The partisanship of mayors has no detectable effect on police spending, police employment, crime, or arrests

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Abstract

In this paper, we examine whether mayors' partisan affiliations lead to differences in crime and policing. We use a large new dataset on mayoral elections and employ three different modern causal inference research designs (a regression discontinuity design centered around close elections and two robust difference-in-differences methods) to determine the causal effect of mayoral partisanship on crime, arrests, and racial differences in arrest patterns in medium and large US cities. We find no evidence that mayoral partisanship affects police employment or expenditures, police force or leadership demographics, overall crime rates, or numbers of arrests. At the same time, we find some suggestive evidence that mayoral partisanship may modestly affect the racial composition of arrests. Overall, the results from our multi-method analyses indicate that local partisan politics has little causal impact on crime and policing.

Keywords: Local politics, policing, race, criminal justice, representation, elections

Introduction

In the lead-up to the 2020 presidential election, former-President Trump blamed increases in crime on Democratic leaders, arguing that Democrat-run cities were "rampant with crime" (*1*). Other Republican politicians have often claimed that increases in crime in large cities are a result of the "soft-on-crime" policies of Democratic leaders in those places (*2*). Much of the national news narrative around elections for prosecutors and mayors has echoed these claims (*3–6*). Yet these arguments from politicians and pundits alike rest upon an empirical assumption that Democratic leadership leads to increases in crime in cities.

Why might local leadership influence criminal justice outcomes? Levitt (*7*) was one of the first to attempt to hurdle potential endogeneity concerns and establish that increased police spending and hiring can lower crime rates. Using newer empirical techniques, recent literature has continued to establish the connection between officers and crime rates (*8–10*). Republican mayors commonly take a "tough on crime" approach and promise that they will reduce local crimes rates by hiring more officers and, often, via punitive policy (*11, 12*). This suggests that the election of Republican leaders may decrease local crime rates. However, political constraints may often curtail an official's ability to employ policy tools to influence local crime rates (*13, 14*). Thus, it is unclear whether mayoral partisanship should impact crime and arrests.

In this paper, we examine this question directly using the most comprehensive data available on local elections, policies, and outcomes and find no evidence that the mainstream narrative around partisanship and crime in cities is correct. We use three different methodological techniques to identify the causal impact of mayoral partisanship. Specifically, we use a regression discontinuity design that focuses on narrowly won elections where we can identify the effect of the mayor's party without confounding from other characteristics of cities. We augment this with two other causal inference techniques – both of which are generalized difference-in-differences methods that leverage changes in mayoral partisanship within cities – to holistically examine the causal impact of partisanship. To do so, we draw on nearly three decades worth of data on mayoral elections in nearly 400 medium and large American cities that encompass 99% of the population in the universe of cities over 75,000 in population that elect mayors. We combine these elections data with fiscal policy data, original data on police leadership, and administrative data on crime and policing. To our knowledge, no previous study has systematically assessed the impacts of mayoral partisanship on the wide set of outcomes within the domain of public safety and policing; therefore, empirical evidence on the matter is lacking.

Overall, our findings show that mayors' partisanship has limited influence on crime and policing. Electing a Democrat rather than a Republican as mayor leads to no detectable impact on police staffing or expenditures on criminal justice, nor does it lead to changes in crime or arrest rates. Racial differences in policing are also mostly unaffected by mayoral partisanship, with a few potential exceptions. Electing a Democratic mayor rather than a Republican mayor appears to marginally decrease the Black share of individuals arrested for several types of crimes, and marginally increase the Black share of law enforcement officers. Yet the first of these results is not consistently robust to alternative measures of calculating racial disparities in arrests nor the use of alternative research designs, and the latter is not verifiable using reliable data.

Our study draws on data spanning cities across the country and multiple decades. There are clear advantages to this approach with regard to statistical power and generalizability. However, this approach limits our analysis to outcomes which are available at that scale. There is increasingly rich data on more finely grained policing outcomes, including, for example, the race of individuals involved in traffic stops and the specific outcomes of those stops: whether people were searched, a citation was issued, or no action was taken (*15, 16*), but these data are not available for a wide enough set of cities or years to be used in our study. While our results largely document similar outcomes across Republican and Democrat-run cities, it remains possible that there are differences in more nuanced outcomes. Our suggestive finding that Democrat-led cities have a higher share of Black officers, which has been documented to have downstream effects for racial disparities in policing (*17, 18*), suggests that there may be effects of local partisanship that are not detectable using the data at hand.

Background

A long line of research in political science demonstrates that parties shape the behavior of national and state level politicians. Republican politicians have more ideologically conservative policy positions and voting records than Democratic politicians in Congress and state legislatures (*19, 20*), and this polarization between parties has only increased over time (*21*). Recent research has suggested that local elections have grown increasingly nationalized (*22, 23*) and partisan effects on policy outcomes extend to the local level (*24–28*). In particular, the election of a Democratic mayor (compared to a Republican mayor) leads to greater municipal expenditures (*24*).

Policing is an important area of local politics where mayoral partisanship may matter. Republican mayors often promise that they will reduce local crimes rates by increasing police spending and making criminal justice policies more punitive (*11, 12*). Democratic mayors and other local politicians often promise to reduce racial inequities in policing (*29*). Much of the national narrative around crime and policing suggests that the Republican party has stronger ownership of crime and public safety as a policy issue (*30*).

However, mayors have important constraints on their ability to unilaterally influence policy (*31, 32*). Mayors often face oversight both from below (by city councils) and above (by states). Potential policy changes are limited by budget restrictions and civil service rules. Limits on mayors' ability to reform policing may be especially pronounced with the rise of police unions' strength in the final decades of the 20th century (*33*) as well as the activity of these interest groups in local politics more broadly (*34*).

Moreover, pledges to increase spending on police often lie in tension with conservatives' larger policy goal of shrinking the size of government, leading to little partisan polarization in mayors' policy preferences in this area (*35*). This may simply be a policy area where politicians are unlikely to diverge along partisan lines in the policies they pursue (*36*). Thus, perhaps it is unsurprising that there is mixed evidence from past research on the effect of mayoral partisanship on police expenditures (*24, 31*).

Yet there has been little prior work on whether mayoral partisanship affects overall crime rates or arrest patterns. Some work has shown it has no impact on rates of murder, larceny, robbery, or burglary (*37*). In contrast, descriptive work finds that Democratically-led cities tend to have higher levels of crime – in large part because of underlying demographic patterns (*37, 38*).

Mayoral partisanship could also affect the racialization of policing. While Democrats and Republicans alike campaign on promises to reduce crime (*39–41*), Democrats tend to focus more than Republicans on reducing racial and socioeconomic inequalities in criminal justice policies (*29, 42*). These inequalities are widespread: previous academic work has shown that Black drivers are more likely to be stopped than white drivers; searches of Black drivers are less likely to produce "contraband," indicating a lower threshold for pulling them over (*15, 43*). Contact with police is more likely to lead to arrest for Black Americans even controlling for contextual factors (*44*), with substantial evidence of this phenomenon in the context of drug arrests in particular (*45, 46*). Black Americans are also more likely to face arrest or experience force at the hands of white officers relative to officers of color (*17, 47, 48*). Recent work integrates such studies with questions about the impact of partisanship. One study, for instance, documents that law enforcement officers are more likely to be Republican than the jurisdictions they police (*49*). In Florida, white Republican officers exhibit a larger racial disparity in policing than white Democratic officers – though disparities grew for both groups in the 2010's (*50*). Thus we might expect that Democratic mayors could reduce racial inequalities in policing relative to Republicans.

Previous theoretical and empirical work thus leaves unanswered important questions about the role of partisanship in cities and its impact on crime, policing, and racial disparities in policing.

Results

In this section, we discuss the impact of mayoral partisanship on criminal justice outcomes in cities (for details on our data and research design, please see the Materials and Methods section below). First, we examine the impact of mayors on overall police expenditures and staffing. Next, we assess how partisanship affects the demographic composition of police forces and the police chiefs who lead those forces. Finally, we examine whether mayoral partisanship affects aggregate crimes and arrests, as well as the racial composition of arrests.

Police Expenditures and Staffing

Republican politicians' tough on crime rhetoric implies they would raise police expenditures and staffing levels relative to Democrats. We evaluate this hypothesis by examining the impact of mayoral partisanship on police employment levels (using the Annual Survey of Public Employment and Payroll data) and related municipal expenditures (using the Historical Database of Individual Government Finances). Figure 1 displays these results. Each point represents the estimated increase in the noted outcome when a Democrat rather than a Republican is mayor – in other words, the causal effect of electing a Democratic mayor rather than a Republican. The bars emanating from each point are 90% and 95% confidence intervals. Each symbol in the figure represents the estimate from a different research design, with the RDD results represented by stars, PanelMatch results represented by crossed circles, and FEct results represented by vertical lines (See Materials and Methods section for details). SI Appendix D also has tabular versions of all results. These tables show the estimates and associated confidence intervals, as well as the bandwidth selected by `rdrobust` and the effective n within that bandwidth, and the number of matched observations for PanelMatch results.

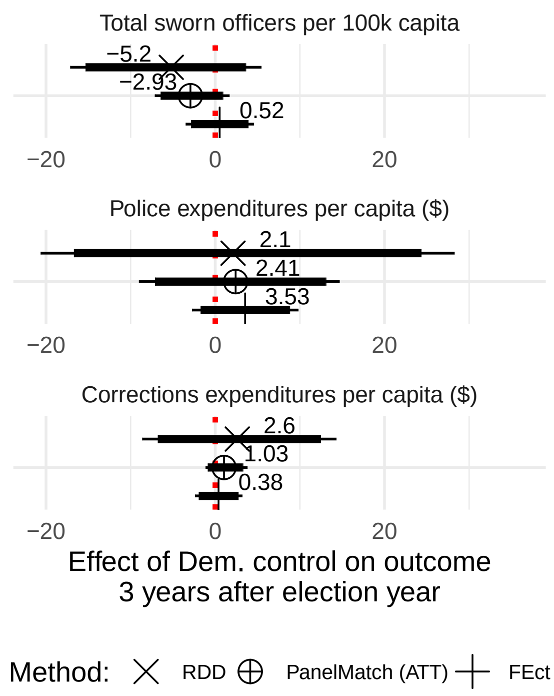


Figure 1: The null effect of mayoral partisanship on municipal police employment and criminal justice spending. Points indicate causal effect estimates from each of our three research designs: from the RDD estimated with `rdrobust` (stars); from PanelMatch (crossed circles); and `fect` (vertical lines), and horizontal lines indicate 90% (thick lines) and 95% (thin lines) confidence intervals, using robust bias-corrected confidence intervals for the RDD.

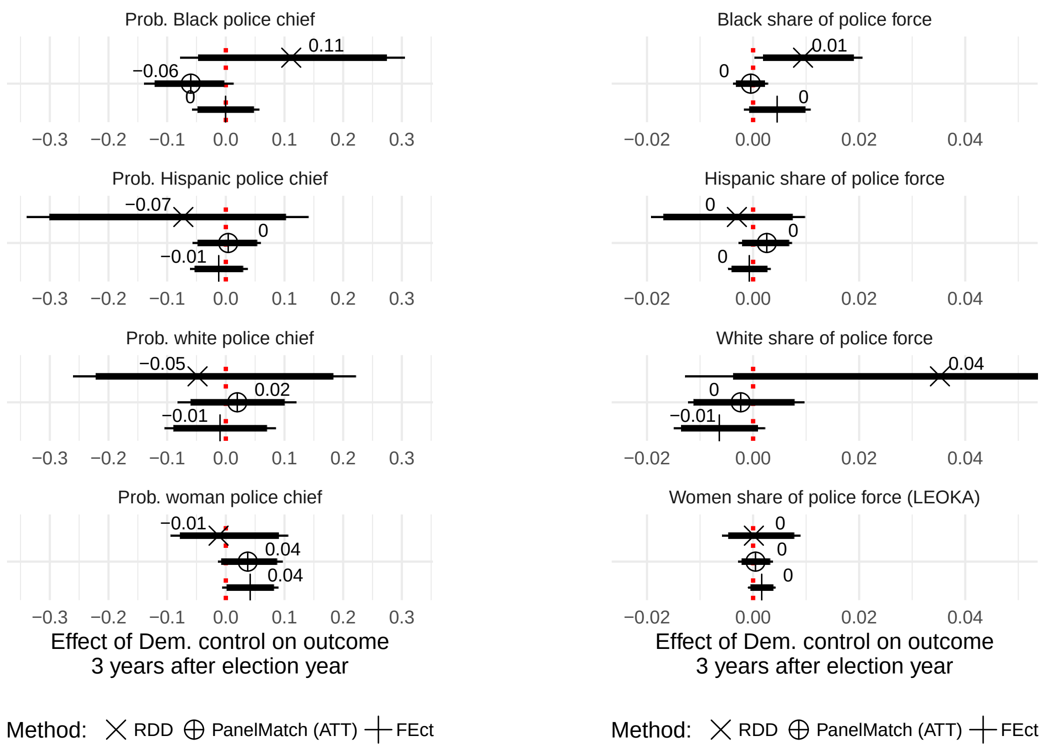
The top set of results in Figure 1 indicates that electing a Democratic mayor has little detectable impact on the per capita total number of police officers employed by a city. We replicate these analyses using RDD on two other datasets that also track the number of police officers employed by a city: the LEMAS and LEOKA datasets. Though we are most inclined to trust the estimates from the Census Bureau's ASPEP rather than the other two datasets, which are based on voluntary opt-in surveys of police departments, we present all three results for the sake of transparency and completeness in SI Appendix E.

The estimated impact on the size of the police force is small and statistically indistinguishable from zero when using all three research designs (RDD, PanelMatch, and `fect`). The largest of these three estimates is roughly equivalent to 2.5 percent of the mean of the outcome, as reported in Table 1; the smallest is one-tenth of that. The second and third sets of results display the estimates of mayoral partisanship on two categories of municipal spending: those directly on police protection as well as expenditures on corrections. In both cases, we find no discernible impact of electing a Democrat as mayor on per capita spending in these policy areas related to criminal justice. The largest estimated effect in the middle panel suggests a 1.2% increase relative to the mean of the outcome. The lack of any partisan impacts on overall policing employment or expenditures suggests that despite Republicans' tough on crime platforms, there may not be substantial effects of mayoral partisanship on policy levers related to criminal justice.

Police Demographics

We next examine the impact on not just the size of the police force, but the demographics of the police. In particular, one crucial tool at the disposal of mayors is (in most places) the power to appoint a police chief who plays a direct role in both police staffing and officers' everyday practices when doing their job. Though our police chiefs data are limited in their time span to the 2010-2022 period, these results do represent the most thorough investigation of police chief demographics with the data available. We also examine the demographic composition of the entire police force. Police force demographics are especially relevant for two reasons: (1) hiring and personnel decisions may be some of the more immediate levers available to a mayor (*51*), including indirectly through police chief selection and (2) a growing body of work documents how demographics of law enforcement officers impact policing outcomes – including on outcomes that we are not able to observe. Black and Hispanic officers make fewer stops and arrests than white officers and are less likely to use force than white officers while agencies run by white officers have higher Black-to-White arrest ratios (*17,47*). Women officers are less likely to search drivers during traffic stops (*52*) and less likely to use force than officers who are men (*17*).

Our analyses of police chief and police force demographics are displayed in Figure 2 using all three of our methodological techniques. The results for police chiefs are shown in panel a of Figure 2. The results provide no consistent evidence that a Democratic mayor is more likely than a Republican mayor to appoint a police chief of any particular demographic group. Our estimates of the causal impact of mayoral partisanship on the probability of having a Black police chief, a Hispanic police chief, a white police chief, or a woman police chief are all statistically indistinguishable from zero. Table 1 shows that 5.8% of police chiefs we have identified are women and 15% are Black. Our largest estimates for each group suggest large (albeit imprecise) relative effects – of nearly three-fourths of the mean in the case of both women as police chiefs and Black police chiefs.



(a) Police chiefs

(b) Police force

Figure 2: The effect of mayoral partisanship on changes in the demographics of police chiefs (panel a) and the police force (panel b) between the election year and the several years after the election. Points indicate causal effect estimates from each of our three research designs: from the RDD estimated with `rdrobust` (stars); from PanelMatch (crossed circles); and `fect` (vertical lines), and horizontal lines indicate 90% (thick lines) and 95% (thin lines) confidence intervals, using robust bias-corrected confidence intervals for the RDD. The horizontal axis of panel b is cut off in order to maintain presentational legibility, but the upper limits of the confidence intervals on the white share of the police extends beyond the plot limits (see Appendix D for full tabular results).

Our results examining the demographics of the police force as a whole (in panel b of Figure 2) using our three different research designs are more mixed. Our RDD indicates that the share of officers who are Black significantly increases by approximately one percentage point as a result of electing a Democrat (rather than a Republican) as mayor ($p = 0.04$); relative to the sample average of 9.5% Black share of police officers, this represents an increase of roughly 9.9%. Yet the two difference-in-differences strategies do not lead to this conclusion, and suggest that changing from a Republican to a Democratic mayor has no effect on the Black share of the police force. And for other racial groups and for gender, none of our research designs indicate that mayoral partisanship influences the demographic makeup of the police force.

Overall Crime, Arrests, and Policing

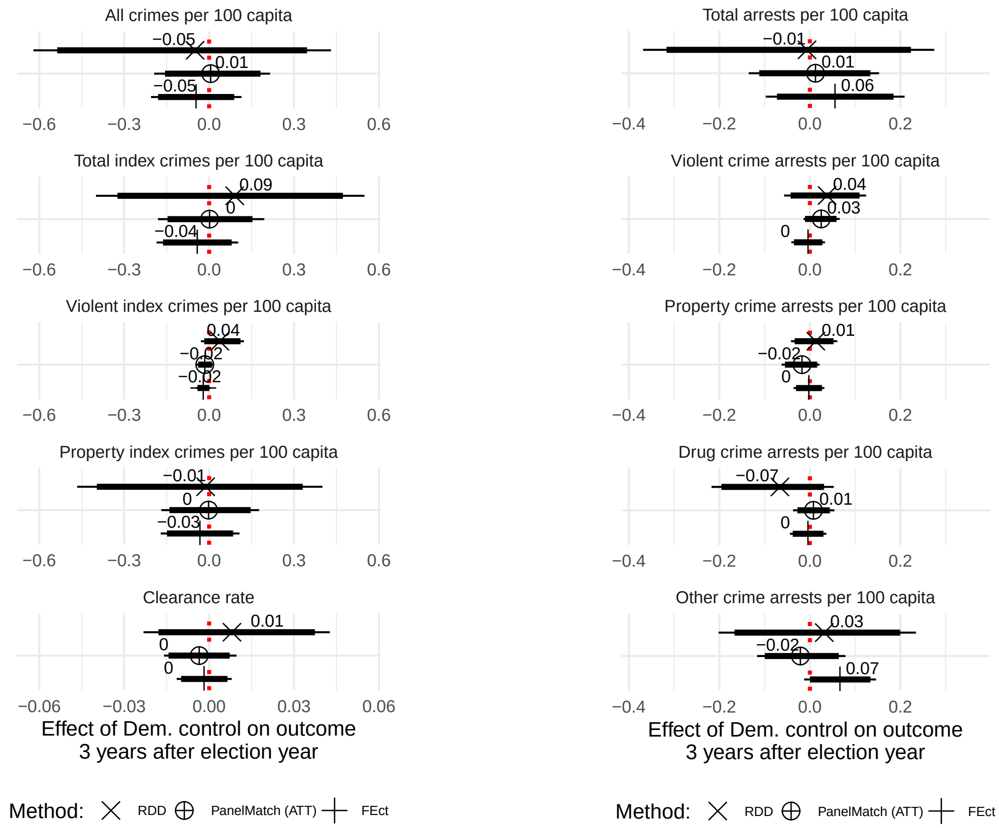
We next examine the empirical impact of mayoral partisanship on overall crime, clearance rates, and arrests in Figure 3. The horizontal axis reports our estimates of the causal impact of Democratic mayors (versus Republican ones) on the overall per capita levels of reported crime and clearance rates (panel a) and overall numbers of arrests (panel b).

Notably, none of the estimates are statistically different than zero. For example, the top point in panel a of Figure 3 shows that electing a Democrat as mayor rather than a Republican has no detectable causal effect on overall levels of crime, and the point estimate is negative from two of our research designs. The confidence interval around this estimate, while wide, allows us to rule out increases in overall crime of more than 0.32 crimes per 100 capita. Even that would represent a small effect relative to the sample average of 6.6 total crimes per 100 capita.

Given concerns about potential underpowered analyses in regression discontinuity analyses in particular (*53*), we also conduct post-hoc power analyses (*54, 55*). These power calculations indicate that the probability of rejecting the null hypothesis were the true effect to be equivalent in size to 0.54 crimes per capita, or half a standard deviation, is relatively high, at 0.87. This – alongside the fact that our two other research designs come to similar conclusions – suggests that we are unable to reject the null hypothesis of no effect on crime not due to a lack of statistical precision but instead the small size of these effects.

We also find no discernible effect of mayoral partisanship on crime when disaggregating to available categories of crimes (index, property, and violent), which we show in the next set of points in panel a of Figure 3. Likewise, we cannot reject the null hypothesis of no change in the clearance rate – the share of reported crimes for which an arrest was made (shown with the bottom points and lines in panel a).

In panel b, we focus on arrests. The estimated impact of Democratic mayors on total arrests per capita is also not statistically distinguishable from zero. Post-hoc power calculations for the RDD analyses indicate that these are relatively precisely estimated nulls, and that the probability of rejecting the null hypothesis were the true effect to be equivalent in size to half a standard deviation in the outcome, or 0.31 arrests per 100 capita, is 0.7. We also test for impacts of the mayor's party on specific categories of arrests. We find that the estimated impacts of partisanship on overall numbers of violent, property, drug, and other crime arrests are close to zero and are all statistically insignificant. Overall – alongside our earlier analyses – our findings indicate that mayoral partisanship is not causally associated with differences in the levels of police



(a) Crimes and clearance rates **(b)** Arrests

Figure 3: The null effect of mayoral partisanship on changes in per capita reported crimes and clearance rate (panel a) and per capita arrests (panel b) between the election year and three years after the election. Points indicate causal effect estimates from each of our three research designs: from the RDD estimated with `rdrobust` (stars); from PanelMatch (crossed circles); and `fect` (vertical lines), and horizontal lines indicate 90% (thick lines) and 95% (thin lines) confidence intervals, using robust bias-corrected confidence intervals for the RDD.

employment, police spending, reported crimes, or arrests.

While mayoral partisanship has no effect on overall crime or arrests, it is possible that it leads to changes in the *way* that police forces act in the conduct of their jobs – and specifically the racial composition of their arrests. Figure S20 in SI Appendix F depicts our causal estimates of mayoral partisanship on the Black share of total arrests as well as the Black share of arrests for each category of crimes separately. We find suggestive evidence that electing a Democratic mayor rather than a Republican mayor marginally decreases the Black share of individuals arrested for several types of crimes. But these findings are not robust across research designs and generally fall below conventional levels of statistical significance.

Discussion

In this paper, we examine whether political control of city governments in the US influences local policing, crime rates, and arrests. Using a large dataset of local elections and three different credible research designs, we are able to disentangle the effect of mayoral partisanship from other city-level characteristics that might affect policing and crime outcomes.

We find no detectable effects of mayoral partisanship on overall police employment and criminal justice expenditures, the demographics of the police, overall levels of crime, numbers of arrests, or the clearance rate of crimes. These results stand in stark contrast to national political rhetoric on policing, crime, and political partisanship. Candidates for political offices at the local, state, and federal level consistently raise crime as an important issue (*56–58*). Voters may hold politicians accountable for these types of outcomes (*59–61*), at least partially as a result of increased news coverage (*62*) – even if this coverage does not always match reality (*63*).

When we examine the effects of city leadership on outcomes in the area of race and policing, we again find no consistent evidence that mayoral partisanship influences the demographic composition of police forces. Previous work on police use-of-force suggests that the racial composition of police officers can have a strong effect on racial disparities in policing activity and violence (*18, 64, 65*). The demographics of the police force might therefore be one way in which local politicians exert control to reduce racial disparities in citizen-police contact. Yet our results yield no consistent evidence when it comes to both police chief and police force demographics across the thorough examination that we conduct using three different research designs.

Republican politicians in particular have claimed that Democrats' "soft-on-crime" policies have led to crime increases in large cities (*2*). National news outlets have pointed to the outcomes of recent recall efforts and elections for prosecutors and mayors to suggest that they represent a backlash to progressive policies on crime (*3–5*). Yet these popular claims ignore the reality that our results make clear: mayoral partisanship of cities does not lead to any detectable causal differences in crime or arrests. If the partisanship of leaders does not influence objective performance metrics, voters may struggle to hold those leaders accountable along partisan lines (*14*). Our results suggest that this may be true for mayors and criminal justice outcomes, and suggest that accountability in local politics must rely on some form of retrospection regardless of party instead.

Overall, our results indicate that politics – and in particular partisan politics – play a limited role in crime and policing. Partisanship has little causal impact on bottom-line outcomes like crime or arrest rates. There is also only limited evidence that partisanship affects the demographics of the police, or the racialized nature of citizen-police interactions. Our analyses are inherently limited in their ability to uncover other mechanisms by which mayoral partisanship might influence police behavior due to a lack of broad intermediate data on mayoral influence or direct policymaking. Nor do our analyses enable us to discern the effect of other influences on policing, such as police union strength or other activity by police unions and affiliated interest groups (*34, 66*).

Future research could focus more on these intermediate steps in the causal chain between elections and criminal justice outcomes. It could also focus on within-party variation in policy-making. For instance, mayors might appoint different types of police chiefs (including known "progressive" or "reform" chiefs) regardless of race, or they may require their police forces to undergo certain types of training to reduce racial biases in policing. Our analyses can neither assess whether these mechanisms are at play, nor the bevy of mechanisms that operate together to result in racialized policing. Overall, our results help build a more complete picture of crime and policing in US cities by providing evidence that local partisanship is not a central driver of differences in crime and policing in U.S. cities.

Materials and Methods

In order to examine the policy effects of the partisan control of city governments, we use data on city mayoral elections and criminal justice policy and outcomes in medium and large cities with a population of more than 75,000 people in 2020. We then leverage three different research designs to identify the causal effect of electing mayors of different parties on crime and policing.

Data

The foundation of our analysis is administrative data on mayoral elections in medium and large cities. The elections data consists of 3,254 individual elections between 1990 and 2021 in 398 cities with at least 75,000 people in 2020 (*67*). These elections data cover 99% of the target population of cities above this population threshold that hold mayoral elections (see SI Appendix A for more details on the coverage of our elections data). Our analysis requires information on each candidate's partisanship even in officially nonpartisan elections. We therefore use data on raw election returns augmented with information about individual candidates' partisanship from matching with a wide range of auxiliary data. These auxiliary data include information from both L2 and TargetSmart's national voter files, as well as campaign finance-based ideology scores (*68, 69*), information on candidates who served in Congress or state legislatures (*20, 70*), and information from previous academic studies (*31, 37*).

We examine the impact of mayoral partisanship on a number of criminal justice outcomes. First, we use data on fiscal policy from the Historical Database of Individual Government Finances to examine the effects of mayoral partisanship on policing-related government expenditures. These data are based on a Census of Governments conducted every five years and the Annual Survey of Governments collected in every non-census year. We adjusted all monetary figures into 2019 dollars based on the consumer price index. We also harness data from the Census Bureau's Annual Survey of Public Employment and Payroll (ASPEP), which records both the number of employees of different types and the payroll expenditures on those employees for local governments.

To examine not just the overall size of the police force but also the composition of the police force, we draw on several sources of data. First, we gather original data on the names (and demographics) of police chiefs in the cities in our elections data for the period 2010-2022. We determined chiefs' gender and racial backgrounds based on the media's identification or self-identification of chiefs in news articles or via biographies on personal or departmental websites that listed race/ethnicity or membership in a group based on race/ethnicity (e.g. "member of the National Organization of Black Law Enforcement Executives"), or in some cases based on multiple photos of chiefs. These data on police chiefs are limited in their time span to the 2010-2022 period due to the difficulty of finding news articles on the subject prior to 2010, so we caution that our results using these data are by no means the definitive answer on partisan control of cities and police chiefs, but they represent an important first attempt at the question.

We also use data on police officer demographics from Law Enforcement Management and Administrative Statistics (LEMAS), a survey of law enforcement agencies administered by the Bureau of Justice Statistics roughly every 4-5 years since 1987. The most recent available wave of the survey is from 2020. These surveys, for most of the survey waves, provide data on the racial demographics of officers as well as their gender. We merge these data with our elections data to create outcomes based on changes in police demographics between the most recent LEMAS survey before the election and the next LEMAS survey after the election. The LEMAS data are an imperfect source of longitudinal information on police forces, given that they are not conducted yearly, so we restrict our use of this outcome variable to surveys within a reasonable time range around the election by only using baseline pre-surveys that were 0-4 years before the election, and post-surveys that were 2-4 years after the election. We supplement this demographic information with data from the FBI's Law Enforcement Officers Killed and Assaulted (LEOKA) survey, which (along with information on violent police-civilian interactions) records the gender of police officers employed by a city.

Our data on crime and policing outcomes are drawn from the FBI Uniform Crime Report (UCR) data (*71, 72*). UCR data are compiled by the FBI based on reports from law enforcement agencies and provide annual agency-level counts of reported crime offenses, clearances, and arrests for a variety of offense types. As we study mayors, we restrict the law enforcement agencies to city police departments. Throughout most of our analysis, we normalize variables that are in levels (rather than proportions) by the city's population.

We report data on crimes overall and in several categories: total "index crimes," which are the eight offenses used by the FBI to produce crime indices (murder, rape, robbery, aggravated assault, burglary, larceny, motor vehicle theft, and arson), violent index crimes, and property index crimes. The latter two are subsets of the eight index offense types. Likewise, we report results on arrests overall and by category, including property crime, violent crime, drug crime, and "other" crime. The "other" category of arrests includes offense types for which enforcement may be particularly subject to law enforcement officer discretion: e.g., vagrancy, loitering, or drunkenness, to name a few.

Finally, we leverage data on the racial composition of arrests in the UCR data. We estimate the *Black share of arrests* based on the number of arrestees coded as Black in the data in a given city-year divided by the total number of arrests in that city-year. We construct this both for overall arrests and each of the categories noted above. We also construct the ratio of Black-to-white arrests as an alternative measure (results shown in Appendix F.1). Note that the arrests data are disaggregated by the age, race, and sex of the arrestee – but not by ethnicity (Hispanic vs. non-Hispanic).

Tables 1 and 2 show descriptive statistics for our main dependent variables that we examine in the remainder of the paper, among the entire sample of city-years since 1990 (in the first column), the city-years when under Democratic mayoral control (second column), Republican mayoral control (third column), or unknown mayoral party control (fourth column). While the majority of mayors in our sample are Democrats, there are also many city-years with Republican mayors.

Table 1: Summary Statistics for Main Outcome Variables, Part 1

	Total	Democratic	Republican	PID Unknown
	(N=12436)	(N=5757)	(N=4143)	(N=2536)
Total sworn officers per 100k capita				
Mean (SD)	200 (87.9)	222 (98.0)	171 (61.1)	167 (59.6)
Median [Min, Max]	181 [0, 969]	202 [0, 969]	160 [0, 620]	153 [0, 403]
Missing	3841 (30.9%)	858 (14.9%)	805 (19.4%)	2178 (85.9%)
Police expenditures per capita ($)				
Mean (SD)	287 (121)	330 (135)	259 (93.1)	236 (89.8)
Median [Min, Max]	265 [0, 1280]	307 [0, 1280]	245 [0, 985]	222 [0, 1240]
Missing	1173 (9.4%)	515 (8.9%)	496 (12.0%)	162 (6.4%)
Corrections expenditures per capita ($)				
Mean (SD)	14.7 (56.0)	21.4 (70.7)	6.43 (28.4)	12.8 (49.2)
Median [Min, Max]	0 [0, 1300]	0 [0, 1150]	0 [0, 276]	0 [0, 1300]
Missing	1173 (9.4%)	515 (8.9%)	496 (12.0%)	162 (6.4%)
Police chief race/ethnicity				
Black	678 (5.5%)	524 (9.1%)	151 (3.6%)	3 (0.1%)
Hispanic	261 (2.1%)	151 (2.6%)	93 (2.2%)	17 (0.7%)
White	4307 (34.6%)	2247 (39.0%)	1783 (43.0%)	277 (10.9%)
Missing	7190 (57.8%)	2835 (49.2%)	2116 (51.1%)	2239 (88.3%)
Police chief gender				
Woman	284 (2.3%)	206 (3.6%)	75 (1.8%)	3 (0.1%)
Man	4880 (39.2%)	2674 (46.4%)	1912 (46.2%)	294 (11.6%)
Missing	7272 (58.5%)	2877 (50.0%)	2156 (52.0%)	2239 (88.3%)
Black share of police force				
Mean (SD)	0.0921 (0.109)	0.124 (0.133)	0.0568 (0.0606)	0.0661 (0.0710)
Median [Min, Max]	0.0555 [0, 0.854]	0.0833 [0, 0.854]	0.0368 [0, 0.446]	0.0377 [0, 0.548]
Missing	1977 (15.9%)	529 (9.2%)	666 (16.1%)	782 (30.8%)
Hispanic share of police force				
Mean (SD)	0.101 (0.148)	0.107 (0.153)	0.106 (0.146)	0.0741 (0.134)
Median [Min, Max]	0.0523 [0, 1.00]	0.0524 [0, 1.00]	0.0621 [0, 1.00]	0.0323 [0, 1.00]
Missing	1977 (15.9%)	529 (9.2%)	666 (16.1%)	782 (30.8%)
White share of police force				
Mean (SD)	0.780 (0.183)	0.739 (0.194)	0.809 (0.170)	0.843 (0.145)
Median [Min, Max]	0.829 [0, 1.00]	0.777 [0, 1.00]	0.855 [0, 1.00]	0.875 [0, 1.00]
Missing	1975 (15.9%)	527 (9.2%)	666 (16.1%)	782 (30.8%)
Woman share of police force				
Mean (SD)	0.106 (0.0489)	0.119 (0.0521)	0.0988 (0.0407)	0.0875 (0.0448)
Median [Min, Max]	0.101 [0, 0.904]	0.114 [0, 0.874]	0.0966 [0, 0.270]	0.0822 [0, 0.904]
Missing	821 (6.6%)	245 (4.3%)	283 (6.8%)	293 (11.6%)

Table 2: Summary Statistics for Main Outcome Variables, Part 2

	Total	Democratic	Republican	PID Unknown
	(N=12436)	(N=5757)	(N=4143)	(N=2536)
All crimes per 100 capita				
Mean (SD)	6.60 (3.26)	6.93 (3.38)	5.73 (2.86)	7.30 (3.27)
Median [Min, Max]	6.08 [0.000629, 21.5]	6.39 [0.0129, 21.5]	5.17 [0.000629, 20.3]	6.97 [0.0746, 21.1]
Missing	680 (5.5%)	264 (4.6%)	226 (5.5%)	190 (7.5%)
Violent crimes per 100 capita				
Mean (SD)	0.670 (0.515)	0.783 (0.558)	0.508 (0.382)	0.678 (0.531)
Median [Min, Max]	0.538 [0, 4.35]	0.651 [0, 4.09]	0.424 [0, 4.35]	0.531 [0, 3.88]
Missing	683 (5.5%)	265 (4.6%)	226 (5.5%)	192 (7.6%)
Property crimes per 100 capita				
Mean (SD)	4.70 (2.37)	4.79 (2.42)	4.12 (2.11)	5.42 (2.41)
Median [Min, Max]	4.29 [0, 16.8]	4.41 [0.00515, 16.8]	3.68 [0, 15.1]	5.12 [0.0707, 16.1]
Missing	680 (5.5%)	264 (4.6%)	226 (5.5%)	190 (7.5%)
Clearance rate				
Mean (SD)	0.265 (0.110)	0.249 (0.110)	0.277 (0.110)	0.281 (0.105)
Median [Min, Max]	0.265 [0, 1.00]	0.248 [0, 0.630]	0.279 [0, 1.00]	0.282 [0, 0.768]
Missing	684 (5.5%)	268 (4.7%)	226 (5.5%)	190 (7.5%)
Total arrests per 100 capita				
Mean (SD)	3.15 (1.77)	3.21 (1.89)	2.78 (1.42)	3.61 (1.88)
Median [Min, Max]	2.81 [0.0279, 19.4]	2.83 [0.0865, 15.9]	2.49 [0.0279, 19.4]	3.30 [0.257, 14.9]
Missing	2749 (22.1%)	1333 (23.2%)	847 (20.4%)	569 (22.4%)
Violent crime arrests per 100 capita				
Mean (SD)	0.815 (0.515)	0.904 (0.581)	0.673 (0.391)	0.852 (0.489)
Median [Min, Max]	0.692 [0, 8.15]	0.753 [0.0601, 6.67]	0.609 [0, 8.15]	0.758 [0.00184, 3.74]
Missing	2749 (22.1%)	1333 (23.2%)	847 (20.4%)	569 (22.4%)
Property crime arrests per 100 capita				
Mean (SD)	0.476 (0.424)	0.473 (0.403)	0.401 (0.332)	0.609 (0.554)
Median [Min, Max]	0.384 [0, 8.39]	0.381 [0.00844, 6.27]	0.331 [0.00559, 5.35]	0.498 [0, 8.39]
Missing	2749 (22.1%)	1333 (23.2%)	847 (20.4%)	569 (22.4%)
Drug crime arrests per 100 capita				
Mean (SD)	0.593 (0.439)	0.653 (0.520)	0.544 (0.336)	0.539 (0.369)
Median [Min, Max]	0.496 [0, 4.89]	0.530 [0, 4.89]	0.476 [0, 2.54]	0.465 [0, 4.47]
Missing	2749 (22.1%)	1333 (23.2%)	847 (20.4%)	569 (22.4%)
Other crime arrests per 100 capita				
Mean (SD)	1.26 (0.944)	1.18 (0.930)	1.17 (0.795)	1.61 (1.11)
Median [Min, Max]	1.04 [0.00401, 9.79]	0.953 [0.00890, 7.67]	0.971 [0.00401, 5.61]	1.35 [0.00705, 9.79]
Missing	2749 (22.1%)	1333 (23.2%)	847 (20.4%)	569 (22.4%)
Mayoral party in power				
Democratic	5757 (46.3%)	5757 (100%)	0 (0%)	0 (0%)
Republican	4143 (33.3%)	0 (0%)	4143 (100%)	0 (0%)
PID Unknown	2536 (20.4%)	0 (0%)	0 (0%)	2536 (100%)

Research Designs

Our first causal inference technique by which we examine the effect of electing mayors of different parties is a regression discontinuity (RD) design. The RDD is a strategy that has been widely employed to estimate the causal effects of elected official identity on political and policy outcomes (*24,31,37,37,73–76*). We view it as providing the clearest causal identification of the impact of mayoral partisanship on criminal justice outcomes. The RD design's main limitation is relatively weak statistical power (*53*). As a result, we augment it with several difference-in-difference designs that examine the effect of switches in mayoral partisanship.

Our regression discontinuity (RD) design exploits the fact that a sharp electoral threshold, 50% of the two-party vote share, determines which party wins mayoral elections. Cities where the mayoral election was won by a Democrat over a Republican (or vice versa) by a very narrow margin are likely to be similar to one another on a host of characteristics other than mayoral partisanship. With some assumptions, this allows us to detect the causal effect of partisanship while avoiding potential confounding from these other characteristics. The RD method therefore focuses on differences in outcomes in very close elections. In practice, the effect of electing a Democratic mayor rather than a Republican mayor is identified by restricting the sample to elections within a bandwidth around the 50% threshold in the Democrats' vote share and estimating the "jump" in outcome variables *at* the threshold – or the elections closest to a tie. This design identifies a *local average treatment effect (LATE)* at the threshold of 50% vote share. Following (*77*), we use a local polynomial nonparametric regression; we outline the estimator in more detail in SI Appendix B. We implement this approach using the `rdrobust` package in `R` (*78*) which selects an optimal bandwidth to minimize mean-squared-error (MSE) in the estimate and adjusts confidence intervals to account for remaining bias from the bandwidth selection procedure.

This approach might raise concerns about the applicability of the estimates from this design to cities where there are not close mayoral elections. This concern is at least partially assuaged by the fact that, of medium and large cities over 75,000 in population in our data, our elections data cover 99% of the population in this target universe, and 89% of those cities in our elections data had a close election at some point and are therefore included in our RD analyses. The coverage of our data and the subsample of cities with close elections is further described in SI Appendix A.

The validity of the RD design depends on the assumption that only the party of the winning candidate — and not the distribution of units' potential outcomes — changes discontinuously at the threshold (*79, 80*). This is often called the "continuity assumption" and involves assuming there is no other endogenous cause of changes in outcomes that occur at the same threshold that triggers the change in treatment status – in our case, changes in partisanship of the winner (*81*). Results from tests in SI Appendix B document that this assumption is likely satisfied in our setting. Consistent with a recent large-scale validation of electoral regression discontinuity (RD) design studies (*82*), we also observe no significant discontinuities in lagged values of the running variable or outcome variables. SI Appendix C shows these placebo results.

In order to increase statistical efficiency, we estimate all RDD treatment effects on changes in outcomes rather than on levels (*80*). Our main analyses focus on the difference between crime and policing outcomes in the election year and three years after the election to account for the lag in time between a politician taking office and their ability to influence policy outcomes.

The RDD – and the other designs we use in this paper – all measure the effects of the compound treatment of partisanship alongside other characteristics that politicians possess in tandem with their partisanship. This technique, however, might also have the downside of potential bias due to compensating differentials (*83*) arising from, for instance, differences in candidate competence that occur in close elections. While the RDD does not enable us to measure the effects of partisanship disentangled from these other characteristics, we are primarily interested in the real-world effects of this bundled party treatment and not the "pure" effect of party on policy outcomes.

Second, we use two recently-developed generalized difference-in-differences strategies to assess the effect of changes in mayoral partisanship on crime and policing outcomes. A basic difference-in-differences model in our setting might take the form:

$$Y_{ct} = \beta Dem_{ct} + \tau_t + \gamma_c + \epsilon_{ct} \tag{1}$$

where Y_{ct} represents our outcome (e.g., per capita numbers of arrests) and Dem_{ct} is an indicator variable equal to one if a city c has a Democratic mayor in year t. Also included are city and year fixed effects (γ_c and τ_t). This basic approach fails to account for recently highlighted biases stemming from two-way fixed effects difference-in-differences models when treatment occurs at different points in time (*84*), which is very much the case in our setting.

Instead, we estimate non-parametric difference-in-differences models using the PanelMatch method (*85*), which compares units with similar treatment histories (i.e. party control) and similar pre-treatment outcomes (e.g. crimes or arrests) that are "treated" with a Democrat taking control of the mayoral office vs. those that are not treated (i.e. a Republican or nonpartisan mayor takes control). Specifically, we match using Mahalanobis distance on lagged outcomes in the three years prior to treatment. This design avoids the negative weights problem in traditional two-way fixed effects methods (*85*). It identifies an *average treatment effect on the treated units* (ATT) across cities and time periods in cities that switch mayoral parties. We also conduct placebo tests for this method by looking at the effects of changes in mayoral partisanship on pre-treatment outcomes in SI Appendix C. Note that we find some non-null placebos, so readers should bear that in mind when evaluating our main results using PanelMatch. However, our main results using PanelMatch are mostly null which could ameliorate concerns of confounding. The downside of PanelMatch's algorithm is that matching treatment and control units reduces our effective sample size and the corresponding statistical power.

To build on these analyses, we also estimate the effects of changes in partisanship within cities over time using the counter-factual fixed effects models developed by (*86*) via the `fect` package in `R`. This avoids the negative weights problem in traditional two-way fixed effects methods and corrects biases induced by treatment effect heterogeneity by not using the treated observations at the modelling stage and by imposing uniform weights on individualistic treatment effects on treated observations (*86*). This method is also based on a difference-in-differences approach and allows us to accommodate switches both from Republican mayors to Democratic mayors, and the reverse. We examine switches from non-Democratic to Democratic control, but results examining switches to Republican control are similar and presented in SI Appendix J. This design identifies an *average treatment effect on treated* (ATT) cities that switch mayoral parties. The `fect` approach also enables us to demonstrate the absence of pre-treatment placebo effects, which we show in SI Appendix C, and to visualize the dynamic trajectory of the post-treatment impact of partisanship.

We present the results of all three of these techniques in the following sections in order to comprehensively examine the questions at hand. While each research design has its own limitations, the three designs together present holistic evidence about the effects of politics on crime and policing. While we have detailed its biases in this context, we also find similar effects using a more traditional two-way fixed effects design.

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Data and Materials Availability

All data needed to evaluate the conclusions in the paper are present in the paper and/or the Supplementary Materials. Replication data and code for this paper is available at the Harvard Dataverse at https://doi.org/10.7910/DVN/LZIIJC.

Author Contributions

All authors contributed equally to conceptualizing the paper and designing the data analysis. J.D.B.K conducted the bulk of the data analysis. C.W. and J.D.B.K collected the elections data. All authors participated in writing and editing the manuscript.

Competing Interests

All authors declare that they have no competing interests.

Supplementary Appendix for
"The partisanship of mayors has no effect on police spending, police employment, crime, or arrests"

Contents

A Elections Data Sample

In this section, we provide further details on our mayoral elections data. Table S1 provides further details on the total elections data gathered as well as those elections used in our RDD analyses. The cities in our mayoral elections dataset encompass 99% of the population in our target universe of medium and large cities that elect mayors. Moreover, the elections that have a Democratic vote share between 40% and 60%, which roughly approximates the effective sample in many of our RDD analyses, covers 67% of the population in our target universe overall. It also covers a broad geographic range, as demonstrated by the comparison between Figure S1, which shows our full sample of cities in our elections data, and Figure S2, which shows those cities with close elections.

Table S1: Summary of Mayoral Elections Data Coverage

Subset	N Cities	N Elections	Min Pop.	Max Pop.	Avg. Pop.	Total Pop.	% of Target Uni. Pop.
All cities	19,481		0	8,804,190	10,526	205,058,014	
Medium and large cities	476		75,102	8,804,190	224,297	106,765,546	
Medium and large cities w/ mayoral elections (target universe)	419		75,102	8,804,190	240,204	100,645,272	100
Medium and large cities in elections dataset	396	3,238	75,102	8,804,190	252,594	100,027,292	99
Two-party contested elections in dataset	285	1,045	75,604	8,804,190	282,038	80,380,921	80
Two-party close elections in dataset	218	501	75,644	8,804,190	303,561	66,176,369	66

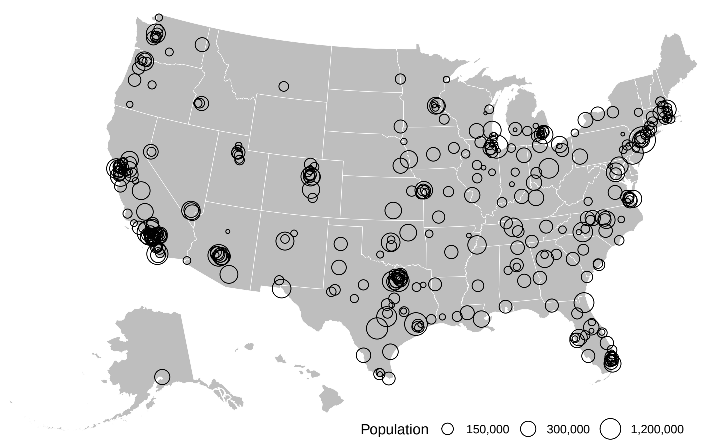


Figure S1: Cities in Elections Dataset

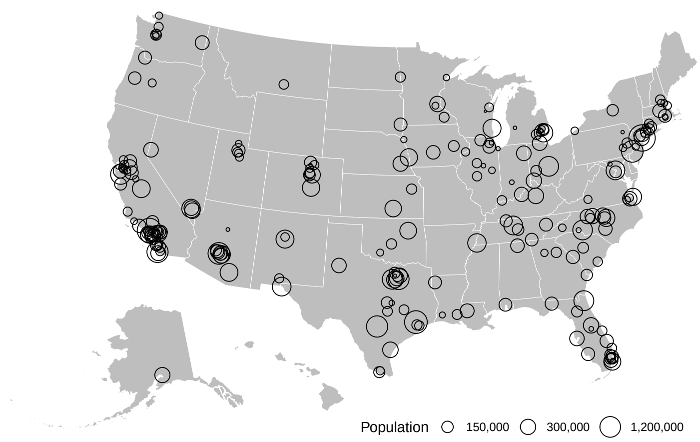


Figure S2: Cities in Dataset with Two-Party Elections and 40% $\leq$ Democratic Voteshare $\leq$ 60% only

Table S2: Summary of Mayoral Candidates Data

	Total	Democratic	Republican	PID Unknown
	(N=6728)	(N=3137)	(N=2375)	(N=1216)
Gender				
Man	4626 (68.8%)	2355 (75.1%)	1886 (79.4%)	385 (31.7%)
Woman	1217 (18.1%)	693 (22.1%)	419 (17.6%)	105 (8.6%)
Missing	885 (13.2%)	89 (2.8%)	70 (2.9%)	726 (59.7%)
Race				
Asian	81 (1.2%)	38 (1.2%)	39 (1.6%)	4 (0.3%)
Black	695 (10.3%)	535 (17.1%)	107 (4.5%)	53 (4.4%)
Caucasian	4910 (73.0%)	2275 (72.5%)	2125 (89.5%)	510 (41.9%)
Hispanic	418 (6.2%)	273 (8.7%)	95 (4.0%)	50 (4.1%)
Other	8 (0.1%)	8 (0.3%)	0 (0%)	0 (0%)
Missing	616 (9.2%)	8 (0.3%)	9 (0.4%)	599 (49.3%)
PID				
Democratic	3137 (46.6%)	3137 (100%)	0 (0%)	0 (0%)
Republican	2375 (35.3%)	0 (0%)	2375 (100%)	0 (0%)
PID Unknown	1216 (18.1%)	0 (0%)	0 (0%)	1216 (100%)

Table S3: Summary of Mayoral Candidates Data (Two-Party Elections with 40% $\leq$ Democratic Voteshare $\leq$ 60% only)

	Total	Democratic	Republican
	(N=1120)	(N=560)	(N=560)
Gender			
Man	847 (75.6%)	392 (70.0%)	455 (81.3%)
Woman	240 (21.4%)	149 (26.6%)	91 (16.3%)
Missing	33 (2.9%)	19 (3.4%)	14 (2.5%)
Race			
Asian	11 (1.0%)	5 (0.9%)	6 (1.1%)
Black	102 (9.1%)	83 (14.8%)	19 (3.4%)
Caucasian	946 (84.5%)	429 (76.6%)	517 (92.3%)
Hispanic	55 (4.9%)	39 (7.0%)	16 (2.9%)
Other	2 (0.2%)	2 (0.4%)	0 (0%)
Missing	4 (0.4%)	2 (0.4%)	2 (0.4%)
PID			
Democratic	560 (50.0%)	560 (100%)	0 (0%)
Republican	560 (50.0%)	0 (0%)	560 (100%)
PID Unknown	0 (0%)	0 (0%)	0 (0%)

B RD Estimator

B.1 RDD Model

We use the `rdrobust` package, which identifies an optimal bandwidth and estimates a local polynomial nonparametric regression. This section provides additional detail on that estimator, using notation and equations as described in Calonico et al. (2014).

Consider a setting with a continuous variable X_i that determines whether a unit is treated or not. A unit is treated if $X_i > \bar{x}$ and is in the control group otherwise. In our setting, X_i is the two-party vote-share and $\bar{x}$=0.5. "Treatment" is the election of a Democratic mayor.

The RD estimator of polynomial order p can be describes as:

$$\hat{\tau}_p(h_n) = \hat{\mu_{+,p}}(h_n) - \hat{\mu_{-,p}}(h_n) \tag{S1}$$

where h is the bandwidth being used, $\hat{\mu_{+,p}}(h_n)$ is the intercept at the threshold $\bar{x}$ for treated units, and $\hat{\mu_{-,p}}(h_n)$ is the intercept at the threshold $\bar{x}$ for control units – with the positive or negative signs denoting approaching the threshold from the right or left, respectively.

More specifically, $\hat{\mu_{+,p}}(h_n) = \mathbf{e_0}'\hat{\beta}_{+,p}$ where:

$$\hat{\beta}_{+,p} = \underset{\beta}{\operatorname{argmin}} \sum_{i=1}^{n} \mathbb{1}(X_i > \bar{x})\{Y_i - r_p(X_i - \bar{x})'\beta\}^2 K_{h_n}(X_i - \bar{x}) \tag{S2}$$

This is all generally the same for $\hat{\mu_{-,p}}(h_n)$ and $\hat{\beta}_{-,p}$, except that $\mathbb{1}(X_i > \bar{x})$ is replaced with $\mathbb{1}(X_i < \bar{x})$. Following Calonico et al.'s notation, $r_p(x) = (1, x, ..., x^p)'$ and $\mathbf{e_0} = (1, 0, ..., 0) \in \mathbb{R}^{p+1}$. K_{h_n} is a kernel function and h_n is a bandwidth.

B.2 Continuity of Observations

In this appendix we present the results of the McCrary test for the continuity of the density of observations across the 50% vote threshold. These tests replicate the RDD framework but using the density of observations as the outcome. If the density of observations were to have a "jump" in numbers across the threshold, it would suggest a potential violation of the assumption that potential outcomes are continuous at the threshold.

In Table S4 below we present the results of the traditional McCrary test using the number of observations within half-percentage-point bins of voteshare. The coefficient in the second line, indicating the change in the number of observations at the threshold, represents the RDD effect on this outcome. We find a null effect, suggesting that the continuity assumption is likely to hold in this context.

Table S4: McCrary Tests

	Dependent variable:
	Number of observations in bin
Voteshare bin	61.265***
	(14.719)
Voteshare ≥ 0.5	0.329
	(1.382)
Voteshare bin $\times$ Voteshare ≥ 0.5	-101.779***
	(20.816)
Constant	11.479***
	(0.977)
Observations	46
R^2	0.415
Note:	*p<0.1; **p<0.05; ***p<0.01

We also present these results visually in Figure S3, which shows the binned number of observations both below and above the 50% vote threshold. Visual inspection corroborates the quantitative results shown in Table S4 that there is no statistically detectable effect on the density of observations at the threshold.

A further check suggested by (*87*) involves conducting a nonparametric test for a discontinuity in the density of the running variable that does not require binning. We present the results from this nonparametric test, estimated using the `R` package `rddensity`, in Table S5 below. Similar to the test discussed earlier, this nonparametric test indicates no evidence of sorting across the threshold.

Table S5: Nonparametric Density Tests

t.statistic	p.value	Effective.N
0.29	0.77	599

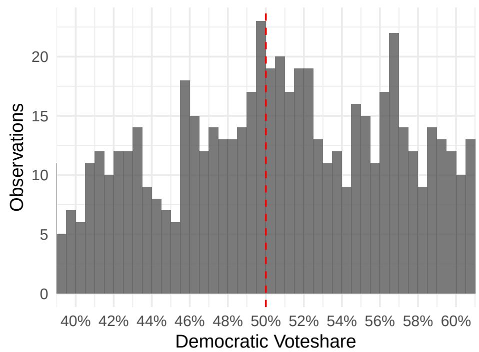


Figure S3: Histograms of observations within half percentage-point bins

In addition, Hartman suggests constructing an equivalence test based on the density of the forcing variable and calculating inverted p-values based on the null hypothesis of a difference in the density to the left and the right of the cutpoint (*88*). We present results using this method in Table S6 below, which show the observed ratio between the density to the left and right of the threshold as well as the equivalence confidence interval and the p-value for the null hypothesis of a jump of greater than 50% in the density across the threshold. This test on our mayoral elections data indicates that the null hypothesis of a substantively important difference in densities can be rejected at the 90% confidence level. More importantly, the equivalence confidence interval suggests that the range in the substantive size of the difference in density across the threshold is fairly small as well.

Table S6: Density Equivalence Tests

Observed.Ratio	Equivalence.Confidence.Interval	p.value
0.94	(0.64, 1.56)	0.07

C Placebo Results

In this section, we assess the effects of mayoral partisanship on several placebo outcomes for each of our research designs. For the RDD, this takes the form of examining the effect of treatment (i.e. narrow mayoral electoral wins by one party over the other) on pre-treatment outcomes. For the DID designs, we analyze pre-treatment trends in outcomes.

C.1 RDD Placebo Tests

For our RDD, we look at the effect of partisanship on lagged versions of the running variable (the democratic share of the vote 4 years prior) in Figure S4; the overall numbers of crimes (Figure S5), arrests (Figure S6), the clearance rate (Figure S7), and the Black share of total arrests (Figure S8).

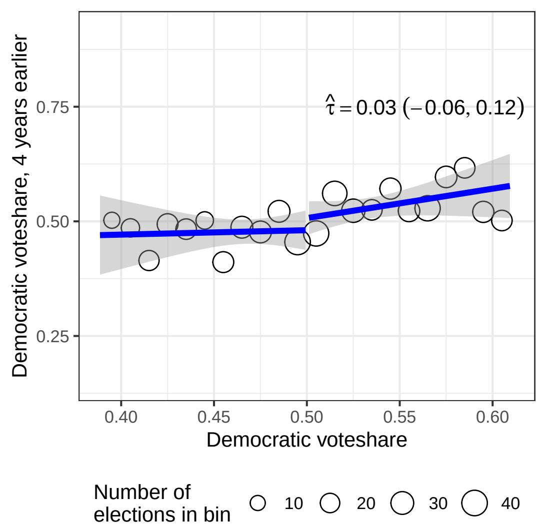


Figure S4: Placebo effect of partisanship on lagged democratic voteshare.

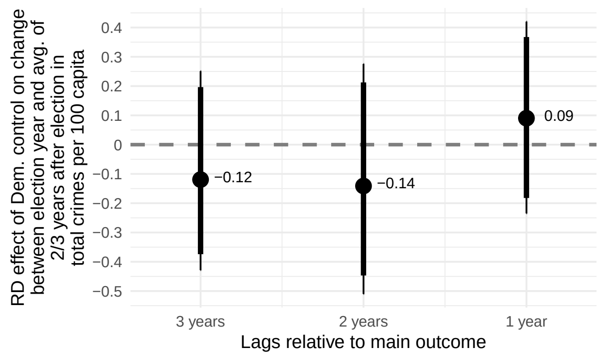


Figure S5: Placebo effect of partisanship on pre-treatment total crimes per 100 capita. Lines indicate robust bias-corrected 90% (thick lines) and 95% (thin lines) confidence intervals.

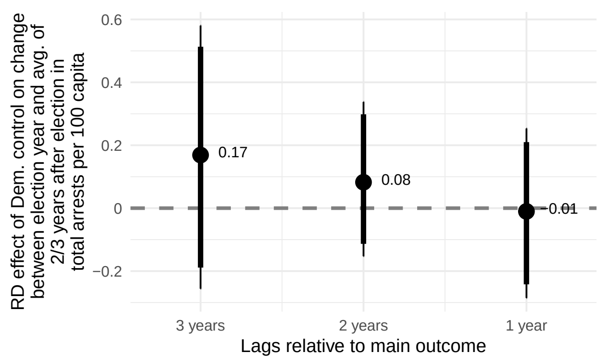


Figure S6: Placebo effect of partisanship on pre-treatment total arrests per 100 capita. Lines indicate robust bias-corrected 90% (thick lines) and 95% (thin lines) confidence intervals.

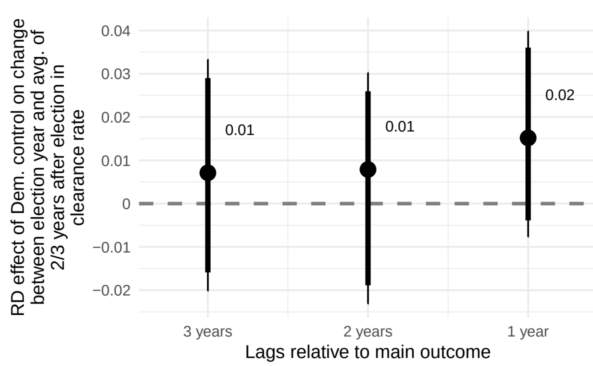


Figure S7: Placebo effect of partisanship on pre-treatment clearance rate. Lines indicate robust bias-corrected 90% (thick lines) and 95% (thin lines) confidence intervals.

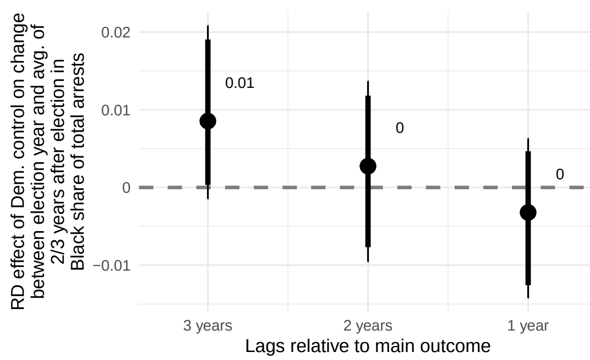


Figure S8: Placebo effect of partisanship on pre-treatment Black share of total arrests. Lines indicate robust bias-corrected 90% (thick lines) and 95% (thin lines) confidence intervals.

C.2 PanelMatch Placebo Tests

For our analyses using PanelMatch, we look at the effect of partisanship on pre-treatment outcomes: total police spending, police employment, total crimes, total arrests, and the Black share of total arrests. Most of these analyses pass placebo tests.

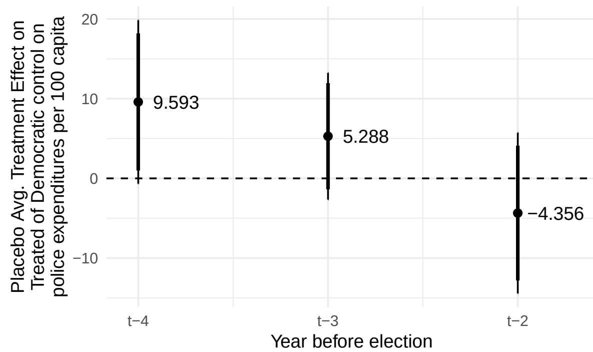


Figure S9: Placebo effect of partisanship on pre-treatment police expenditures. Lines indicate 90% (thick lines) and 95% (thin lines) bootstrapped confidence intervals.

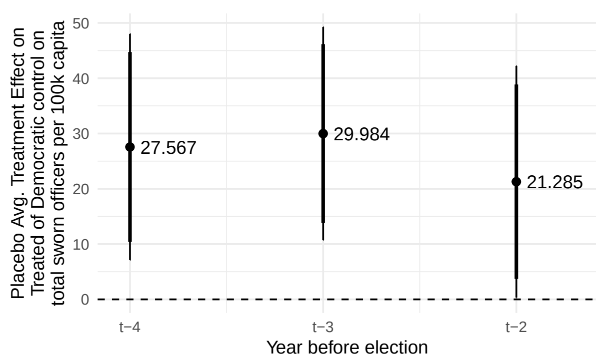


Figure S10: Placebo effect of partisanship on pre-treatment police employment. Lines indicate 90% (thick lines) and 95% (thin lines) bootstrapped confidence intervals.

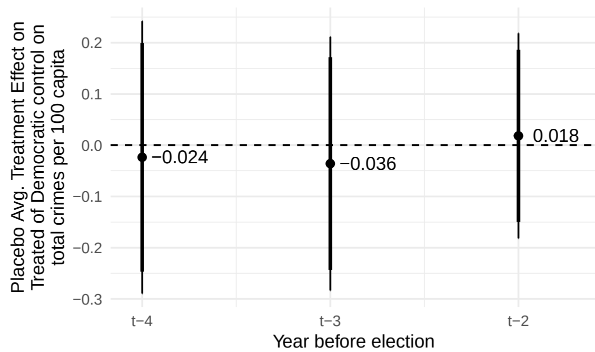


Figure S11: Placebo effect of partisanship on pre-treatment total crimes per 100 capita. Lines indicate 90% (thick lines) and 95% (thin lines) bootstrapped confidence intervals.

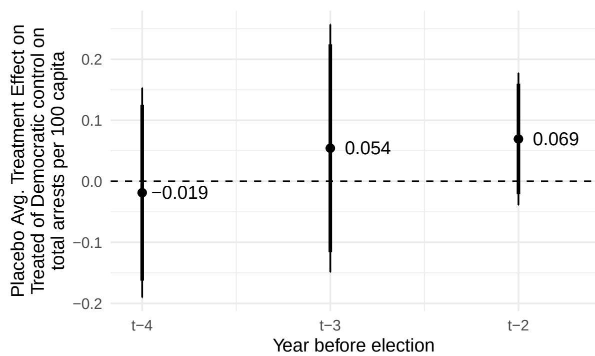


Figure S12: Placebo effect of partisanship on pre-treatment total arrests per 100 capita. Lines indicate 90% (thick lines) and 95% (thin lines) bootstrapped confidence intervals.

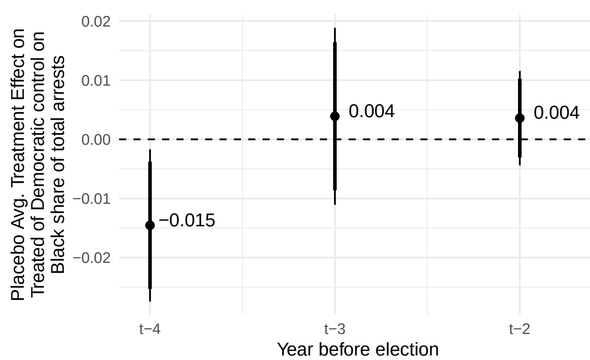


Figure S13: Placebo effect of partisanship on pre-treatment Black share of total arrests. Lines indicate 90% (thick lines) and 95% (thin lines) bootstrapped confidence intervals.

C.3 FEct Placebo Tests

For our analyses using FEct, we similarly look at the effect of partisanship on pre-treatment outcomes (total police spending, police employment, total crimes, total arrests, and the Black share of total arrests). None of the analyses show significant placebo effects and all of them pass equivalency tests, which validates the parallel trends assumption underlying the difference-in-differences design.

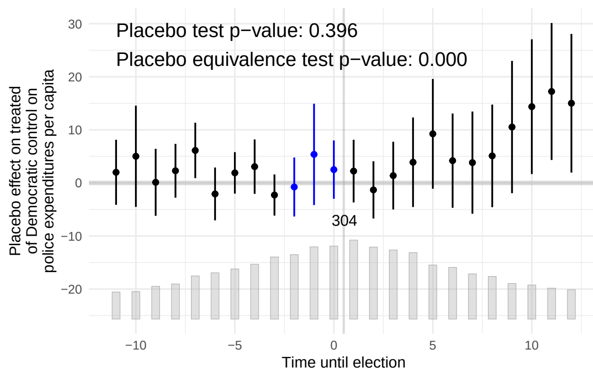


Figure S14: Placebo effect of partisanship on pre-treatment police expenditures. Lines indicate 90% (thick lines) and 95% (thin lines) bootstrapped confidence intervals.

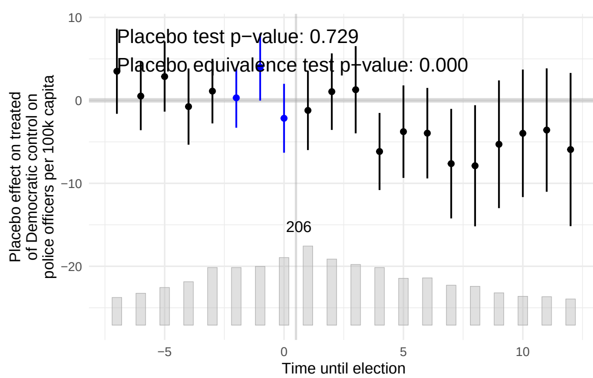


Figure S15: Placebo effect of partisanship on pre-treatment police employment. Lines indicate 90% (thick lines) and 95% (thin lines) bootstrapped confidence intervals.

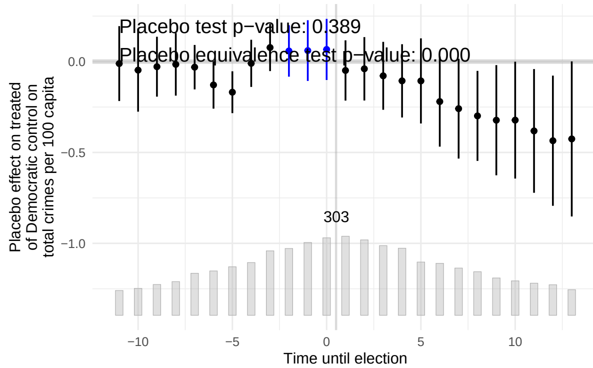


Figure S16: Placebo effect of partisanship on pre-treatment total crimes per 100 capita. Lines indicate 90% (thick lines) and 95% (thin lines) bootstrapped confidence intervals.

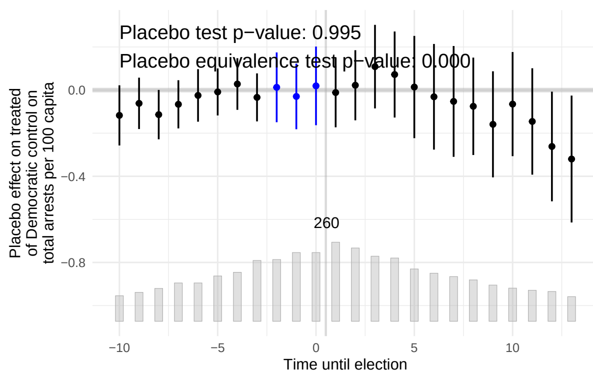


Figure S17: Placebo effect of partisanship on pre-treatment total arrests per 100 capita. Lines indicate 90% (thick lines) and 95% (thin lines) bootstrapped confidence intervals.

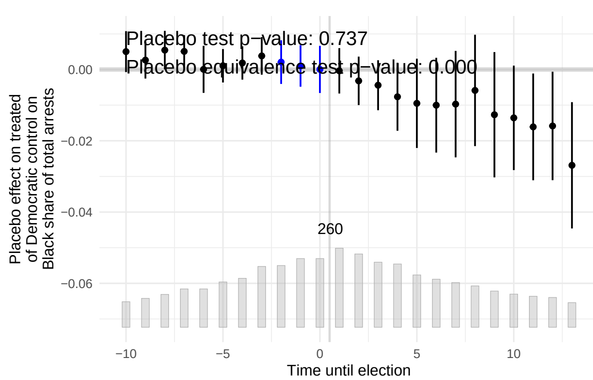


Figure S18: Placebo effect of partisanship on pre-treatment Black share of total arrests. Lines indicate 90% (thick lines) and 95% (thin lines) bootstrapped confidence intervals.

D Main Results in Tabular Format

In this appendix we present the results from the main analyses of the effect of mayoral partisanship on a number of outcomes. First, in Tables S7, S8, and S9 we present the results from Figure 1 in the main manuscript, showing the effects of partisanship on overall police employment and expenditures three years after the election. Then, in Tables S10, S11, S12, S13, S14, and S15, we present the results showing the effects of partisanship on police chief and police force demographics from Figure 2 in the main manuscript. Tables S16, S17, S18, S19, S20, and S21 display the full results analyzing the effect of mayoral partisanship on overall crime levels, clearance rates, and arrests from Figure 3 in the main manuscript.

Table S7: RDD Analyses: Employment and Municipal Expenditures

DV	Coef	p-value	BW	Obs
Total Sworn Officers per 100k capita (ASPEP)	-5.2	0.308	10.51	353
	(-17.135, 5.405)			
Police Exp. per capita	2.102	0.76	11.27	454
	(-20.631, 28.249)			
Corrections Exp. per capita	2.603	0.629	14.14	527
	(-8.625, 14.273)			

Table S8: PanelMatch Analyses: Employment and Municipal Expenditures

Outcome	Estimate	95% CI	N Matched Obs.
Total sworn officers per 100k capita	-2.93	(-7.14, 1.65)	147
Police expenditures per capita ($)	2.41	(-9.02, 14.67)	219
Corrections expenditures per capita ($)	1.03	(-1.15, 3.77)	219

Table S9: FEct Analyses: Employment and Municipal Expenditures

Outcome	Estimate	95% CI	N Obs.
Total sworn officers per 100k capita	0.52	(-3.49, 4.52)	213
Police expenditures per capita ($)	3.53	(-2.73, 9.79)	309
Corrections expenditures per capita ($)	0.38	(-2.39, 3.16)	309

Table S10: RDD Analyses: Police Chief Demographics

DV	Coef	p-value	BW	Obs
Black police chief	0.111	0.245	12.67	247
	(-0.078, 0.304)			
Hispanic police chief	-0.072	0.417	8.64	181
	(-0.339, 0.141)			
White police chief	-0.048	0.874	11.45	234
	(-0.26, 0.221)			
Woman police chief	-0.012	0.911	11.87	228
	(-0.094, 0.106)			

Table S11: PanelMatch Analyses: Police Chief Demographics

Outcome	Estimate	95% CI	N Matched Obs.
Prob. Black police chief	-0.06	(-0.14, 0.01)	78
Prob. Hispanic police chief	0.00	(-0.06, 0.06)	78
Prob. white police chief	0.02	(-0.08, 0.12)	78
Prob. woman police chief	0.04	(-0.01, 0.1)	76

Table S12: FEct Analyses: Police Chief Demographics

Outcome	Estimate	95% CI	N Obs.
Prob. Black police chief	-0.00	(-0.06, 0.06)	75
Prob. Hispanic police chief	-0.01	(-0.06, 0.04)	75
Prob. white police chief	-0.01	(-0.1, 0.08)	75
Prob. woman police chief	0.04	(-0.01, 0.09)	75

Table S13: RDD Analyses: Police Officer Demographics

DV	Coef	p-value	BW	Obs
Black share of police force	0.009	0.044	14.22	351
	(0, 0.021)			
Hispanic share of police force	-0.003	0.52	13.91	347
	(-0.019, 0.01)			
White share of police force	0.035	0.13	13.74	345
	(-0.013, 0.1)			
Women share of police force (LEOKA)	0	0.688	9.82	443
	(-0.006, 0.009)			

Table S14: PanelMatch Analyses: Police Officer Demographics

Outcome	Estimate	95% CI	N Matched Obs.
Black share of police force	-0.00	(0, 0)	230
Hispanic share of police force	0.00	(0, 0.01)	230
White share of police force	-0.00	(-0.01, 0.01)	230
Women share of police force (LEOKA)	0.00	(0, 0)	242

Table S15: FEct Analyses: Police Officer Demographics

Outcome	Estimate	95% CI	N Obs.
Black share of police force	0.00	(0, 0.01)	282
Hispanic share of police force	-0.00	(0, 0)	282
White share of police force	-0.01	(-0.01, 0)	282
Women share of police force (LEOKA)	0.00	(0, 0)	317

Table S16: RDD Analyses: Crime

DV	Coef	p-value	BW	Obs
All crimes per 100 capita	-0.05 (-0.621, 0.429)	0.72	10.23	443
Total index crimes per 100 capita	0.09 (-0.4, 0.547)	0.76	10.92	464
Violent index crimes per 100 capita	0.038 (-0.029, 0.121)	0.229	8.42	383
Property index crimes per 100 capita	-0.012 (-0.466, 0.399)	0.879	10.72	453
Clearance rate	0.008 (-0.023, 0.042)	0.564	10.1	445

Table S17: PanelMatch Analyses: Crime

Outcome	Estimate	95% CI	N Matched Obs.
All crimes per 100 capita	0.01	(-0.19, 0.21)	237
Total index crimes per 100 capita	0.00	(-0.18, 0.19)	238
Violent index crimes per 100 capita	-0.02	(-0.05, 0.01)	236
Property index crimes per 100 capita	-0.00	(-0.17, 0.18)	239
Clearance rate	-0.00	(-0.02, 0.01)	241

Table S18: FEct Analyses: Crime

Outcome	Estimate	95% CI	N Obs.
All crimes per 100 capita	-0.05	(-0.21, 0.11)	307
Total index crimes per 100 capita	-0.04	(-0.19, 0.1)	311
Violent index crimes per 100 capita	-0.02	(-0.04, 0)	307
Property index crimes per 100 capita	-0.03	(-0.17, 0.11)	311
Clearance rate	-0.00	(-0.01, 0.01)	318

Table S19: RDD Analyses: Arrests

DV	Coef	p-value	BW	Obs
Total arrests per 100 capita	-0.006	0.773	8.33	290
	(-0.368, 0.274)			
Violent crime arrests per 100 capita	0.038	0.469	12.34	383
	(-0.057, 0.123)			
Property crime arrests per 100 capita	0.013	0.727	11.96	379
	(-0.042, 0.06)			
Drug crime arrests per 100 capita	-0.066	0.229	9.61	330
	(-0.217, 0.052)			
Other crime arrests per 100 capita	0.032	0.885	9.42	322
	(-0.201, 0.234)			

Table S20: PanelMatch Analyses: Arrests

Outcome	Estimate	95% CI	N Matched Obs.
Total arrests per 100 capita	0.01	(-0.13, 0.15)	158
Violent crime arrests per 100 capita	0.03	(-0.01, 0.07)	158
Property crime arrests per 100 capita	-0.02	(-0.06, 0.02)	158
Drug crime arrests per 100 capita	0.01	(-0.04, 0.05)	158
Other crime arrests per 100 capita	-0.02	(-0.12, 0.08)	158

Table S21: FEct Analyses: Arrests

Outcome	Estimate	95% CI	N Obs.
Total arrests per 100 capita	0.06	(-0.1, 0.21)	249
Violent crime arrests per 100 capita	-0.00	(-0.04, 0.03)	249
Property crime arrests per 100 capita	-0.00	(-0.04, 0.03)	249
Drug crime arrests per 100 capita	-0.00	(-0.04, 0.04)	249
Other crime arrests per 100 capita	0.07	(-0.01, 0.15)	249

E Alternative Employment Outcome Measurements

In the main manuscript (Figure 1) we present RDD analyses of the effect of Democratic control on policing employment using the Census Bureau's Annual Survey of Public Employment and Payroll (ASPEP). However, both the LEMAS and LEOKA datasets also track the number of sworn police officers employed by a police force. We also analyze these independent measurements of employment, and present results including these alternative data sources alongside our finances results in Figure S19 and Table S22 below.

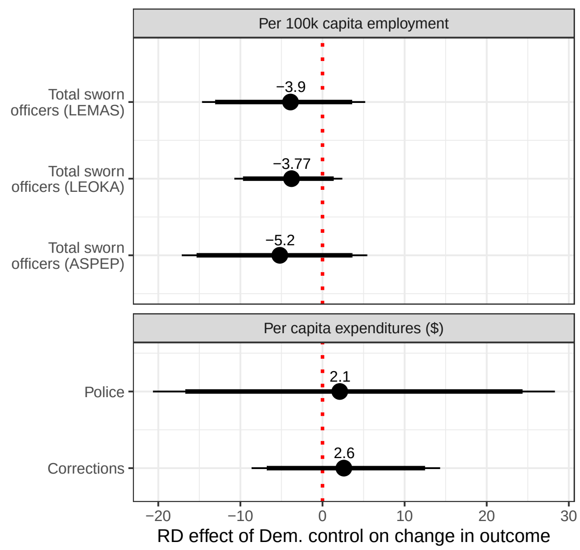


Figure S19: The effect of mayoral partisanship on municipal police employment and criminal justice spending including alternative employment outcome measurements. Points indicate estimates from the regression discontinuity design using the robust bandwidth selection procedure estimated with `rdrobust` and lines indicate robust bias-corrected 90% (thick lines) and 95% (thin lines) confidence intervals.

Table S22: RDD Analyses: Employment and Municipal Expenditures

DV	Coef	p-value	BW	Obs
Total Sworn Officers per 100k capita (LEMAS)	-3.898	0.346	9.13	406
	(-14.657, 5.145)			
Total Sworn Officers per 100k capita (LEOKA)	-3.774	0.21	8.15	372
	(-10.716, 2.361)			
Total Sworn Officers per 100k capita (ASPEP)	-5.2	0.308	10.51	353
	(-17.135, 5.405)			
Police Exp. per capita	2.102	0.76	11.27	454
	(-20.631, 28.249)			
Corrections Exp. per capita	2.603	0.629	14.14	527
	(-8.625, 14.273)			

F Analyses of Racialized Arrest Patterns

As we describe in the manuscript, we also examine the effects of partisan control on racialized arrest patterns, as measured by the share of people arrested for crimes who are Black. We present descriptive statistics for these variables (the Black share of arrests for all types of crimes, as well as a number of sub-types of crimes) in Table S23.

Table S23: Summary Statistics for Racialized Arrest Outcome Variables

	Total	Democratic	Republican	PID Unknown
	(N=12436)	(N=5757)	(N=4143)	(N=2536)
Black share of total arrests				
Mean (SD)	0.285 (0.228)	0.368 (0.244)	0.207 (0.172)	0.228 (0.212)
Median [Min, Max]	0.219 [0, 0.916]	0.344 [0, 0.916]	0.156 [0, 0.873]	0.140 [0, 0.861]
Missing	2748 (22.1%)	1332 (23.1%)	847 (20.4%)	569 (22.4%)
Black share of violent crime arrests				
Mean (SD)	0.331 (0.240)	0.411 (0.252)	0.256 (0.191)	0.277 (0.232)
Median [Min, Max]	0.280 [0, 1.07]	0.401 [0, 1.07]	0.216 [0, 0.919]	0.207 [0, 0.905]
Missing	2750 (22.1%)	1333 (23.2%)	848 (20.5%)	569 (22.4%)
Black share of property crime arrests				
Mean (SD)	0.317 (0.237)	0.396 (0.252)	0.244 (0.190)	0.262 (0.220)
Median [Min, Max]	0.269 [0, 1.00]	0.381 [0, 0.948]	0.193 [0, 0.894]	0.202 [0, 1.00]
Missing	2749 (22.1%)	1332 (23.1%)	847 (20.4%)	570 (22.5%)
Black share of drug crime arrests				
Mean (SD)	0.314 (0.258)	0.401 (0.270)	0.228 (0.199)	0.262 (0.256)
Median [Min, Max]	0.241 [0, 1.00]	0.376 [0, 1.00]	0.169 [0, 0.914]	0.158 [0, 0.952]
Missing	2780 (22.4%)	1342 (23.3%)	853 (20.6%)	585 (23.1%)
Black share of other crime arrests				
Mean (SD)	0.229 (0.205)	0.307 (0.225)	0.157 (0.144)	0.173 (0.181)
Median [Min, Max]	0.160 [0, 1.00]	0.265 [0, 0.921]	0.107 [0, 1.00]	0.0922 [0, 0.833]
Missing	2748 (22.1%)	1332 (23.1%)	847 (20.4%)	569 (22.4%)
Mayoral party in power				
Democratic	5757 (46.3%)	5757 (100%)	0 (0%)	0 (0%)
Republican	4143 (33.3%)	0 (0%)	4143 (100%)	0 (0%)
PID Unknown	2536 (20.4%)	0 (0%)	0 (0%)	2536 (100%)

The results from the analyses using all three of our research designs looking at the changes in outcomes between the election year and three years after the election are shown in Figure S20 and Tables S24, S25, and S26.

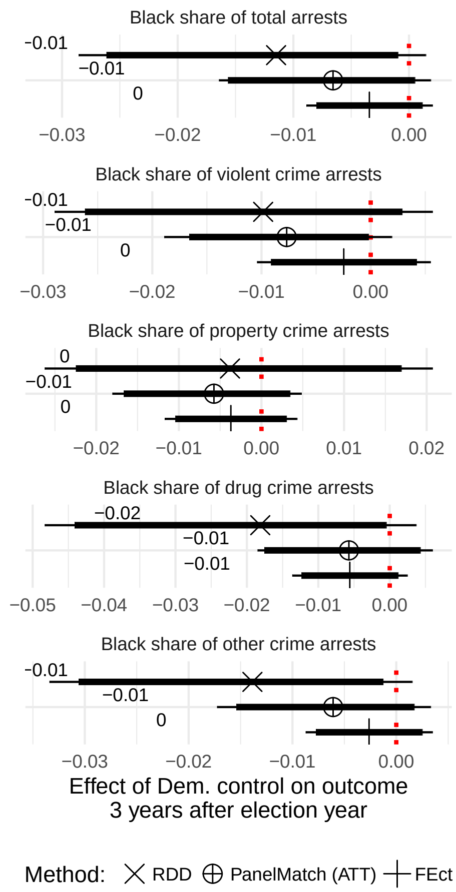


Figure S20: The effect of mayoral partisanship on the change in the Black share of arrests between the election year and three years after the election. Points indicate causal effect estimates from each of our three research designs: from the regression discontinuity design using the robust bandwidth selection procedure estimated with `rdrobust` (stars); from PanelMatch (crossed circles); and `fect` (vertical lines), and horizontal lines indicate 90% (thick lines) and 95% (thin lines) confidence intervals, using robust bias-corrected confidence intervals for the RDD.

Table S24: RDD Analyses: Racial Composition of Arrests (Shares)

DV	Coef	p-value	BW	Obs
Black share of all arrests	-0.011	0.077	10.29	344
	(-0.029, 0.001)			
Black share of violent crime arrests	-0.01	0.187	11.04	362
	(-0.029, 0.006)			
Black share of property crime arrests	-0.004	0.817	14.35	428
	(-0.026, 0.021)			
Black share of drug crime arrests	-0.018	0.093	12.23	380
	(-0.048, 0.004)			
Black share of other crime arrests	-0.014	0.074	11.22	366
	(-0.033, 0.002)			

Table S25: PanelMatch Analyses: Racial Composition of Arrests (Shares)

Outcome	Estimate	95% CI	N Matched Obs.
Black share of total arrests	-0.01	(-0.02, 0)	158
Black share of violent crime arrests	-0.01	(-0.02, 0)	158
Black share of property crime arrests	-0.01	(-0.02, 0)	158
Black share of drug crime arrests	-0.01	(-0.02, 0.01)	158
Black share of other crime arrests	-0.01	(-0.02, 0)	158

Table S26: FEct Analyses: Racial Composition of Arrests (Shares)

Outcome	Estimate	95% CI	N Obs.
Black share of total arrests	-0.00	(-0.01, 0)	249
Black share of violent crime arrests	-0.00	(-0.01, 0.01)	249
Black share of property crime arrests	-0.00	(-0.01, 0)	249
Black share of drug crime arrests	-0.01	(-0.01, 0)	249
Black share of other crime arrests	-0.00	(-0.01, 0)	249

F.1 Robustness Checks: Alternative Outcomes of Black Arrest Ratios and Per Capita Numbers of Black Arrests

In the previous section, we presented analyses of the effect of mayoral partisanship on the Black share of arrests. However, in this section we conduct RDD analyses using an alternative outcome of the natural log of the Black *ratio* of arrests – i.e. the number of Black arrests over the number of White arrests.

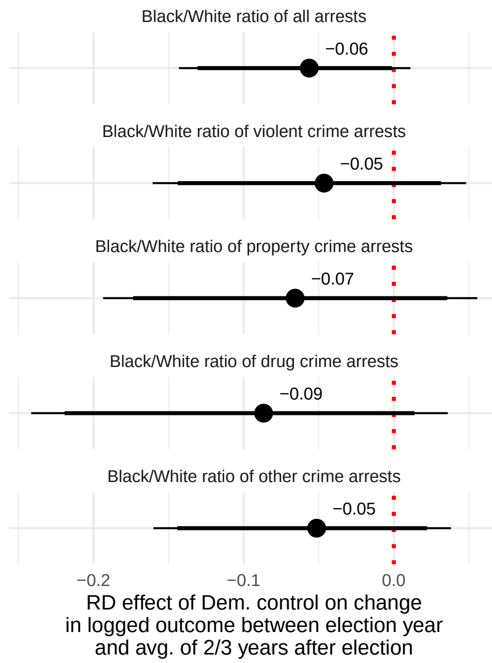


Figure S21: The effect of mayoral partisanship on the change in the natural log of the Black/White ratio of arrests two and three years after an election. Points indicate estimates from the RDD using change outcomes and using a robust bandwidth selection procedure estimated using `rdrobust`. Lines indicate robust bias-corrected 90% (thick lines) and 95% (thin lines) confidence intervals.

We also use an alternative outcome of the Black number of arrests (in per 100 capita terms) rather than the Black share of arrests in Figure S22 and Table S28.

Table S27: RDD Results: Racial Composition of Arrests (Ratios)

DV	Coef	p-value	BW	Obs
Black/White ratio of all arrests	-0.056 (-0.143, 0.011)	0.092	13.3	436
Black/White ratio of violent crime arrests	-0.046 (-0.161, 0.048)	0.289	12.82	423
Black/White ratio of property crime arrests	-0.066 (-0.194, 0.055)	0.276	16.37	495
Black/White ratio of drug crime arrests	-0.087 (-0.241, 0.036)	0.145	11.05	388
Black/White ratio of other crime arrests	-0.051 (-0.16, 0.038)	0.226	14.38	462

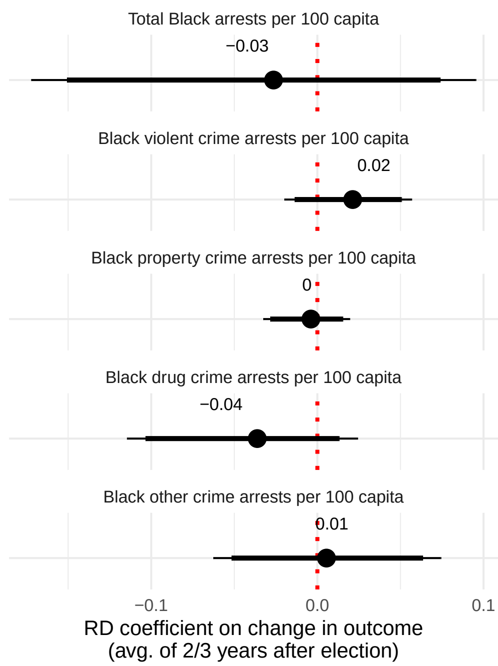


Figure S22: The effect of mayoral partisanship on the change in the per 100 capita number of Black arrests between the election year and the average of two and three years after the election. Points indicate estimates from the RDD using change outcomes and using a robust bandwidth selection procedure estimated using `rdrobust`. Lines indicate robust bias-corrected 90% (thick lines) and 95% (thin lines) confidence intervals.

Table S28: RDD Results: Racial Composition of Arrests (PC Number)

DV	Coef	p-value	BW	Obs
Total Black arrests per 100 capita	-0.026 (-0.172, 0.095)	0.574	8.62	317
Black violent crime arrests per 100 capita	0.021 (-0.02, 0.057)	0.345	11.1	390
Black property crime arrests per 100 capita	-0.004 (-0.033, 0.019)	0.622	12.08	407
Black drug crime arrests per 100 capita	-0.036 (-0.115, 0.024)	0.202	9.55	349
Black other crime arrests per 100 capita	0.005 (-0.063, 0.074)	0.867	9.28	341

G Robustness Checks for RDD

G.1 Alternative Polynomials

Though our main RDD results use first-order polynomials, as a robustness check, in this section we present the results of RDD analyses using higher order polynomials as well as a simple difference in averages within the optimally-selected bandwidth (i.e., a 0-order polynomial) between cities that elected a Democrat versus those that did not. The results are similarly null for our main crime, arrests, and clearance rate outcomes, as shown in Figure S23, indicating that our main results are not simply an artifact of functional form. We also find similarly suggestive negative effects on the racial composition of arrests, as shown in Figure S24.

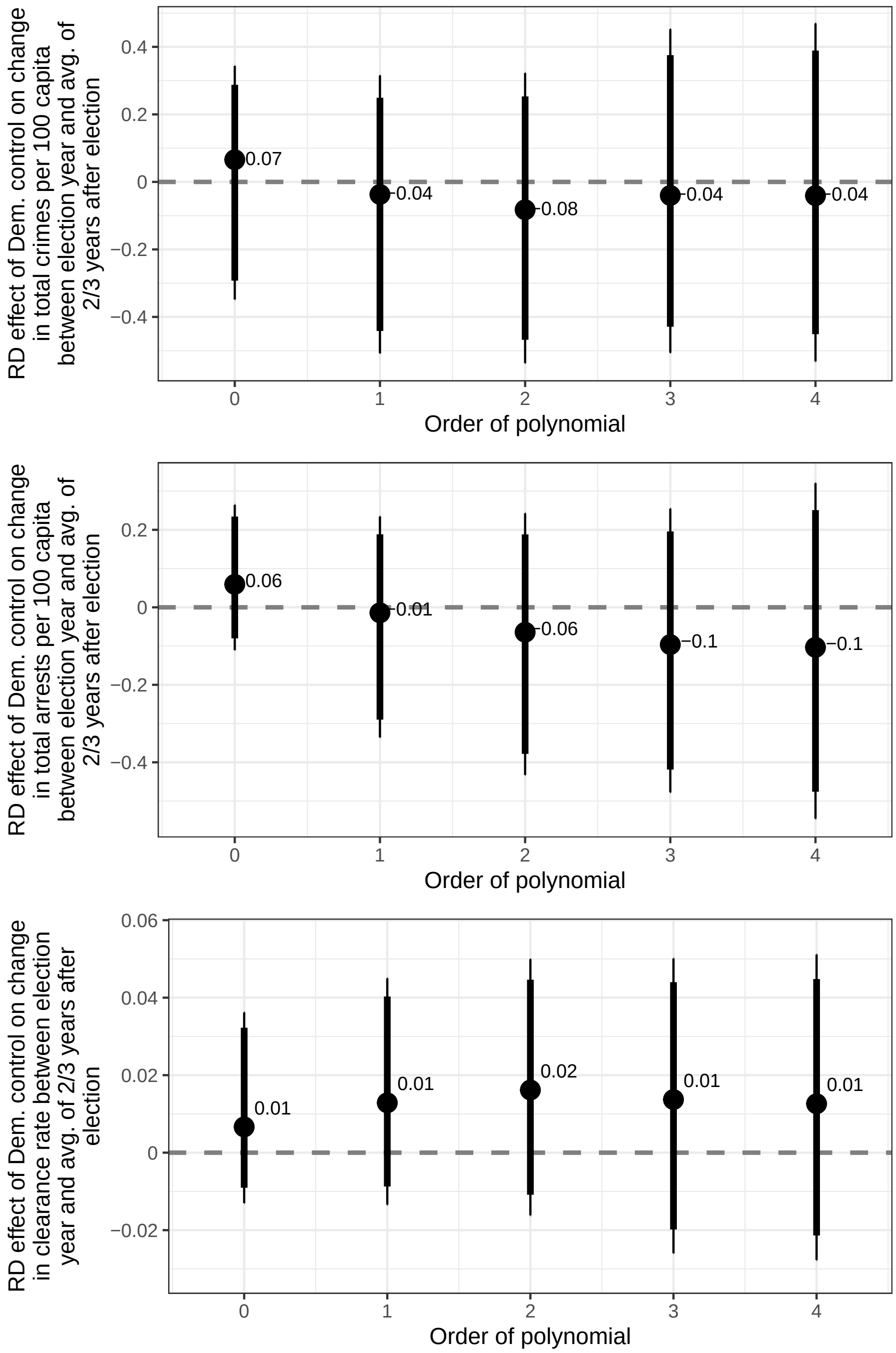


Figure S23: RDD effect on crime, arrests, and the clearance rate, with lower- and higher-order polynomials. Bars show 95% (thin lines) and 90% (thick lines) robust confidence intervals.

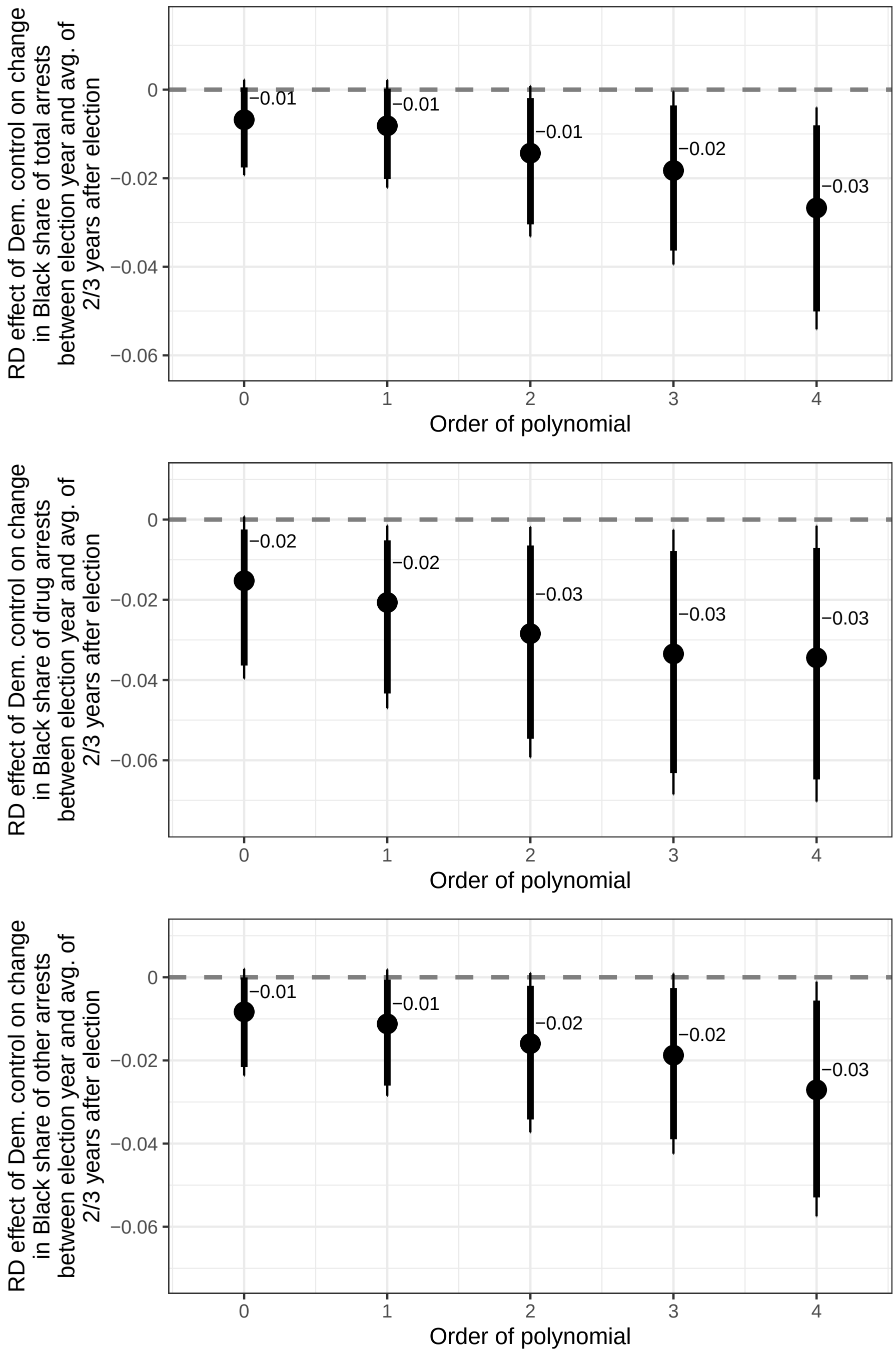
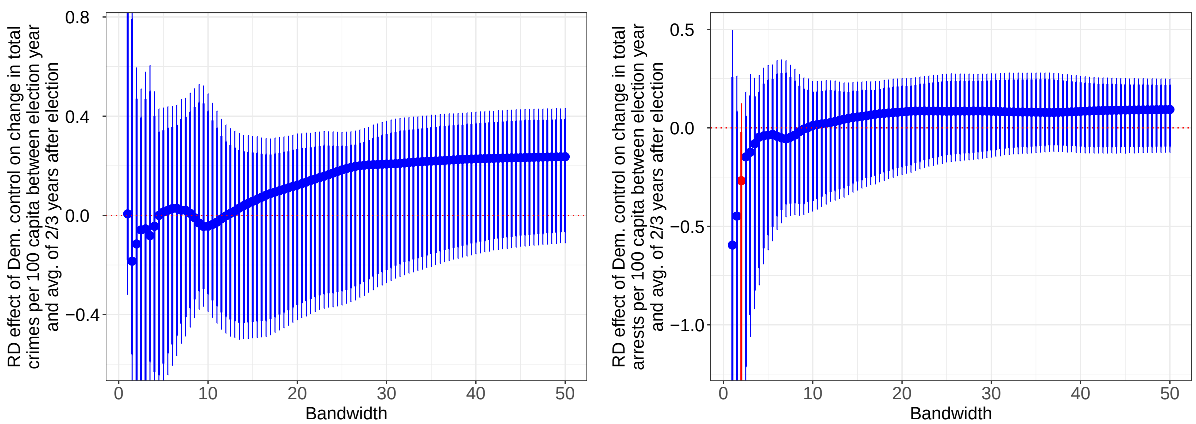


Figure S24: RDD effect on Black share of arrest types, with lower- and higher-order polynomials. Bars show 95% (thin lines) and 90% (thick lines) robust confidence intervals.

G.2 Robustness Checks: Alternative Bandwidths

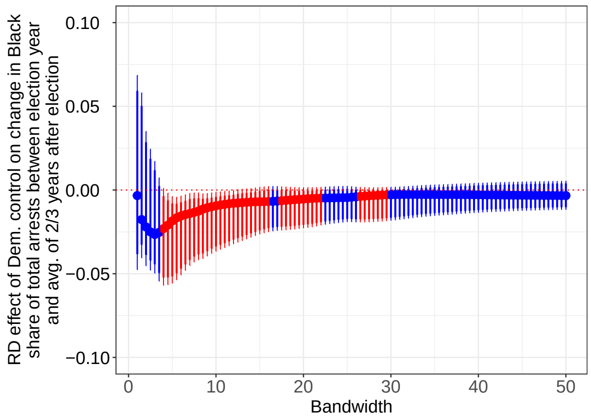
Though our main RDD results use the MSE-optimized bandwidth as selected by `rdrobust` (*77*), as another robustness check, in this section we present the results of analyses using alternative RDD bandwidths ranging from one percentage point on either side to 50 percentage points (i.e. the entire range of Democratic voteshare). The results for our main analyses of crime and arrests are similarly null for nearly all bandwidths, as shown in Figure S25. Meanwhile, the results for our analyses of the racialized patterns in arrest rates are similarly negative and statistically significant for at least some range of bandwidths, as shown in Figure S26.



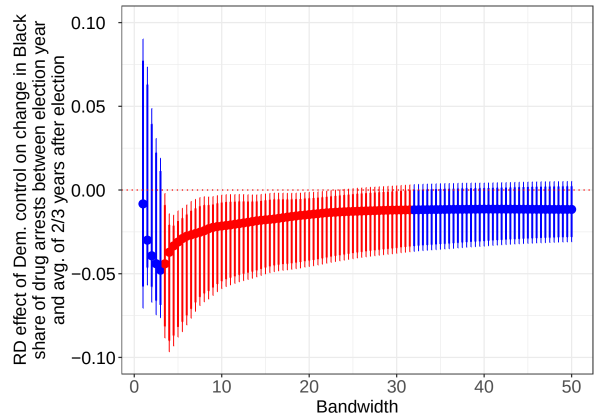
(a) Total crimes per 100 capita

(b) Total arrests per 100 capita

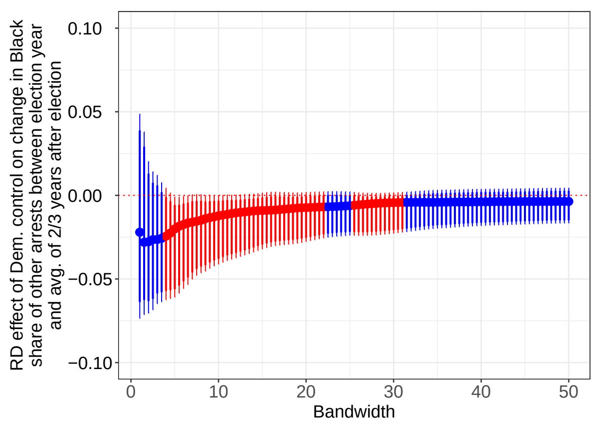
Figure S25: RDD effects on total crimes (panel a) and arrests (panel b) per 100 capita, with alternative bandwidths increasing by half a percentage point from 1 to 50 percentage points. Points indicate traditional point estimates and lines indicate bias-corrected 90% (thick lines) and 95% (thin lines) confidence intervals. Red points and lines indicate estimates that are significant at the 90% confidence level, while blue indicate those that are not.



(a) Black share of total arrests



(b) Black share of drug crime arrests



(c) Black share of other crime arrests

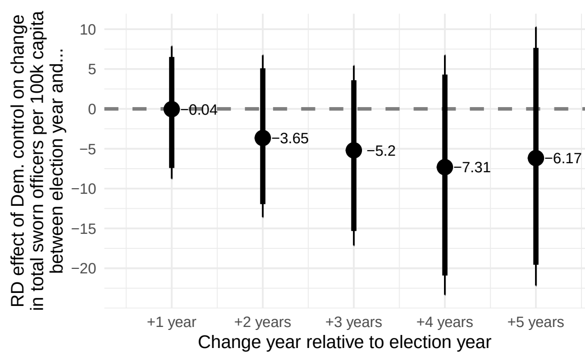
Figure S26: RDD effect on Black share of total arrests (panel a), drug crime arrests (panel b), and other crime arrests (panel c), with alternative bandwidths increasing by half a percentage point from 1 to 50 percentage points. Points indicate traditional point estimates and lines indicate bias-corrected 90% (thick lines) and 95% (thin lines) confidence intervals. Red points and lines indicate estimates that are significant at the 90% confidence level, while blue indicate those that are not.

H Long-run Effects of Partisanship

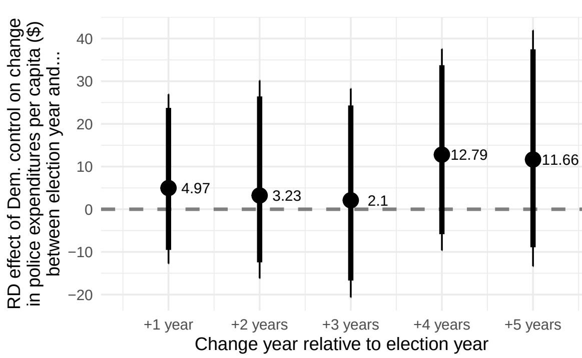
In this section, we test for the effect of mayoral partisanship on the outcomes that we examine using alternative outcome measures at different timelines after the election. For the RDD, we do this by calculating changes between the election and different post-election years and averaging over different post-election years, and for the two DID designs we do this by displaying dynamic treatment effect plots as well as pooled ATTs.

In the figures that follow, we conduct RDD and DID analyses similar to an event study, and assess the timeline in which effects appear using outcomes that measure (for the RDD) the change between the election year and a variety of different years after the election, or (for the DIDs) the outcomes in different years following the election. These analyses corroborate the findings in the body of our manuscript, and indicate no detectable impacts of partisanship on expenditures, staff, police chief and police force[1] demographics, crime, the clearance rate, arrests, or the racial composition of arrests. However, these analyses also suggest that – if there are any effects on the racial composition of arrests – they generally peak 4 years after the election. If partisanship influences these outcomes at all, changes in this outcome may take longer to appear due to time needed for more proximate policy levers to change beforehand.

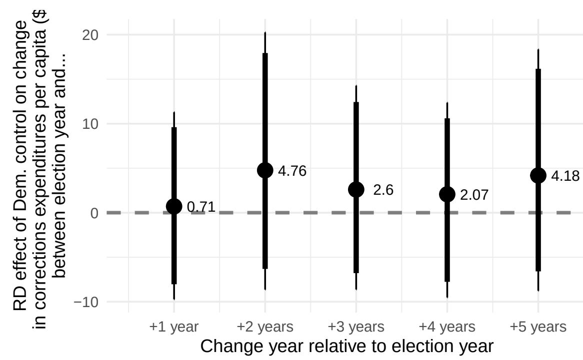
[1] Note that for police force demographics, the LEMAS survey data which contains these outcomes do not allow us to calculate changes between the election year and the individual years following the election, so we do not display the RDD results for these outcomes here, but our two DID designs make use of the single survey years for which we do have data following the election years without the need to calculate change outcomes.



(a) Total sworn officers per 100 capita

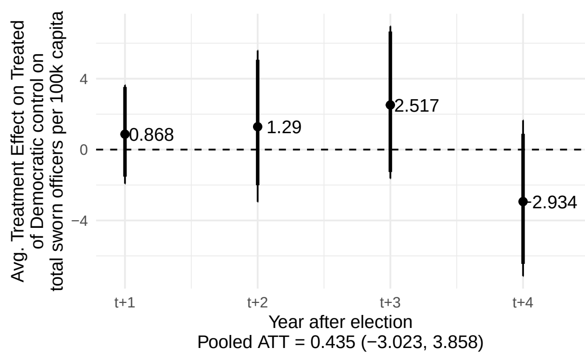


(b) Police expenditures per capita ($)

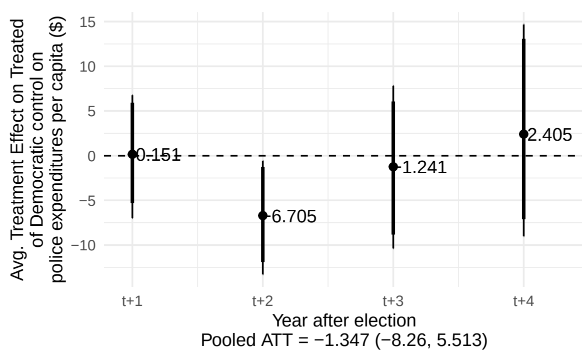


(c) Corrections expenditures per capita ($)

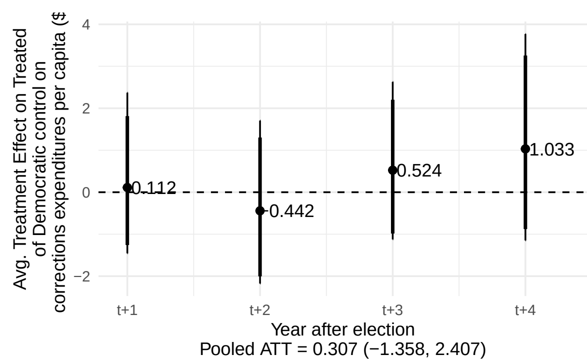
Figure S27: Long-term RDD effects of partisanship on the change in criminal justice staffing (panel a) and expenditures on police (panel b) and corrections (panel c). Thick bars show 90% robust confidence intervals and thin bars show 95% robust confidence intervals.



(a) Total sworn officers per 100 capita

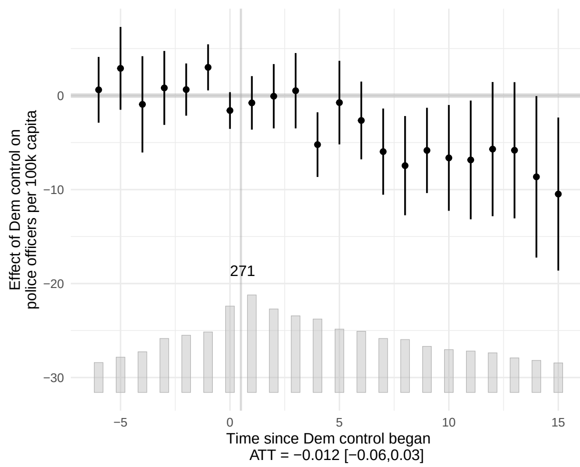


(b) Police expenditures per capita ($)

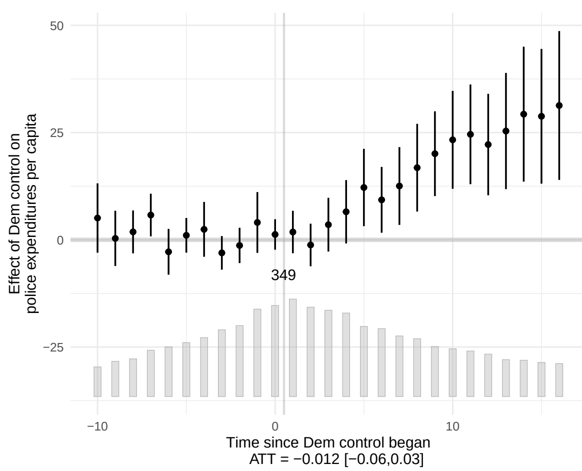


(c) Corrections expenditures per capita ($)

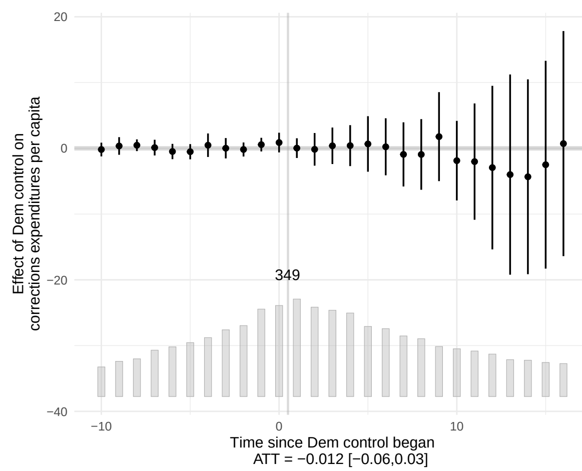
Figure S28: Long-term PanelMatch effects of partisanship on criminal justice staffing (panel a) and expenditures on police (panel b) and corrections (panel c). Thick bars show 90% robust confidence intervals and thin bars show 95% robust confidence intervals.



(a) Total sworn officers per 100 capita

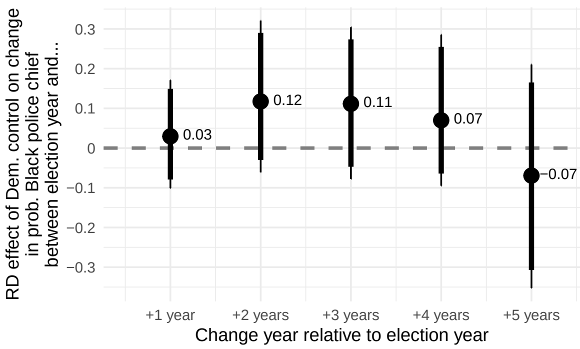


(b) Police expenditures per capita ($)

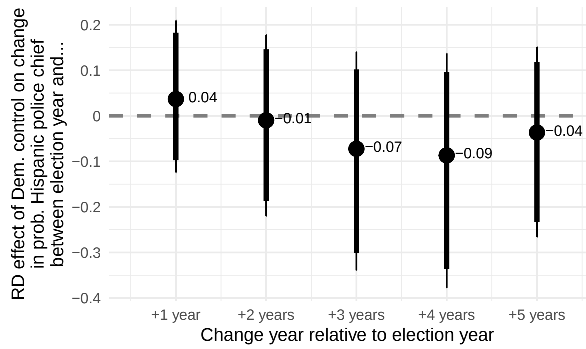


(c) Corrections expenditures per capita ($)

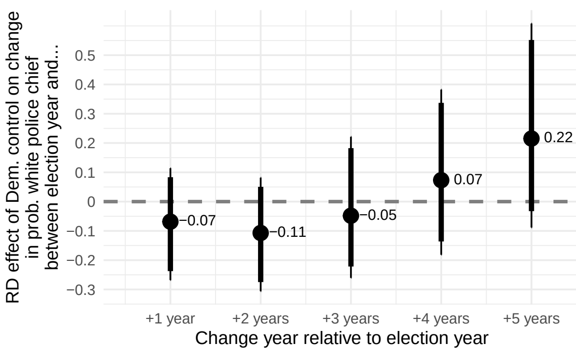
Figure S29: Long-term FEct effects of partisanship on criminal justice staffing (panel a) and expenditures on police (panel b) and corrections (panel c). Thick bars show 90% robust confidence intervals and thin bars show 95% robust confidence intervals.



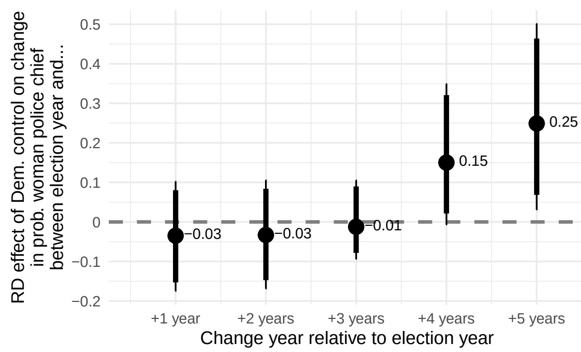
(a) Prob. Black police chief



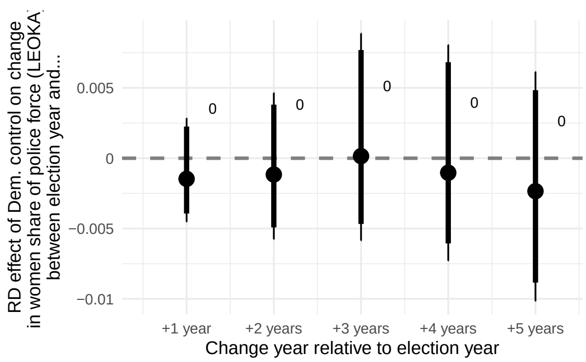
(b) Prob. Hispanic police chief



(c) Prob. white police chief

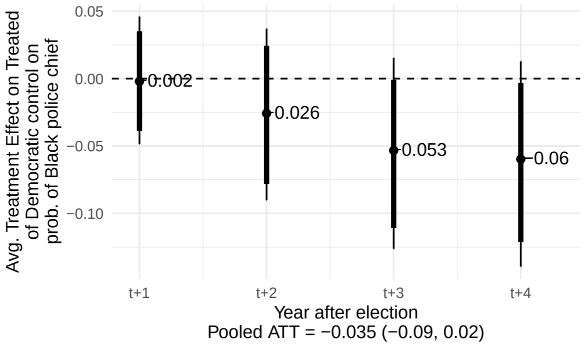


(d) Prob. woman police chief

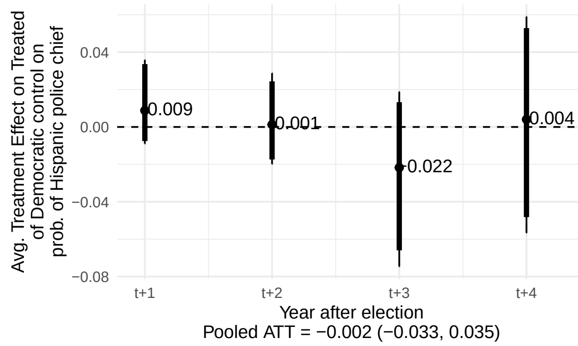


(e) Women share of police force (LEOKA)

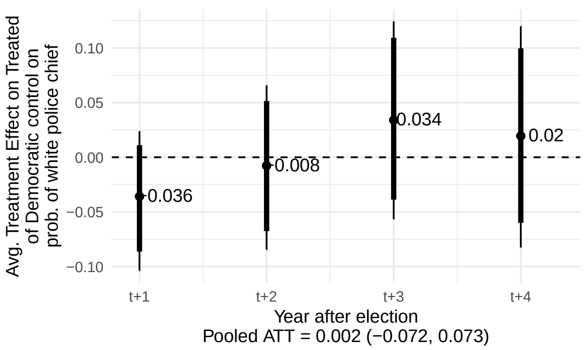
Figure S30: Long-term RDD effects of partisanship on the change in police chief (panels a-d) and police force (panel e) demographics. Thick bars show 90% robust confidence intervals and thin bars show 95% robust confidence intervals.



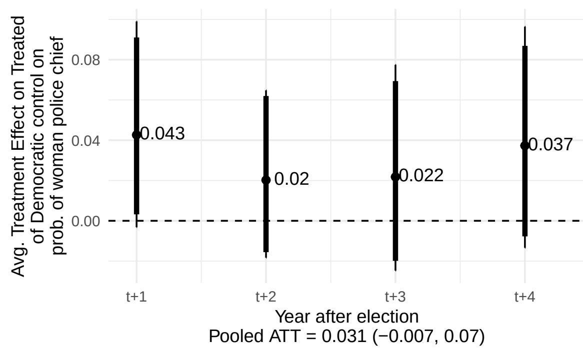
(a) Prob. Black police chief



(b) Prob. Hispanic police chief

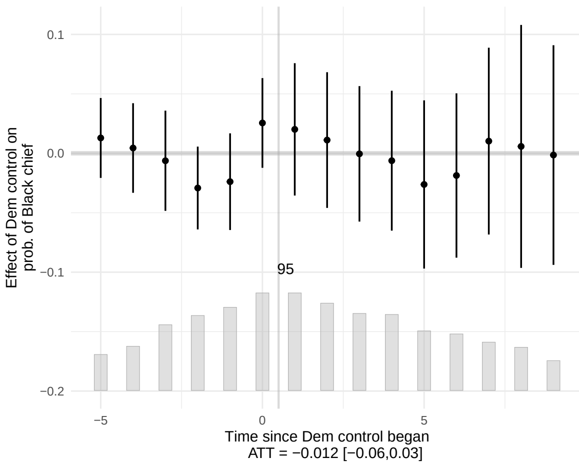


(c) Prob. white police chief

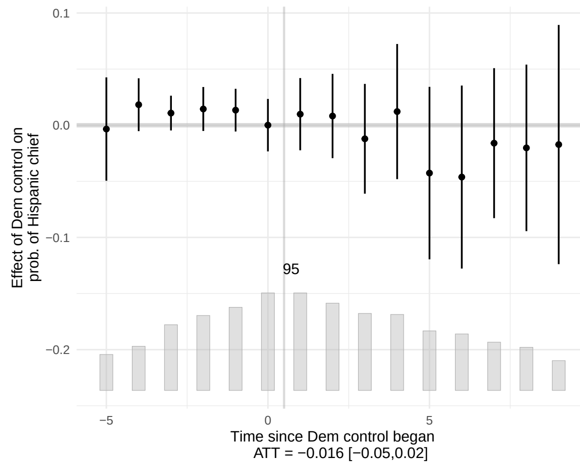


(d) Prob. woman police chief

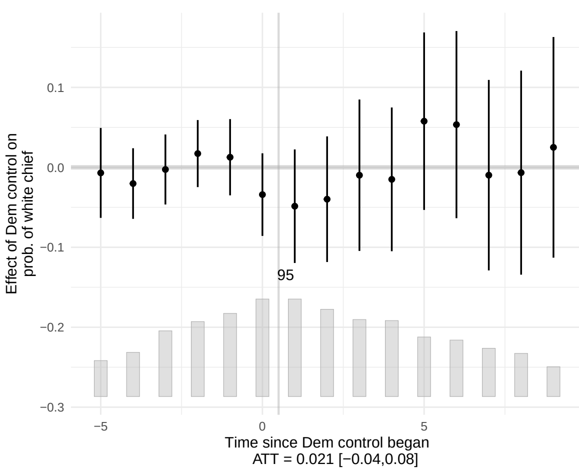
Figure S31: Long-term PanelMatch effects of partisanship on police chief demographics: the probability of a Black chief (panel a), Hispanic chief (panel b), white chief (panel c), and woman chief (panel d). Thick bars show 90% robust confidence intervals and thin bars show 95% robust confidence intervals.



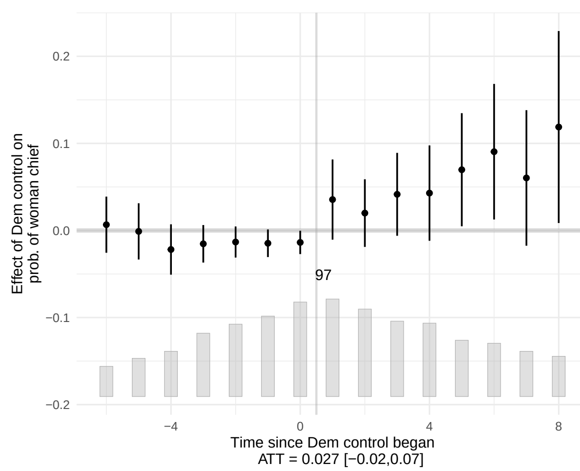
(a) Prob. Black police chief



(b) Prob. Hispanic police chief

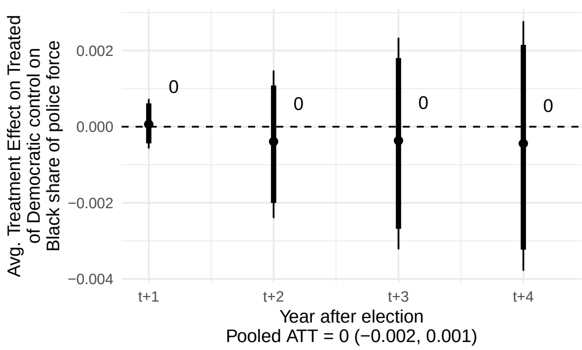


(c) Prob. white police chief

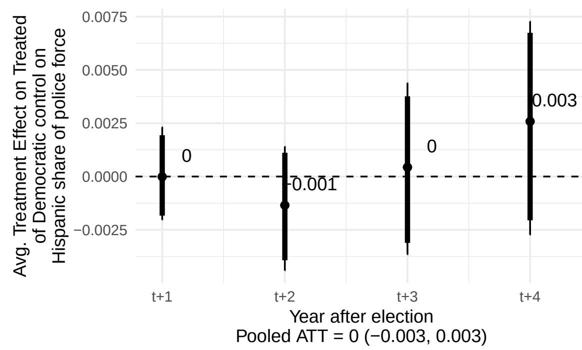


(d) Prob. woman police chief

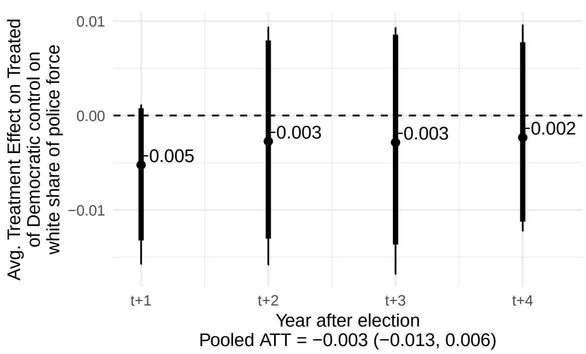
Figure S32: Long-term FEct effects of partisanship on police chief demographics: the probability of a Black chief (panel a), Hispanic chief (panel b), white chief (panel c), and woman chief (panel d). Thick bars show 90% robust confidence intervals and thin bars show 95% robust confidence intervals.



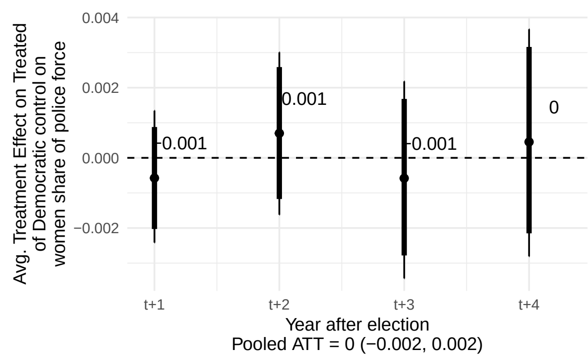
(a) Black share of police force



(b) Hispanic share of police force

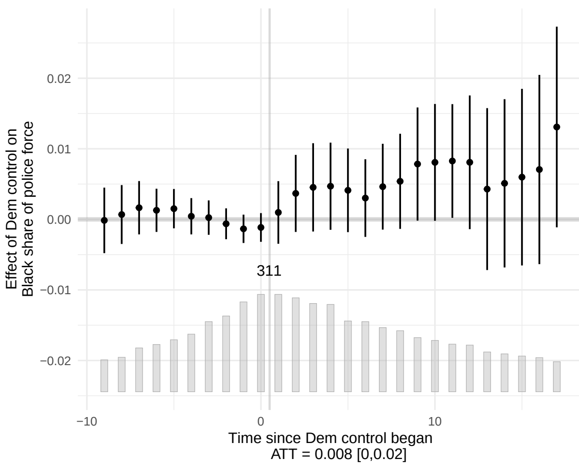


(c) White share of police force

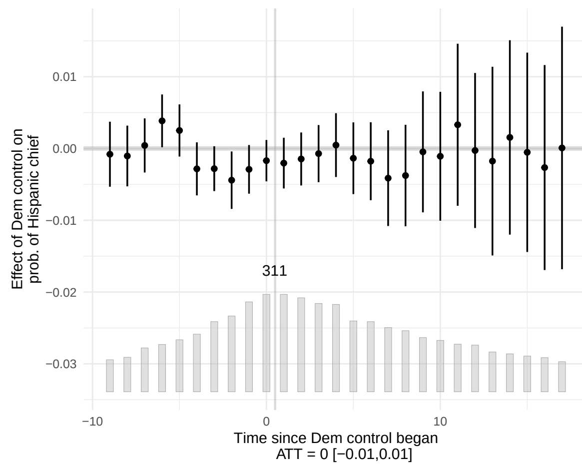


(d) Women share of police force (LEOKA)

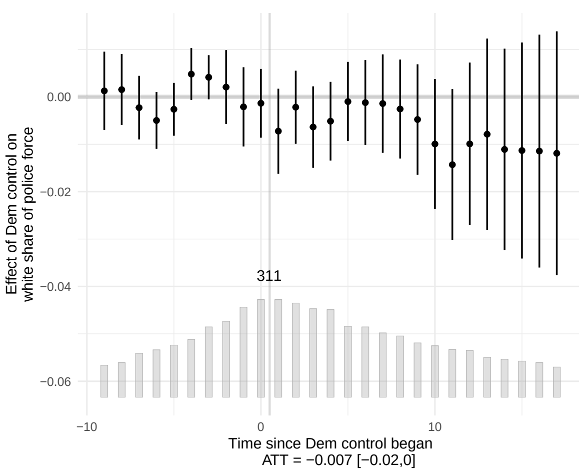
Figure S33: Long-term PanelMatch effects of partisanship on police force demographics: the Black share of the police force (panel a), Hispanic share of the police force (panel b), white share of the police force (panel c), and women share of the police force (panel d). Thick bars show 90% robust confidence intervals and thin bars show 95% robust confidence intervals.



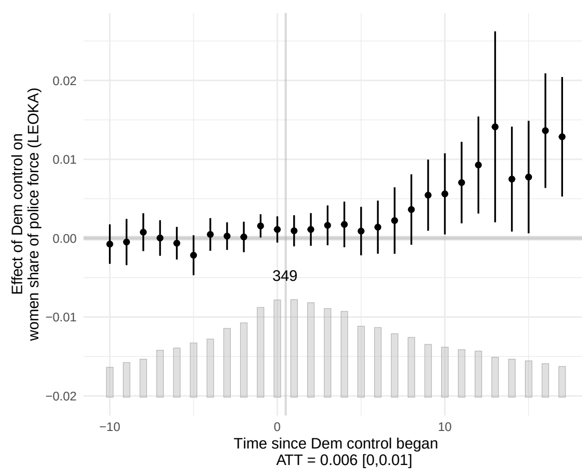
(a) Black share of police force



(b) Hispanic share of police force

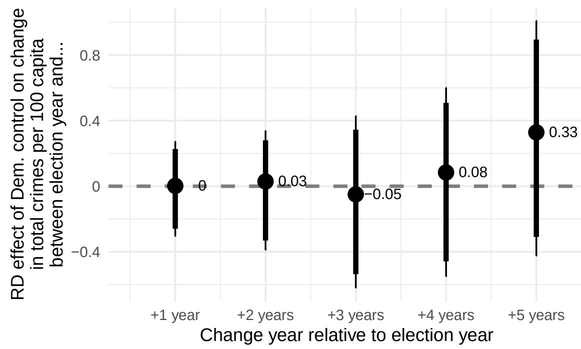


(c) White share of police force

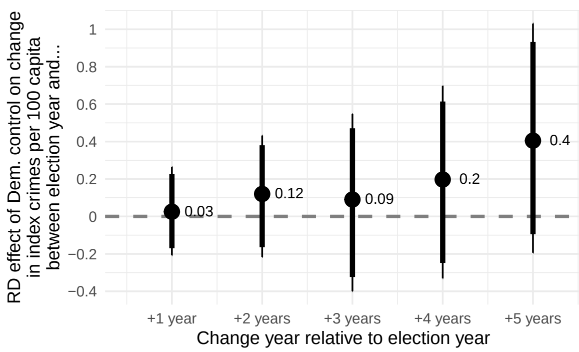


(d) Women share of police force (LEOKA)

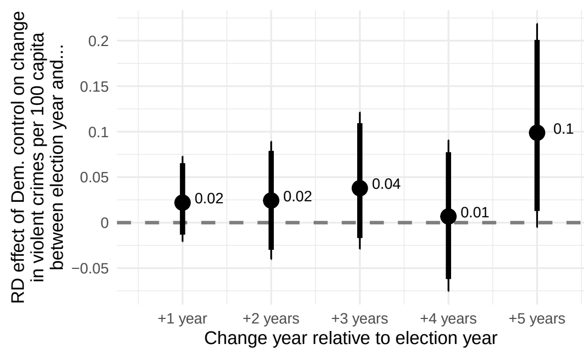
Figure S34: Long-term FEct effects of partisanship on police force demographics: the Black share of the police force (panel a), Hispanic share of the police force (panel b), white share of the police force (panel c), and women share of the police force (panel d). Thick bars show 90% robust confidence intervals and thin bars show 95% robust confidence intervals.



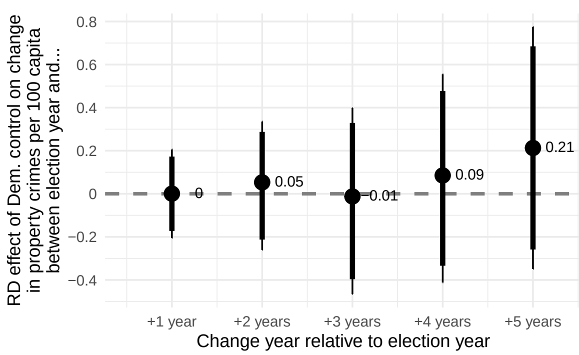
(a) Total crime per 100 capita



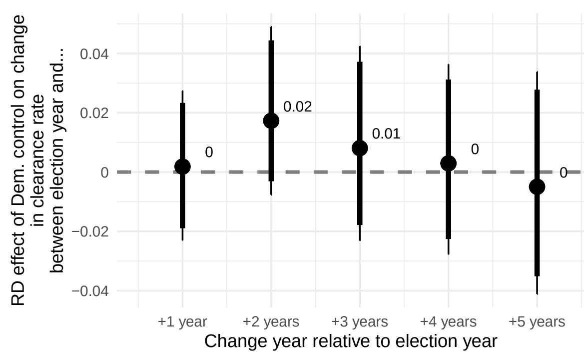
(b) Index crimes per 100 capita



(c) Violent crime per 100 capita

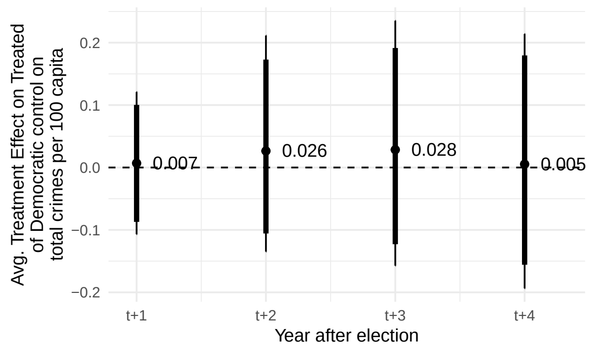


(d) Property crimes per 100 capita

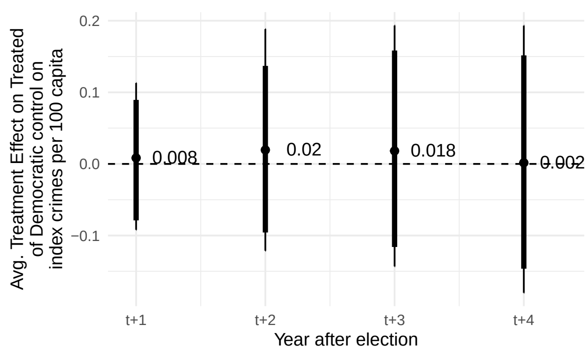


(e) Clearance rate

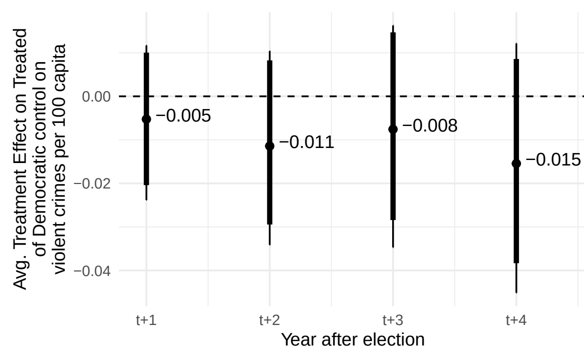
Figure S35: Long-term RDD effects of partisanship on the change in total crimes (panel a), index crimes (panel b), violent crimes (panel c), property crimes (panel d), and clearance rate (panel e). Thick bars show 90% robust confidence intervals and thin bars show 95% robust confidence intervals.



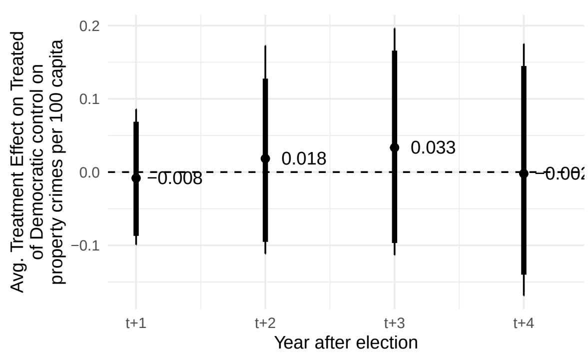
(a) Total crime per 100 capita



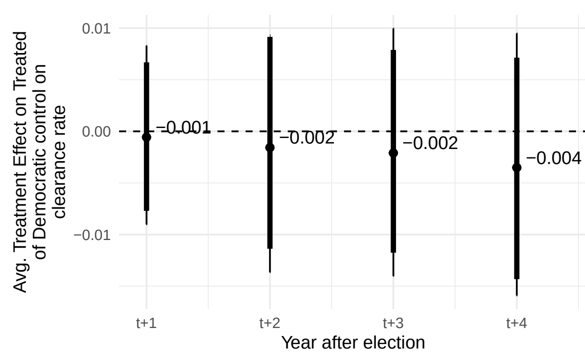
(b) Index crimes per 100 capita



(c) Violent crime per 100 capita

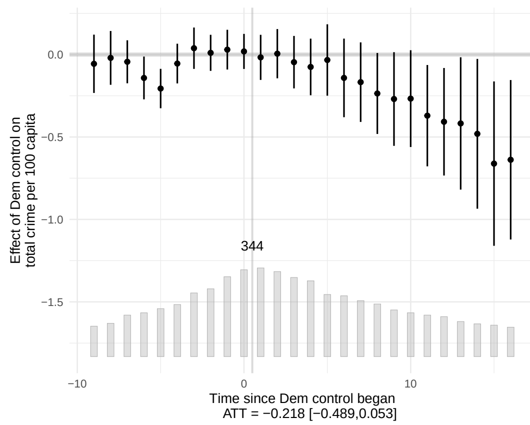


(d) Property crimes per 100 capita

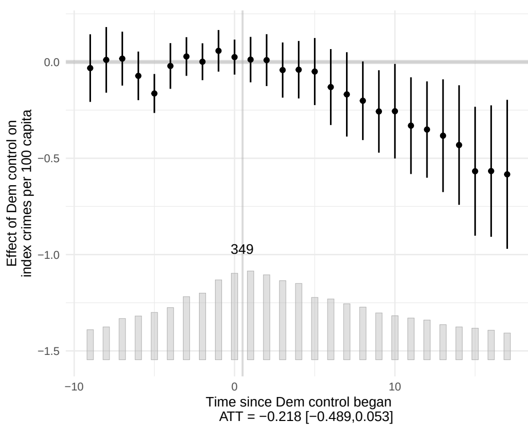


(e) Clearance rate

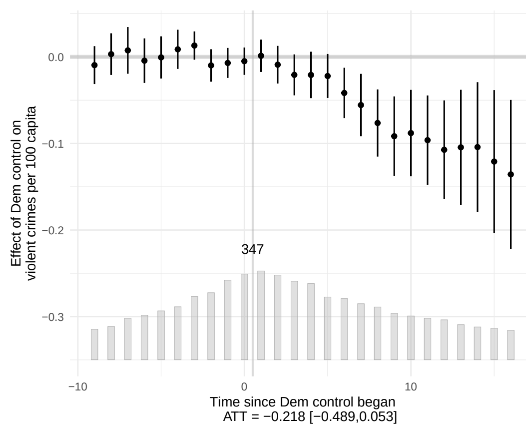
Figure S36: Long-term PanelMatch effects of partisanship on total crimes (panel a), index crimes (panel b), violent crimes (panel c), property crimes (panel d), and clearance rate (panel e). Thick bars show 90% robust confidence intervals and thin bars show 95% robust confidence intervals.



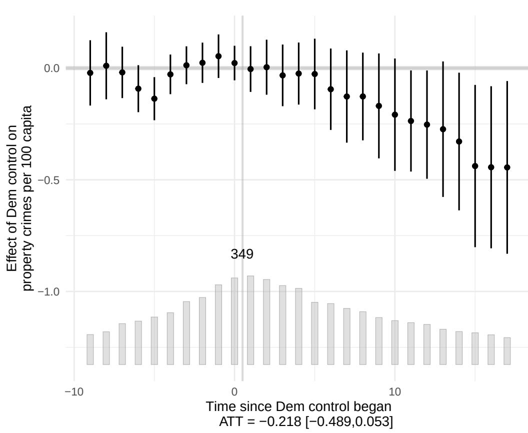
(a) Total crime per 100 capita



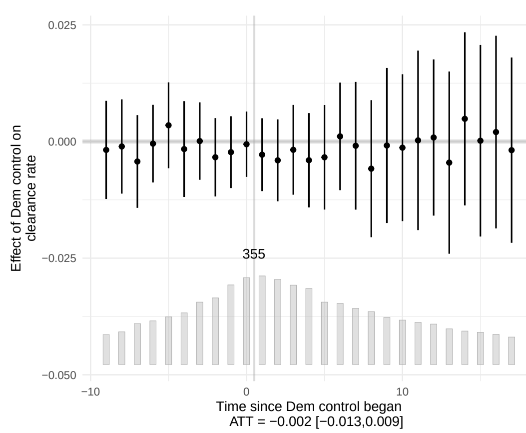
(b) Index crimes per 100 capita



(c) Violent crime per 100 capita

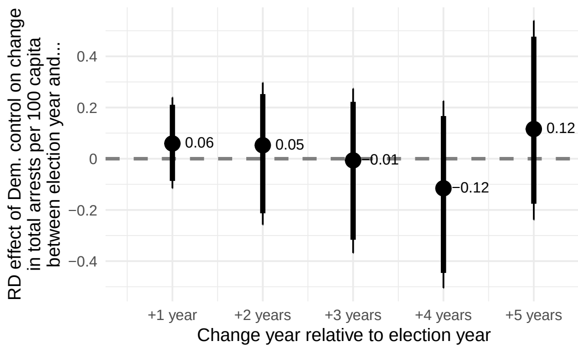


(d) Property crimes per 100 capita

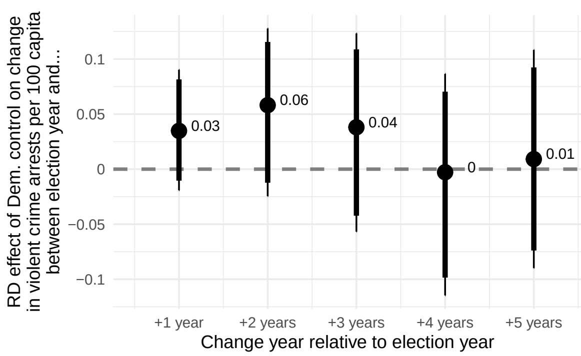


(e) Clearance rate

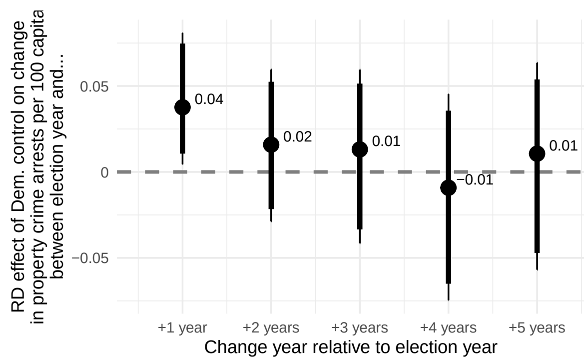
Figure S37: Long-term FEct effects of partisanship on total crimes (panel a), index crimes (panel b), violent crimes (panel c), property crimes (panel d), and clearance rate (panel e). Thick bars show 90% robust confidence intervals and thin bars show 95% robust confidence intervals.



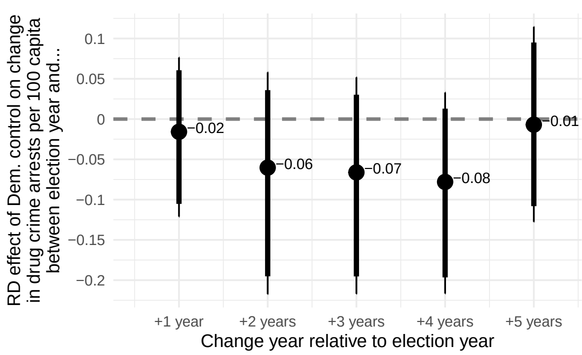
(a) Total arrests per 100 capita



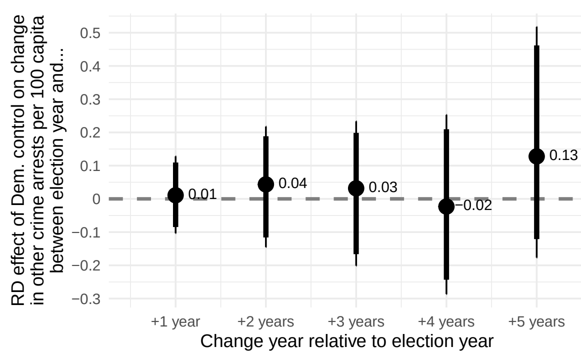
(b) Violent crime arrests per 100 capita



(c) Property crime arrests per 100 capita

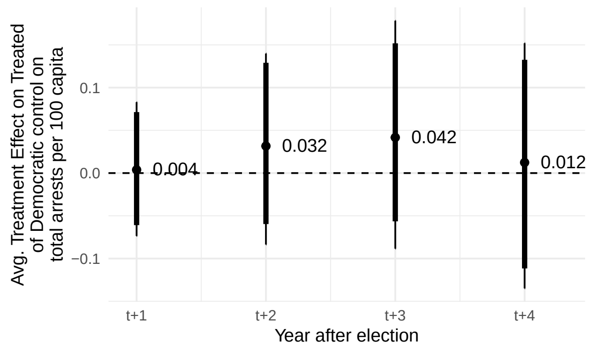


(d) Drug crime arrests per 100 capita

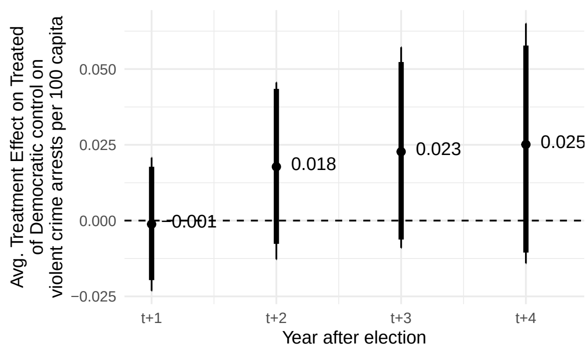


(e) Other crime arrests per 100 capita

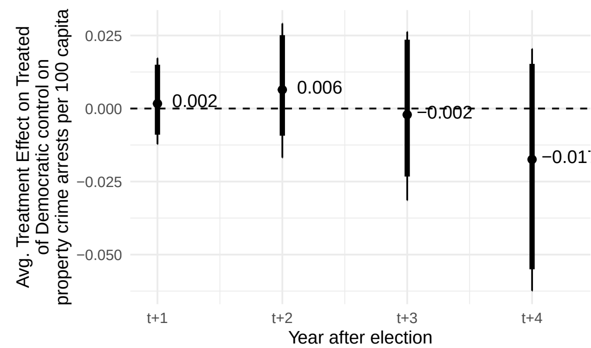
Figure S38: Long-term RDD effects of partisanship on the change in total arrests (panel a), violent crime arrests (panel b), property crime arrests (panel c), drug crime arrests (panel d), and other crime arrests (panel e). Thick bars show 90% robust confidence intervals and thin bars show 95% robust confidence intervals.



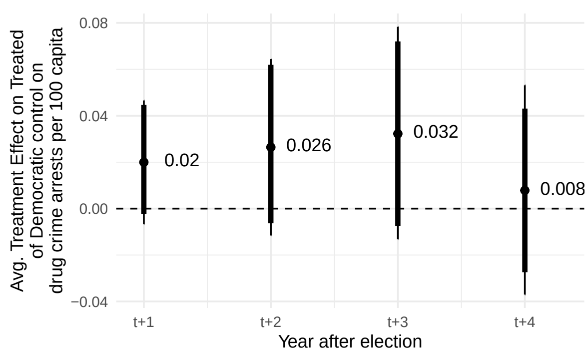
(a) Total arrests per 100 capita



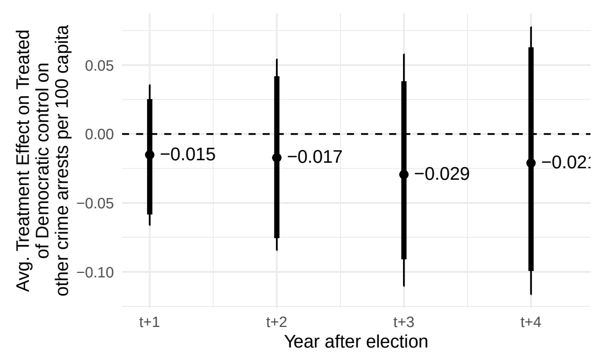
(b) Violent crime arrests per 100 capita



(c) Property crime arrests per 100 capita

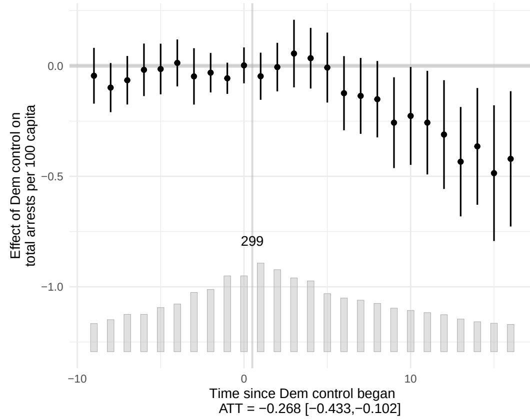


(d) Drug crime arrests per 100 capita

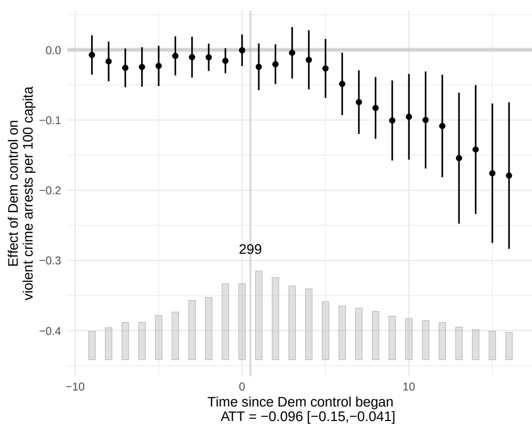


(e) Other crime arrests per 100 capita

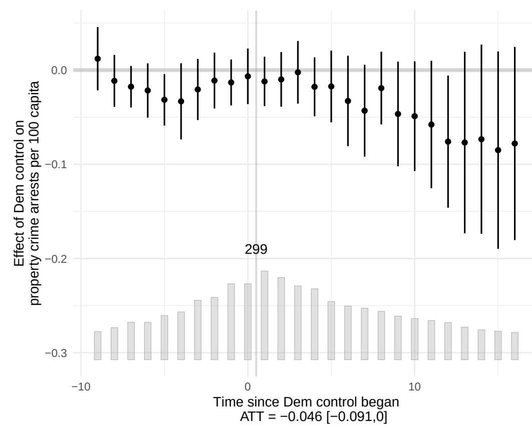
Figure S39: Long-term PanelMatch effects of partisanship on total arrests (panel a), violent crime arrests (panel b), property crime arrests (panel c), drug crime arrests (panel d), and other crime arrests (panel e). Thick bars show 90% robust confidence intervals and thin bars show 95% robust confidence intervals.



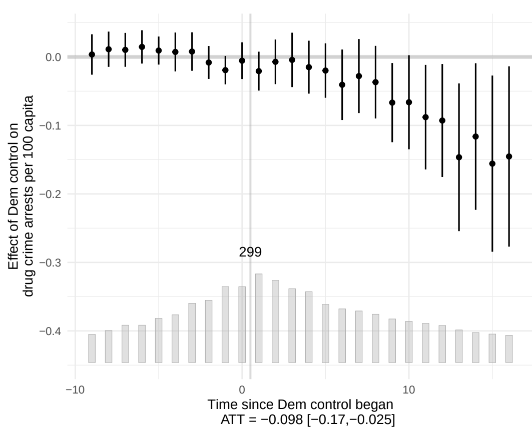
(a) Total arrests per 100 capita



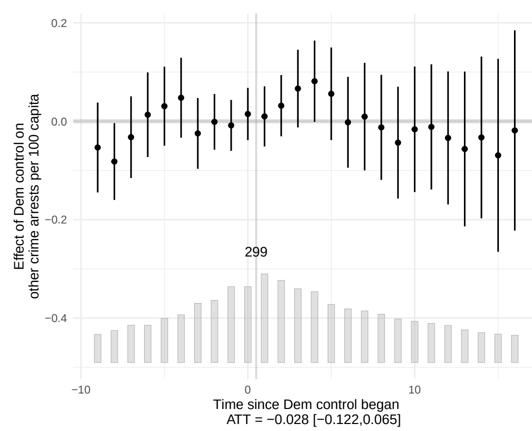
(b) Violent crime arrests per 100 capita



(c) Property crime arrests per 100 capita

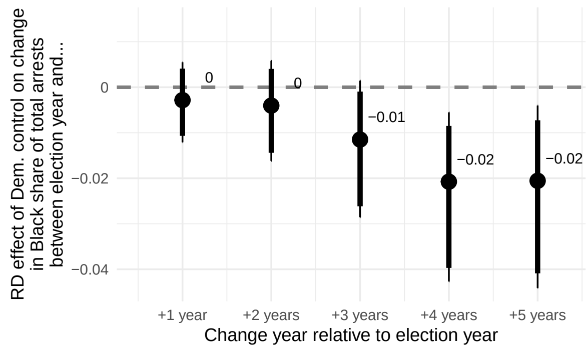


(d) Drug crime arrests per 100 capita

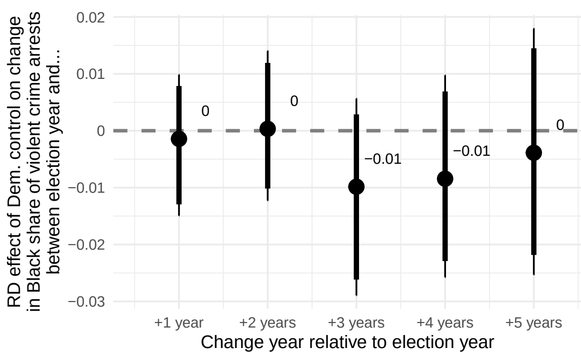


(e) Other crime arrests per 100 capita

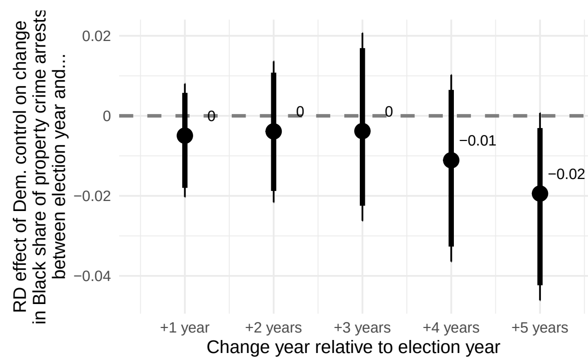
Figure S40: Long-term FEct effects of partisanship on total arrests (panel a), violent crime arrests (panel b), property crime arrests (panel c), drug crime arrests (panel d), and other crime arrests (panel e). Thick bars show 90% robust confidence intervals and thin bars show 95% robust confidence intervals.



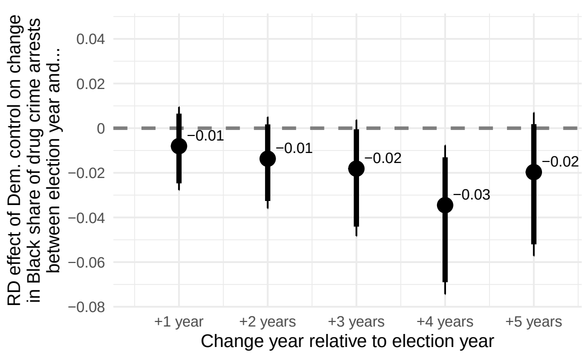
(a) Black share of total arrests



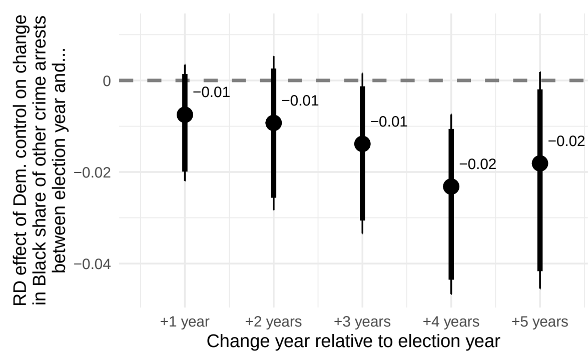
(b) Black share of violent crime arrests



(c) Black share of property crime arrests

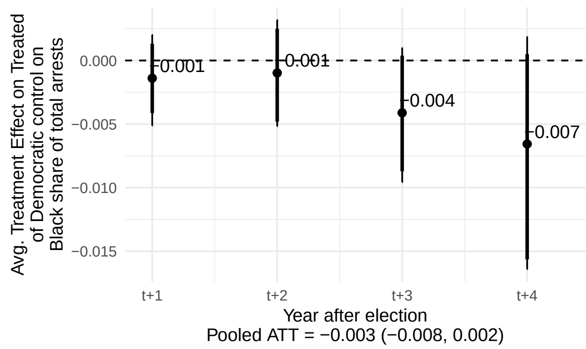


(d) Black share of drug crime arrests

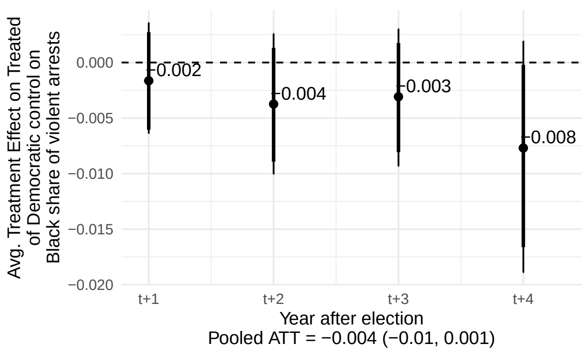


(e) Black share of other crime arrests

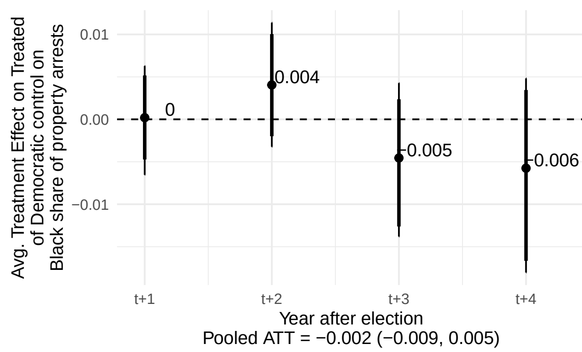
Figure S41: Long-term RDD effects of partisanship on the change in the Black share of total arrests (panel a), violent crime arrests (panel b), property crime arrests (panel c), drug crime arrests (panel d), and other crime arrests (panel e). Thick bars show 90% robust confidence intervals and thin bars show 95% robust confidence intervals.



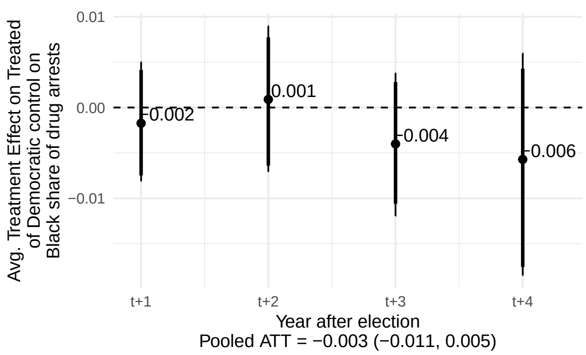
(a) Black share of total arrests



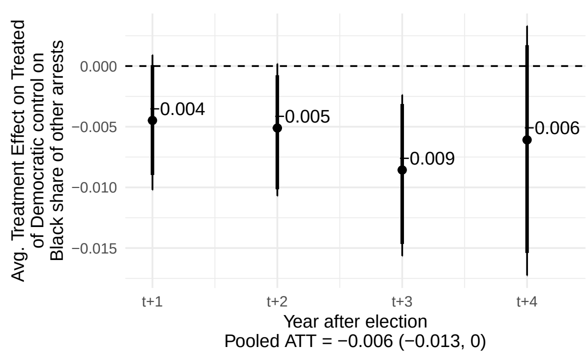
(b) Black share of violent crime arrests



(c) Black share of property crime arrests

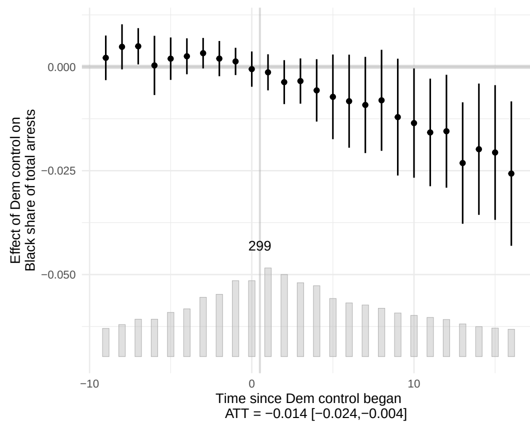


(d) Black share of drug crime arrests

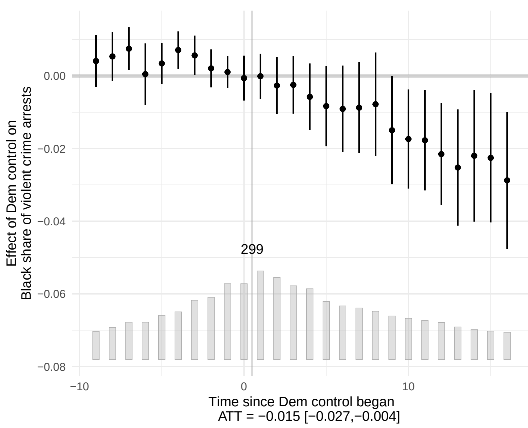


(e) Black share of other crime arrests

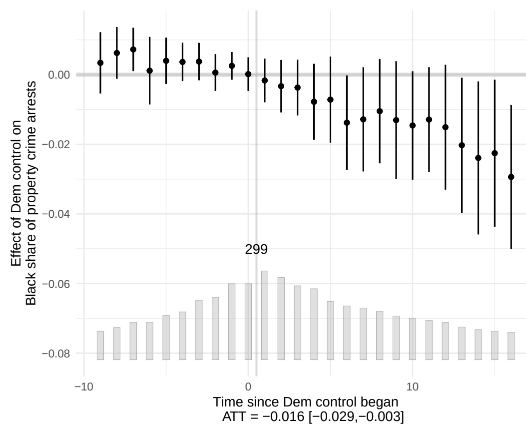
Figure S42: Long-term PanelMatch effects of partisanship on the Black share of types of total arrests (panel a), violent crime arrests (panel b), property crime arrests (panel c), drug crime arrests (panel d), and other crime arrests (panel e). Thick bars show 90% robust confidence intervals and thin bars show 95% robust confidence intervals.



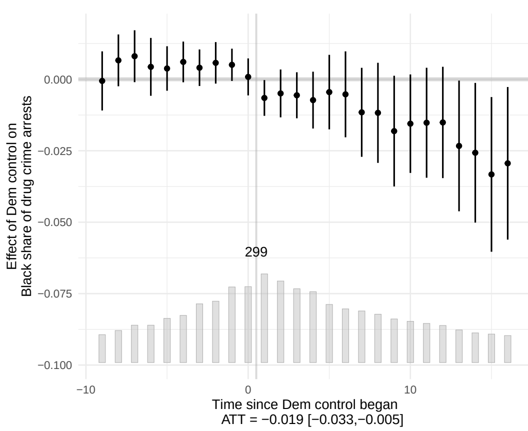
(a) Black share of total arrests



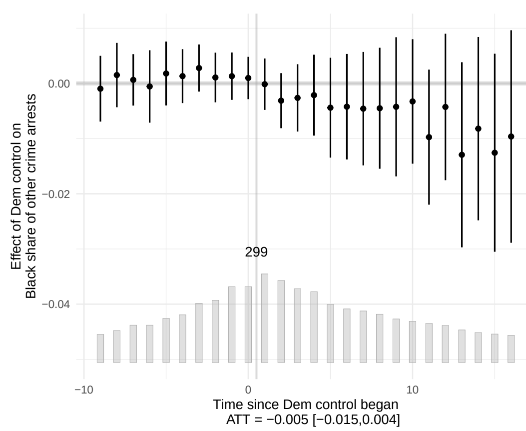
(b) Black share of violent crime arrests



(c) Black share of property crime arrests



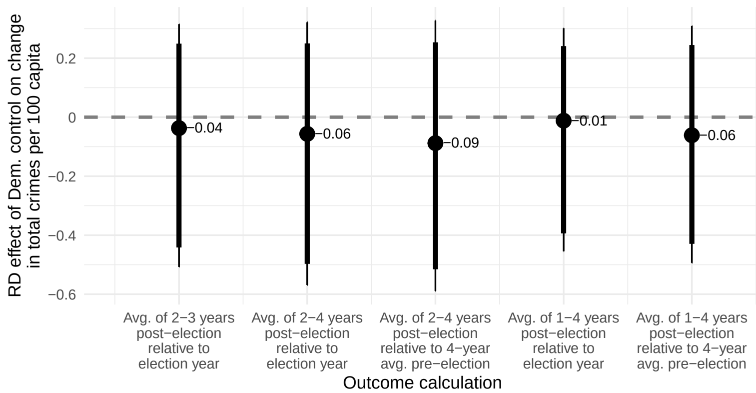
(d) Black share of drug crime arrests



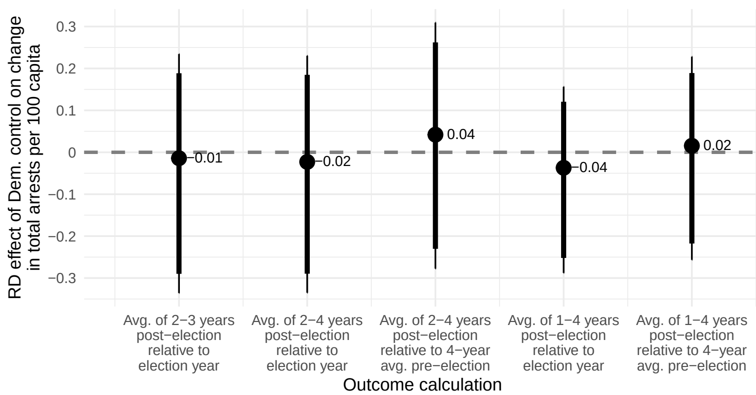
(e) Black share of other crime arrests

Figure S43: Long-term FEct effects of partisanship on the Black share of types of total arrests (panel a), violent crime arrests (panel b), property crime arrests (panel c), drug crime arrests (panel d), and other crime arrests (panel e). Thick bars show 90% robust confidence intervals and thin bars show 95% robust confidence intervals.

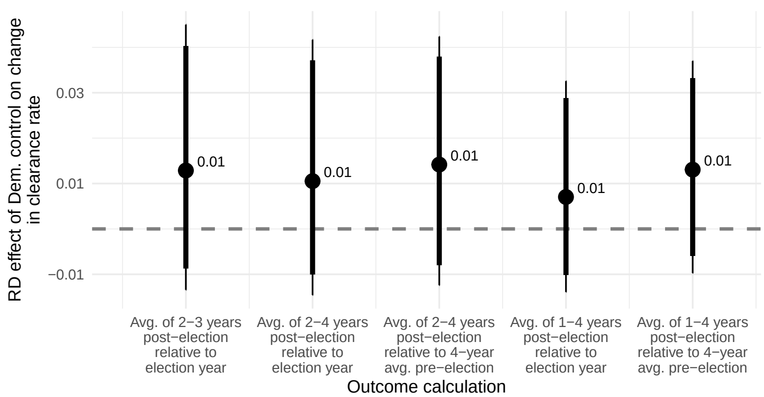
In addition, we display results from RDD analyses averaging outcome changes over multiple years to conserve statistical power. To do so, we average the change in outcomes between the election year and the year two years later, and the change in outcomes between the election year and the year three years later. We also average across the 2-4 years after the election, or use the change between the four years prior to the election and the years after the election. We conduct these analyses for overall crime, arrests, and clearance rates and display the results in Figure S44, as well as for racialized arrest patterns in Figure S45. These analyses all indicate that the change outcome calculation method makes little difference for our overall conclusions. Though the racialized arrest pattern results indicate statistically significant decreases in the Black share of arrests for drug and other crimes, as we discuss in the main manuscript, these results are not robust to the use of our two other research designs.



(a) Total crimes per 100 capita

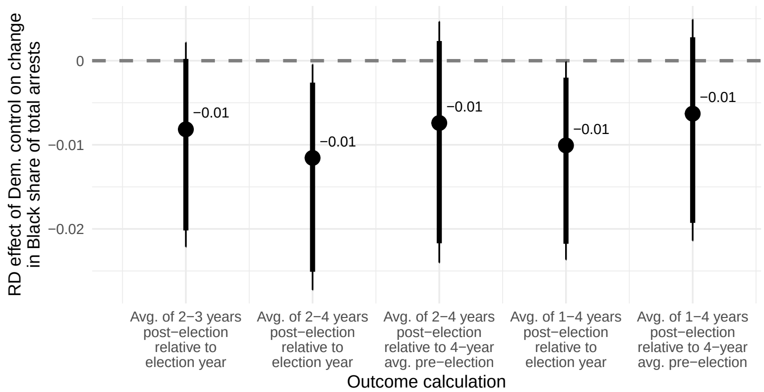


(b) Total arrests per 100 capita

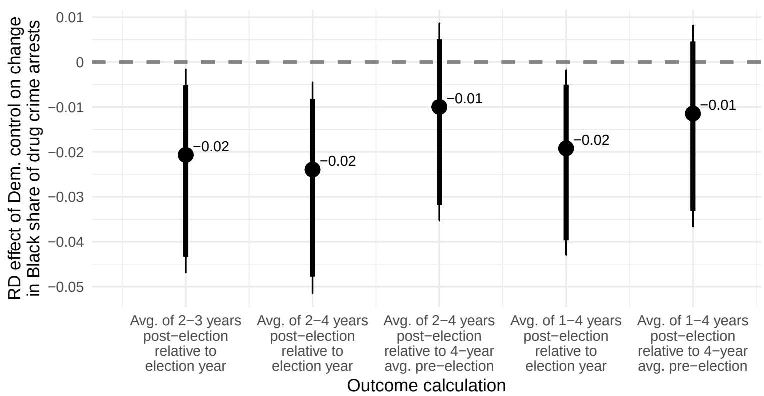


(c) Clearance rate

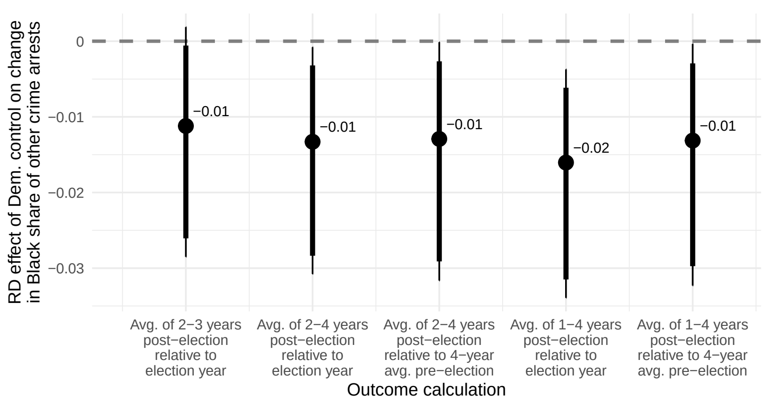
Figure S44: Effect of mayoral partisanship on the change in overall crime (panel a), arrests (panel b), and the clearance rate (panel c) using alternative change measures. Thick bars show 90% robust confidence intervals and thin bars show 95% robust confidence intervals.



(a) Black share of total arrests



(b) Black share of drug crime arrests



(c) Black share of other crime arrests

Figure S45: Effect of mayoral partisanship on the change in the Black share of total arrests (panel a), drug crime arrests (panel b), and other crime arrests (panel c) using alternative change measures. Thick bars show 90% robust confidence intervals and thin bars show 95% robust confidence intervals.

I Heterogeneity of Results by Institution

I.1 Heterogeneity by Form of Government

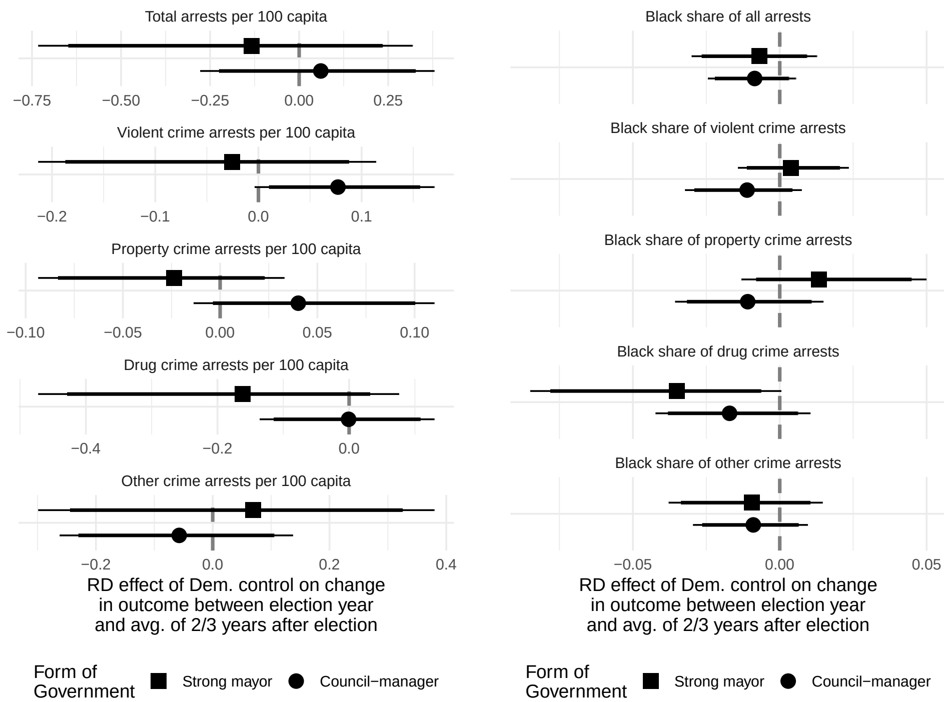


Figure S46: RDD effect on overall arrests and Black share of arrest types, by form of government. Points indicate traditional point estimates and lines indicate bias-corrected 90% (thick lines) and 95% (thin lines) confidence intervals, with filled squares indicating strong mayor cities and filled circles indicating council-manager cities.

I.2 Heterogeneity by Partisan Elections

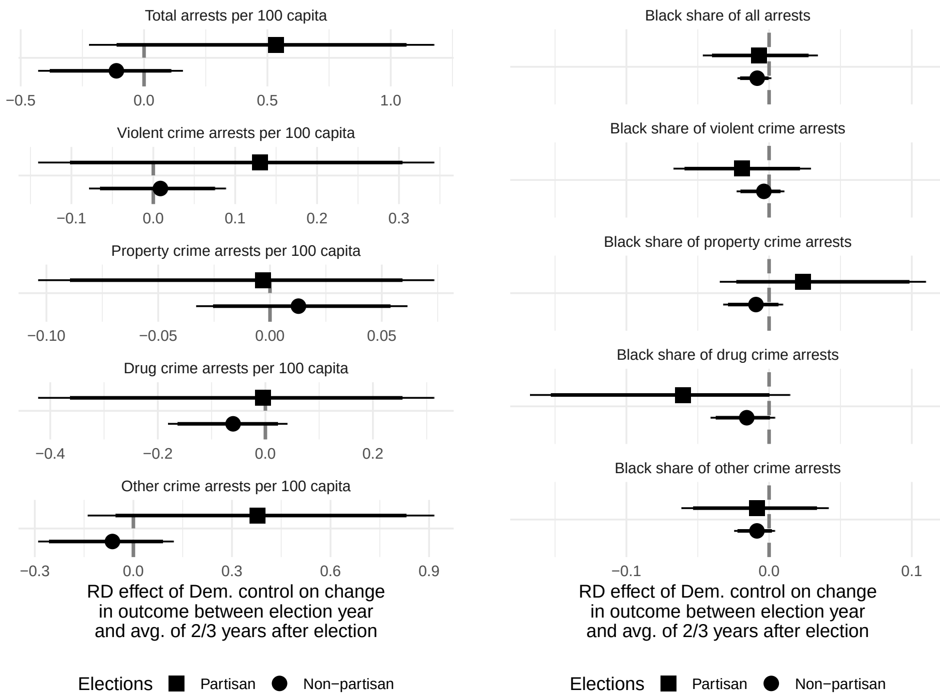


Figure S47: RDD effect on overall arrests and Black share of arrest types, by partisan ballots. Points indicate traditional point estimates and lines indicate bias-corrected 90% (thick lines) and 95% (thin lines) confidence intervals, with filled squares indicating cities with partisan elections and filled circles indicating cities with nonpartisan elections.

I.3 Heterogeneity by Decade

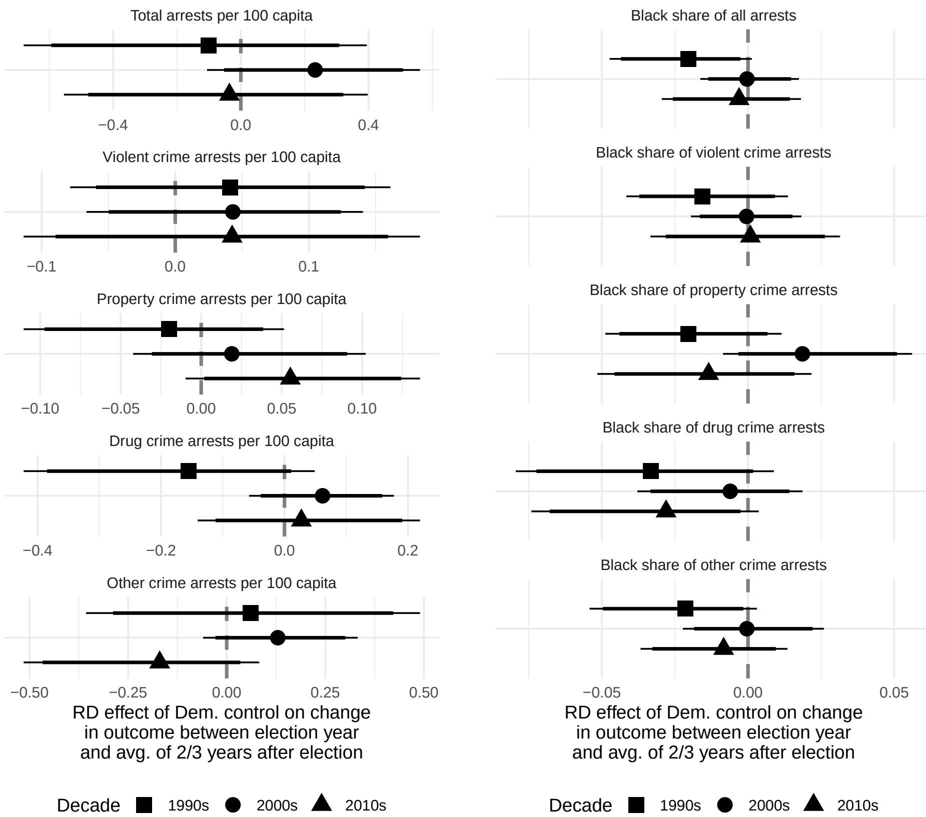


Figure S48: RDD effect on overall arrests and Black share of arrest types, by decade. Points indicate traditional point estimates and lines indicate bias-corrected 90% (thick lines) and 95% (thin lines) confidence intervals, with squares indicating elections in the 1990s, triangles indicating elections in the 2000s, and filled circles indicating elections in the 2010s.

J Effects of Switches to Republican Control

In the main body of the manuscript, we analyze the effect of Democratic mayoral victories rather than Republican mayoral victories (the RDD results) and of switches from non-Democratic mayors to Democratic mayors. In this appendix, we instead present results from analyses of the effect of close Republican rather than Democratic victories (i.e. the negative of the main RDD results) and difference-in-differences analyses analyzing switches from non-Republican mayors to Republican mayors. These results exhibit slight differences from those presented in the main body of the paper, but largely corroborate the null effects of partisanship on crime and policing outcomes.

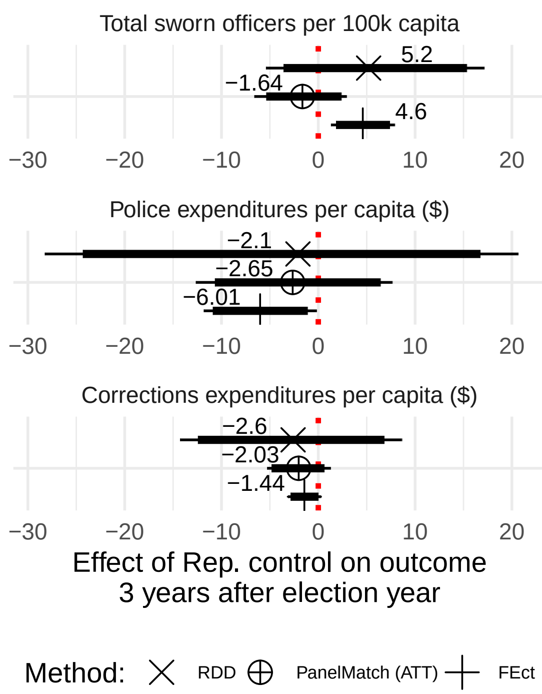
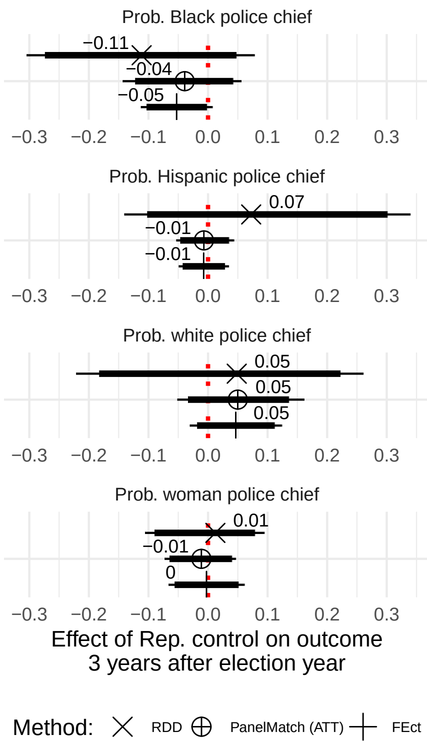
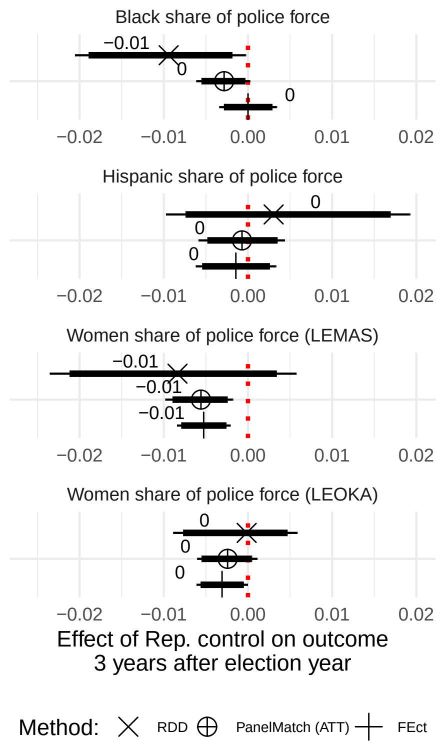


Figure S49: The effect of mayoral partisanship (switches to Republican mayors) on municipal police employment and criminal justice spending. Points indicate causal effect estimates from each of our three research designs: from the regression discontinuity design using the robust bandwidth selection procedure estimated with `rdrobust` (stars); from PanelMatch (crossed circles); and `fect` (vertical lines), and horizontal lines indicate 90% (thick lines) and 95% (thin lines) confidence intervals, using robust bias-corrected confidence intervals for the RDD.

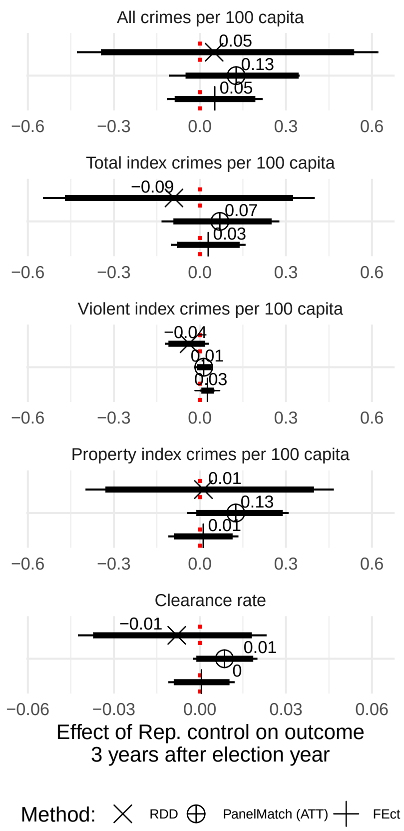


(a) Police chiefs

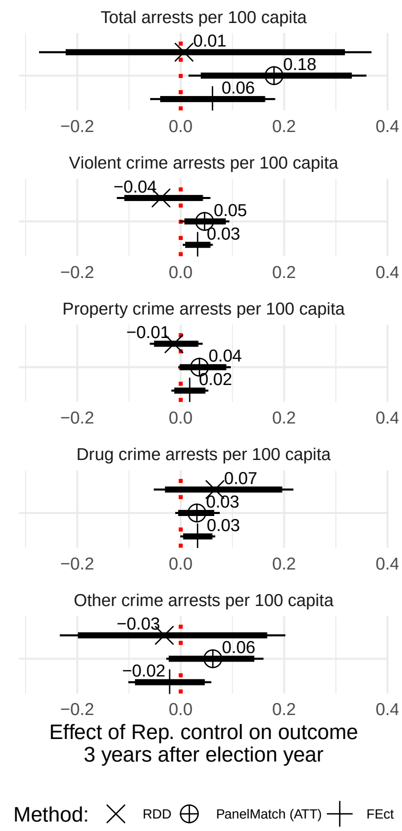


(b) Police force

Figure S50: The effect of mayoral partisanship (switches to Republican mayors) on changes in the demographics of police chiefs (panel a) and the police force (panel b) between the election year and the several years after the election. Points indicate causal effect estimates from each of our three research designs: from the regression discontinuity design using the robust bandwidth selection procedure estimated with `rdrobust` (stars); from PanelMatch (crossed circles); and `fect` (vertical lines), and horizontal lines indicate 90% (thick lines) and 95% (thin lines) confidence intervals, using robust bias-corrected confidence intervals for the RDD.



(a) Crimes and clearance rates



(b) Arrests

Figure S51: The null effect of mayoral partisanship (switches to Republican mayors) on changes in per capita reported crimes and clearance rate (panel a) and per capita arrests (panel b) between the election year and three years after the election. Points indicate causal effect estimates from each of our three research designs: from the regression discontinuity design using the robust bandwidth selection procedure estimated with `rdrobust` (stars); from PanelMatch (crossed circles); and `fect` (vertical lines), and horizontal lines indicate 90% (thick lines) and 95% (thin lines) confidence intervals, using robust bias-corrected confidence intervals for the RDD.

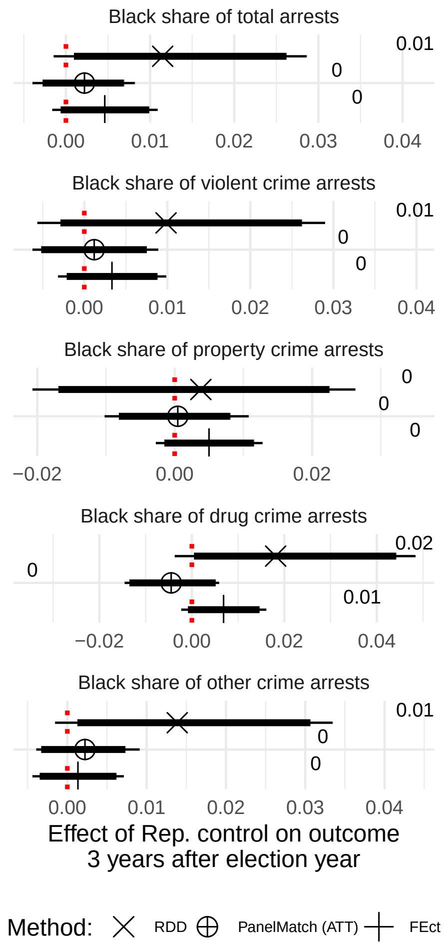


Figure S52: The effect of mayoral partisanship (switches to Republican mayors) on the change in the Black share of arrests between the election year and three years after the election. Points indicate causal effect estimates from each of our three research designs: from the regression discontinuity design using the robust bandwidth selection procedure estimated with `rdrobust` (stars); from PanelMatch (crossed circles); and `fect` (vertical lines), and horizontal lines indicate 90% (thick lines) and 95% (thin lines) confidence intervals, using robust bias-corrected confidence intervals for the RDD.