police protection may have differential effects across racial groups.[4]

With these facts in mind, I consider the potential policy feedback associated with police

[3] It should be noted that the "police manpower" literature is primarily interested in measuring the effects of police force size on crime and arrests. Studies of police force composition (e.g., race and gender composition of police) estimate differential effects by race and gender on a number of outcomes, including victimization and arrests across groups (Donohue and Levitt 2001; McCrary 2007; Miller and Segal 2019; Harvey and Mattia 2022).
[4] As noted earlier, national victimization rates suggest that rates of violent victimization among Hispanics are closer to Whites than Blacks. Thus, this section focuses on the predicted effects of police on Black Americans.

numbers. Most obviously, victims are made worse off as they incur the direct costs of a specific crime. However, the costs of crime are also borne by those at risk of victimization. That is, broader neighborhoods and communities are negatively impacted as individuals siphon resources towards precautionary measures meant to decrease victimization risk. In the most extreme cases, these precautionary measures may take the form of private gun ownership (legal or otherwise) (Lacombe et al. 2022). The lack of adequate protection from violence pushes individuals out of public life as they attempt to reduce their victimization risk (Moffett-Bateau 2023), and in some instances, may push people out of their communities altogether (Xie and McDowall 2008; Katz, Kling, Liebman 2001). In summary, crime makes citizens worse off and deteriorates communal ties.

The instability and uncertainty created by crime constrain the ability of individuals and communities to engage in politics. Rather than invest time, knowledge, and effort into learning about specific candidates, contests, and other ballot measures, individuals who are on the margin of violence may choose to allocate their available resources to matters more directly relevant to their immediate well-being. Therefore, the provision of police may free up significant material, psychological, and cognitive resources insofar as police reduce crime and victimization risk. In other words, physical safety and certainty about one's physical safety are valuable resources that potential voters draw upon when engaging in politics (Verba et al. 1993; Brady, Verba, and Schlozman 1995). Due to inequalities in protection from violence, the effects of police should be most pronounced for Black Americans. Thus, Hypothesis 1 states:

H1: *An increase in police numbers increases community-level turnout among Black voters — reducing Black-White disparities in voting.*

It is worth emphasizing that this expectation is contingent on police reducing levels of overall violence. That is, if more police translate to less criminal violence, then turnout should increase.

However, given that the security gap faced by Black Americans is defined by both civilian *and state* violence, it is possible that police provision does not always imply a narrowing of the security gap. If for instance, the provision of police raises overall levels of state violence and crime does not decrease, then one should not expect police hires to increase turnout.

Even though non-Black racial groups have lower rates of victimization, it remains possible that changes in crime may be felt by all racial groups. After all, as reviewed earlier, crime is costly, and it is likely that lower rates of disorder improve the ability of all citizens to participate in social and civic life. This expectation is reflected in Hypothesis 2 (H2).

H2: *An increase in police numbers increases community-level turnout among all racial groups*.

Finally, as mentioned in previous sections, men are at particular risk of criminal victimization, with Black men experiencing the highest rates of criminal victimization. Therefore, it is worth turning our attention to the intersection of gender and race. I expect that Black men will be mobilized by expansions in police numbers due to their disproportionate exposure to crime and their over-representation among homicide victims. Hypothesis 3 (H3) is thus:

H3: *An increase in police numbers increases community-level turnout among Black men — reducing Black-White disparities in voting*.

As in H1, this hypothesis is contingent on a violence-reduction benefit. Indeed, in the absence of such benefit, Black men have the highest risk of *demobilization* due to their higher likelihood of being stopped by police. In other words, Black men are most likely to feel the socio-political costs of police, especially if the benefits of additional police are small.

A potential complication of these hypotheses is that the expectations laid out in H1-H3 share some overlap with existing theories of policy threat. Scholars of race and ethnic politics have shown that local contexts marked by discriminatory or harmful policies targeted at specific

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groups may exhibit higher rates of turnout due to *counter*-mobilization among members of the targeted group (Barreto and Woods 2005; White 2016; Nuamah and Ogorzalek 2021; Morris and Shoub 2023). Thus, results that show heightened Black political participation may be consistent with theories of policy threat and studies that show counter-mobilization in response to police killings of unarmed civilians (Ang and Tebes 2024; Burch 2022; Morris and Shoub 2023). An important distinction between the theory articulated here and existing policy threat frameworks is that the latter predicts heightened turnout across *all* targeted groups. That is, policy threat suggests that Hispanic turnout should increase alongside Black turnout, due to Hispanic Americans' status as a targeted group for policing and immigration enforcement in certain contexts (White 2016; Aslan and Yang 2022; Luh 2022). Although, a null effect on Hispanic turnout would not be dispositive proof against a theory of policy threat, it suggests that a more thorough adjudication of mechanisms is required.

The mechanism most consistent with policy threat frameworks in the context of policing is the rate of adversarial encounters with police (e.g., arrests, stops, etc.); in contrast, the primary mechanism in my theory is criminal violence. Thus, we can distinguish mobilization from counter-mobilization by identifying the mechanism responsible for any observed change in turnout. Overall, then, while policy threat emphasizes, as the name implies, the potential of negative or punitive dimensions of policy to mobilize voters, this theory argues that public safety drives political engagement. In the next section I describe specific tests that I rely on to adjudicate between the crime and contact mechanisms.

4 Data and Research Design

To estimate the effect of police on voter turnout, I constructed a panel dataset of voter turnout at the municipal level and municipal police numbers from 2004 to 2014. To do so, I merge publicly available replication data provided by Mello (2019) with historical voting records curated by

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Catalist, LLC. To harmonize voting records with law enforcement agency data maintained by the FBI's UCR program, I used the Law Enforcement Agency Identifiers Crosswalk to identify each municipality (census place) associated with each municipal law enforcement agency included in Mello (2019). I then identified all census blocks within each census place (municipality). Due to the decennial census, this process was conducted twice, as census place boundaries are subject to change across decades. Catalist then identified voters residing in the relevant census block and census block groups and aggregated vote counts for the 2004-2014 elections to the municipal level.[5] Counts were provided separately for each racial group. Due to of concerns of post-treatment bias (Nyhan, Skovron, and Titiunik 2017; Montgomery, Nyhan, and Torres 2018), only voters with a registration date prior to 11/02/2004 were included in the sample, and voters who moved or changed their residential address were excluded for the same reason.[6] In total, voter data was collected for 3,632 municipalities for election years 2004-2014, resulting in 21,792 election-municipality observations. With the exception of Hawaii, all states plus D.C. are included in the data.[7] Aggregate turnout at the municipal level was then merged with agency-level variables, including the main variable of interest, the rate of sworn police officers, and data for two potential mechanisms, violent crime and arrest rates.[8] To account for level demographic

[5] Approximately 2,900 census block groups could not be attributed to a single municipality because these block groups overlapped municipal boundaries. Rather than excluding these voters (approximately 3% of voting records) or the affected municipalities from the analysis, I randomly assigned each census block group to one of its potential parent municipalities. Voters in these block groups were then included in the aggregate count for the randomly chosen parent city. The process was repeated 15 times, creating 15 different sets of vote counts. The results presented in this manuscript are consistent across all 15 iterations of the data.

[6] The earliest historical data maintained by Catalist dates back to 2008. To collect data from the two prior elections, the vote histories of registered 2008 voters were used to identify voters with a registration date prior to the 2004 election. Only voters with an initial registration date prior to this date cutoff were included in the study.

[7] This sample does not include every municipality originally included in Mello's sample due to missing voter records. Table 1A in the Appendix provides descriptive characteristics for included and excluded municipalities.

[8] Due to proprietary concerns on behalf of Catalist LLC, only aggregate turnout rates for each racial group, not vote counts, are available for replication.

and economic differences across municipalities, shares of Black, Hispanic, and young men are included, along with per capita income and the unemployment rate.

The number of sworn police officers, were originally collected from the FBI's Law Enforcement Officers Killed and Assaulted (LEOKA). Data on violent index crimes (i.e., murder, rape, aggravated assault, and robbery) were collected from the FBI's UCR program. The reliability and accuracy of the UCR crime data has been the subject of intensive study (for an example, see Maltz and Weiss 2006); however, as noted by others, the UCR crime data is closely related to both self-reported victimization data collected through the National Crime Victimization Survey and independent data collection efforts by the CDC (Clegg and Usmani 2019).[9] In addition to the FBI's crime data, index arrest counts are from the FBI's Arrests file. The rates for sworn officers, crimes, and arrests are calculated using the population estimates provided in the UCR data.

Demographic data for the share of Black, Hispanic, and young males residents at the county level come from the Surveillance, Epidemiology, and End Results (SEER) program maintained by the National Institutes of Health. Contextual economic measures include county per capita income and unemployment rates collected from the Bureau of Economic Analysis and Bureau of Labor Statistics, respectively.[10]

The present study leverages exogenous variation in police numbers commonly used to estimate the effect of police on crime (Evans and Owens 2007; Mello 2019; Weisburst 2019; Chalfin et al. 2022). Although one could rely on a selection on observables approach to estimate

[9] The UCR data does, however, contain errors (i.e., extreme outliers) and missing observations. Thus, an additional advantage of using replication data provided by Mello (2019) is maintaining continuity across studies in terms of data cleaning procedures.

[10] County level estimates are used as there is no available annual data for economic or demographic variables at the municipal level.

the police-voting relationship, there are several reasons to expect that a naive comparison across municipalities would not yield a credible estimate of the effect of police on voter participation. First, local tax revenues are a significant source of funding for municipal police agencies; thus, municipalities with a broader tax base are better able to hire larger police forces. If unobserved economic differences also correspond to levels of turnout across municipalities, this could bias the estimated effect of police on behavior. Second, minority group size or an influx of minority residents may cause White residents or policymakers to push for additional police hires (Kent and Jacobs 2005; Vargas and McHarris 2017; Derenoncourt 2022). Given the well documented effects of minority group share on electoral participation (Fraga 2016), a selection on observables approach is unlikely to yield an unbiased estimate of the quantity of interest if covariate adjustment fails to adequately account for the pull-factors that draw racial minorities to some municipalities over others.

In this study, I use a federal hiring grant administered by the Office of Community-Oriented Policing Services (COPS) as an instrument for the number of sworn law enforcement officers employed by municipal police departments. Data on agencies that applied for the hiring grant and the final award decision were collected from the COPS office. In the next section I briefly describe the history and details of the COPS hiring program before turning to the empirical strategy.

Background on the COPS Hiring Program

The data collected by Mello (2019) contain a sample of police agencies that applied to the COPS Hiring Program (CHP) in 2009. The program assists state and local police agencies in the hiring and retaining of sworn police officers. Originally established through the Violent Crime Control and Law Enforcement Act of 1994 as part of the Clinton administration's goal to put 100,000 additional officers on the street, grant awards covered 75% of entry-level salary and fringe benefits for a period of three years, with a 25% local match requirement. Funding for the CHP waned in the lead-up to 2009. Starting in 1998, Congress shifted COPS resources away from the CHP in favor of non-hiring grants that provided funds to state and local agencies for new equipment and technologies and programs aimed at combating drug production (James 2013). From 2005 through 2008, appropriations for the CHP never exceeded 20 million dollars.

Congressional priorities shifted once more in the wake of the Great Recession. Concerned that constrained local fiscal budgets would cause police layoffs and that a weak economy would exacerbate crime, Congress included $1 billion in appropriations for the CHP in the American Recovery and Reinvestment Act (ARRA). Unlike prior years, the 2009 round of awards covered the full cost associated with hiring or retaining police officers for the three-year period. The application process was open to state, local, and tribal law enforcement agencies. Applicants were assigned a score by the COPS office based on the agency's responses to the application questionnaire. Scores were based on a weighted total of three categories, an agency's crime need, fiscal need, and use of community policing techniques. The COPS office funded applicants in descending order until funds were exhausted. The CHP funding process thus provides a unique opportunity to estimate the effect of police on turnout, as grant awards were decided quasi-randomly, especially for those municipalities that were on either side of the funding cut-off.

Table 1 presents descriptive statistics of the sample of municipalities in 2009. The data includes only municipalities whose police agency applied for the CHP funds for which I have voter turnout numbers. The average municipality in my sample had approximately 33,000 residents. The average share of Black and Hispanic residents across all cities and towns was

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approximately 10-11%, and the share of young men (15-29 years old) was 22%. There were approximately 23 police officers employed for every 10,000 residents. Table 1 also presents turnout rates in the 2008 election for each racial group.

Table 1. Descriptive Characteristics.

	CHP Funds	No Funds	Total
Population (Ten Thousands)	6.996 (21.74)	2.467 (15.29)	3.295 (16.74)
Unemployment Rate	9.552 (4.020)	6.976 (3.127)	7.447 (3.454)
Household Income (Ten Thousands)	3.960 (1.112)	5.334 (2.164)	5.083 (2.082)
Percent Black	20.76 (22.51)	7.753 (12.38)	10.13 (15.59)
Percent Hispanic	15.19 (20.67)	10.05 (14.92)	10.99 (16.25)
Percent Young Male	23.54 (5.874)	21.60 (6.909)	21.95 (6.773)
Police Per 10,000	26.10 (10.94)	22.69 (11.26)	23.32 (11.28)
Violent Crimes Per 10,000	93.20 (51.00)	56.83 (42.35)	63.47 (46.24)
Property Crimes Per 10,000	497.4 (228.2)	267.6 (162.0)	309.7 (197.1)
Black Turnout	0.751 (0.137)	0.763 (0.189)	0.761 (0.180)
White Turnout	0.801 (0.0943)	0.830 (0.0898)	0.824 (0.0914)
Hispanic Turnout	0.644 (0.195)	0.708 (0.205)	0.696 (0.205)
Asian Turnout	0.653 (0.221)	0.684 (0.256)	0.677 (0.249)
Officers Funded Per 10,000	1.679 (1.601)	0 (0)	0.307 (0.943)
Funding Per Capita	29.60 (23.83)	0 (0)	5.411 (15.32)

There are notable differences along observables between municipalities that received the CHP grant award and those that did not. Treated cities were larger, had higher unemployment, lower median family income, and had younger and more diverse populations. They also had higher rates of police employment and crime and lower rates of turnout for all groups than comparison municipalities.

Empirical Strategy

To estimate the effect of police on voter turnout, I use an instrumented difference-in-differences (DDIV) strategy. The DDIV approach requires a familiar set of assumptions from both traditional difference-in-differences (DiD) and instrumental variables frameworks, including a set of exclusion restrictions, a monotonicity assumption, and parallel trends assumptions (Hudson, Hull, and Liebersohn 2017). Before discussing the DDIV estimation strategy in more detail, I turn to the plausibility of the assumptions.

The exclusion restrictions in the DDIV setting require that the instrument only affects the outcome (i.e., turnout) through the treatment (i.e., the rate of sworn police officers). The validity of the exclusion restriction is not guaranteed. Two plausible ways that the exclusion restriction could be violated in this context include flypaper effects, in which intergovernmental transfers lead to additional spending over and above the amount of the transfer. For example, grantee agencies may have received more funds from local or state sources (non-CHP related) than they would have received in the absence of the grant. A second way the exclusion restriction could be violated would be if agencies that received the CHP award hired additional civilian staff to complement the work done by the newly hired or retained officers. Mello (2019) provides evidence that the 2009 round of the CHP did not affect per capita expenditures on police by state and local governments nor did grant receipt increase the number of civilian employees on payroll at grantee agencies, providing some assurance that the exclusion restrictions are tenable in this context. The monotonicity assumption, while untestable, is likely to hold in this context given the fiscal constraints imposed on local governments by the Great Recession. That is, applicants that did not receive grant funds would not have been able to hire or retain the number of officers that would have been provided to them through the terms of the CHP grant.

Finally, the parallel trends assumption requires that treated and untreated municipalities would have exhibited similar changes in turnout in the absence of the COPS grant. Figure 1 plots trends in voter turnout by racial group for both groups of municipalities. Visually, the trends in turnout for all racial groups look well matched. Due to the limited number of time periods, I do not explicitly test the parallel trends assumption; instead, I include an alternative specification that addresses concerns of a potential violation of the assumption (Wing, Simon, Bello-Gomez 2018). Unlike standard DiDs, the DDIV scales the DiD on the outcome by a DiD effect on the treatment:

$$\beta = \frac{E[Y_{i1} - Y_{i0} | D_i = 1] - E[Y_{i1} - Y_{i0} | D_i = 0]}{E[S_{i1} - S_{i0} | D_i = 1] - E[S_{i1} - S_{i0} | D_i = 0]}, \quad (1)$$

where Y is the outcome (turnout), D is a binary instrument (CHP grant), and S is the treatment of interest (number of sworn officers). Following Mello (2019), I estimate Equation 2:

$$Y_{jit} = \beta Police_{it} + \phi_i + k_t + \lambda(t)_i + \varepsilon \quad (2)$$

where $Police_{it}$ is the number of sworn police officers per 10,000 residents in municipality i at time t instrumented by $Treated_i \times Post_t$. *Treated* is an indicator that takes on a value of 1 for all municipalities that are awarded the COPS grant, and 0 for applicant municipalities that fell below the funding cutoff. *Post* takes on a value of 1 for all election years after 2009, and 0 otherwise. Y_{jit} represents turnout for group j, in municipality i, in year t. Furthermore, ϕ_i represents municipality fixed effects; k_t represents time fixed effects, which vary by city size;

and $\lambda(t)_i$ represents municipality-year fixed effects. The main parameter of interest, β, is the effect of hiring an additional police officer on voter turnout for group *j*.

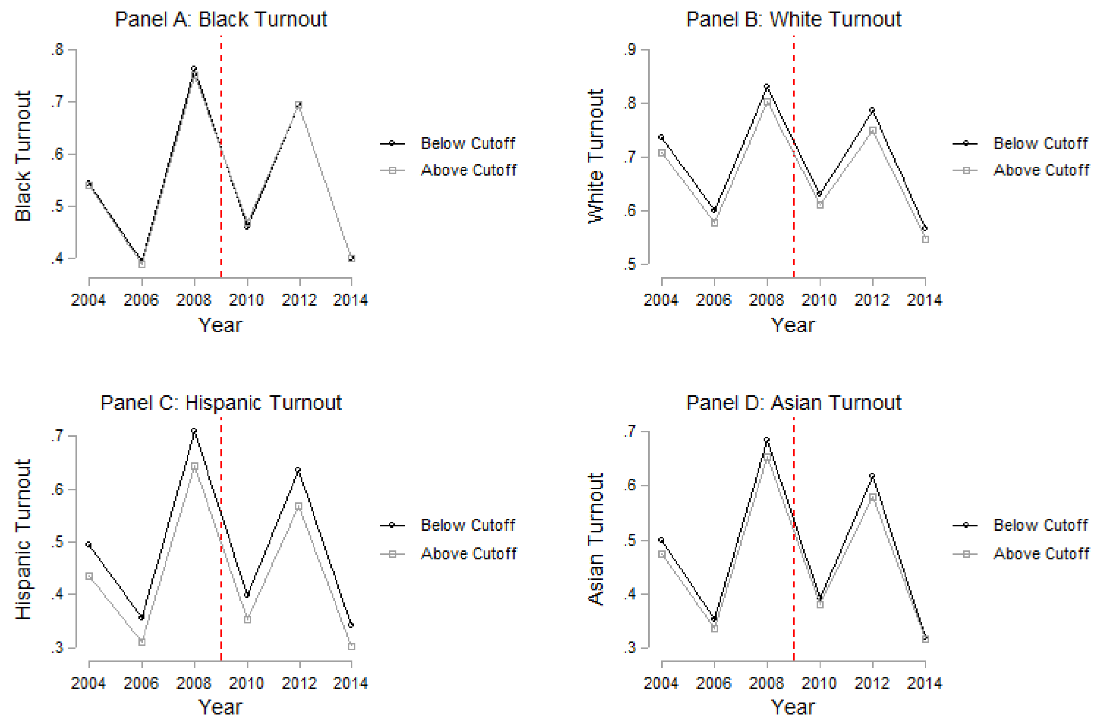

Figure 1. Time trends in voter turnout across applicant municipalities. In all panels, the gray line corresponds to turnout in municipalities that received federal funding through the COPS grant and the black line represents applicant municipalities that did not.

To address a potential parallel trends violation, I validate the results produced by Equation 2 using a triple difference (DDD) approach. In the DDD specification, *Y* represents the difference in turnout rates between Black or Hispanic voters and White voters, with positive values indicating greater minority turnout and negative values reflecting greater White participation. The DDD specification addresses the possibility of a time-varying confounder by adding an additional comparison group that is exposed to the time-varying confounder but is arguably *not* exposed to treatment (Wing, Simon, Bello-Gomez 2018). In so doing, the DDD differences away the confounder. The time trend for the Black-White and Hispanic-White turnout gaps can be found in the Appendix (Figure 1A). It is important to note that this model implicitly assumes that minority voters, but not White voters, are affected by police. To the extent that White voters are affected by police force size in the same direction as minority voters, the estimates of the effect of police are biased towards zero.

5 Results

The central theoretical argument presented in this paper is that police hires can increase turnout through their effects on crime. Therefore, I first present estimates of the effect of an additional police officer on turnout and racial turnout gaps. Then I turn my attention to the effect of police on crime. If police do not meaningfully affect crime, then this is not a plausible mechanism for the main results. Next, I use CHP disbursement as an instrument for the violent crime and homicide rates to estimate the effects of criminal violence on turnout. Finally, I conduct a similar analysis using index arrests to test extant theories of policing.

Figure 2 presents the main results. I find that an additional police hire increases Black turnout by approximately 2 percentage points (see Table 2A).[11] Point estimates for all other groups are statistically indistinguishable from zero.[12] The estimates from the DDD specifications are consistent with the initial estimates (Figure 3 and Tables 4A). An additional officer narrows the Black-White gap by 2 percentage points, but there is no comparable effect for the Hispanic-White gap. These results run contrary to expectations put forth by extant theories of policing that suggest police demobilize racial minorities; however, they are consistent with H1. That is, if police improve local living conditions, then Black turnout increases.

Moreover, counter-mobilization is inconsistent with these results. As stated earlier, policy threat leads us to expect mobilization among both Black and Hispanic voters because there is considerable evidence that Black and Hispanic Americans experience discrimination at the hands

[11] Abbreviated regression tables not included in the main text can be found in Appendix A. Tables with full regression results are included in Appendix E.
[12] Tables 3A and 3E provide corresponding OLS estimates.

of police. The null effect on Hispanic turnout is, therefore, inconsistent with a policy threat channel but, importantly, is not out of step with the crime hypothesis, as Hispanic Americans are much less exposed to criminal violence than Black Americans. In short, these findings provide support for the main hypothesis.

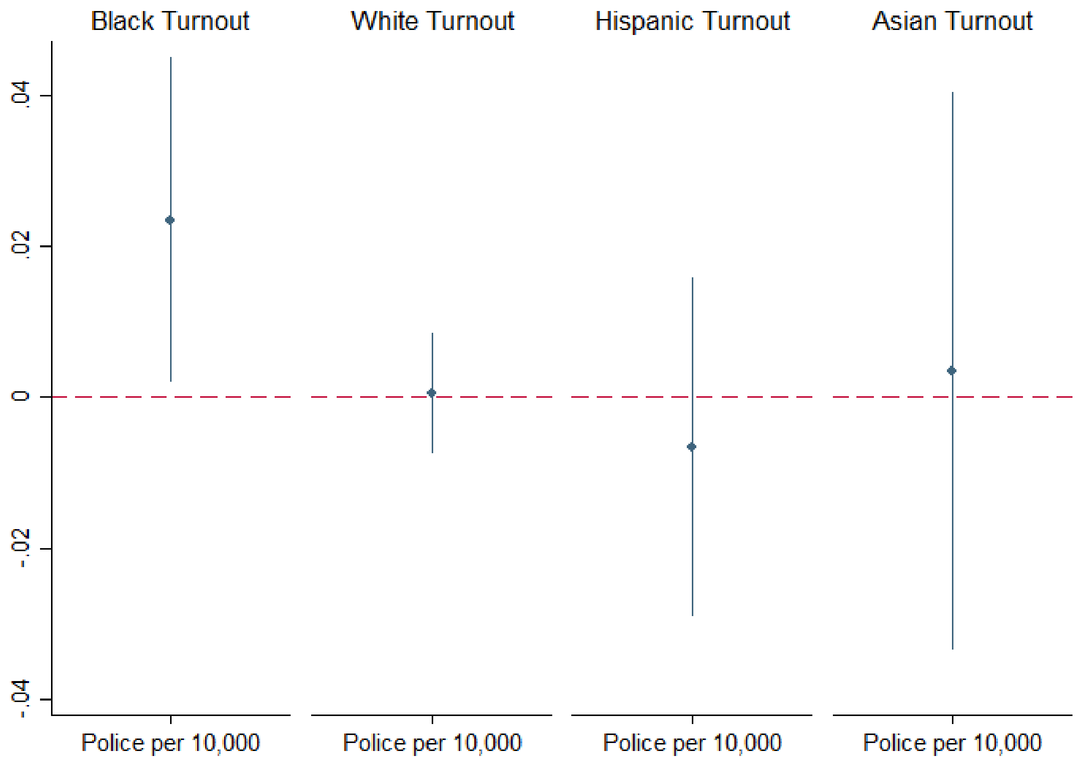

Figure 2. Estimated effects of police on turnout by racial group. Point estimates are presented with 95% confidence intervals. For complete regression results, see Table 1E.

Still, to adequately adjudicate between these two competing theories, it is necessary to consider whether increased Black turnout can be attributed to crime or arrests. To directly test whether changes in crime induced by the CHP change voter turnout, I use the CHP as an instrument for rates of violent crime and homicides.[13] That is, I use the CHP as a source of exogenous variation in violent crime and homicide to estimate the effects of violence on Black

[13] Tables 5A and 4E show the DDIV estimates for the effect of police on the overall violent crime rate and each violent crime type separately. As in previous studies, the results show that police reduce violent crime.

turnout.[14] Columns 1 and 2 in Table 2 show the first-stage estimates from two separate equations along with their corresponding F-statistics.[15] An F-statistic of 6.03 suggests that the CHP is a weak instrument for the overall violent crime rate. However, the CHP is a strong predictor of homicide, with an F-statistic of 13.04. Columns 4 and 6 report the point estimates from DDIV models for which the dependent variable is Black turnout and the Black-White turnout gap, respectively. The results are in line with my theoretical expectations: an additional murder increases the Black-White turnout gap by approximately 9 percentage points. Violence significantly reduces political engagement among Black voters.

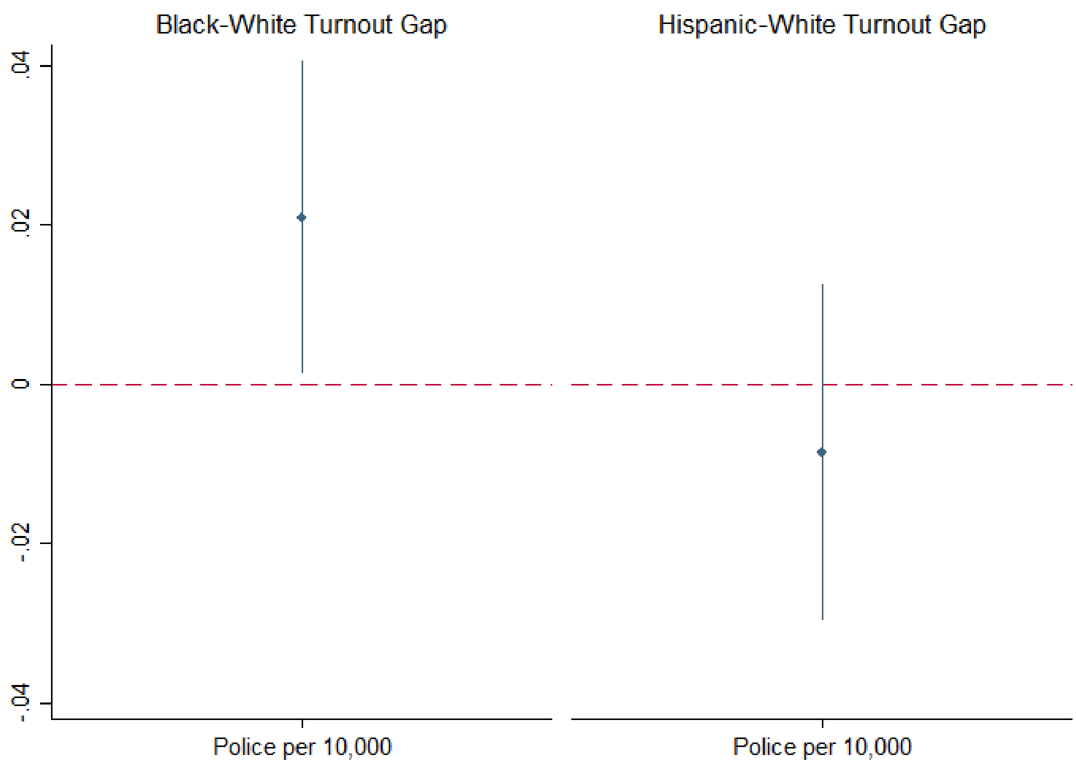

Figure 3. Triple difference estimates of the effect of police on Black-White and Hispanic-White turnout gaps. See Table 2E.

[14] Prior work has used CHP disbursement as an instrument for violence (Sharkey and Torrats-Espinosa 2017).
[15] I limit this analysis only to municipalities that have registered Black voters, so these point estimates slightly differ from the estimated effects reported in Tables 5A and 4E, which use the entire sample.

Table 2. Estimates of Effects of Violence on Turnout

	(1) Violent Crime	(2) Murder	(3) Black Turnout	(4) Black Turnout	(5) B-W Gap	(6) B-W Gap
Funded x Post	-3.927**	-0.196***				
	(1.599)	(0.0544)				
Violent per 10,000			-0.00552*		-0.00490*	
			(0.00318)		(0.00287)	
Murder per 10,000				-0.110**		-0.0981**
				(0.0547)		(0.0499)
F-Stat	6.03	13.04	-	-	-	-
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations (City-Years)	20526	20526	20526	20526	20526	20526

Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$. Covariates include county-level unemployment, log per capita income, percent Black, percent Hispanic, and percent young male. Columns 1 and 2 show the first stage relationship between CHP disbursement and the rate of overall violent crime and homicide, respectively. Columns 3-6 present DDIV estimates of the effect of an additional violent crime or homicide per 10,000 on Black turnout (Columns 3 and 4) and the Black-White turnout gap (Columns 5 and 6). For full results, see Table 5E.

Still, it may be the case that the police-turnout relationship can be attributed to changes in police-civilian contacts. To address this concern, I estimate the effect of police on index arrests. It is worth noting here that the effect on arrests can only be estimated using a subsample of applicant municipalities that report arrests to the FBI's UCR program, and as in Table 2, I also limit the analysis to municipalities with registered Black voters. Columns 1 and 2 in Table 3 report DDIV estimates of the effect of police on arrests for violent and property arrests. The point estimates are negative but statistically indistinguishable from zero, suggesting that there is no effect of police on arrests for index crimes. In other words, the CHP is, at best, a weak instrument for arrests. Indeed, the F-statistics for the first-stage regressions reported in columns 3 and 4 (2.27 and .26, respectively) show that this is the case. In this context, using the CHP as an instrument for arrests would provide an inconsistent estimate of the effect of arrests on turnout. Overall, then, I do not have evidence that police-civilian interactions mediate the effect of police on Black turnout. Still, it remains possible that interactions not measured here (e.g., traffic stops, police questioning, or misdemeanor arrests) can account for some of the police-turnout relationship, but this is beyond the limits of this paper.[16]

Finally, I test whether the main results or the estimated effects of violence on Black turnout are robust to the inclusion of non-CHP spending in the ARRA. After all, it may be the case that localities that received CHP funds also received additional federal aid from other sources, and it is this non-CHP spending that accounts for the results presented here. Tables 1C and 2C show that, even after adjusting for log per capita ARRA spending, the effects of an additional police hire and homicide remain significant.

Overall, these results provide strong evidence that crime conditions the relationship

[16] In a separate analysis, I also show that CHP disbursement has no relationship with the number of police killings in my sample. These results are available upon request.

between policing institutions and voter behavior. Thus, not only does incorporating crime into existing theoretical frameworks produce theoretical predictions distinct from those in existing literature, but this study also shows that voters do, in fact, respond to changes in criminal violence differently than they do to police-civilian interactions.

Table 3. Estimates of Effects of Police and CHP on Arrests

	(1) Violent Arrests	(2) Property Arrests	(3) Violent Arrests	(4) Property Arrests
Police per 10,000	-1.146 (0.815)	-1.090 (2.155)		
High x Post			-1.102 (0.731)	-1.049 (2.061)
Controls	Yes	Yes	Yes	Yes
F-Stat	-	-	2.27	.26
Observations (City-Years)	18570	18570	18570	18570

Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$ Covariates include county-level unemployment, log per capita income, percent Black, percent Hispanic, and percent young male. Columns 1 and 2 present DDIV estimates of the effect of police on index arrests for violent and property crimes, respectively. Columns 3 and 4 show the first stage relationship between CHP disbursement and the rate of violent and property arrests, respectively. See Table 6E.

Robustness: Proximity to the Cutoff and Race-Gender Subgroups

To address the possibility that the results presented above are merely an artifact of time-varying unobserved differences between treated and untreated municipalities, I conduct the main analysis using subsets of the sample that vary in their proximity to the funding cutoff. Leveraging standardized application scores provided by Mello (2019), I estimate a reduced form equation using only cities that fall within varying bandwidths from the funding cutoff.

Figure 4 shows the estimates from this analysis for all racial groups. Bandwidth estimates include all municipalities that fall within the specified bandwidth. Bandwidths closer to zero on the x-axis represent estimates using only municipalities close to the funding cutoff, for which random assignment to the CHP grant is most plausible. If the main results are due to underlying differences unrelated to grant disbursement, then the reduced form coefficients should be close to

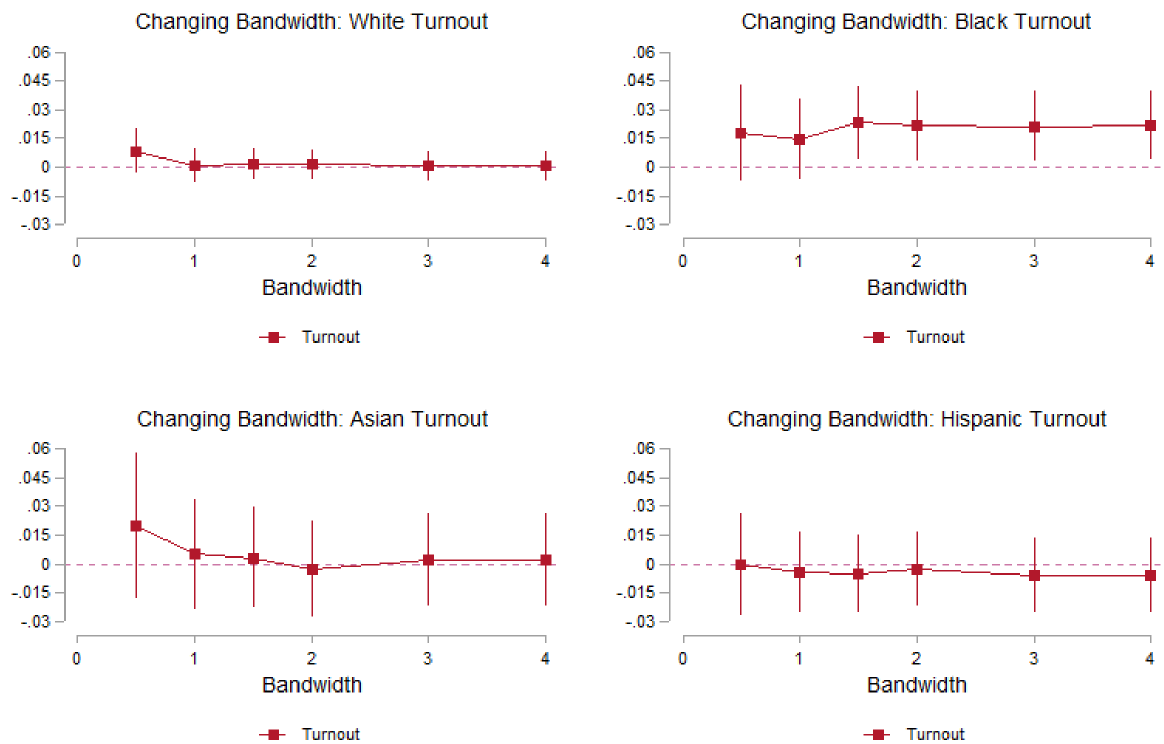

Figure 4. Estimated effects of police on turnout by proximity to the funding cutoff. In each panel, coefficients are plotted for subsamples of municipalities. The x-axis denotes standard deviations from the funding cut-off. Scores closer to zero represent cities that fell closer to the funding cutoff. See Tables 7E-10E for regression estimates.

zero for the narrowest bandwidths and grow only as I include municipalities with scores further from the funding cutoff. Conversely, if the estimated coefficients are positive when using the narrowest bandwidths, then changes in turnout can be more readily attributed to grant disbursement.

The point estimates for all four groups are remarkably consistent across all bandwidths, though estimates from narrower bandwidths are noisier due to smaller sample sizes. In the case of Black turnout, the estimated coefficient using the narrowest bandwidth (n = 904) is .017 and .021 when using the full sample. For White and Asian turnout, the point estimate is positive for the narrowest bandwidth (.008 and .02, respectively), but these estimates fail to achieve conventional levels of statistical significance.

Importantly, a weakly significant positive coefficient for the narrowest bandwidth for White turnout implies that the Black-White turnout gap is *not* narrowed by the CHP, as both

Black and White turnout are moving in tandem in response to CHP receipt. Indeed, when conducting the same analysis using the Black-White turnout gap as the dependent variable, the reduced form estimates are positive and statistically significant only when using wider bandwidths that include cities further from the cutoff (see Figure 2B). This suggests that the narrowing of the Black-White turnout gap shown above in Figure 3 is due to unobserved differences between funded and unfunded municipalities, not CHP disbursement.

However, this analysis masks significant heterogeneity in political responses to police. Turning our attention to the intersection of race and gender, I replicate the bandwidth analysis using the Black-White turnout gap among men and women voters, separately. Figure 5 plots the results. As can be seen in the right panel in Figure 5, the Black-White turnout gap persists among women in municipalities nearest the funding cutoff, but within this same group of municipalities, the disparity narrows among men by approximately 3 percentage points. These findings are consistent with H3 that states that Black men should be particularly sensitive to police hires.

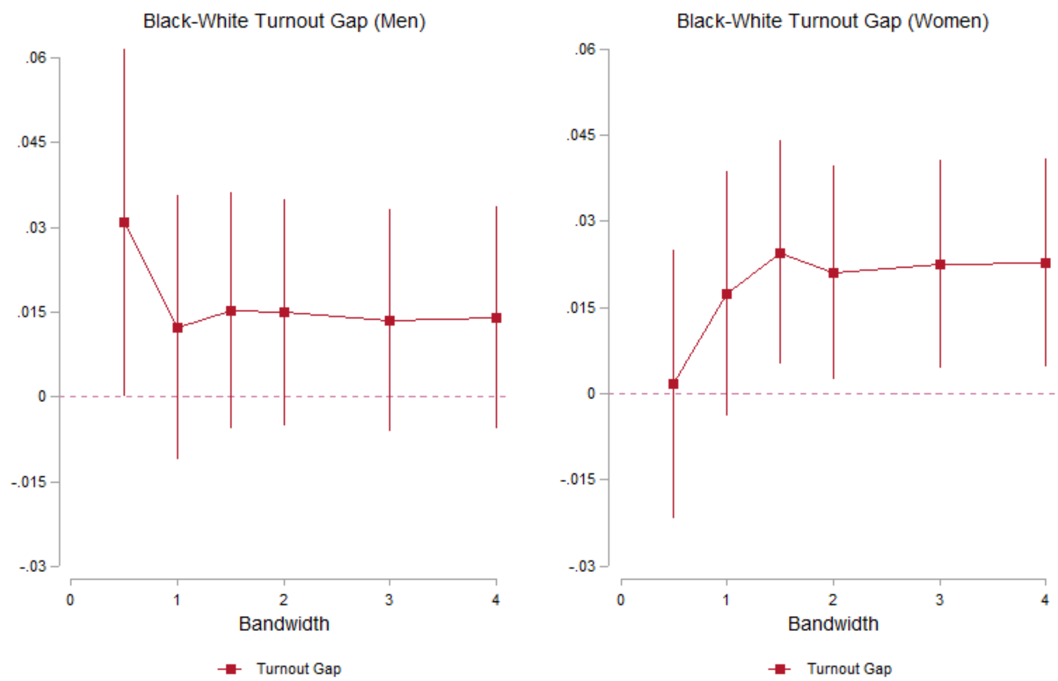

Figure 5. Estimated effects of police on turnout by proximity to the funding cutoff. In each panel, coefficients from reduced form equations are plotted for the Black-White turnout gap corresponding to each gender. The x-axis denotes standard deviations from the funding cut-off. Scores closer to zero represent cities that fell closer to the funding cutoff. See Tables 13E and 14E.

To further examine these results, I re-estimate the reduced form equation using only municipalities near the funding cutoff (within 0.5 standard deviations) for each of the four race-gender groups. The results are plotted in Figure 6.

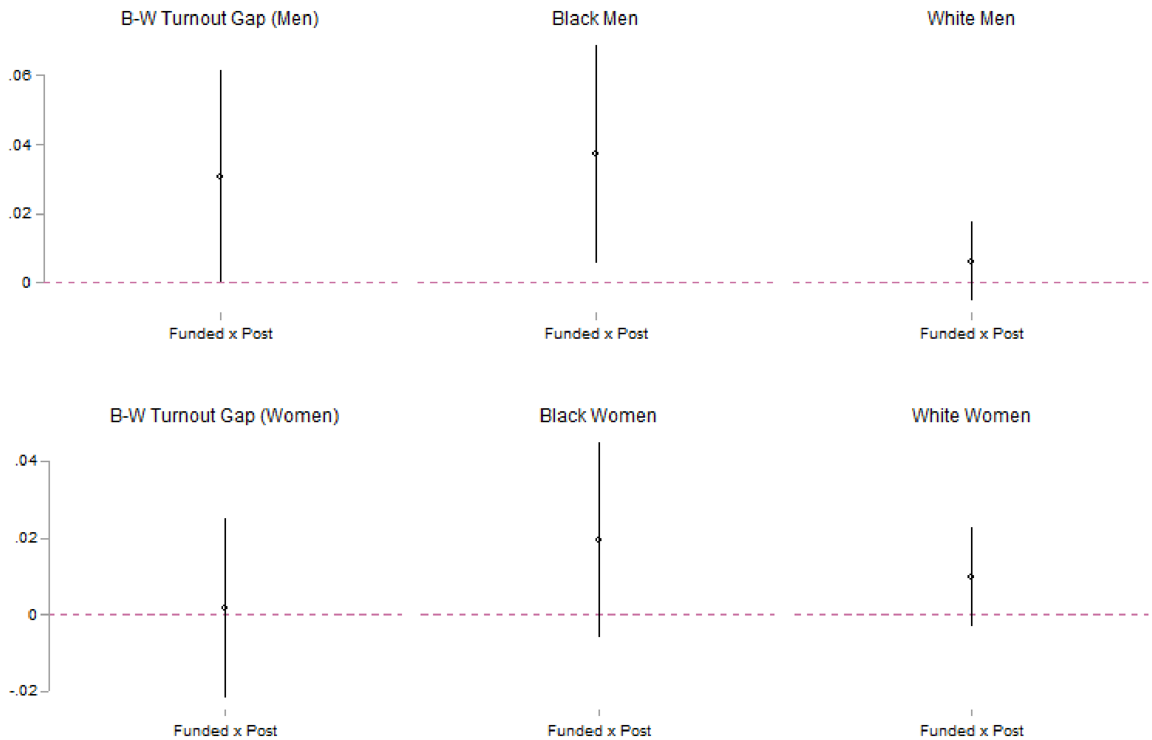

Figure 6. Reduced form estimates of the effect of police on race-gender groups in municipalities within 0.5 standard deviations of the funding cut-off. The top panel shows the estimated effects of police on the Black-White turnout gap among men and male turnout by race (Black and White). The bottom panel shows the corresponding estimates for women voters. See Table 15E.

As expected, the coefficient for Black male turnout is positive. Thus, the narrowing of the Black-White turnout gap is due to higher rates of turnout among Black male voters. There is no statistically significant effect for White men, White women, or Black women. When limiting the analysis to municipalities most plausibly randomly assigned to receive federal funding for local police, only Black male voters are responding to changes in police numbers.

6 Discussion and Conclusion

Using a natural experiment, I provide evidence that police can have profound effects on the civic engagement of Americans. I find that additional police have a mobilizing effect on Black turnout. In addition, I find that political mobilization among Black voters is a downstream consequence of lower rates of criminal violence. Finally, I find that in municipalities most plausibly randomly assigned to a federal hiring grant that subsidized municipal police hires, turnout among Black men increases. In short, as police departments become better staffed, they can more capably meet the needs of their jurisdiction, which in turn, facilitates the political engagement of those most exposed to crime.

An important contribution of this paper is that it highlights how crime influences the way police shape the political lives of Americans. In standard theories of the policy feedback of criminal-justice institutions, increasing police surveillance is likely to deteriorate the political capacities of subjugated communities that are routinely in contact with policing authorities. These theories predict that additional police demobilize potential minority voters, as adversarial encounters push this group out of politics. In contrast, this study empirically tests the effects of police on registered voters, a group of individuals that is not necessarily exposed to routine encounters with police but may still discern the consequences of crime in their community for their own livelihoods. Incorporating crime into the analysis reveals that the political consequences of police are more complicated than suggested by extant studies. My findings highlight that there is likely significant intra-racial variation in political responses to police.

A limitation of the present study is that it does not consider all members residing in a municipality, as not all individuals appear in standard administrative voter datasets. Thus, it remains possible that significant heterogeneity in political responses exists within racial groups that are unobserved in state voter files. As we are better able to identify non-registered individuals with personal experiences within criminal-justice institutions (Doleac et al. 2023), we will be better positioned to understand potential underlying heterogeneity in responses to police. Indeed, it may be the case that justice-impacted individuals are (de-)mobilized in ways consistent with extant studies while politically engaged individuals are mobilized by reductions in crime.

Finally, this paper has practical implications for policymakers across the country. In recent years, police agencies across the country have reported difficulties in recruiting or maintaining the size of their police forces. To address this staffing shortage, agencies have offered generous hiring bonuses, changed recruiting standards, and some have even attempted to recruit officers from other departments (Klemko 2023). Although law enforcement officials express concern that this shortage will leave agencies ill-equipped to meet the crime-need of their service areas, some advocates of policing reform see this as an opportunity to shift funds towards alternatives to traditional policing. As local policymakers debate the appropriate level of police for their communities, they will weigh a number of factors they believe will be most affected by policing services. However, local leaders are unlikely to consider the consequences changing police numbers may have on constituents' ability to express their political voice. That is, in choosing to hire more or fewer police, municipalities may unintentionally affect who decides to vote in their communities.

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ONLINE APPENDIX

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Appendix A. Tables.

Appendix A contains tables that directly correspond to figures or estimates mentioned in the main text. Regression tables in this section are abbreviated. For full regression tables, see Appendix E**.**

Table 1A. Characteristics by Treatment Status and (Non-)Missingness

	CHP Funds (Included)	CHP Funds (Missing)	No Funds (Included)	No Funds (Missing)	Total
Population (Ten Thousands)	7.547	3.192	2.787	0.884	3.295
	(22.26)	(17.32)	(16.70)	(2.820)	(16.74)
Unemployment Rate	9.493	9.961	6.926	7.227	7.447
	(3.800)	(5.304)	(2.986)	(3.737)	(3.454)
Family Income (Ten Thousands)	4.026	3.510	5.462	4.699	5.083
	(1.081)	(1.219)	(2.159)	(2.075)	(2.082)
Percent Black	20.79	20.56	7.838	7.331	10.13
	(22.03)	(25.71)	(12.21)	(13.22)	(15.59)
Percent Hispanic	15.49	13.14	10.33	8.632	10.99
	(20.49)	(21.88)	(14.81)	(15.41)	(16.25)
Percent Young Male	23.73	22.27	21.62	21.50	21.95
	(5.570)	(7.564)	(6.708)	(7.831)	(6.773)
Police Per 10,000	25.47	30.49	22.03	25.97	23.32
	(10.87)	(10.45)	(10.56)	(13.78)	(11.28)
Violent Crimes Per 10,000	91.05	108.1	55.07	65.52	63.47
	(49.58)	(58.02)	(42.09)	(42.61)	(46.24)
Property Crimes Per 10,000	499.3	484.8	268.4	264.2	309.7
	(222.9)	(262.4)	(159.2)	(175.3)	(197.1)
Officers Funded Per 10,000	1.525	2.739	0	0	0.307
	(1.441)	(2.162)	(0)	(0)	(0.943)
Funding Per Capita	27.76	42.32	0	0	5.411
	(21.81)	(32.07)	(0)	(0)	(15.32)
N	691	100	2941	595	4327

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Table 2A. Main DDIV Results

	(1) Police	(2) Black	(3) White	(4) Hispanic	(5) Asian
Funded x Post	0.957*** (0.191)				
Police per 10,000		0.0235** (0.0110)	0.000554 (0.00406)	-0.00670 (0.0115)	0.00339 (0.0189)
F-Stat	25.04	-	-	-	-
Controls	Yes	Yes	Yes	Yes	Yes
Observations (City-Years)	21792	20526	21762	20556	17406

Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$. Column 1 shows the first stage relationship between CHP disbursement and the rate of sworn police. Columns 2-5 present DDIV estimates of the effect of an additional police hire on aggregate turnout for each racial group. Covariates include county-level unemployment, log per capita income, percent Black, percent Hispanic, and percent young male. These results are reported in Figure 2 of the main text. See Table 1E for full regression results.

Table 3A. Main OLS Results

	(1) OLS Black	(2) OLS: White	(3) OLS: Hispanic	(4) OLS: Asian
Police per 10,000	0.000498 (0.000616)	-0.0000238 (0.000271)	-0.0000975 (0.000811)	-0.0000522 (0.00120)
Controls	Yes	Yes	Yes	Yes
Observations (City-Years)	20526	21762	20556	17406

Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$. Covariates include county-level unemployment, log per capita income, percent Black, percent Hispanic, and percent young male. See Table 3E for full regression results.

Table 4A. Triple Difference Results

	(1) Black-White Gap	(2) Hispanic-White Gap
Police per 10,000	0.0209** (0.0100)	-0.00868 (0.0108)
Controls	Yes	Yes
Observations (City-Years)	20526	20550

Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$. Covariates include county-level unemployment, log per capita income, percent Black, percent Hispanic, and percent young male. These results are reported in Figure 3. See Table 2E for full regression results.

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Table 5A. Estimates of Police Effect on Violent Crime.

	(1) Violent Crime	(2) Agg Assault	(3) Robbery	(4) Rape	(5) Murder
Police per 10,000	-3.639*	0.170	-2.711***	-0.436**	-0.216***
	(1.860)	(1.504)	(0.678)	(0.222)	(0.0704)
Observations	21792	21792	21792	21792	21792

Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$. Covariates include county-level unemployment, log per capita income, percent Black, percent Hispanic, and percent young male. This table presents DDIV estimates of the effect of an additional police hire on overall violent crime and each violent crime category. See Table 4E for full regression results.

Appendix B. Figures.

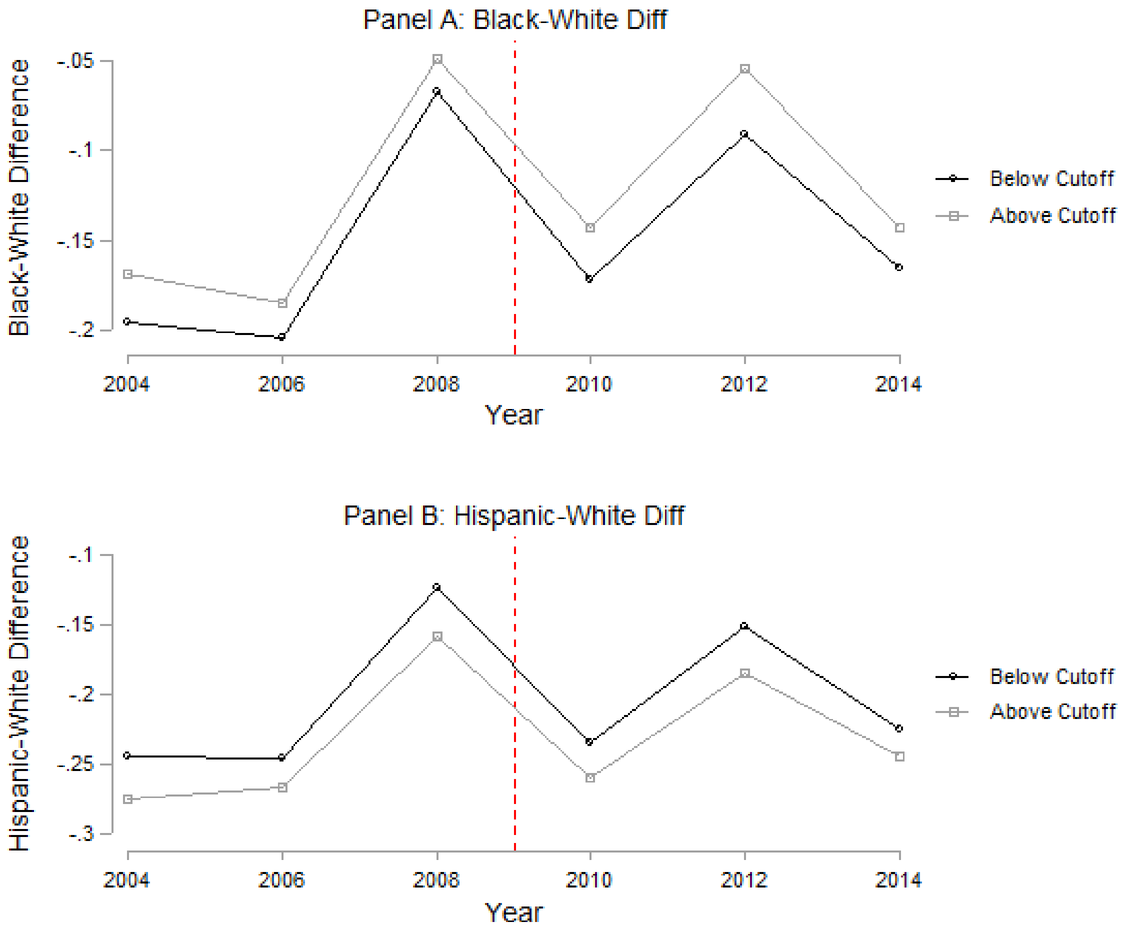

Figure 1B. Times trends in turnout gaps between minority and White voters across municipalities. Treated municipalities are denoted in gray.

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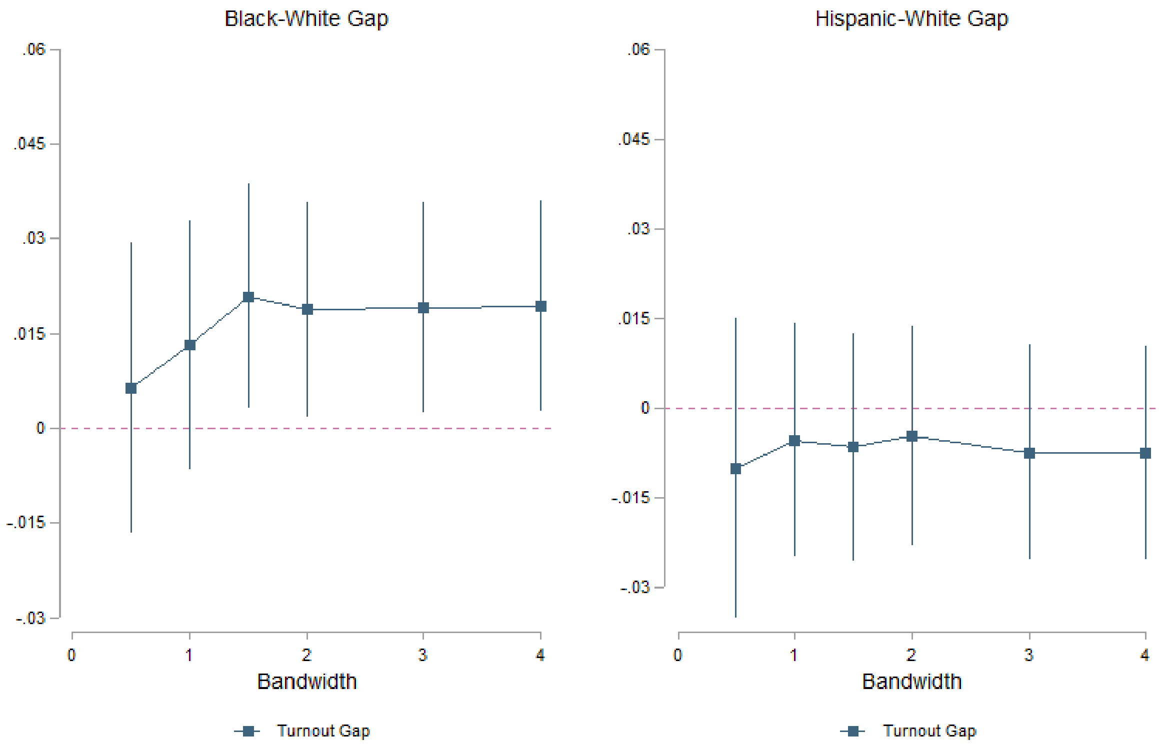

Figure 2B. Estimated effects of police on Black-White and Hispanic-White turnout gaps by proximity to the funding cutoff. In each panel, coefficients are plotted for subsamples of municipalities. The x-axis denotes standard deviations from the funding cut-off. Scores closer to zero represent cities that fell closer to the funding cutoff. See Tables 11E & 12E for full regression results.

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Appendix C. Additional Robustness Check.

Because the CHP grant was allocated, in part, due to local economic conditions, it may be the case that municipalities that received COPS funding, also received other forms of federal aid. This additional federal aid could in effect serve as a time-varying confounder that could account for the results presented in this manuscript. To address this concern, I use additional data provided by Mello (2019) that contains information on non-Department of Justice ARRA spending allocated to localities. Spending data was first attributed to individual zip codes, which were then aggregated to the municipal level; however, this data is not available for the full sample. Tables 1C and 2C provide estimates for the main relationships explored in this paper for the full sample and for the subset of municipalities for which data on ARRA spending is available.

Table 1C provides evidence that even after adjusting for log per capita ARRA spending, the reported DDIV effects on Black turnout and the Black-White turnout gap remain statistically significant. Similarly, Table 2C provides evidence that ARRA spending cannot account for the reported effects of homicide on Black turnout or the Black-White turnout gap.

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Table 1C. Adjusting for ARRA Funds

	(1) Black Turnout (Full)	(2) Black Turnout (ARRA)	(3) Black Turnout (ARRA)	(4) B-W Gap (Full)	(5) B-W Gap (ARRA)	(6) B-W Gap (ARRA)
Police per 10,000	0.0235**	0.0311**	0.0326**	0.0209**	0.0251**	0.0257**
	(0.0110)	(0.0125)	(0.0130)	(0.0100)	(0.0112)	(0.0116)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
ARRA Funds	No	No	Yes	No	No	Yes
Cities	3632	2733	2733	3632	2733	2733
Observations (City-Years)	20526	15498	15498	20526	15498	15498

Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$. Covariates include county-level unemployment, log per capita income, percent Black, percent Hispanic, and percent young male. All columns present DDIV estimates. Columns 1-3 present estimates of the police-turnout relationship for the full sample, the ARRA sample, and the ARRA sample after adjusting for the log of ARRA dollars, respectively. Columns 4-6 present the corresponding estimates for the police-turnout gap relationship.

Table 2C. Adjusting for ARRA Funds

	(1) Black Turnout (Full)	(2) Black Turnout (ARRA)	(3) Black Turnout (ARRA)	(4) B-W Gap (Full)	(5) B-W Gap (ARRA)	(6) B-W Gap (ARRA)
Murder per 10,000	-0.110**	-0.144**	-0.145**	-0.0981**	-0.116**	-0.114**
	(0.0547)	(0.0634)	(0.0627)	(0.0499)	(0.0562)	(0.0552)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
ARRA Funds	No	No	Yes	No	No	Yes
Cities	3632	2733	2733	3632	2733	2733
Observations (City-Years)	20526	15498	15498	20526	15498	15498

Standard errors in parentheses * $p < .1$, ** $p < .05$, *** $p < .01$. Covariates include county-level unemployment, log per capita income, percent Black, percent Hispanic, and percent young male. Columns 1-3 present estimates of the police-turnout relationship for the full sample, the ARRA sample, and the ARRA sample after adjusting for the log of ARRA dollars, respectively. Columns 4-6 present the corresponding estimates for the police-turnout gap relationship.

Appendix D. Heterogeneity.

The exposure of localities to the Great Recession varied significantly. Therefore, the CHP may have made a more meaningful difference in the size of the police force in cities and towns that were relatively more exposed to the economic downturn. Consequently, turnout in these same localities should have been the most affected by CHP disbursement. In an exploratory analysis, I test whether the changes in the Black-White turnout gap were larger in municipalities with more exposure to the Great Recession. Following Mello (2019), I use two approaches. First, I separate municipalities into quintiles based on their change in unemployment from 2007 to 2009. I then interacted the quintiles with the CHP instrument. The second approach interacts the change in unemployment with the instrument. Figure 1D shows the results. Consistent with expectations, places with a larger change in unemployment experienced relatively larger increases in police and a larger narrowing of the Black-White turnout gap.

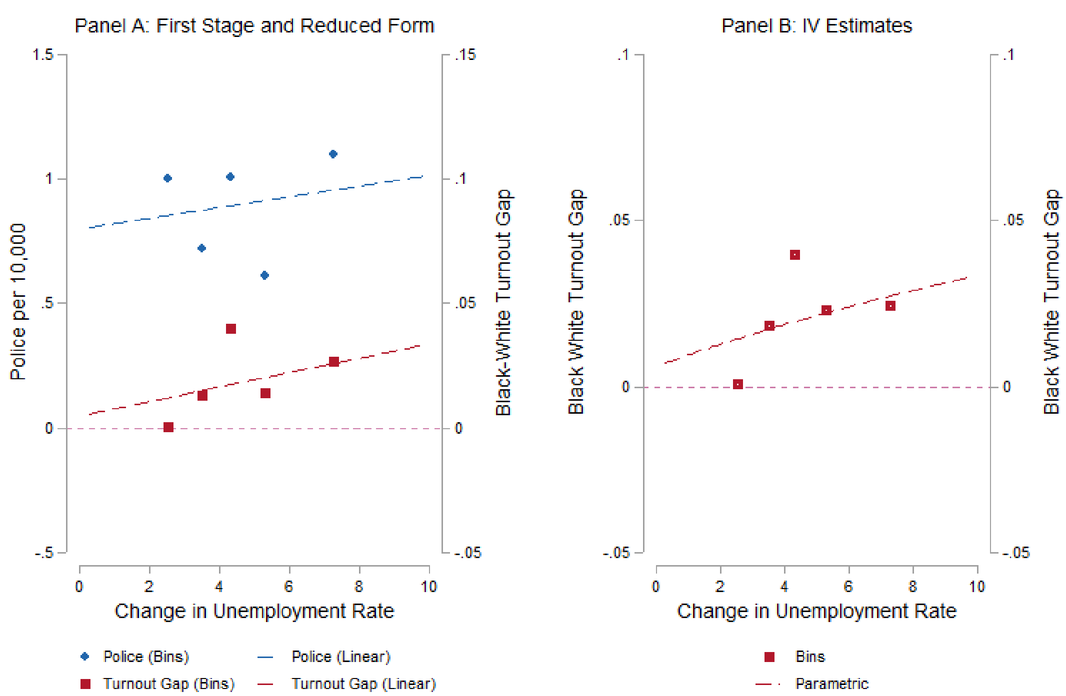

Figure 1D. Heterogeneous effects by change in unemployment.

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Appendix E. Full Regression Tables with Covariates.

Table 1E. Main DDIV Results

	(1) Police	(2) IV: Black	(3) IV: White	(4) IV: Hispanic	(5) IV: Asian
Funded x Post	0.957***				
	(0.191)				
Police per 10,000		0.0235**	0.000554	-0.00670	0.00339
		(0.0110)	(0.00406)	(0.0115)	(0.0189)
Log Per Capita Income	0.161	-0.120**	-0.0239	0.00847	-0.155**
	(0.808)	(0.0483)	(0.0211)	(0.0431)	(0.0697)
Unemployment	-0.0531*	0.00486***	0.00171**	0.00666***	0.000662
	(0.0292)	(0.00174)	(0.000677)	(0.00173)	(0.00237)
Percent Black	10.59	-1.653**	-1.721***	-0.618	-1.730*
	(17.96)	(0.668)	(0.325)	(0.706)	(0.913)
Percent Hispanic	33.87**	-0.677	0.705*	1.753**	-0.716
	(13.31)	(0.767)	(0.365)	(0.743)	(1.173)
Percent Young Male	7.400	1.447	0.669*	0.591	0.532
	(16.68)	(0.905)	(0.400)	(0.910)	(1.396)
F-Stat	25.04	-	-	-	-
Observations (City-Years)	21792	20526	21762	20556	17406

Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

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Table 2E. Triple Difference: Turnout Gap Results

	(1) Black-White Gap	(2) Hispanic-White Gap
Police per 10,000	0.0209**	-0.00868
	(0.0100)	(0.0108)
Log Per Capita Income	-0.0829*	0.0234
	(0.0447)	(0.0431)
Unemployment	0.00322**	0.00466***
	(0.00163)	(0.00169)
Percent Black	0.0902	0.997
	(0.568)	(0.670)
Percent Hispanic	-1.645**	1.129*
	(0.728)	(0.605)
Percent Young Male	0.720	-0.0213
	(0.848)	(0.854)
Observations (City-Years)	20526	20550

Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 3E. Main OLS Results

	(1) OLS: Black	(2) OLS: White	(3) OLS: Hispanic	(4) OLS: Asian
Police per 10,000	0.000498	-0.0000238	-0.0000975	-0.0000522
	(0.000616)	(0.000271)	(0.000811)	(0.00120)
Log Per Capita Income	-0.0966**	-0.0237	0.00750	-0.152**
	(0.0410)	(0.0209)	(0.0428)	(0.0672)
Unemployment	0.00407**	0.00169**	0.00692***	0.000621
	(0.00159)	(0.000661)	(0.00168)	(0.00237)
Percent Black	-1.416***	-1.715***	-0.673	-1.723*
	(0.515)	(0.321)	(0.686)	(0.914)
Percent Hispanic	0.149	0.724**	1.541**	-0.565
	(0.579)	(0.352)	(0.626)	(0.881)
Percent Young Male	1.784**	0.673*	0.563	0.557
	(0.798)	(0.400)	(0.895)	(1.392)
Constant	1.486***	0.963***	0.188	2.298***
	(0.463)	(0.234)	(0.475)	(0.747)
Observations (City-Years)	20526	21762	20556	17406

Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 4E. Estimates of Police Effect on Violent Crimes

	(1) Violent Crime	(2) Agg Assault	(3) Robbery	(4) Rape	(5) Murder
Police per 10,000	-3.639*	0.170	-2.711***	-0.436**	-0.216***
	(1.860)	(1.504)	(0.678)	(0.222)	(0.0704)
Log Per Capita Income	6.460	5.356	0.541	0.954	-0.0358
	(7.640)	(6.646)	(2.369)	(0.820)	(0.267)
Unemployment	-0.398	-0.203	-0.108	-0.0539	-0.0256**
	(0.309)	(0.268)	(0.0961)	(0.0341)	(0.0103)
Percent Black	209.6	72.86	108.0*	12.60	4.080
	(153.0)	(133.2)	(56.58)	(16.02)	(5.769)
Percent Hispanic	36.10	-141.2	129.4***	24.24	5.370
	(119.2)	(98.28)	(44.56)	(15.81)	(6.701)
Percent Young Male	51.05	1.271	57.49	-2.476	-0.0903
	(159.2)	(133.5)	(52.23)	(17.48)	(7.899)
Observations	21792	21792	21792	21792	21792

Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

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Table 5E. Estimates of Effects of Violence on Turnout

	(1) Violent Crime	(2) Murder	(3) Black Turnout	(4) Black Turnout	(5) B-W Gap	(6) B-W Gap
Funded x Post	-3.927**	-0.196***				
	(1.599)	(0.0544)				
Violent per 10,000			-0.00552*		-0.00490*	
			(0.00318)		(0.00287)	
Murder per 10,000				-0.110**		-0.0981**
				(0.0547)		(0.0499)
Log Per Capita Income	5.435	-0.0418	-0.0719	-0.107**	-0.0402	-0.0709
	(7.145)	(0.219)	(0.0598)	(0.0496)	(0.0537)	(0.0444)
Unemployment	-0.0993	-0.0135*	0.00317	0.00223	0.00172	0.000883
	(0.286)	(0.00767)	(0.00235)	(0.00212)	(0.00217)	(0.00196)
Percent Black	198.0	2.732	-0.317	-1.108	1.278	0.575
	(145.2)	(4.303)	(1.042)	(0.755)	(0.953)	(0.610)
Percent Hispanic	-74.98	0.869	-0.217	0.293	-1.236	-0.783
	(102.5)	(3.305)	(0.850)	(0.696)	(0.791)	(0.632)
Percent Young Male	9.775	3.778	1.851	2.214**	1.079	1.401
	(142.2)	(5.136)	(1.126)	(1.039)	(1.014)	(0.917)
F-Stat	6.03	13.04	-	-	-	-
Observations (City-Years)	20526	20526	20526	20526	20526	20526

Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

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Table 6E. Estimates of Effects of Police and CHP on Arrests

	(1) Violent Arrests	(2) Property Arrests	(3) Violent Arrests	(4) Property Arrests
Police per 10,000	-1.146	-1.090		
	(0.815)	(2.155)		
Funded x Post			-1.102	-1.049
			(0.731)	(2.061)
Log Per Capita Income	4.030	-6.982	3.510	-7.477
	(3.105)	(8.217)	(2.901)	(7.974)
Unemployment	-0.189	0.522*	-0.140	0.568*
	(0.118)	(0.311)	(0.106)	(0.310)
Percent Black	37.29	162.2	22.79	148.4
	(73.25)	(199.1)	(69.71)	(196.2)
Percent Hispanic	-33.43	-118.1	-64.20	-147.3
	(47.05)	(127.3)	(39.63)	(114.6)
Percent Young Male	-15.71	110.5	-34.22	92.90
	(60.99)	(182.0)	(56.21)	(179.3)
Controls	Yes	Yes	Yes	Yes
F-Stat	-	-	2.27	.26
Observations (City-Years)	18570	18570	18570	18570

Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 7E. Bandwidth Estimates: White Turnout

	(1)	(2)	(3)	(4)	(5)	(6)
	0.5	1	1.5	2	3	4
Funded x Post	0.00834	0.000461	0.00176	0.00148	0.000496	0.000532
	(0.00596)	(0.00452)	(0.00412)	(0.00393)	(0.00390)	(0.00389)
Log Per Capita Income	-0.108**	-0.0840***	-0.0477**	-0.0397*	-0.0252	-0.0238
	(0.0450)	(0.0287)	(0.0241)	(0.0220)	(0.0211)	(0.0210)
Unemployment	0.000280	0.00119	0.00174**	0.00177**	0.00169**	0.00168**
	(0.00119)	(0.000903)	(0.000769)	(0.000707)	(0.000666)	(0.000664)
Percent Black	-1.930***	-1.590***	-1.634***	-1.598***	-1.721***	-1.715***
	(0.590)	(0.405)	(0.360)	(0.319)	(0.322)	(0.321)
Percent Hispanic	0.346	0.539	0.376	0.482	0.719**	0.724**
	(0.522)	(0.361)	(0.369)	(0.346)	(0.354)	(0.353)
Percent Young Male	1.045	0.813	0.672	0.711*	0.669*	0.673*
	(0.699)	(0.523)	(0.451)	(0.408)	(0.400)	(0.400)
Observations	5646	10662	15066	18354	21612	21762

Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$. Column name corresponds to bandwidth used. For instance, estimates under 0.5 are derived from a reduced form equation using only units 0.5 standard deviations from the funding cutoff.

Table 8E. Bandwidth Estimates: Black Turnout

	(1) 0.5	(2) 1	(3) 1.5	(4) 2	(5) 3	(6) 4
Funded x Post	0.0175	0.0143	0.0231**	0.0214**	0.0214**	0.0217**
	(0.0128)	(0.0107)	(0.00980)	(0.00940)	(0.00918)	(0.00918)
Log Per Capita Income	-0.0751	-0.0973**	-0.109**	-0.113***	-0.101**	-0.102**
	(0.0629)	(0.0494)	(0.0444)	(0.0430)	(0.0412)	(0.0410)
Unemployment	0.00307	0.00331*	0.00386**	0.00369**	0.00377**	0.00372**
	(0.00262)	(0.00199)	(0.00178)	(0.00170)	(0.00163)	(0.00162)
Percent Black	-1.612**	-1.421**	-1.612***	-1.559***	-1.426***	-1.409***
	(0.802)	(0.593)	(0.556)	(0.511)	(0.516)	(0.515)
Percent Hispanic	0.765	0.209	0.235	-0.115	0.201	0.197
	(0.867)	(0.637)	(0.566)	(0.595)	(0.582)	(0.580)
Percent Young Male	0.835	1.857*	1.773**	1.966**	1.768**	1.797**
	(1.084)	(0.954)	(0.897)	(0.830)	(0.798)	(0.798)
Observations	5424	10200	14310	17328	20382	20526

Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$. Column name corresponds to bandwidth used. For instance, estimates under 0.5 are derived from a reduced form equation using only units 0.5 standard deviations from the funding cutoff.

Table 9E. Bandwidth Estimates: Hispanic Turnout

	(1) 0.5	(2) 1	(3) 1.5	(4) 2	(5) 3	(6) 4
Funded x Post	-0.000265	-0.00444	-0.00478	-0.00258	-0.00579	-0.00576
	(0.0134)	(0.0106)	(0.0103)	(0.00997)	(0.00978)	(0.00978)
Log Per Capita Income	-0.0564	-0.0535	-0.0243	-0.00706	0.00423	0.00890
	(0.0539)	(0.0470)	(0.0480)	(0.0458)	(0.0430)	(0.0429)
Unemployment	0.00644***	0.00776***	0.00828***	0.00787***	0.00699***	0.00702***
	(0.00242)	(0.00198)	(0.00194)	(0.00179)	(0.00171)	(0.00171)
Percent Black	-0.975	-0.799	-0.861	-0.679	-0.667	-0.673
	(0.900)	(0.741)	(0.676)	(0.706)	(0.686)	(0.686)
Percent Hispanic	1.781**	2.175***	1.791**	1.579**	1.568**	1.530**
	(0.703)	(0.547)	(0.712)	(0.669)	(0.628)	(0.623)
Percent Young Male	0.508	0.0822	-0.319	0.640	0.636	0.562
	(1.097)	(0.994)	(0.963)	(0.929)	(0.894)	(0.894)
Observations	5406	10152	14292	17364	20406	20556

Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$ Column name corresponds to bandwidth used. For instance, estimates under 0.5 are derived from a reduced form equation using only units 0.5 standard deviations from the funding cutoff.

Table 10E. Bandwidth Estimates: Asian Turnout

	(1) 0.5	(2) 1	(3) 1.5	(4) 2	(5) 3	(6) 4
Funded x Post	0.0202	0.00511	0.00306	-0.00262	0.00201	0.00225
	(0.0193)	(0.0147)	(0.0133)	(0.0128)	(0.0124)	(0.0124)
Log Per Capita Income	-0.345**	-0.271***	-0.172**	-0.161**	-0.152**	-0.153**
	(0.137)	(0.0932)	(0.0784)	(0.0730)	(0.0680)	(0.0675)
Unemployment	0.00164	0.000823	0.000651	0.000965	0.000605	0.000581
	(0.00385)	(0.00307)	(0.00272)	(0.00256)	(0.00241)	(0.00240)
Percent Black	-1.923	-2.668**	-2.036**	-1.646*	-1.712*	-1.723*
	(1.269)	(1.133)	(0.986)	(0.940)	(0.915)	(0.914)
Percent Hispanic	-2.267	-1.270	-1.151	-0.917	-0.523	-0.564
	(1.385)	(0.997)	(0.923)	(0.861)	(0.878)	(0.877)
Percent Young Male	0.678	-1.013	0.0756	0.864	0.537	0.558
	(1.694)	(1.647)	(1.534)	(1.411)	(1.394)	(1.392)
Observations	4752	8868	12162	14700	17286	17406

Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$. Column name corresponds to bandwidth used. For instance, estimates under 0.5 are derived from a reduced form equation using only units 0.5 standard deviations from the funding cutoff.

Table 11E. Bandwidth Estimates: Black-White Turnout Gap

	(1)	(2)	(3)	(4)	(5)	(6)
	0.5	1	1.5	2	3	4
Funded x Post	0.00637	0.0132	0.0209**	0.0187**	0.0190**	0.0193**
	(0.0117)	(0.0100)	(0.00908)	(0.00869)	(0.00846)	(0.00846)
Log Per Capita Income	0.0285	-0.0157	-0.0541	-0.0711*	-0.0639*	-0.0668*
	(0.0506)	(0.0439)	(0.0419)	(0.0399)	(0.0386)	(0.0384)
Unemployment	0.00302	0.00229	0.00228	0.00218	0.00226	0.00221
	(0.00239)	(0.00183)	(0.00166)	(0.00161)	(0.00154)	(0.00154)
Percent Black	0.408	0.199	0.0738	0.0861	0.298	0.307
	(0.493)	(0.415)	(0.418)	(0.388)	(0.414)	(0.414)
Percent Hispanic	0.229	-0.533	-0.574	-1.034*	-0.866	-0.869
	(0.842)	(0.596)	(0.536)	(0.562)	(0.552)	(0.550)
Percent Young Male	-0.155	1.165	0.985	1.234	1.007	1.031
	(0.869)	(0.820)	(0.812)	(0.764)	(0.737)	(0.737)
Observations	5424	10200	14310	17328	20382	20526

Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$. Column name corresponds to bandwidth used. For instance, estimates under 0.5 are derived from a reduced form equation using only units 0.5 standard deviations from the funding cutoff.

Table 12E. Bandwidth Estimates: Hispanic-White Turnout Gap

	(1) 0.5	(2) 1	(3) 1.5	(4) 2	(5) 3	(6) 4
Funded x Post	-0.00999	-0.00531	-0.00649	-0.00459	-0.00745	-0.00748
	(0.0128)	(0.00993)	(0.00967)	(0.00934)	(0.00914)	(0.00914)
Log Per Capita Income	0.0129	-0.00480	0.00386	0.0119	0.0208	0.0240
	(0.0536)	(0.0456)	(0.0479)	(0.0453)	(0.0427)	(0.0425)
Unemployment	0.00586**	0.00623***	0.00617***	0.00583***	0.00508***	0.00513***
	(0.00235)	(0.00198)	(0.00189)	(0.00174)	(0.00166)	(0.00165)
Percent Black	0.707	0.669	0.697	0.840	0.938	0.925
	(0.807)	(0.691)	(0.618)	(0.667)	(0.645)	(0.645)
Percent Hispanic	1.339*	1.515***	1.357**	1.061**	0.882*	0.841*
	(0.709)	(0.544)	(0.532)	(0.498)	(0.466)	(0.464)
Percent Young Male	-0.112	-0.606	-0.899	-0.00129	0.0205	-0.0582
	(1.014)	(0.954)	(0.899)	(0.867)	(0.838)	(0.840)
Observations	5400	10146	14286	17358	20400	20550

Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$. Column name corresponds to bandwidth used. For instance, estimates under 0.5 are derived from a reduced form equation using only units 0.5 standard deviations from the funding cutoff.

Table 13E. Bandwidth Estimates: Black-White Turnout Gap Among Men

	(1)	(2)	(3)	(4)	(5)	(6)
	0.5	1	1.5	2	3	4
Funded x Post	0.0308*	0.0123	0.0152	0.0149	0.0135	0.0139
	(0.0157)	(0.0119)	(0.0107)	(0.0102)	(0.00999)	(0.00998)
Log Per Capita Income	0.0566	-0.00160	-0.0297	-0.0633	-0.0567	-0.0623
	(0.0767)	(0.0652)	(0.0531)	(0.0513)	(0.0497)	(0.0496)
Unemployment	0.000339	0.00294	0.00362*	0.00321	0.00342*	0.00350*
	(0.00309)	(0.00244)	(0.00211)	(0.00203)	(0.00197)	(0.00197)
Percent Black	0.594	-0.0682	0.377	0.437	0.861*	0.868*
	(0.640)	(0.585)	(0.518)	(0.483)	(0.522)	(0.523)
Percent Hispanic	0.380	-1.190	-0.960	-1.231*	-0.957	-0.902
	(0.969)	(0.760)	(0.663)	(0.695)	(0.677)	(0.676)
Percent Young Male	-0.507	1.203	1.052	0.944	0.523	0.499
	(1.118)	(1.056)	(0.961)	(0.916)	(0.913)	(0.912)
Observations	5256	9834	13668	16524	19428	19554

Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$. Column name corresponds to bandwidth used. For instance, estimates under 0.5 are derived from a reduced form equation using only units 0.5 standard deviations from the funding cutoff.

Table 14E. Bandwidth Estimates: Black-White Turnout Gap Among Women

	(1)	(2)	(3)	(4)	(5)	(6)
	0.5	1	1.5	2	3	4
Funded x Post	0.00163	0.0173	0.0245**	0.0210**	0.0225**	0.0227**
	(0.0119)	(0.0108)	(0.00991)	(0.00949)	(0.00923)	(0.00923)
Log Per Capita Income	-0.00453	-0.0334	-0.0567	-0.0565	-0.0563	-0.0560
	(0.0584)	(0.0477)	(0.0478)	(0.0447)	(0.0434)	(0.0431)
Unemployment	0.00523**	0.00167	0.00165	0.00193	0.00113	0.000940
	(0.00252)	(0.00204)	(0.00185)	(0.00176)	(0.00169)	(0.00169)
Percent Black	0.324	0.327	-0.0108	-0.244	-0.228	-0.188
	(0.548)	(0.478)	(0.475)	(0.456)	(0.477)	(0.478)
Percent Hispanic	0.558	0.438	0.0213	-0.661	-0.583	-0.640
	(0.814)	(0.637)	(0.634)	(0.637)	(0.639)	(0.637)
Percent Young Male	-0.0778	0.465	0.564	0.960	0.745	0.839
	(0.889)	(0.898)	(0.941)	(0.878)	(0.873)	(0.878)
Observations	5304	9954	13926	16824	19740	19884

Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$. Column name corresponds to bandwidth used. For instance, estimates under 0.5 are derived from a reduced form equation using only units 0.5 standard deviations from the funding cutoff.

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Table 15E. Reduced form estimates near the cut-off (0.5) for race-gender groups

	(1) B-W Gap (Men)	(2) Black men turnout	(3) White men turnout	(4) B-W Gap (Women)	(5) Black women turnout	(6) White women turnout
Funded x Post	0.0308*	0.0372**	0.00614	0.00163	0.0196	0.00978
	(0.0157)	(0.0161)	(0.00591)	(0.0119)	(0.0129)	(0.00649)
Log Per Capita Income	0.0566	-0.0442	-0.106**	-0.00453	-0.107	-0.0939**
	(0.0767)	(0.0842)	(0.0434)	(0.0584)	(0.0693)	(0.0465)
Unemployment	0.000339	0.00115	0.000818	0.00523**	0.00460*	-0.000508
	(0.00309)	(0.00328)	(0.00117)	(0.00252)	(0.00269)	(0.00126)
Percent Black	0.594	-1.262	-1.930***	0.324	-1.747**	-1.917***
	(0.640)	(0.883)	(0.573)	(0.548)	(0.829)	(0.626)
Percent Hispanic	0.380	0.894	0.294	0.558	1.095	0.199
	(0.969)	(1.027)	(0.483)	(0.814)	(0.812)	(0.696)
Percent Young Male	-0.507	0.456	1.040	-0.0778	0.613	1.192
	(1.118)	(1.256)	(0.683)	(0.889)	(1.111)	(0.762)
Observations	5256	5256	5634	5304	5304	5646

Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$