Political Power of Bureaucratic Agents: Evidence from Policing in New York City

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Abstract

To what extent can bureaucrats manipulate public service provision for explicitly political ends? A growing body of work highlights the immense ability of bureaucrats to influence governments through campaign contributions, endorsements, collective bargaining, and organized election turnout. I explore a more fundamental mechanism of bureaucratic influence: bureaucrats strategically shirking responsibilities to leverage voters. Politicians depend on bureaucrats to achieve policy goals. This gives the latter leverage over the former. If bureaucrats deviate in their preferences from politicians and are organized in cohesive unions with strong tenure protections, they can collectively reduce effort to exert political pressure. I use data on New York Police Department (NYPD) 911 response times together with council members' preferences on the FY2021 $1 billion cut to the NYPD's budget. Employing difference-in-differences and spatial difference-in-discontinuities designs, I find that police disproportionately reduced effort in districts of non-aligned politicians by slowing response times. This study informs the theoretical debate on principal-agent relationships in government and highlights the importance of organized political interests to explain policing in US cities.

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1 Introduction

During the pandemic, governments in many US cities found themselves in contentious public clashes with law enforcement unions over requirements that officers receive COVID19 vaccines. Although the coronavirus caused many casualties among the rank-and-file, with more officers dying from COVID19 than from gunfire (Medina, 2021), many police officers and their unions resisted vaccinations, threatening work stoppages and lawsuits. Law enforcement officers and their union representatives claimed that vaccine mandates violate their rights. For instance, in October 2021 the head of Chicago's largest police union, John Catanzara, called on its 11,000 union members to ignore the city's requirement to report their vaccination status stating, "it is the city's clear attempt to force officers to 'Chicken Little, the sky is falling' into compliance. Do not fall for it. Hold the line." (Honderich, 2021). Expecting that officers would refuse to submit to the mandate, he added that "it's safe to say the city of Chicago will have a police force at 50 percent or less for this weekend coming up. [...] Whatever happens because of the manpower issue, that falls at the mayor's doorstep." (Bosman, 2021).

In this paper, I examine how divergent policy preferences of bureaucrats and their political principals incentivize bureaucrats to protest unwanted policy change. While politicians decide on policy choices, they must invariably rely on bureaucrats to enact policies, e.g., to enforce the law, ensure safe communities, teach our children, or distribute social services. I postulate that this dependence of politicians on bureaucrats' efforts vests the latter with political power over the former. Voters base their assessments of incumbents on policy choices and outcomes but face challenges in attributing responsibility for poor public service provision. For instance, when a community experiences worse public safety following a police reform, voters find it difficult to determine whether poor public safety result from bad policy or poor service provision by the police post-reform. If bureaucrats differ in their preferences from elected politicians and are shielded from political control, they can exploit

this uncertainty about political responsibilities and their central role in government. I argue that under these conditions bureaucrats can strategically shirk to exert political influence on non-aligned incumbents.

I build on a growing body of work demonstrating how bureaucrats function as powerful interest groups in American politics. Prior research has overwhelmingly focused on how public sector unions influence governments by entering politics *explicitly*, e.g., through collective bargaining (Moe, 2009, 2011; Anzia and Moe, 2015; Paglayan, 2019; Zoorob, 2019), union endorsements (Moe, 2006; Hartney and Flavin, 2011; Hartney, 2022), electoral mobilization of their members (Leighley and Nagler, 2007; Anzia, 2014; Flavin and Hartney, 2015), political contributions (Moe, 2011; DiSalvo, 2015), or direct lobbying (Anzia, 2022). In contrast, this article illuminates a more fundamental mechanism of influence for bureaucrats and their unions. I focus on bureaucrats' central role in politician-voter accountability relationships as service providers and demonstrate how bureaucrats strategically shirk their responsibilities to instrumentalize voters' influence on politicians—without entering politics explicitly.

I focus on the US municipal police. Anecdotal evidence suggests that police unions influence local and national politics through lobbying, litigation, or participating in electoral campaigns (Blumgart, 2020; Zoorob, 2019). Yet, little is known about how police officers adjust their day-to-day activities to affect their elected principals and the policy choices they make in office. Applying my theoretical argument, I expect that the police reduce their effort to exert political pressure on non-aligned local elected officials. In so doing, the police can affect voters' perceptions of the quality of security provision and their evaluations of incumbents.

I test this argument in the context of the unprecedented cut to NYPD's budget in July 2020. Faced with strained resources due to the coronavirus and growing public demand for police reforms after George Floyd's death, the New York City Council voted to reduce the funding of America's largest police force for fiscal year 2021 by $1 billion—a substantial reduction relative to the 2020 budget of $5.6 billion. While 32 City Council members voted

in favor of the budget cut, an unusually high number of 17 councilors and police unions in NYC opposed the new budget. Using geocoded data on more than nine million 911 calls, I test whether police response times increased in the districts of anti-police politicians after the budget vote. The NYPD budget cut following George Floyd's death certainly marked a unique moment in American history. Yet, it is similar to police reforms of many other major US cities in 2020.[1] Additionally, the details of the policy allow me to identify the effect of political misalignment on bureaucratic resistance. Hence, the unusual nature of the policy shock is a feature of this article, rather than a flaw.

A natural threat to inference is that police behavior might diverge across aligned and non-aligned districts after the budget cut due to other trends (e.g., differences in traffic levels or resource allocation across police precincts). To overcome this, I employ a triple difference-in-differences design where I compare response times across non-aligned and aligned districts within the same police precincts, before and after the budget vote and across agencies. I use response times of firefighters to 911 medical emergency calls to account for time-specific trends in response times across districts. Firefighters are largely comparable to police officers in their unionization rates and local government structures. Yet, unlike funding for the NYPD, the adopted budget of the Fire Department of the City of New York (FDNY) increased relative to previous fiscal years. Since firefighters had little reason to organize politically to exert pressure on city council members, emergency medical services (EMS) response times can serve as a credible counterfactual in bureaucrats' reactions to 911 calls absent electorally motivated behavior. In a supplementary analysis, I also use spatial difference-in-discontinuities regressions, where I estimate differences in response times across council districts with opposing budget votes in a spatial regression discontinuity design (RDD) before and after the budget vote.

Consistent with my theoretical argument, I find that response times in non-aligned districts increased by about one minute and 30 seconds for NYPD calls compared to FDNY calls

[1]Figure A1 shows the distribution of budget cuts across each US state's largest cities between FY2020 and FY2021.

after the budget vote—a substantial increase relative to the average 911 response time of 13.4 minutes prior to the budget vote. The size and precision of this treatment effect is robust to accounting for the available budget in each police precinct, demand for police presence and police-related protests. Supplementary analyses suggest that the effect is driven by delays for longer calls where police have more discretion, including crimes not in progress, disputes and vehicle accidents. Further, I provide qualitative evidence from official statements and social media posts by NYPD police unions to substantiate how police organizations targeted non-aligned politicians by leveraging their influence on voters.

This research makes three main contributions. First, this study adds to our understanding of the strategic interactions among politicians, bureaucrats, and voters, and their effects on public policy and accountability. By highlighting how bureaucrats shirk responsibilities for political leverage, this article speaks to the theoretical literature on bureaucratic delegation and empirical studies on public sector unions. It demonstrates that bureaucrats are powerful interest groups within government by the mere fact of being bureaucrats. While prior research has highlighted a variety of ways for bureaucrats to exert political influence, scholars could underestimate bureaucrats' full political power if they primarily focused on official channels, such as lobbying, campaign spending or turnout.

Second, a growing body of work on the political economy of bureaucracy shows that politicians' power over bureaucrats in patronage systems induces bureaucrats to act as electoral brokers for politicians by working harder and actively boosting politicians' chances of re-election (Pierskalla and Sacks, 2019; Brierley, 2020). I show that the converse can hold in professionalized bureaucracies where bureaucrats' careers are independent of political influence: Electoral accountability incentivizes bureaucrats to *reduce* effort to put pressure on non-aligned politicians. Scholars and practitioners in public administration generally advocate for bureaucracies to be strongly independent from political authorities (Rauch and Evans, 2000). Yet, this study raises questions about whether a strict political insulation of civil servants necessarily prevents electorally motivated behavior of bureaucrats, thus speak-

ing to recent research on the strategic politicking of bureaucrats (Potter, 2019) and the political preferences of career executives (Bolton et al., 2020).

Lastly, this study expands the growing literature on the politics of policing. While recent studies have taken more interest in local policing, particularly its impact on minority communities (Lerman and Weaver, 2014; Ba et al., 2021), few scholars study police as a political institution within government, accountable to and incentivized by other governmental actors (Mummolo, 2018; Goldstein et al., 2020; Cook and Fortunato, 2022). This study, in contrast, recognizes law enforcement agencies as political players within local government and offers both a theoretical and empirical account of how their relationship with local elected officials structures police incentives.

2 Shirking for Political Leverage

A long theoretical tradition in bureaucratic politics uses top-down principal-agent models to describe the relationships between political authorities and non-elected bureaucrats (see Huber and Shipan (2011); Moe (2012) for a review). Politicians—the principals—lack the expertise and time to implement and enforce policy and therefore delegate authority to expert bureaucrats—the agents. These canonical accounts assume that diverging preferences between politicians and bureaucrats induce bureaucrats to be non-compliant with the principals' intentions and shirk their duties (e.g., Brehm and Gates (1997); Epstein and O'Halloran (1999); Huber and Shipan (2002)).

A limitation of this standard view on bureaucracy is its focus on the dyadic relationships between politicians and bureaucrats. Importantly, it disregards that political principals in a democratic setting are *elected* and thus vulnerable to the behavior of bureaucrats (Moe, 2006). Consequently, in traditional models of bureaucracy, shirking arises because bureaucrats have idiosyncratic preferences and abilities to work towards politicians' goals (i.e., standard problems of moral hazard and averse selection), not because it allows bureaucrats to leverage their influence on voters. By omitting the fact that citizens base their assessment

of elected politicians partly on the quality of bureaucratic service provision, the canonical account understated the ability of bureaucrats to turn the delegation relationship to their own benefit. This article addresses this gap by examining the dynamic of *shirking for political leverage* and how it affects public service provision and electoral accountability.

The insight that the standard top-down account of bureaucracy underestimates bureaucrats' political power is not new. Moe (2006) famously argued that because bureaucrats can influence the electoral process, they can affect who their principals are and what policies they choose in office. That is, the electoral vulnerability of politicians turns them into "agents of the agents." To illustrate bureaucrats' electoral power, Moe (2006) uses teachers' unions as an example and shows that union endorsements significantly boost election prospects of candidates running for Californian school boards. Similarly, an extensive subsequent literature shows that bureaucrats—particularly their public sector unions—are one of the most influential interest groups on all levels of government (e.g., Anzia (2014); DiSalvo (2015); Flavin and Hartney (2015); Hartney (2022)). Yet, importantly, this work exclusively considers *explicit* routes of political influence for bureaucrats, e.g., through collective bargaining and lobbying, union endorsements, or electoral mobilization of their members. In contrast, I postulate that bureaucrats can bring pressure to bear on elected officials by the mere virtue of being central players in government.

Motivated by re-election incentives, political representatives use public policy to cater to their voters and donors. Yet, since voters rarely observe politicians' performance directly, they generally base their evaluations of elected representatives on policy outcomes as implemented by bureaucrats (Ujhelyi, 2014). Without perfect information about the inner workings of government, voters face challenges in attributing responsibility for poor service provision to bureaucrats vis-à-vis politicians. This imperfect information allows bureaucrats

to sabotage the public payoff for political purposes.[2] If incumbents enact policies that bureaucrats dislike, bureaucrats may shirk their duties in the constituencies of such non-aligned politicians. This allows them to obtain their main objective (i.e., ensure favorable policy) in one of two ways, either by damaging the reputation of certain incumbents and thus jeopardizing their electoral chances or, less severe, by pushing politicians to revisit unwanted policies through public pressure.[3] For instance, bureaucrats can protest unwanted policies by delaying the execution of policy instead of working diligently. Even more extreme, bureaucrats may actively sabotage the political agenda of their principals, for example, by obstructing policy implementation to prevent possible reforms (Brehm and Gates, 1997). Thus, because elected officials inherently depend on bureaucratic agents and voters have imperfect information about political responsibilities, electoral accountability can deteriorate public service provision through bureaucratic sabotage.[4]

This is not to say that all groups of bureaucrats act politically or are equally powerful across different political systems. In fact, existing research on US federal bureaucrats' resistance suggests that militancy in the bureaucracy has been limited across different presidencies (Brehm and Gates, 1997; Golden, 2000). I, therefore, highlight several scope conditions for my argument.[5]

First, whether bureaucrats are willing to exert political pressure depends on the degree

[2]Note that if voters can perfectly attribute poor service quality to shirking bureaucrats, the dynamics change significantly and bureaucratic resistance cannot be sustained in equilibrium. Perfectly informed voters either never punish politicians for anti-bureaucratic policies, which renders politically motivated shirking ineffective, or punish politicians for anti-bureaucratic policies with certainty to avoid bureaucratic shirking, which induces politicians to refrain from such policies in the first place.

[3]A natural question is why politicians wouldn't anticipate and acquiesce to bureaucrats' threat of shirking. As we formally show in Heo and Wirsching (2023), incumbents engage in reform despite the possibility of sabotage either because they care about the policy enough or because they can improve their electoral chances through reform, relying on the voters' uncertainty about political responsibility and the uncertainty about bureaucrats' ability to sabotage.

[4]While I focus on the political mobilization of bureaucrats as a reason for shirking, my claim is not that it is the *only* or even the most important reason for agency loss in the public sector. Following related work (Forand et al., 2022), I assume other bureaucratic shirking to be a function of bureaucrats' varying public service motivation and thus largely exogenous to bureaucrats' alignment with politicians. Empirically, I address alternative explanations related to morale effects in Section 7.

[5]Since these conditions remain fixed in my empirical setting, I cannot test their importance for the theoretical mechanism. I leave this to future research.

of their job protections. If politicians can influence the appointments, promotion, and transfers of bureaucrats in patronage systems, bureaucratic agents depend on the re-election and continuous support of their political principals. This political dependence of bureaucrats automatically aligns the incentives of bureaucrats and politicians (Ujhelyi, 2014). In contrast, politicians lose most of their direct influence on bureaucrats' careers and actions if bureaucrats are selected through competitive examinations and enjoy civil service protections, including job tenure, collective bargaining, and standardized pay scales. This makes it easier for bureaucrats who disagree with politicians to diverge from the intended policy without risking their jobs. Hence, bureaucratic resistance should only exist in professionalized, independent bureaucracies, not in systems of political patronage.

Second, to be better able to exert pressure, bureaucrats need to develop mechanisms to overcome collective action problems in their strategic behavior. Public sector unions often serve this purpose, as they pool employees' resources, streamline political goals, and reduce the possibility for selective punishment of individual bureaucrats. A strong union, therefore, enables bureaucrats to *collectively* resist the agenda of their political principals.[6]

Third, the mechanism depends on the observability of public goods provision. Bureaucrats can only take advantage of politicians' electoral vulnerability if voters are well aware of the quality of public services but are unsure who is to blame for any deterioration. For example, while voters might be less aware of the output of employees in a city's office of labor relations, they are often more immediately affected by and informed about the behavior of street-level bureaucrats, such as police officers or social workers.

Given these conditions, bureaucrats are likely better able to exert political pressure on the sub-national level. Local public sector unions often form more cohesive interest groups than their larger federal counterparts (Moe, 2006; Anzia, 2022). Further, unlike federal bu-

[6]In principle, the mechanism allows for bureaucrats to shirk *in isolation*. However, while bureaucrats might not fear retribution for atomic shirking due to strong tenure protections in professionalized bureaucracies, they likely do not have sufficient efficacy in moving voters' beliefs and might have incentives to free-ride on other bureaucrats' shirking. Additionally, individual bureaucrats likely lack sufficient knowledge about the politics of public goods provision to engage in strategic shirking.

reaucrats, local bureaucrats frequently interact with their constituents, thus allowing citizens to directly observe public service provision.

Lastly, bureaucrats' capacity to engage in politically motivated shirking is inherently limited. Public sector employees are often found to exert effort without significant monetary incentives because they tend to be intrinsically motivated to perform (Brehm and Gates, 1997; Forand et al., 2022). Similarly, better public services often facilitate bureaucrats' jobs. For instance, as lower crime rates reduce the need for constant policing, police officers benefit from a sufficient level of effort. Additionally, if bureaucrats engaged in constant shirking, this strategy would lose its valuable signaling effect, and bureaucrats would risk alienating voters and politicians and could thus trigger more unwanted policies instead of advancing their causes.

3 Bureaucratic Resistance of US Municipal Police

To test this theoretical argument, I focus on the behavior of US municipal police. Police forces in the US are agents of local elected governments, where chiefs of municipal police usually report to their city councils and mayors and receive their funding from their city's budget. Yet, professionalization and formal independence of police departments across the country, together with the nature of policing, reduce politicians' ability to control police. Policing generally requires high levels of autonomy and discretion, since the task environment of the police is often ambiguous and demands officers' individual choices (Wilson, 1978).

Rank-and-file employees of law enforcement agencies are generally well organized in powerful unions with strong tenure protections. In 2020, for instance, 56% of the 764,141 police officers in the US were unionized, compared to only 25% of employees in the public sector overall and 6% in the private sector (Hirsch and Macpherson, 2021).[7] Police unions tend

[7]Four states (Georgia, North Carolina, South Carolina, and Tennessee) forbid police collective bargaining. In another four states (Alabama, Colorado, Mississippi and Wyoming) no state statutes or case laws govern collective bargaining and the actual legality of collective bargaining depends on local laws (Sanes and Schmitt, 2014).

to be characterized by a cohesive "police culture" with high levels of in-group solidarity, often manifested in a norm of mutual protection and cover-ups of bureaucratic transgressions (Zoorob, 2019). These dense and cohesive unions make police networks particularly conducive to collective action.

Additionally, police forces have strong policy preferences. Unlike most unions, police unions have gravitated towards right-wing policies throughout American history, often resisting criminal justice reform initiatives. The major Fraternal Order of Police (FOP), for example, has supported legislation that turns the killing of police officers into a hate crime and has backed the "Police Bill of Rights," which protects officers accused of misconduct in several states (Zoorob, 2019). Like other public sector unions, police unions also have strong vested interests in maintaining the material benefits from government work, including large budgets, fringe benefits, and their political autonomy and discretion (cf. Moe (2015)).

There is ample anecdotal evidence that police forces are powerful agents who are willing and able to exert political pressure on their principals. When preferences of policymakers and police diverge over contract negotiations, funding issues, or oversight, US municipal leaders often report facing a unique kind of militancy from police unions that is unknown to interactions with other local interest groups (Blumgart, 2020). Besides lobbying, litigating, of picketing, police unions increasingly use their ability to play on the public's fear of crime during confrontations with local officials. A common tactic is to publicly and vocally warn that local politicians are courting danger by acting against the interests of local police forces. For instance, in response to proposed cuts to police budgets, police forces employed billboards with slogans such as, "Welcome to the 2^{nd} most dangerous city in California - Stop laying off cops" (in Stockton, California) or "Danger: enter at your own risk, this city does not support public safety" (in Memphis, Tennessee) (Blumgart, 2020).

Anecdotal evidence also suggests that police officers use work slowdowns and strategic depolicing for political ends. While strikes by law enforcement are not permissible in virtually all US states (Sanes and Schmitt, 2014), police can shirk their daily responsibilities to exert

political influence. By avoiding certain areas or activities (such as traffic stops), they aim to voice discontent and intensify their pressure on local politicians. For example, when proposing a budget cut to the local police department in 2018, Minneapolis City Council member Steve Fletcher received complaints from business owners and constituents, indicating that officers were delaying response times to calls for service in his district (Blumgart, 2020). As the politician put it:

> "They'd show up 45 minutes later and say, 'Well, we would have loved to come, but talk to your council member about why we can't.' Many of my constituents were given the very strong impression by MPD [the Minneapolis Police Department] that we had somehow just created a situation where they couldn't respond to 911 calls. [...] This is the challenging thing about having a group of employees who are authorized to use force, and who we rely on in very vulnerable situations. There's that kind of implied reminder that officers can use independent judgment to use force on you or not, create consequences for you or not, protect you or not. That does create leverage, and that leverage can be exploited." (Blumgart, 2020)

Yet, little scholarly work has explicitly examined the existence and, more importantly, the political nature of police shirking. Interviews with small samples of officers indicate that they believe police shirking happens and that individuals engage in this behavior for various reasons, including civil litigation, new laws regulating police behavior, or riots (Oliver, 2017; Nix et al., 2018). However, quantitative evidence on the phenomenon is mixed. For instance, while some scholars find declines in proactive policing following public protests (Shjarback et al., 2017; Roman et al., 2023) or pattern-or-practice investigations by the Department of Justice (Devi and Fryer, 2020), others find little or no evidence of such behavior on the aggregate (Chanin and Sheats, 2018; Marier and Fridell, 2020).

My theoretical claim and empirical analysis deviate from this existing work on de-policing in two important ways. First, instead of characterizing effort shirking as a blunt instrument for police to oppose criticism by civilians and public officials across an entire city, I examine how police use shirking to target specific non-aligned politicians. Second, to study de-policing on the aggregate, prior work compares police behavior before and after major events, such

as large-scale protests or investigations that affected the entire agency. This poses major challenges for causal identification. Specifically, these events go hand-in-hand with other changes in city policies that affect police behavior. For example, public protests directed at policing often entail changes to police management and resources, and shifts in police performance could result from workforce issues rather than strategic shirking. Hence, before-after designs used in prior research likely suffer from numerous confounders. As I stipulate in greater detail in Section 4, I account for such spurious correlation by leveraging *within-jurisdiction* variation in the political environment and behavior of the police.

Taken together, I expect that the police aim to punish politicians and affect public policy by exerting lower effort in areas where incumbents run on police-reform agendas. By evoking perceptions of deteriorating safety and higher crime rates among the public, police can exert latent political pressure on politicians. Rising public safety concerns could trigger complaints by constituents to their political representatives and may push politicians to revisit their reform agendas. While police likely do not aim to influence elections *directly* through shirking, these mechanisms can also have detrimental electoral consequences for incumbents. The police can paint specific incumbents in these areas as "soft on crime" and anti-police—labels that elected officials try to avoid and for which they tend to be punished at the polls (Huber and Gordon, 2004; Drago et al., 2019). Additionally, by reducing their effort in policing districts of non-aligned politicians, the police can intensify the salience of public safety issues for voters and thus increase the importance of their own agendas in local electoral campaigns.

4 Empirical Case, Data, and Research Design

4.1 NYPD's 2021 Budget Cut

For the empirical analysis I focus on the behavior of NYPD officers in response to the significant cuts to the NYPD budget in FY2021. On June 30, 2020, the New York City Council

agreed to a grim budget for the following fiscal year that sharply reduced municipal services. The NYPD experienced the most significant cut in its funding, as the City Council reduced its budget by about $1 billion and imposed hiring freezes for police officers (Rubinstein and Mays, 2020). In particular, in an attempt to reform the NYPD organization and placate calls to defund the police, council members reduced overtime payments by 67%, eliminated the July 2020 police academy class of roughly 1,160 officers, cancelled hiring plans for traffic enforcement agents and civilian positions, and transferred several responsibilities from the police department to other city agencies (including school safety and monitoring of illegal vending) (City of New York, 2020; Rubinstein and Mays, 2020). Yet, since the latter component was not officially part of the FY2021 adopted budget, the final cut amounted to $415 million, with most of the savings due to reductions in both civilian and uniformed overtime ($328 million) (Citizens Budget Commission, 2020).

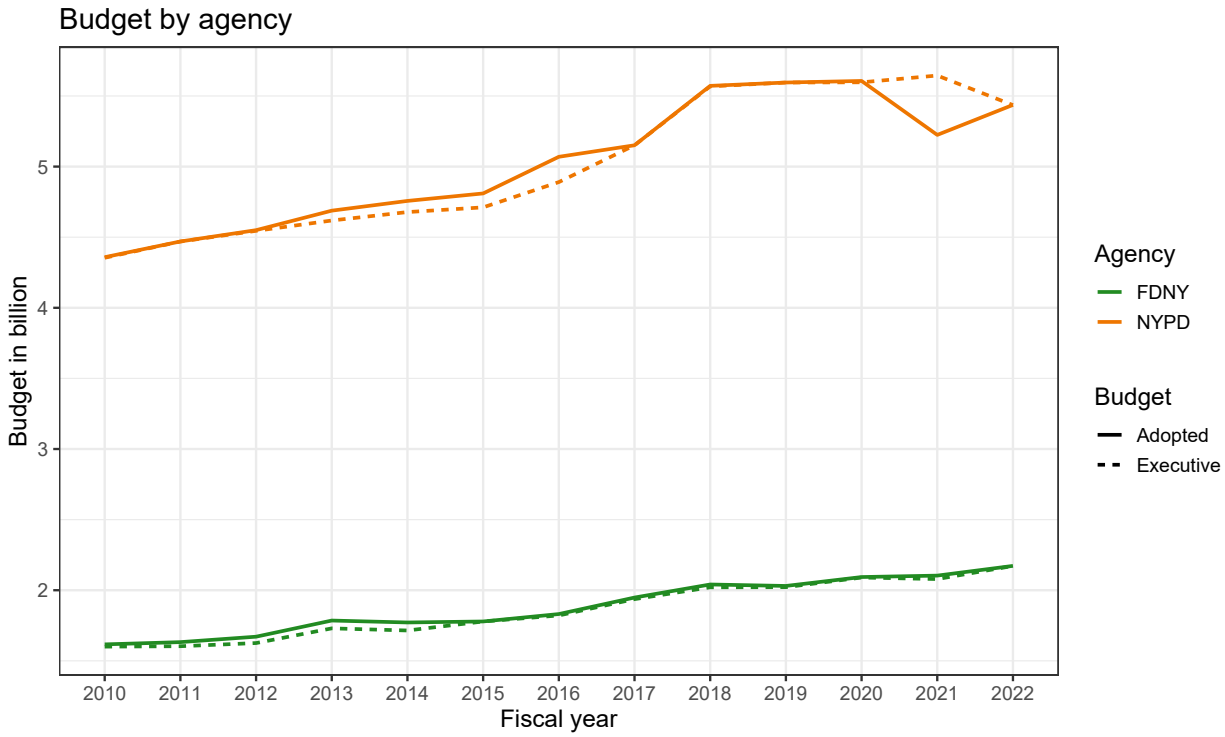
Accompanied by growing public scrutiny and prolonged protests outside city hall in the week before the vote publicly known as "Occupy City Hall", the FY2021 budget became a highly contentious issue in the NYC Council, especially in light of the 2021 local elections. The budget negotiations primarily centered on the question of how deeply to cut the NYPD's budget and the hefty reduction in police funding became the decisive feature of council members' voting behavior (Coltin, 2020; Rubinstein and Mays, 2020). The final vote on the budget proposal was unusually divided, with 32 council members in favor and 17 members voting against the reductions in police funding. In contrast, during the previous three years, the City Council had approved the budget unanimously.

The scope of the budget adjustment was unprecedented and largely unexpected. As Figure 1 illustrates, NYPD's operating budget increased in almost all years prior to FY2021. Additionally, former NYC mayor Bill De Blasio's executive budget proposal in April 2020 included a minimal cut of only $24 million, and although the mayor promised on June 7 to shift some of the NYPD's budget to social services and youth programs, he declined to specify the amount of cuts (Coltin, 2020). Just weeks before the budget deadline, city

council leaders agreed on June 12 to set a goal of $1 billion in cuts to the NYPD budget and De Blasio eventually approved their proposal on June 23 (Coltin, 2020; New York City Council, 2020). The Police Benevolent Association (PBA), the NYPD's largest police union, promptly voiced dissent against the proposal, threatening that

> "For decades, every time a city agency failed at its task, the city's answer was to take the job away and give it to the NYPD. If the City Council wants to give responsibility back to those failing agencies, that's their choice. But they will bear the blame for every victim, for every New Yorker in need of help who falls through the cracks. They won't be able to throw cops under the bus anymore."[8]

Figure 1: Operating Budget of NYPD and FDNY Over Time



Note: The executive budget is based on the mayor's submission of a proposed budget in April each year. The adopted budget is the finalized budget in each fiscal year that the City Council votes on. Source: NYC City Council Expense and Contract Budget Resolutions, Fiscal Years 2010-2022.

Police unions play an important role in NYC politics and the operations of the NYPD.

[8]PBA President Patrick Lynch on Twitter, June 12, 2020. https://twitter.com/NYCPBA/status/1271576847399235584?ref_src=twsrc%5Etfw%7Ctwcamp%5Etweetembed%7Ctwterm%5E1271576847399235584%7Ctwgr%5E%7Ctwcon%5Es1_&ref_url=http%3A%2F%2Fgothamist.com%2Fnews%2Fcouncil-unveils-proposal-to-cut-1-billion-from-nypd-budget-identifying-inefficiencies.

In addition to the PBA, which represents all sworn NYPD officers (about 24,000), there are four major police unions representing various ranks of NYPD employees (the Detectives' Endowment Association, the Sergeants Benevolent Association, the Lieutenants Benevolent Association, and the Captains' Endowment Association). These organizations function as private corporations supported by their members' dues, are responsible for negotiating NYPD contracts, provide legal services, and administer health and welfare benefits to their members. Additionally, they are publicly known for their inflammatory media presence and their lobbying activities to influence NYC legislation and local elections.[9]

The funding changes in the NYPD had significant implications for rank-and-file employees at the agency. The NYPD spends most of its annual budget on personnel. For instance, in FY2020, 92% of the operating budget was for personnel services, while the remainder was assigned to purchase supplies, materials, and other services for the agency's operations.[10] Additionally, overtime spending is an important source of officers' income. In FY2020, overtime spending totaled $635 million, 44% of all citywide overtime expenses (Citizens Budget Commission, 2020). In the same year, the median share of overtime pay out of total pay for NYPD employees amounted to 12%.[11]

As a result of the budget cut, overtime pay per NYPD employee dropped by 45% between FY2020 and FY2021, in sharp contrast to its steady growth over previous years. Similarly, the civilian and uniformed headcount at the NYPD decreased by 11% and 3% in FY2021, respectively. While this trend was partly a result of increased retirement of police officers across the nation following George Floyd's death in May 2020, the new budget slashed the number of employees substantially through vacancy reductions for traffic enforcement agents, hiring freezes for non-safety personnel, and cancellations of the FY21 academy and cadet classes (Citizens Budget Commission, 2020).[12]

[9]For instance, during the 2021 elections, the PBA told its members to list specific candidates for NYC mayor and the PBA Super PAC spent more than $450,000 to swing several City Council races in favor of police allies (Blau, 2021).

[10]https://www1.nyc.gov/assets/omb/downloads/pdf/erc6-20.pdf.

[11]Calculated from FY2020 NYC payroll data.

[12]See Figure A2 for more details.

4.2 Measuring Police Behavior: Calls for Service

To measure police behavior and effort, I use fine-grained data on 911 calls for service, namely officer response times to calls (i.e., the time between when the call was logged in the dispatch system and when officers arrived at the scene). These data are suitable to test my theory for several reasons. First, officers spend a substantial amount of their time responding to 911 calls (Neusteter et al., 2020). Most of the incidents are noncriminal in nature—citizens make calls to complain or request that an officer perform a welfare check. As a result, police officers have a considerable amount of discretion in when and how they respond to these calls for service, which is often reflected in a large variation in dispatcher and officer response times to calls across departments and incidents (Neusteter et al., 2020). Second, earlier studies indicate that neighborhood characteristics, including the economic wealth and demographics of residents in an area, affect call patterns and police officers' response times (Cihan et al., 2012; Lee et al., 2017). Yet, no previous work has considered the effect of the *political* characteristics of neighborhoods on officer behavior in response to calls for service. Third, officers' response times to calls are related to people's perceptions of the quality of policing. Using different response time surveys across various US cities, several studies have found negative correlations between response times and respondents' evaluations of police performance (Pate et al., 1976; Parks, 1984). Additionally, some work suggests that shorter response times are associated with higher arrest rates (Cihan et al., 2012; Lee et al., 2017; Blanes i Vidal and Kirchmaier, 2017). There are further technical advantages to using calls for service data to measure police effort. In addition to the timing and chronology of each call, the data includes detailed information on the location of the incidence and classifications for the call type and priority level. This allows me to geocode each call and assign it to a specific political district.[13]

[13]Besides 911 call data, I collected various additional data for supplementary analyses presented below. Table A1 in the Appendix lists all data sets together with the relevant sources.

4.3 Council Members' Voting Behavior

Figure 2 shows the distribution of council members' voting behaviors on the budget proposal across NYC's 51 council districts.[14] The map illustrates that both "yes" and "no" votes are distributed across the city, and districts with opposite voting patterns share a border in several instances. Additionally, these district borders cut across NYPD precinct boundaries. As I discuss in greater detail in Section 4.4, this allows me to compare changes in 911 response times across districts *within* police precincts using precinct fixed effects in my empirical designs.

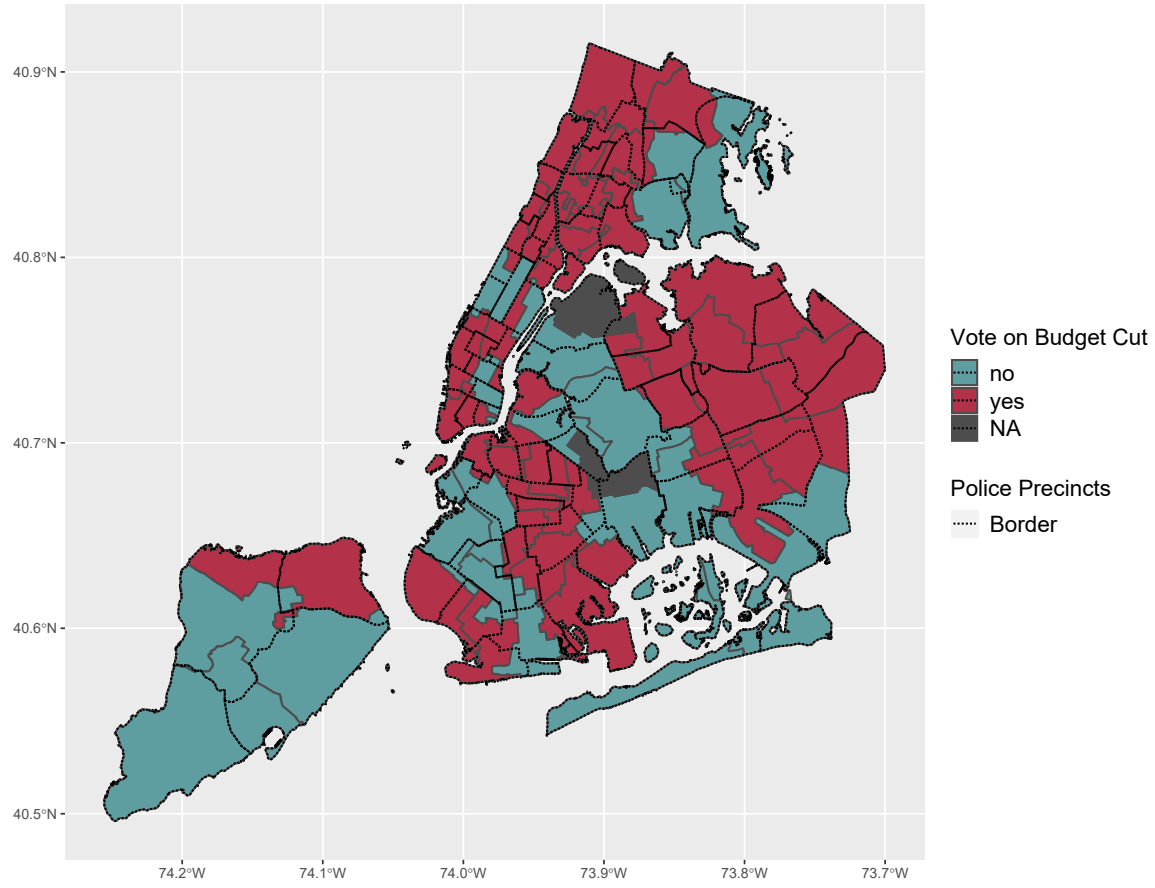
To provide some information on possible factors influencing a council member's voting behavior, Table A2 shows summary statistics of district characteristics. Unsurprisingly, districts in favor of the budget cut are somewhat more progressive and more crime-ridden. These areas had significantly larger minority populations; higher vote shares for President Biden in 2020; and more valid felony, misdemeanor, and violation complaints.

4.4 Triple Difference-in-Differences Design

To identify the effect of preference alignment between the NYPD and New York City Council members on police behavior my main specification leverages the fine-grained geographic information on 911 calls in a difference-in-differences (DiD) model. I compare response times in districts of council members in favor of the budget cut to response times in districts of council members who voted against the budget reduction, before and after the vote on June 30. This implies that the 32 council members who supported the significant cut to the NYPD's funding are deemed to be non-aligned with police preferences, while the 17 representatives who opposed the policy remained aligned with the NYPD's general interests. Yet, in a simple DiD model, it is inherently difficult to distinguish politically motivated shirking of police officers from general time-specific dynamics across districts (e.g., differences in traf-

[14] One council seat (37) was vacant at the time of the vote and one member (Costa Constantinides) was absent from the session.

Figure 2: NYC Council Votes on 2021 Budget



fic). To account for time-specific trends in response times, I additionally use response times of firefighters to 911 medical emergency calls as my third control dimension. Firefighters are largely comparable to police officers in their unionization rates and local government structure. Yet, unlike funding for the NYPD, Figure 1 shows that the adopted budget of the FDNY increased relative to the planned budget in April 2020 and the operating budget in previous fiscal years. Hence, since firefighters had little reason to organize politically to exert pressure on City Council members, EMS response times can serve as a credible

counterfactual in bureaucrats' reactions to 911 calls absent electorally motivated behavior.

Thus, I estimate the following econometric model:

$$\begin{aligned}\text{response time}_{icpda} = \beta_1 \text{yes vote}_c \times \text{after vote}_d \times \text{NYPD}_a + \mathbf{X'_{icpda}}\rho \\ + \delta_c + \eta_p + \gamma_d + \nu_a + \varepsilon_{icpda}\end{aligned} \tag{1}$$

where response time_{icpda} is the response time of call i in district c, day d and agency a, yes vote_c is an indicator equal to 1 if council member of district c voted in favor of the budget cut, after vote_d indicates whether a call happened after June 30, 2020 and NYPD_a indicates whether the NYPD or the FDNY responded to the 911 call. $\mathbf{X_{icpda}}$ is a vector of covariates, including the total number of calls per day and other proxies for crime rates and demand for police presence. δ_c, η_p, γ_d and ν_a are district, police precinct, date and agency fixed effects, respectively.

Importantly, police precinct fixed effects alleviate concerns that the estimated treatment effect is driven by mechanical changes in the number of available patrol officers due to reductions in staffing, overtime, or voluntary retirements. Since such police patrol management is organized on the police precinct level, the within-precinct DiD setup ensures that mechanical changes in response times for a given precinct are subsumed by delays among portions of the precinct that voted against the budget cut. *Differential* increases in response times in yes-voting regions of the precinct can thus help identify delays resulting from politically motivated behavior of police. This design implies that only precincts with variation in the vote patterns *within* the precinct boundaries contribute to the estimated treatment effect. 62 of the 77 NYPD precincts in my sample respond to both treatment and control districts, thus ensuring that the effective sample is close to the overall sample. Additionally, district fixed effects account for differences in district characteristics (see Table A2). To the extent that these characteristics and their influence on NYPD response times stay constant across my sample period, my treatment effect estimates remain unbiased. I cluster standard errors

ε_{icpda} on the district level.[15]

Figure 3: Visual Representation of DiD Identification, Hypothetical

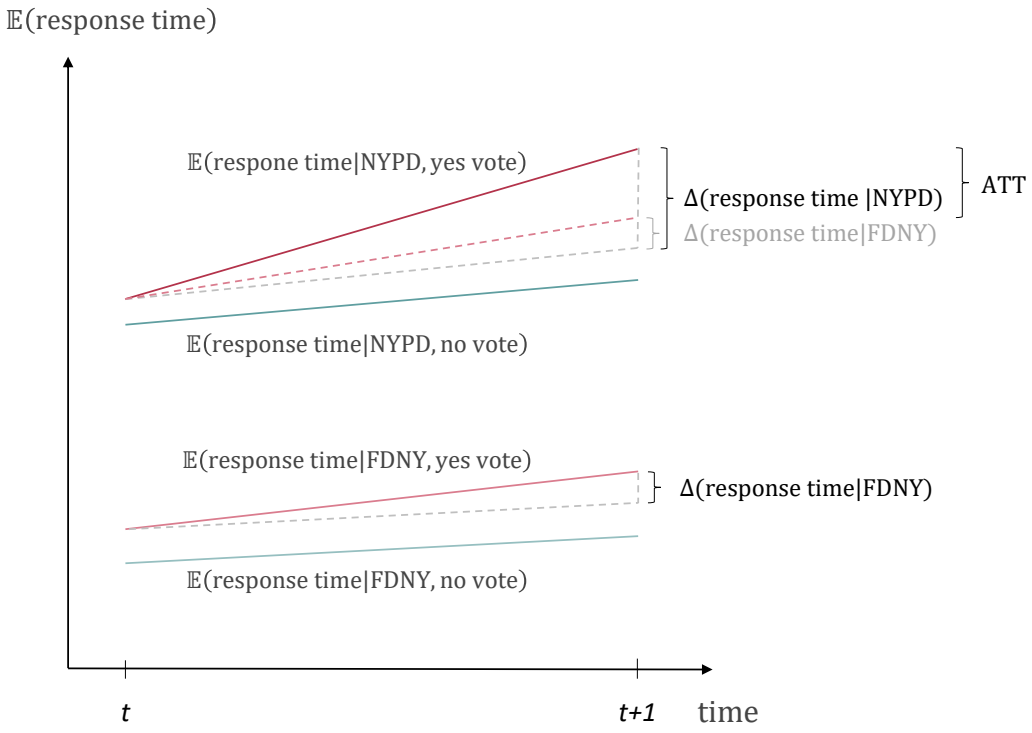


Figure 3 provides a graphical representation of the triple DiD identification strategy. While a simple DiD design would only rely on the divergent trends in NYPD response times within yes-voting districts vis-à-vis no-voting districts over time (i.e. Δ(response time|NYPD)), the triple DiD design incorporates the corresponding trends in FDNY response times in order to estimate the causal effect of the budget vote on bureaucrats' behavior ($ATT = \Delta(\text{response time}|\text{NYPD}) - \Delta(\text{response time}|\text{FDNY})$). The identifying assumption of this design is that *differences* in response times between NYPD and FDNY officers across treatment and control districts would have followed similar trends in the absence of the budget

[15]For the main analysis, I remove response times for calls between May 30 - June 15, when numerous and large protests took place in NYC across several locations as a response to George Floyd's killing. Consequently, response times were on average almost three minutes (22%) longer between May 30 and June 15, 2020, than in previous months. Table A3 presents estimation results including these strong outliers, showing that the main results largely hold with the full sample of calls.

vote.[16]

4.5 Spatial Difference-in-Discontinuities Design

The triple DiD design crucially hinges on the validity of the parallel trends assumption. This might be complicated by the fact that police might shirk shortly *before* the vote in the hope to influence council members' voting behavior. Although this dynamic might be alleviated by the heightened public attention to the issue of policing before the vote, which reduced the ability of police to shift politicians' positions on the issue, it can pose challenges to my triple DiD identification strategy. To leverage more cross-sectional variation, I therefore supplement the analysis with a spatial difference-in-discontinuities design. As shown in Figure 4, I use a spatial RDD design to compare NYPD response times in close proximity to the council district borders that separated yes and no voting members. For each 911 call I calculate the minimum distance to a separating border to construct the running variable. To provide estimates for the changes in these RDD estimates before and after the vote, I split my sample along the date of the budget vote.[17] For both time periods, the resulting model is estimated as follows:

$$\begin{aligned}\text{response time}_{icpd} = \alpha + \tau \text{yes vote}_c + \beta_- \text{distance}_{icpd} + \beta_+ \text{yes vote}_c \times \text{distance}_{icpd} \\ + \eta_p + \varepsilon_{icpd}\end{aligned} \tag{2}$$

where response time$_{icpd}$ is the response time of call i in district c and day d, yes vote$_c$ is an indicator equal to 1 if council member of district c voted in favor of the budget cut. distance$_{icpd}$ represents the distance of call i to the border distinguishing these two categories of districts, and contains only units distance$_{icpd} \in [-h; h]$, where $-h$ and h denote the MSE-

[16] I also estimate simple DiD models, separately for the NYPD and FDNY. Reassuringly, the results in Table A4 indicate that there is a positive ATT estimate for NYPD 911 calls, while the estimate for FDNY is smaller, negative and statistically insignificant.

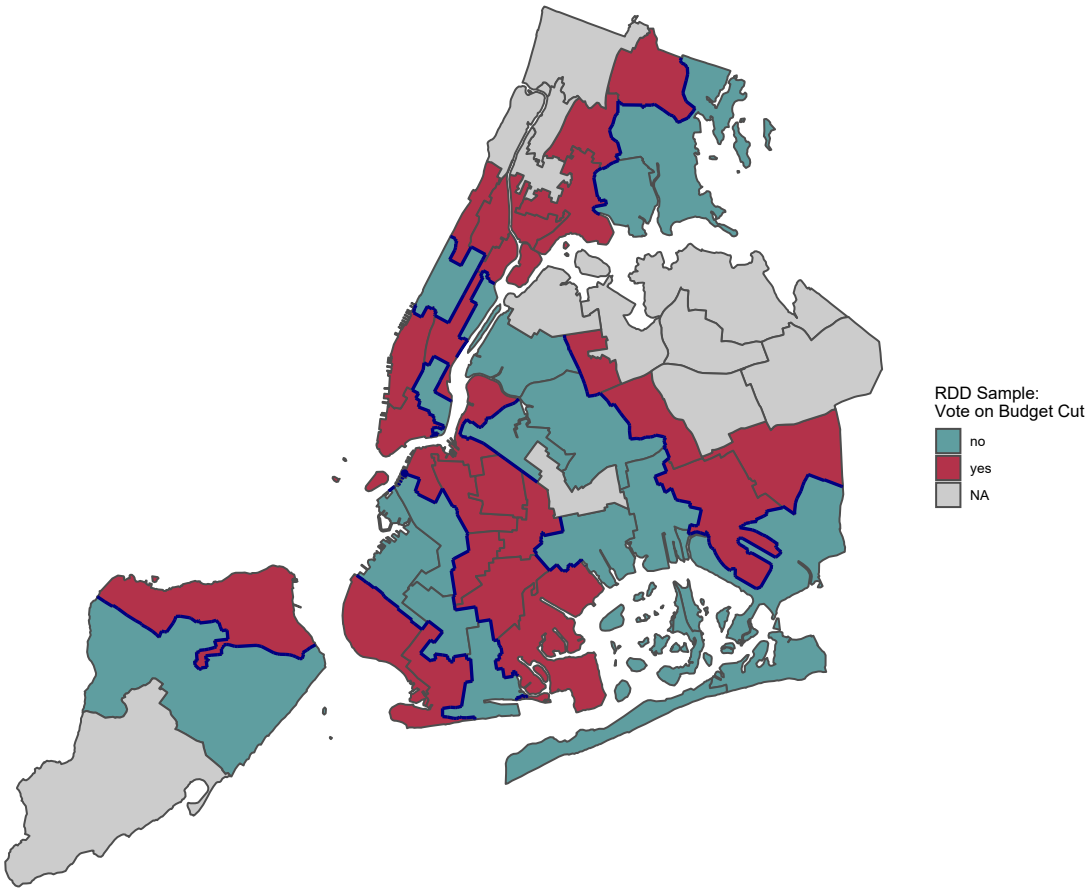
[17] As before I exclude dates affected by the George Floyd protest (May 29 - June 15, 2020). Additionally, to avoid concerns about anticipatory police behavior right before the vote, I also exclude calls between June 16 - June 30, 2020.

optimal bandwidths to the left and right of the border, respectively. The model is estimated using local linear regression with a triangular kernel (Calonico et al., 2014). NYPD precinct fixed effects again ensure comparisons of calls within the same police administration. I use Monte Carlo simulations to provide confidence intervals of the difference in RDD estimates (King et al., 2000).

A few clarifying comments are warranted. Like all spatial RDD settings that rely on administrative borders, estimates of τ likely suffer from compound treatment problems, since many characteristics beyond a council member's vote change discontinuously along district borders, such as road quality or demographics. Yet, this is less problematic in a *difference*-in-discontinuities design. To the extent that these characteristics and their effect on NYPD response times stay constant across the periods before and after the vote, the *difference* in the RDD treatment effects remains unbiased. Yet, if other determinants of NYPD response times change over time along the separating border, the difference in RDD estimates represents an estimate for the heterogeneity in the treatment effect across periods, rather than a full-fledged causal moderation analysis. To alleviate these concerns, I estimate RDD estimates where I match observations across periods using coarsened exact matching on either side of the cutoff on relevant time-variant covariates, including call type and the number of calls per day on the zip code level.[18]

[18]Figure A3 and Figure A4 show the resulting balance in these covariates after matching.

Figure 4: RDD Sample



5 Results

5.1 Triple Difference-in-Differences Design

Figure 5 depicts the raw trends in average 911 response times across different types of districts over time and for different agencies. The dynamics in police behavior seem to corroborate the general theory. The figure provides some graphical evidence that NYPD average response times were elevated after and in the two weeks before the budget vote, and

more so in non-aligned council districts and relative to FDNY-EMS calls. The figure also highlights cyclical trends in response times (e.g., due to COVID19 waves), which my triple DiD design accounts for.

Figure 5: Trends in 911 Response Time across Districts

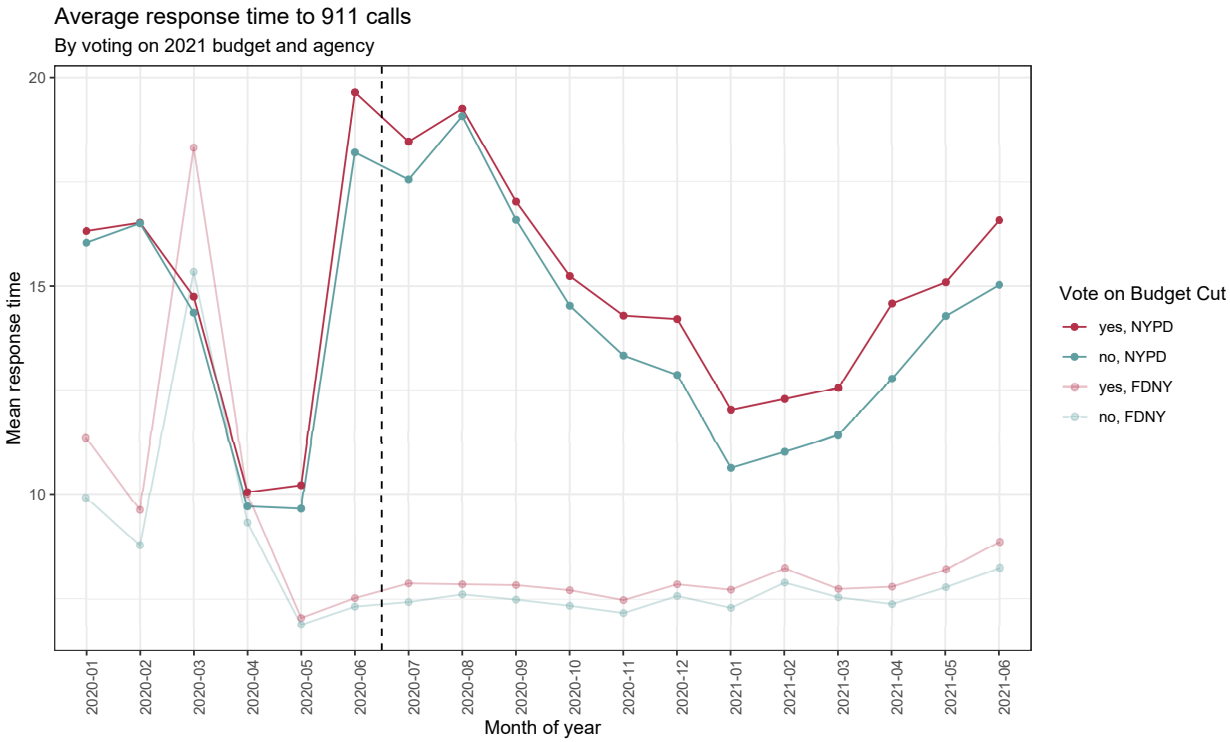


Table 1 evaluates trends in police 911 response times using the triple DiD model in Equation (1). We find that NYPD on average took about 5 minutes longer in their response times in aligned districts than FDNY before the budget vote (NYPD). After the vote, response times went up by about 2.5 minutes in aligned districts (after vote × NYPD). Most importantly, in line with the theory the triple DiD estimate is positive, suggesting that response times in non-aligned districts increased by about one minute and 30 seconds more for NYPD calls than for FDNY calls after the budget vote (yes vote × after vote × NYPD). With an average response time of about 13.4 minutes throughout the sample period, this increase is substantial. Similarly, an additional minute in police response times is large enough to elicit public concern. For instance, after examining data of overall NYPD response times in 2020, then-Brooklyn Borough President Eric Adams said that "[a] minute in policing is a lifetime,

when you are wrestling with someone, when you are being robbed, that extra 60 seconds is the difference between an apprehension or even a person's life" (Gross, 2020). The size and precision of this treatment effect is largely robust to further controlling for the demand for police presence (in Models (2)-(5)). Model (2) accounts for the total number of calls in districts and precincts per day. Similarly, Model (3) and (4) proxy demand for police presence using the total number of shootings in districts and precincts each day and the number of valid felony, misdemeanor, and violation crimes reported to the NYPD. Model (5) incorporates fixed effects for the official importance level of NYPD and FDNY calls. This separates critical and serious crime incidents from non-critical crimes and non-crime calls for NYPD, and life-threatening from non-life-threatening emergencies for FDNY.

To evaluate pre-treatment trends, I re-estimate Equation (1) in an event study setup:

$$\text{response time}_{icpda} = \sum_{\tau \in [-6,11]} \beta_\tau \text{yes vote}_c \times \text{NYPD}_a + \mathbf{X}'_{\mathbf{icpda}}\rho + \delta_c + \eta_p + \gamma_d + \nu_a + \varepsilon_{icpda} \tag{3}$$

Figure A5 shows the respective treatment effects by month. While the estimates are imprecise, there is some indication of pre-treatment divergence in 911 response times, particularly in June 2020. This could be the result of ramifications from the George Floyd protests and police anticipating politicians' positions on the budget leading up to the official vote on June 30. In fact, on June 12 council speaker Johnson together with the leaders of city council caucuses and the chairs of the committees on finance, capital budget, and public safety published a joint statement to announce the $1 billion cut to NYPD spending (New York City Council, 2020), and many council members published their vote intentions around that time.[19]

[19] https://docs.google.com/spreadsheets/d/1DAan2yEhaO8Mt9VmADAxNbCwhX8usfsSL51Pw9m4Fh0/edit#gid=2032235041.

Table 1: Effect of Approving 2021 Budget on 911 Response Times

	(1)	(2)	(3)	(4)	(5)
yes vote × after vote × NYPD	1.480**	1.432*	1.479**	1.485**	1.473*
	(0.734)	(0.732)	(0.735)	(0.735)	(0.742)
NYPD	4.689***	7.316***	4.686***	4.694***	3.525***
	(0.696)	(0.738)	(0.697)	(0.697)	(0.701)
yes vote × NYPD	-0.276	-0.278	-0.276	-0.279	-0.235
	(1.107)	(1.066)	(1.108)	(1.107)	(1.099)
after vote × NYPD	2.522***	2.627***	2.528***	2.516***	2.563***
	(0.477)	(0.474)	(0.478)	(0.477)	(0.485)
yes vote × after vote	-0.803	-0.841	-0.809	-0.804	-0.796
	(0.627)	(0.635)	(0.636)	(0.625)	(0.636)
total calls (log)		-1.799***			
		(0.225)			
# of shootings (log)			2.192***		
			(0.383)		
# of complaints (log)				0.339**	
				(0.160)	
District FE	✓	✓	✓	✓	✓
Police Precinct FE	✓	✓	✓	✓	✓
Date FE	✓	✓	✓	✓	✓
Call Importance FE					✓
Observations	9,286,084	9,286,084	9,286,084	9,286,084	9,286,084
Mean of DV	13.346	13.346	13.346	13.346	13.346
Adj. R^2	0.032	0.032	0.032	0.032	0.032

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Dependent variable: Response time in minutes. Coefficients for yes vote_c and after vote_d absorbed by district and day fixed effects, respectively. Call importance fixed effects account for the two main levels of call importance for NYPD and FDNY calls: (1) Critical and serious crime incidents and life-threatening medical emergencies, (2) Non-critical crimes and non-crime incidents and non-life threatening medical emergencies. Cluster robust standard errors in parentheses, by district (49).

5.2 Spatial Difference-in-Discontinuities Design

To alleviate concerns about anticipatory behavior and the validity of the parallel trends assumption, I turn to the spatial difference-in-discontinuity as a secondary analysis. Table 2 shows the results. Interestingly, the negative RDD estimates in both periods suggest that NYPD officers respond faster to calls in treatment districts (yes votes) compared to neigh-

boring control districts (no votes), both before and after the vote. This might be attributed to systematic differences in these neighborhoods that determine response times, including traffic, road quality etc.[20] More importantly for my argument, the difference in the RDD estimates is positive and significant. In line with previous results, the model suggests that for neighborhoods in close proximity to the district borders NYPD slowed down by about 68 seconds per call in yes voting districts relative to no voting districts after the budget vote.

Table 2: Effect of Approving 2021 Budget on 911 Response Times
Spatial Difference-in-Discontinuities

	Before Vote	After Vote	Difference
yes vote (robust bias-corrected)	-2.756	-1.625	1.131
	(-3.14; -2.371)	(-1.878; -1.373)	(0.891; 1.810)*
Precinct FE	✓	✓	
Matched Sample	✓	✓	
Kernel	Triangular	Triangular	
Bandwidth	mserd	mserd	
BW_est	206.566	203.972	
Obs_left	599,725	1,411,730	
Obs_right	1,254,137	2,844,357	

Dependent variable: Response time in minutes. 95% confidence intervals shown in parentheses. * 95% CIs from Monte Carlo simulations.

6 Mechanisms

What type of calls do officers use for leverage shirking? Police might rely on two main ways to drive up response times. First, officers can reduce pro-active policing by minimizing the number of officer-initiated calls (Roman et al., 2023). If police encounter events that warrant a police response, they can log calls themselves. These officer-initiated calls are characterized by response times close to zero in my data. As Table A6 indicates, I find no evidence that NYPD officer-initiated calls decreased disproportionately in non-aligned districts.

[20] Table A8, for instance, indicates that calls in yes-voting parts of the RDD sample are slightly closer to the precinct headquarter, thus presumably shortening the amount of travel necessary.

Second, increases in response times in non-aligned districts might be driven by late arrivals and "no shows" of officers to calls where police have sufficient discretion in how they address the incident and face fewer costs for shirking. To evaluate this mechanism, Figure 6 depicts quantile treatment effects.[21] The estimated treatment effects are largest at the upper end of the response time distribution, while remaining small and insignificant for other calls. Non-aligned districts faced more particularly long calls (response times increased by 8.1% [1.8 minutes], 10% [3 minutes] and 15% [6.7 minutes] for the 85th, 90th and 95th quantiles, respectively).[22] Average response times by call-type, location and period in Figure A6 further indicate that these long response times are predominantly clustered among calls where police have broad discretion in how they address the incident, such as crimes not in progress, disputes and vehicle accidents.

My argument also highlights the role of police unions as the crucial bureaucratic actor in targeting non-aligned officials and organizing politically motivated shirking. There is ample qualitative evidence supporting this notion. In the months following the budget vote, NYPD's police unions engaged in various smear campaigns against council members who had supported the budget cut. A common tactic was to leverage crime incidents in their districts and connect these to council members' support of the budget on social media.[23] Similarly, police unions publicly defamed council members and their decisions on public safety policies in their districts. For instance, the Lieutenants Benevolent Associated used a video installation outside of a council member's office, shaming him for "anti-cop laws" and proclaiming that the council member "voted to defund the police among other anti-police, and anti-public safety bills. He doesn't care about the well being [sic] of his constituents, he cares about bowing to a hashtag!"[24] Police unions also weren't shy to call

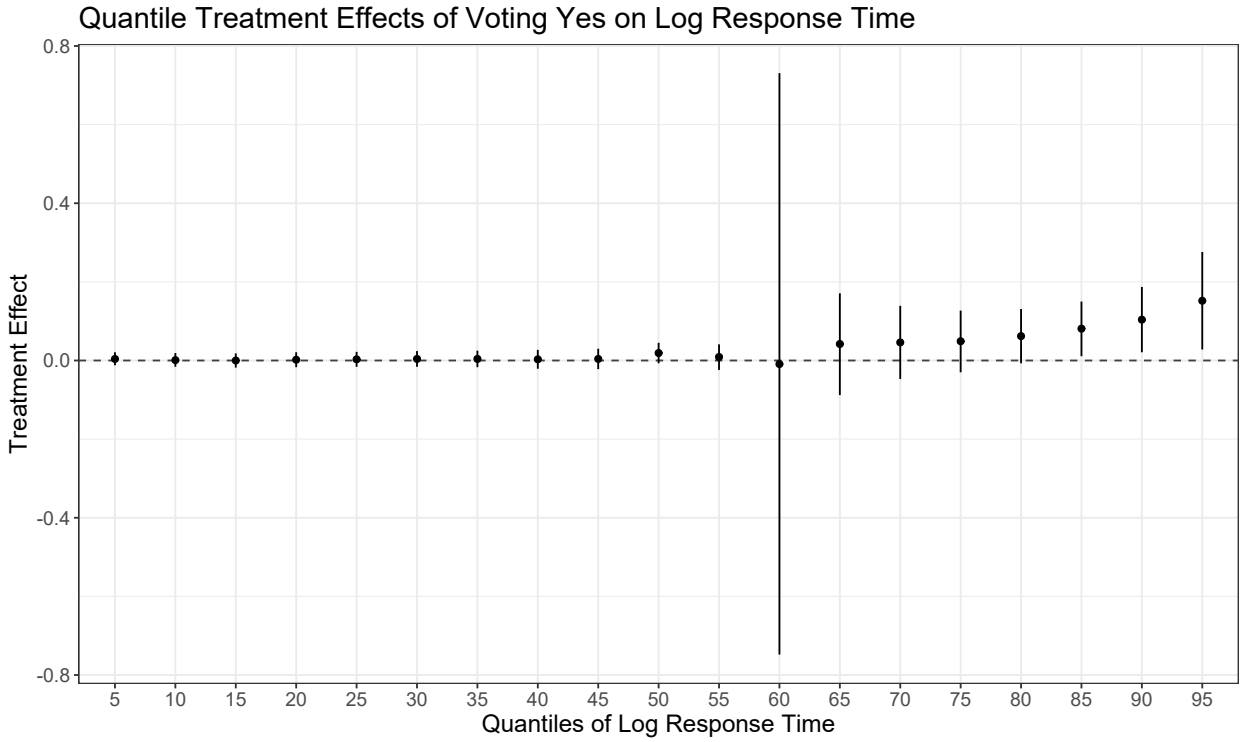
[21] To ensure better comparability of effect sizes across quantiles, the underlying models use log response times.

[22] To alleviate concerns that these effects are driven by a few outliers in response times, Table A5 shows the robustness of my DiD results using winsorized response times.

[23] https://twitter.com/NYCPBA/status/1288122515898822657; https://twitter.com/NYCPBA/status/1311704141224345603; https://twitter.com/SBANYPD_Archive/status/1334693569601351680; https://twitter.com/SBANYPD_Archive/status/1277424114249146374.

[24] https://twitter.com/lbanypd/status/1377297021036589074.

Figure 6: Quantile Treatment Effects



Note: Depicted are quantile treatment effects, estimated using recentered influence functions with 90% confidence intervals.

on voters to punish council members and the mayor for their public safety policies following the budget cut. Besides endorsing specific candidates for races in the 2021 city elections[25], NYPD's police unions campaigned against incumbent officials using slogans such as "We will say it again: the Mayor and the City Council have surrendered the city to lawlessness. Things won't improve until New Yorkers hold them responsible"[26] or "keep voting Democrat and you'll have war zones.. just ask Chicago, Detroit, Baltimore!"[27] What is more, as concerns about rising response times arose in the public discourse, police unions attributed the blame for reduced public service quality to the city council and the mayor.[28] When then-council member Ritchie Torres and then-borough president Eric Adams called for an

[25] https://twitter.com/NYCPDDEA/status/1407332800345346054.

[26] https://twitter.com/NYCPBA/status/1277671870205169665.

[27] https://twitter.com/SBANYPD_Archive/status/1277424114249146374.

[28] https://twitter.com/NYCPBA/status/1277671870205169665; https://twitter.com/NYCPBA/status/1300206634279620611.

investigation into longer response times and a possible NYPD slowdown in September 2020, police unions reacted with personal insults, leading council members to call for resignations among union officials.[29] In sum, this qualitative evidence substantiates how police unions targeted non-aligned politicians in their campaigning in the wake of the budget cut, leveraged their influence on voters in their political messages and intended to affect the 2021 NYC elections.

Police unions can pass on their political strategy to the relevant police forces in at least two ways. First, they may coordinate with their rank-and-file members directly. Anecdotal evidence suggests that NYPD's police unions regularly communicate with their members, giving them instructions on how to vote and contribute in local elections and how to adjust their work effort politically. For instance, following mayor DeBlasio's police reform endeavors in the wake of Eric Garner's death in 2014, the PBA circulated a bulletin instructing its members that "Starting IMMEDIATELY: At least two units are to respond to EVERY call, no matter the condition or severity, no matter what type of job is pending, or what the opinion of the patrol supervisor happens to be."[30] Second, to put pressure on certain council members, police unions could urge precinct management to adjust deployment within precincts. Precinct chiefs have considerable discretion in how to allocate resources across their precincts and police officers have been shown to be highly responsive to managerial directives (Mummolo, 2018). Unfortunately, precinct-level deployment information is unavailable for the NYPD, thus precluding a direct test of this conjecture for my case.[31]

How did citizens react to the strategic shirking of police officers? While systemic data on citizens' complaints about NYPD behavior is unavailable[32], I illustrate possible downstream electoral consequences of the budget vote and presumably the police resistance in Appendix D. Analyzing changes in vote shares of incumbent council members between 2017

[29] https://twitter.com/RitchieTorres/status/1303400519302631431.

[30] https://investortimes.com/freedomoutpost/nypds-cop-union-become-wartime-police-department-two-officers-slain/; It is unclear whether there was similar communication in 2020.

[31] The NYPD rejected my respective freedom of information request (FOIL-2022-056-24147).

[32] Data available from the Civilian Complaint Review Board only includes misconduct and use of force by NYPD officers.

and 2021 elections, I provide some suggestive evidence that council members approving the budget cut lost more votes in their electoral districts than council members voting no.

7 Alternative Explanations and Robustness

In this section, I address several alternative explanations for my findings. First, I consider whether the results are an artifact of citizens' call patterns. If citizens interacted differently with the police after the policy change in certain areas, one may suspect that the number of calls and the distribution of call types changed, giving rise to sample selection problems and phantom counterfactuals (Slough, 2023). Particularly, if individuals in non-aligned districts had different likelihoods to call the police or only call for minor incidents that take longer to respond a priori, this could explain increases in response times in these council districts. Yet, the NYPD call data suggests that this is unlikely to occur. Figure A7 shows the average daily number of calls per district for treatment and control areas. Evidently, the daily number of calls moves almost in tandem in the treatment and control districts, both before and after the budget vote. The gap between the call volume in "yes" vs. "no" voting districts decreases slightly after the $1 billion budget cut, thus making it presumably harder to find longer response times in treatment districts following the policy change.[33] To further evaluate whether citizens' reporting behavior differed along the treatment dimension, Figure A8 depicts the type distribution of 911 calls across districts and periods. The frequency of different types of calls as well as the difference in the occurrence of call types across treatment and control districts remains largely unchanged before and after the budget vote. This alleviates concerns that the estimated increase in response times is driven by differences in citizens' propensity to call the police for specific types of incidents. Similarly, Table A7 indicates that the distance of call location to NYPD precinct headquarters did not increase post budget vote, thus assuaging concerns that divergent trends in call proximity

[33] While there were on average 99 more calls in "yes" voting districts than in "no" voting districts before the budget cut daily, this gap reduced to 89 calls after the vote (t-value of a two-sample T-test: 7.82).

explains response time differences.

Additionally, it is possible that public outrage following George Floyd's death in May 2020 gave rise to differences in policing, either due to changes in civilian behavior or officers' motivation to retaliate against public criticism. Hence, if politicians' voting behavior actually captured changes in citizens' opinions about the police, the estimates may be the result of simultaneity bias, where policing is a function of public outrage, civil disorder, and mistrust of officers rather than politicians' votes. To assuage these concerns, I geocoded all police-related protests in NYC since January 2020.[34] This data provides an imperfect, yet valuable fine-grained measure of citizens' outcry about policing in NYC over time. Figure A9 depicts the location of the 1,989 protests that occurred between January 2020 and June 2021. While there seems to be some clustering of protests among non-aligned districts, especially outside of Manhattan, the figure also indicates that protests took place across the entire city. Table A9, in turn, shows estimates of the DiD model after accounting for the daily number of anti-police protests in a district and precinct in various ways. Reassuringly, the treatment effect estimates remain robust to this alternative explanation of increases in 911 response times.

Finally, I address alternative explanations of my findings related to the motivation of police. According to my argument, the increase in response times in yes-voting districts is driven by politically motivated shirking, where police leverage their influence on voters' perceptions of incumbents to punish elected officials. However, one can think of two alternative less strategic motivations for increased shirking in non-aligned areas. First, police forces in yes-voting districts might have lower morale after the budget cut, which could drive down their incentives to improve 911 response times. Second, officers in non-aligned districts could avoid engagement after the budget cut because they do not want to draw attention to themselves or risk becoming the focus of a civil inquiry. Importantly, if these alternative

[34]The raw data comes from the Crowd Counting Consortium Dataset, a collaborative effort led by Jeremy Pressman and Erica Chenoweth to collect publicly available data on political crowds reported in the United States, including marches, protests, strikes, demonstrations, riots, and other actions at https://github.com/nonviolent-action-lab/crowd-counting-consortium.

mechanisms hold, shirking should increase monotonically with the degree of non-alignment, i.e. shirking should be *highest* in areas with very pro-reform incumbents and voters.

In the case of politically motivated police behavior, in contrast, shirking depends on the ability of police to affect voters' beliefs, which presumably decreases with anti-police sentiment in neighborhoods. That is, in areas where voters are more critical of police forces and likely blame police for slow responses, police have fewer incentives to shirk for political reasons. In more moderate non-aligned districts, in contrast, voters are less lopsided in their opinions about police. Law enforcement officers can therefore gamble on voters misattributing blame for increasing response times, and shirking can help to damage the reputation of non-aligned politicians. Politically motivated shirking thus implies a non-monotonicity between the intensity of non-alignment and the incentives for shirking.

To distinguish these accounts, I leverage councilors' different reasons to vote "no" on the budget. While eight council members opposed the budget cut because they perceived it to be too much, nine councilors voted against the budget because they thought the reductions to the NYPD's funding were not enough. Incumbents and voters in "no, not enough" areas tend to be more progressive than in "no, too much" and "yes" voting districts; they have a majority non-white population and more than 85% of them supported Biden in the 2020 presidential election on average (see Table A10). Figure A10 shows the treatment effect estimates from the triple DiD design after disaggregating different types of "no" votes. The estimates suggest a non-monotonicity in the treatment effects along the intensity of non-alignment: While there is a positive, albeit small and insignificant treatment effect for "yes" vs. "no, too much" districts, the differences in response time trends between "no, not enough" and "no, too much" districts is (imprecisely) estimated to be *negative*. The difference in these treatment effect estimates is significant at conventional levels, suggesting that the main results in Table 1 are mainly driven by differences in response times between "yes" and "no, not enough" districts. Taken together, while this non-monotonicity is difficult to square with the morale and avoidance mechanisms, it is consistent with politically motivated

shirking and the fact that shirking may have divergent implications for law enforcement depending on voters' latent preferences towards police and officers' ability to use politicians as scape goats.

8 Conclusion

> *"Most disturbing to me was a near constant refrain that I heard from constituents calling SPD [Seattle Police Department] for help that they were told by officers that 'the council has tied their hands'. Of course individual council members don't decide what laws SPD enforces or doesn't enforce. We aren't in the chain of command."*
> – Lisa Herbold, Seattle City Council member (Blumgart, 2020)

This study explains why and when police officers in cities like Seattle reduce their effort in responding to citizens' calls for service. I have argued that bureaucrats can – under certain conditions – leverage their influence on public policy to exercise power over the political authorities to whom they answer. By strategically and collectively shirking their duties in certain areas, bureaucratic agents can protest unwanted policy choices, exert pressure on political authorities, and affect the policies they make while in office. As I have argued, bureaucrats' willingness and capacity to exercise such political power largely depend on the degree of preference misalignment with their political principals as well as their tenure protection and unionization. Focusing on municipal police and using data on 911 response times of NYPD officers as a case study, I find empirical evidence that largely supports this view. Relative to the FDNY response times and compared to calls in FY2020, NYPD officers took about 90 seconds longer to respond to calls in city council districts that had voted for the $1 billion cut to the NYPD budget – a policy that police unions in NYC heavily condemned.

This research provides new insights into issues of political representation and the role of bureaucrats as interest groups within government. To the best of my knowledge, this is the first study to exploit exogenous variation in the preference alignment of bureaucrats and politicians to study bureaucrats' political leverage through shirking. Adding to existing evidence on police unions' involvement in local politics (Anzia, 2022), this study highlights

the importance of political power to explain police behavior in US cities.

Additionally, this study informs the policy debate on the desirability of strong police unions. Recent work has considered how union pressure to protect officers from termination may attract more extreme officers or allow for more biased policing tactics, thus leading to worse policing outcomes (Clark, 2021). This study highlights another aspect of this debate. Tenure protection of local law enforcement officers allows the police to flex their muscle vis-à-vis non-aligned elected superiors to push back against unwanted police reforms. If well-organized police unions manage to exert sufficient pressure on reform-oriented incumbents through work slowdowns, lobbying activities, or recall campaigns, meaningful police reform may remain elusive – despite broad public support for such measures.

Lastly, by raising questions about who is controlling whom in politician-bureaucrat relationships, this study has important implications for our understanding of principal-agent dependencies between elected authorities and their bureaucratic agents. To be sure, this study does not provide evidence for the claim that elected government is run by a "deep state" of embedded, biased bureaucrats who work to thwart legitimate political agendas. Prior research has repeatedly shown that civil servants in the American bureaucracy are, for the most part, individuals dedicated to serving the public and tend to be more responsive than resistant to changes in political administrations (Brehm and Gates, 1997; Golden, 2000). Rather, this study aims to characterize the dynamics that *can* give rise to bureaucratic resistance.

While the study focuses on a single city employing the largest US police force, similar dynamics of police resistance likely apply in many other US cities. 45% of each state's largest cities reduced the share of their police budget for fiscal year 2021, with cuts to the police budget ranging up to 12.1% and 9.7% in Albuquerque and Seattle, respectively.[35] In light of the strong police unions in these major cities and their open resistance to these budgetary changes, the bureaucratic power play and reduced public service of police forces shown in this

[35] https://www.smartcitiesdive.com/news/calls-to-defund-the-police-are-upending-fy21-budgets-heres-how/581163/.

study likely represents a broader phenomenon across US cities. Using data from a larger set of jurisdictions, future work may address this conjecture explicitly. It is also worth examining the broader applicability of my argument across types of bureaucracies. Invoking the scope conditions of my argument, we should expect possible bureaucratic resistance in other local, politically independent and well-organized bureaucracies. For instance, subsequent work might study whether and how progressive teachers and their unions resisted recent restrictive policies of local school boards, such as book bans and educational gag orders.

There remain several open questions this study cannot address. First, my argument and analysis abstract away from internal hierarchies of local law enforcement. Precinct management has considerable discretion in allocating resources across neighborhoods and research shows that police administrators have significant influence on officers' behavior (Mummolo, 2018). Hence, it is possible that the disengagement of NYPD officers in certain districts is partly due to changes in how administrators assigned forces within their precincts. While data limitations hampered the consideration of this aspect in my case study, future work on officer resistance may shed light on this open question.

Second, this study remains agnostic about the broader welfare implications of politically motivated behavior of police. Although work slowdowns reduce the public utility of citizens calling for help, these reductions might be offset by utility increases for individuals subject to police interventions. If work slowdowns are clustered in overpoliced areas, the net impact of police shirking might not be negative overall. I leave these considerations for future research.

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Appendix: Supporting Information for *Political Power of Bureaucratic Agents*

A Data Description

Table A1: Description of Data Sources

Data Set	Description	Source
NYPD 911 call data	This dataset documents entries into the NYPD 911 system, ICAD. The data is collected from the ICAD system which call takers and dispatchers use to communicate with callers and the NYPD. Each record represents an entry into the system. The data includes entries generated by members of the public as well as self-initiated entries by NYPD Members of Service. I use the longitude and latitude of each incident to geolocate each call. The sample spans from January 1, 2020, to June 30, 2021 ($N = 9,417,637$).	https://data.cityofnewyork.us/Public-Safety/NYPD-Calls-for-Service-Year-to-Date-/n2zq-pubd; https://data.cityofnewyork.us/Public-Safety/NYPD-Calls-for-Service-Historic-/d6zx-ckhd
EMS 911 call data (FDNY administered)	The EMS Incident Dispatch Data file contains data on individual emergency medical service calls in NYC, generated by the EMS Computer Aided Dispatch System. The data spans from the time the incident is created in the system to the time the incident is closed in the system. It covers information about the incident as it relates to the assignment of resources and the Fire Department's response to the emergency. The sample spans from January 1, 2020, to May 31, 2021 ($N = 1,755,487$).	https://data.cityofnewyork.us/Public-Safety/EMS-Incident-Dispatch-Data/76xm-jjuj
Vote share Biden 2020, by district	I aggregate valid vote counts for President Biden in the 2020 general election in each electoral district on the City Council district level and calculate vote shares in each district.	https://vote.nyc/page/election-results-summary-2020
Census demographics, by district	Various demographics on the City Council district level, collected from the US census Bureaus' decennial dissemination for 2010	https://data.cityofnewyork.us/City-Government/Census-Demographics-at-the-NYC-City-Council-distri/ye4r-qpmp

Continued on next page

Data Set	Description	Source
Valid violation, misdemeanor and felony complaints	This dataset includes all valid felony, misdemeanor, and violation crimes reported to the New York City Police Department (NYPD) since 2006. I aggregate the number of complaints on the precinct-district level.	https://data.cityofnewyork.us/Public-Safety/NYPD-Complaint-Data-Historic/qgea-i56i; https://data.cityofnewyork.us/Public-Safety/NYPD-Complaint-Data-Current-Year-To-Date-/5uac-w243
Shooting incidents	This is a breakdown of every shooting incident that occurred in NYC going back to 2006 through the end of the previous calendar year. This data is manually extracted every quarter and reviewed by the Office of Management Analysis and Planning before being posted on the NYPD website. Each record represents a shooting incident in NYC and includes information about the event, the location and time of occurrence. In addition, information related to suspect and victim demographics is also included. I aggregate the number of shootings on the precinct-district level.	https://data.cityofnewyork.us/Public-Safety/NYPD-Shooting-Incident-Data-Year-To-Date-/5ucz-vwe8; https://data.cityofnewyork.us/Public-Safety/NYPD-Shooting-Incident-Data-Historic-/833y-fsy8
Police related protests	I use all police-related protests located in NYC as identified by the Crowd Counting Consortium (CCC). The CCC collects publicly available data on political crowds reported in the United States, including marches, protests, strikes, demonstrations, riots, and other actions. Based on the address information for each protest, I geolocate police-related protests using Google's Geocoding API. I verified the accuracy of the geocoding by manually checking 100 random protests. This exercise warranted manual adjustments to 604 protests (24% of all protests in the sample).	https://sites.google.com/view/crowdcountingconsortium/about
USPS address changes	I use monthly data on change of address requests published by the United States Postal Service on the ZIP-code level. I assign each City Council district to a ZIP code based on either (1) the ZIP code with the largest share of the district area or according to (2) the average of all ZIP codes within a district, weighted by their respective share of the district area.	https://about.usps.com/who/legal/foia/library.htm

B Tables

Table A2: Summary Statistics - Covariates by Voting Behavior

	Vote on Budget Cut			
	yes	no	difference	
	mean	mean	est.	t-value
Council member characteristics				
Black candidate	37.50	23.53	-13.97	(-1.02)
Vote share last election	82.86	78.69	-4.18	(-0.89)
Win margin, last election	68.90	60.73	-8.17	(-0.92)
Term limited	59.38	64.71	5.33	(0.36)
Experience (in years)	6.09	5.59	-0.51	(-0.56)
Geographic characteristics (pretreatment)				
Vote share Biden 2020 [a]	79.81	67.74	-12.07*	(-1.95)
Share of white population [b]	26.47	46.71	20.25**	(2.57)
Share of black population [b]	27.95	14.17	-13.78*	(-1.95)
Share of hispanic population [b]	29.49	24.78	-4.71	(-0.82)
Share of female population [b]	52.84	52.30	-0.54	(-0.91)
Share of population over 65 [b]	12.16	12.53	0.37	(0.43)
Share of population over 18 [b]	78.28	78.60	0.33	(0.20)
Share of renter occupied households [b]	70.20	64.71	-5.48	(-1.05)
Number of George Floyd protests [c]	4.41	3.12	-1.29	(-0.97)
Number of violation complaints [d]	677.28	540.59	-136.69*	(-1.90)
Number of misdemeanor complaints [d]	2227.75	1621.88	-605.87***	(-2.88)
Number of felony complaints [d]	1330.91	1008.88	-322.02**	(-2.23)
Number of shootings [e]	15.81	9.29	-6.52	(-1.54)
Number of districts	32	17	49	

Sources: [a] Official Electoral Results, [b] Census Demographics, [c] Crowd Counting Consortium, [d] NYPD Complaint Data, [e] NYPD Shooting Incident Data.

Table A3: Effect of Approving 2021 Budget on 911 Response Times, Including May 30 - June 15

	(1)	(2)	(3)	(4)	(5)
yes vote × after vote × NYPD	1.252*	1.208*	1.250*	1.257*	1.247*
	(0.682)	(0.680)	(0.684)	(0.684)	(0.691)
NYPD	5.193***	7.772***	5.190***	5.199***	4.050***
	(0.695)	(0.740)	(0.696)	(0.696)	(0.703)
yes vote × NYPD	-0.044	-0.049	-0.044	-0.048	-0.007
	(1.140)	(1.097)	(1.141)	(1.139)	(1.133)
after vote × NYPD	2.020***	2.123***	2.026***	2.012***	2.058***
	(0.449)	(0.444)	(0.450)	(0.450)	(0.457)
yes vote × after vote	-0.719	-0.755	-0.724	-0.721	-0.715
	(0.580)	(0.589)	(0.589)	(0.576)	(0.588)
total calls (log)		-1.766***			
		(0.230)			
# of shootings (log)			2.331***		
			(0.427)		
# of complaints (log)				0.444***	
				(0.165)	
District FE	✓	✓	✓	✓	✓
Police Precinct FE	✓	✓	✓	✓	✓
Date FE	✓	✓	✓	✓	✓
Call Importance FE					✓
Observations	9,540,116	9,540,116	9,540,116	9,540,116	9,540,116
Mean of DV	13.423	13.423	13.423	13.423	13.423
Adj. R^2	0.032	0.033	0.032	0.032	0.032

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Dependent variable: Response time in minutes. Coefficients for yes vote_c and after vote_d absorbed by district and day fixed effects, respectively. Cluster robust standard errors in parentheses, by district (49).

Table A4: Effect of Approving 2021 Budget on 911 Response Times, Simple DiD models

	(1)	(2)	(3)	(4)	(5)	(6)
		NYPD			FDNY	
yes vote × after vote	0.683*	0.603	0.699*	-0.857	-0.853	-0.840
	(0.393)	(0.389)	(0.398)	(0.582)	(0.582)	(0.584)
total calls (log)		-1.748***			0.245***	
		(0.281)			(0.088)	
District FE	✓	✓	✓	✓	✓	✓
Police Precinct FE	✓	✓	✓	✓	✓	✓
Date FE	✓	✓	✓	✓	✓	✓
Call Importance FE			✓			✓
Observations	7,369,246	7,369,246	7,369,246	1,916,838	1,916,838	1,916,838
Mean of DV	14.508	14.508	14.508	8.880	8.880	8.880
Adj. R^2	0.034	0.035	0.036	0.107	0.107	0.142

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Dependent variable: Response time in minutes. Coefficients for yes vote_c and after vote_d absorbed by district and day fixed effects, respectively. Cluster robust standard errors in parentheses, by district (49).

Table A5: Effect of Approving 2021 Budget on 911 Response Times
Winsorized Response Times

	(1) 1-99 pct.	(2) 1-99 pct., by day
yes vote × after vote × NYPD	1.180*	1.247*
	(0.591)	(0.637)
NYPD	4.074***	4.046***
	(0.631)	(0.635)
yes vote × NYPD	-0.433	-0.504
	(0.977)	(0.968)
after vote × NYPD	2.306***	2.412***
	(0.401)	(0.433)
yes vote × after vote	-0.673	-0.723
	(0.528)	(0.572)
District FE	✓	✓
Police Precinct FE	✓	✓
Date FE	✓	✓
Observations	9,286,084	9,286,084
Mean of DV	12.542	12.588
Adj. R^2	0.041	0.042

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Dependent variable: Response time in minutes. Coefficients for yes vote_c and after vote_d absorbed by district and day fixed effects, respectively. Cluster robust standard errors in parentheses, by district (49).

Table A6: Effect of Approving 2021 Budget on Probability of Officer-Initiated Calls

	(1)	(2)	(3)	(4)
	Response Time = 0		Response Time < 0.15	
yesvote × postvote × NYPD		0.013		0.002
		(0.014)		(0.009)
NYPD		-0.012***		0.552***
		(0.002)		(0.011)
yesvote × NYPD		-0.003		-0.007
		(0.002)		(0.017)
postvote × NYPD		0.356***		-0.006
		(0.008)		(0.007)
yesvote × postvote	0.011	-0.002**	0.001	-0.001
	(0.014)	(0.001)	(0.009)	(0.001)
District FE	✓	✓	✓	✓
Police Precinct FE	✓	✓	✓	✓
Date FE	✓	✓	✓	✓
Observations	7,369,246	9,286,084	7,369,246	9,286,084
Mean of DV	0.250	0.201	0.569	0.454
Adj. R^2	0.315	0.322	0.039	0.232

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Dependent variable: Dummy for zero or < 0.15 response time. Columns (1) and (3) only include NYPD calls. Coefficients for yes vote_c and after vote_d absorbed by district and day fixed effects, respectively. Cluster robust standard errors in parentheses, by district (49).

Table A7: Call Distance to NYPD Precinct Headquarters

	(1)	(2)
yes vote	-310.027*	-297.809*
	(165.521)	(165.444)
yes vote × after vote		-17.275
		(15.115)
Police Precinct FE	✓	✓
Date FE	✓	✓
Observations	8,888,313	8,888,313
Mean of DV	1259.463	1259.463
Adj. R^2	0.406	0.406

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Cluster robust standard errors in parentheses, by district (49).

Table A8: Call Distance to NYPD Precinct Headquarters
RDD Sample (within 200 meter bandwidth)

	(1)	(2)
yes vote	-71.362	-69.061
	(47.572)	(49.917)
yes vote × after vote		-3.248
		(14.533)
Police Precinct FE	✓	✓
Date FE	✓	✓
Observations	1,080,830	1,080,830
Mean of DV	1111.790	1111.790
Adj. R^2	0.781	0.781

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Cluster robust standard errors in parentheses, by district (49).

Table A9: Effect of Approving 2021 Budget on 911 Response Times
Accounting for Protests

	(1)	(2)	(3)
yes vote × after vote × NYPD	1.479**	1.448*	1.422*
	(0.734)	(0.727)	(0.725)
NYPD	4.689***	4.644***	4.797***
	(0.696)	(0.691)	(0.708)
yes vote × NYPD	-0.276	-0.279	-0.323
	(1.107)	(1.109)	(1.101)
after vote × NYPD	2.523***	2.653***	2.681***
	(0.477)	(0.484)	(0.486)
yes vote × after vote	-0.802	-0.769	-0.750
	(0.628)	(0.618)	(0.617)
# of protests (log)	0.090	-6.007***	
	(0.462)	(1.210)	
after vote × # of protests (log)		8.323***	
		(1.873)	
NYPD × # of protests (log)		7.923***	
		(1.885)	
after vote × NYPD × # of protests (log)		-10.735***	
		(2.893)	
# of protests (log) (June 2020)			0.514
			(0.967)
after vote × # of protests (log) (June 2020)			1.147*
			(0.612)
NYPD × # of protests (log) (June 2020)			-1.172
			(2.057)
after vote × NYPD × # of protests (log) (June 2020)			-1.668
			(1.111)
District FE	✓	✓	✓
Police Precinct FE	✓	✓	✓
Date FE	✓	✓	✓
Observations	9,286,084	9,286,084	9,286,084
Mean of DV	13.346	13.346	13.346
Adj. R^2	0.032	0.032	0.032

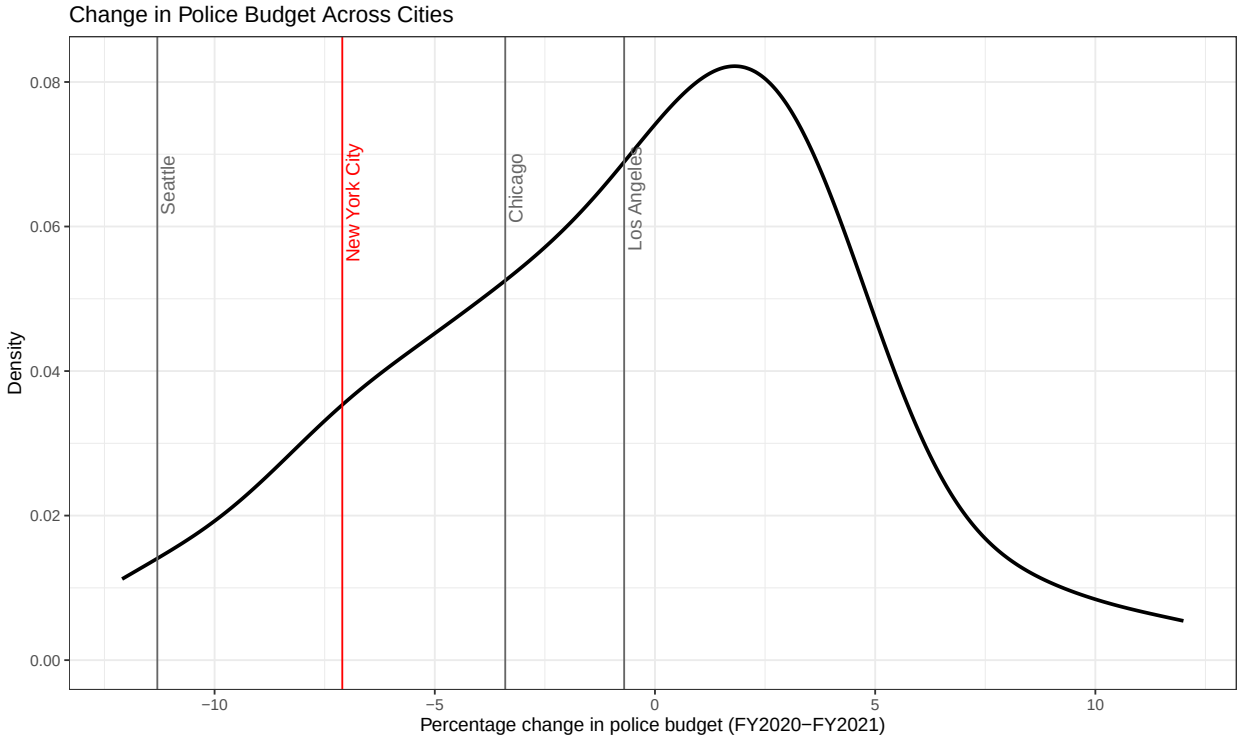
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Dependent variable: Response time in minutes. Coefficients for yes vote_c and after vote_d absorbed by district and day fixed effects, respectively. Cluster robust standard errors in parentheses, by district (49).

Table A10: Summary Statistics - Covariates by Voting Behavior

	Voting behavior		
	yes	no, not enough	no, too much
	mean	mean	mean
Council member characteristics			
Black candidate	37.50	33.33	12.50
Vote share last election	82.86	89.58	66.44
Win margin, last election	68.90	81.71	37.13
Term limited	59.38	77.78	50.00
Experience (in years)	6.09	6.56	4.50
Geographic characteristics (pretreatment)			
Vote share Biden 2020	79.81	85.04	48.27
Share of white population	26.47	39.79	54.50
Share of black population	27.95	20.17	7.42
Share of hispanic population	29.49	24.71	24.86
Share of female population	52.84	52.76	51.78
Share of population over 65	12.16	11.59	13.59
Share of population over 18	78.28	80.33	76.65
Share of renter occupied households	70.20	73.37	54.98
Number of George Floyd protests	4.41	5.00	1.00
Number of violation complaints	677.28	585.44	490.13
Number of misdemeanor complaints	2227.75	1849.33	1366.00
Number of felony complaints	1330.91	1213.56	778.63
Number of shootings	15.81	13.89	4.13
Number of districts	32	9	8

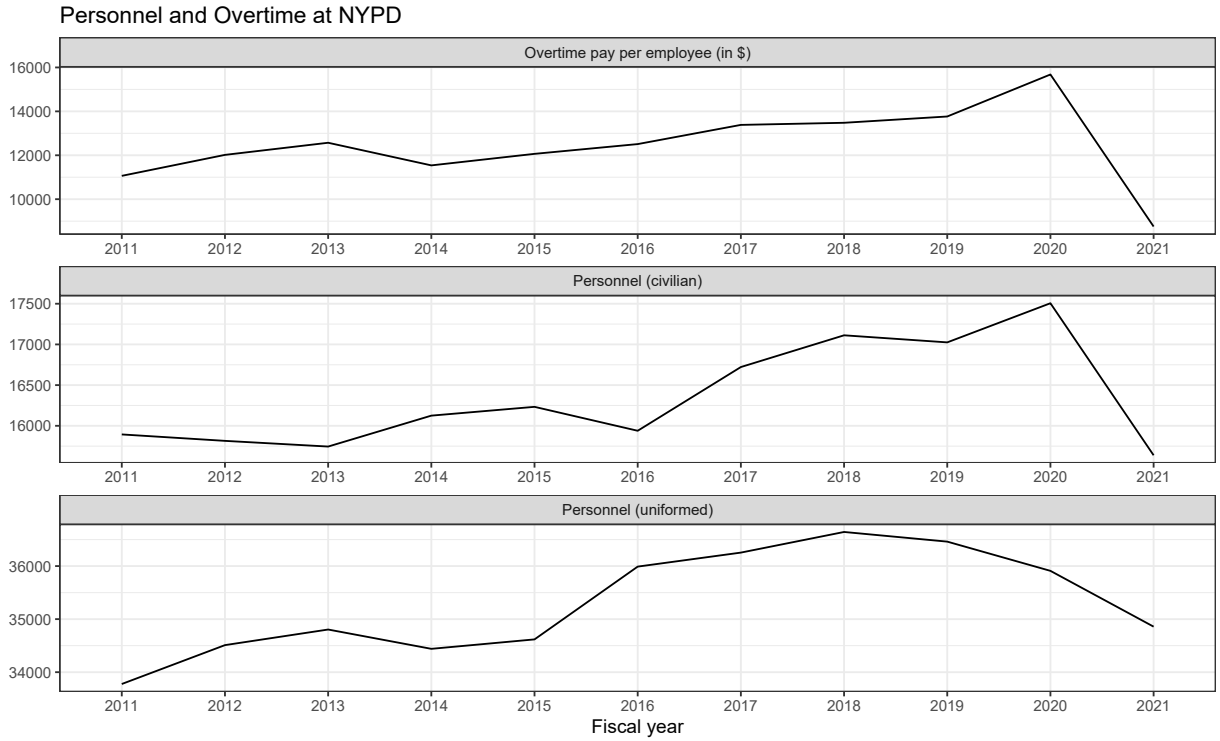
C Figures

Figure A1: Distribution of Police Budget Cuts Across Major US Cities in 2020



Note: The figure depicts changes in police budgets across all US state's largest cities, between fiscal years 2020 and 2021 (in percentages). Source: https://www.smartcitiesdive.com/news/calls-to-defund-the-police-are-upending-fy21-budgets-heres-how/581163/

Figure A2: Development of Personnel at NYPD



Note: The figure depicts NYPD resources from the FY2015, FY2020 and FY2021 Mayor's Management Reports (MMR), including paid overtime per employee, civilian personnel and uniformed personnel.

Figure A3: Balance of Matched RDD Sample - Major Call Types

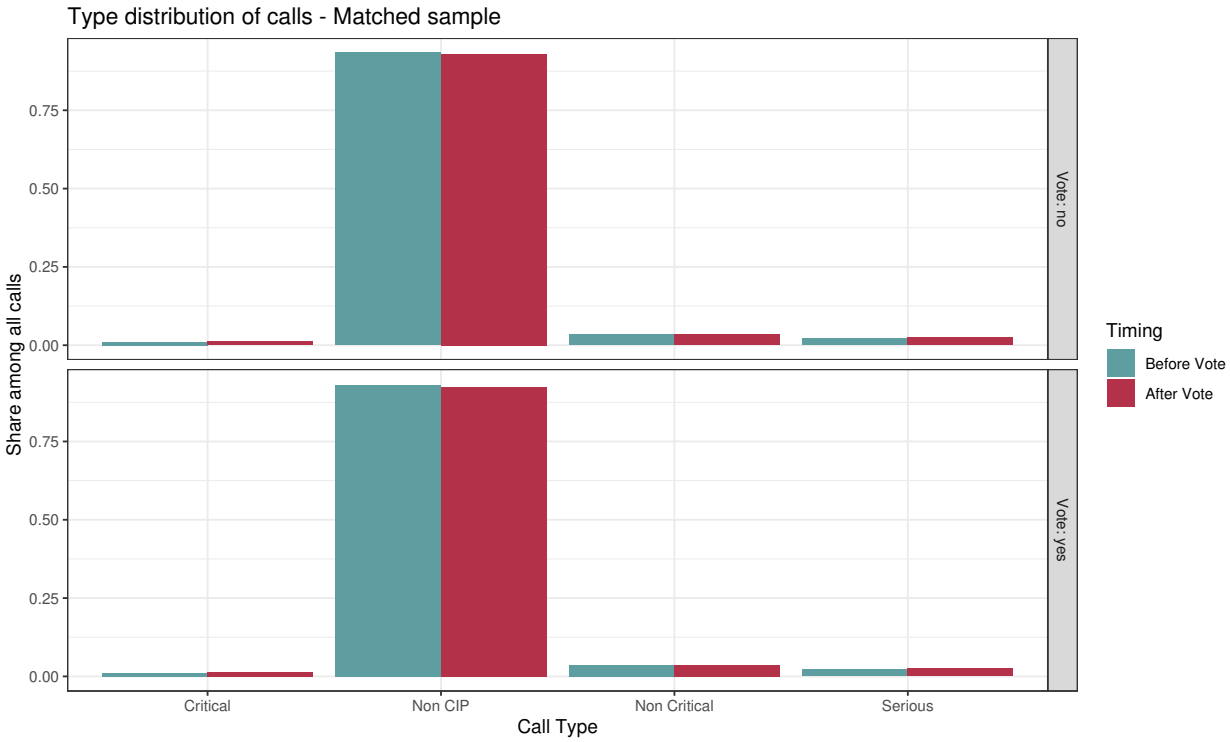


Figure A4: Balance of Matched RDD Sample - Daily Call Volume by Zip Code

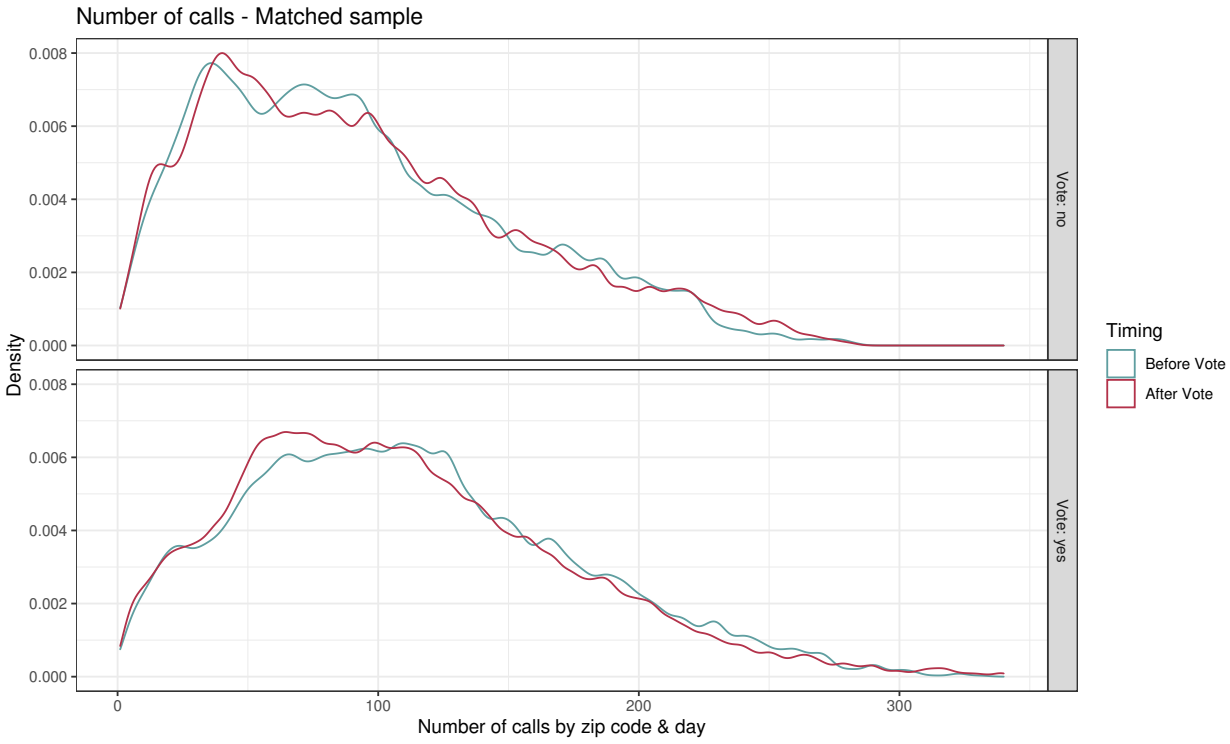
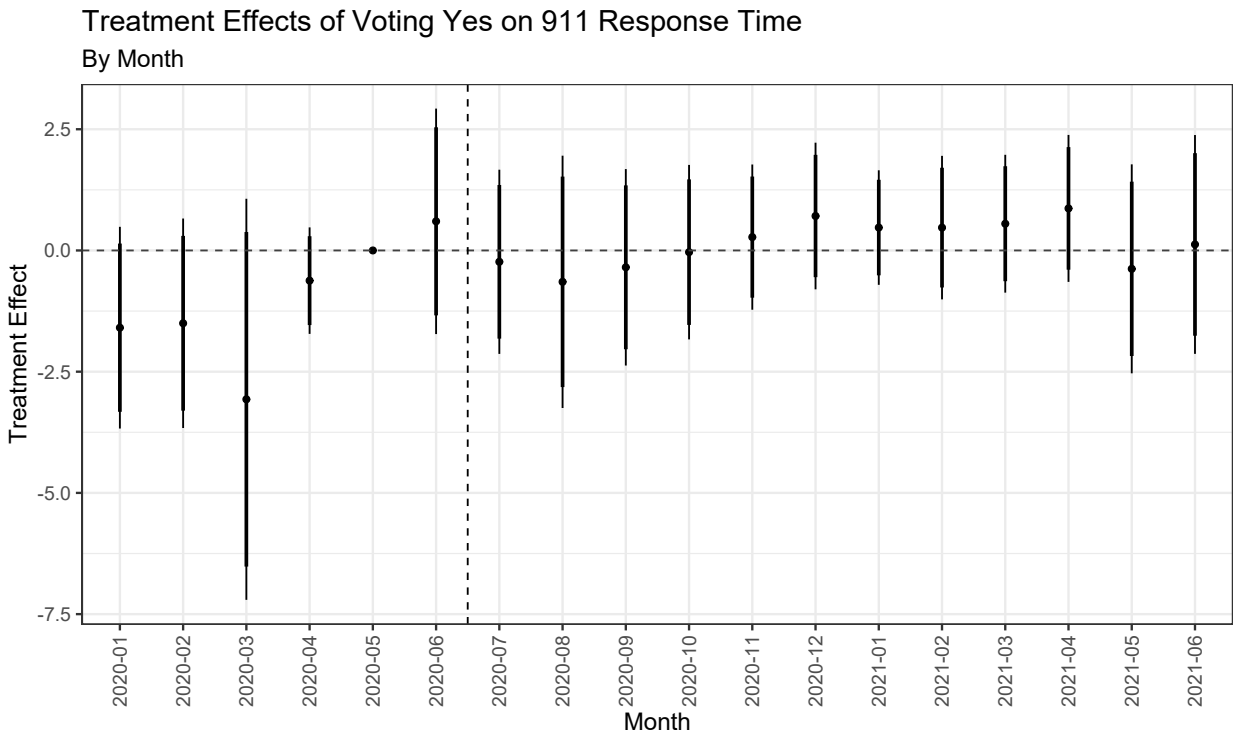


Figure A5: Monthly Treatment Effects



Note: Depicted are month-specific treatment effects, based on estimations of Equation (3) with 90% and 95% confidence intervals.

Figure A6: Average Response Times across Call Types, Districts and Periods

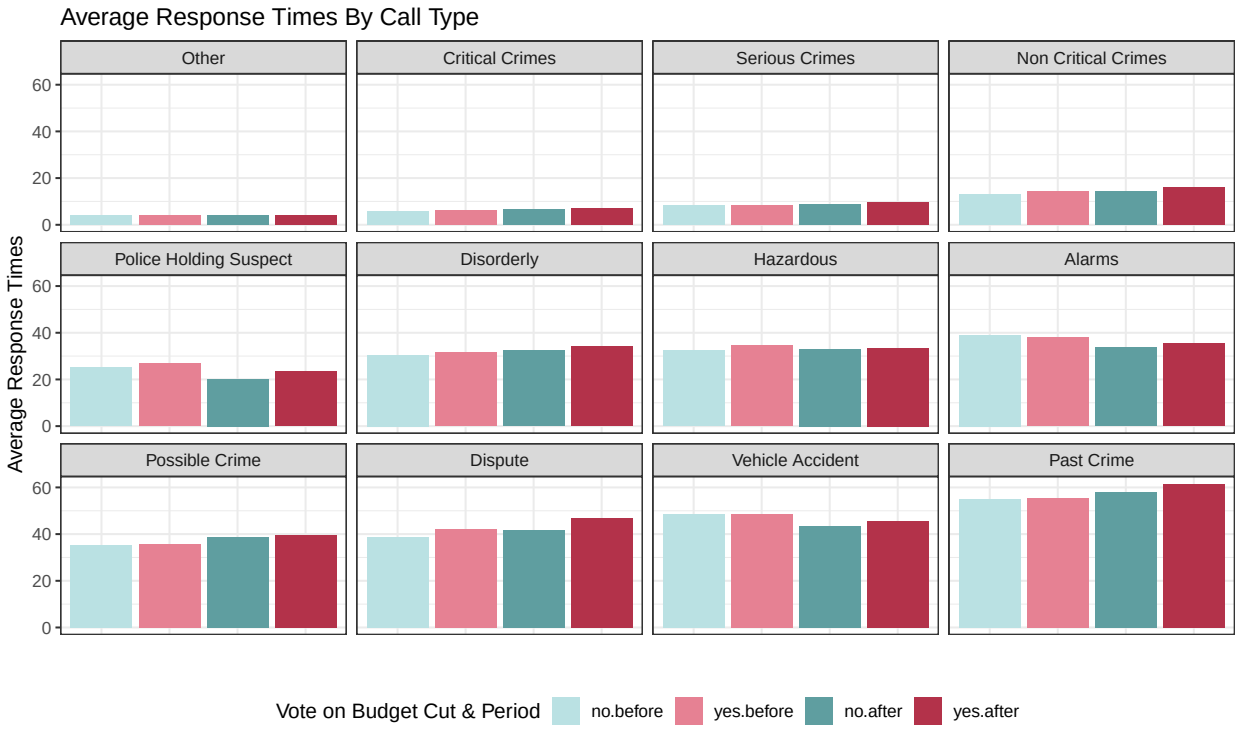


Figure A7: Trends in Amount of 911 NYPD Calls across Districts

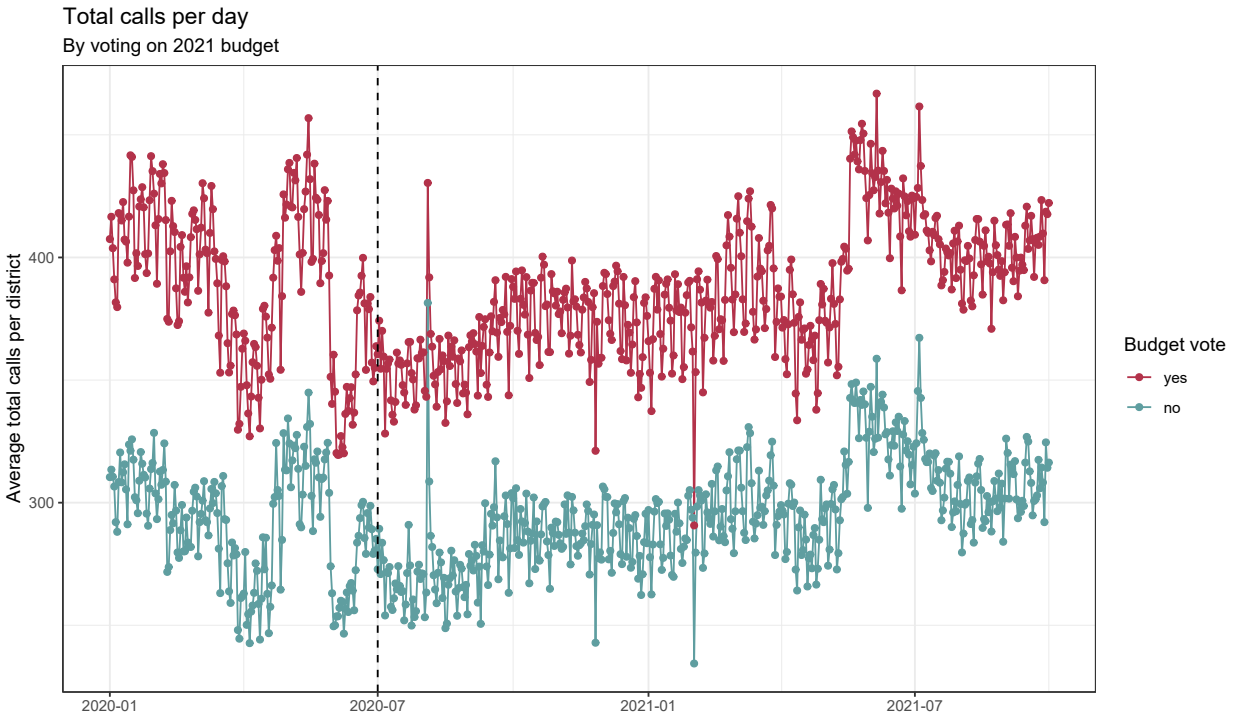


Figure A8: Distribution of 911 Call Types, by Period and District

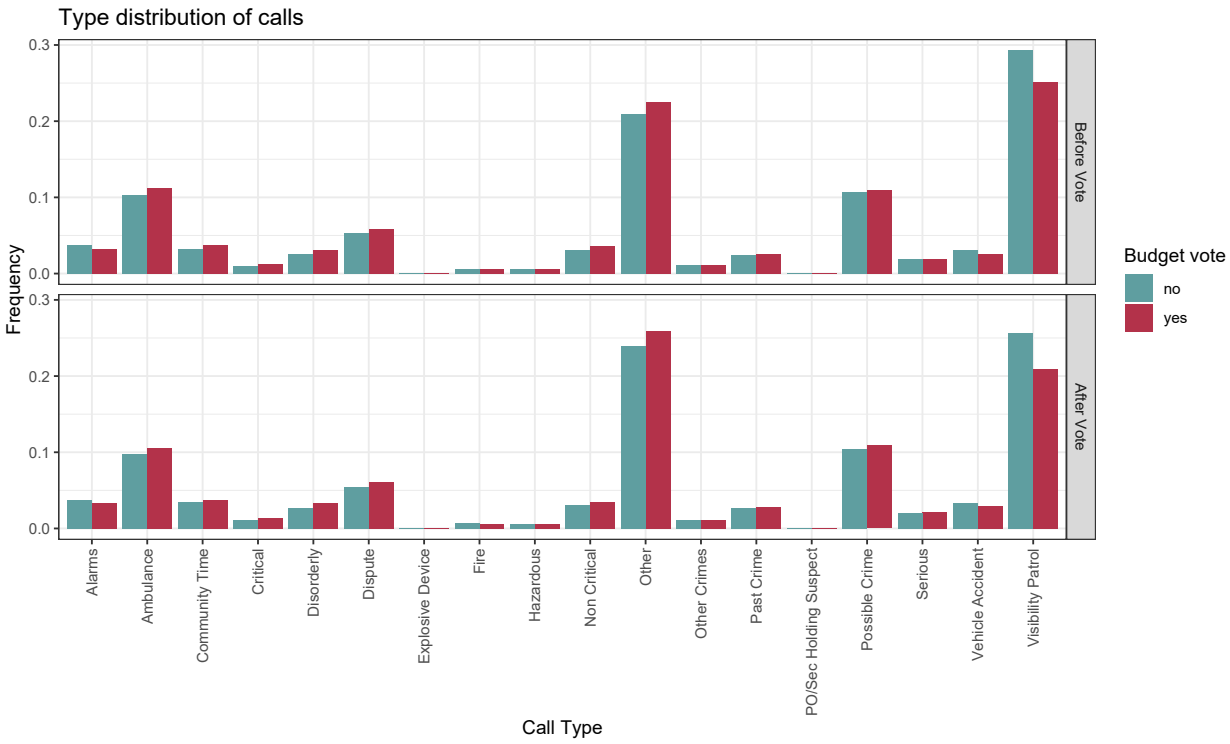


Figure A9: Location of Police-Related Protests

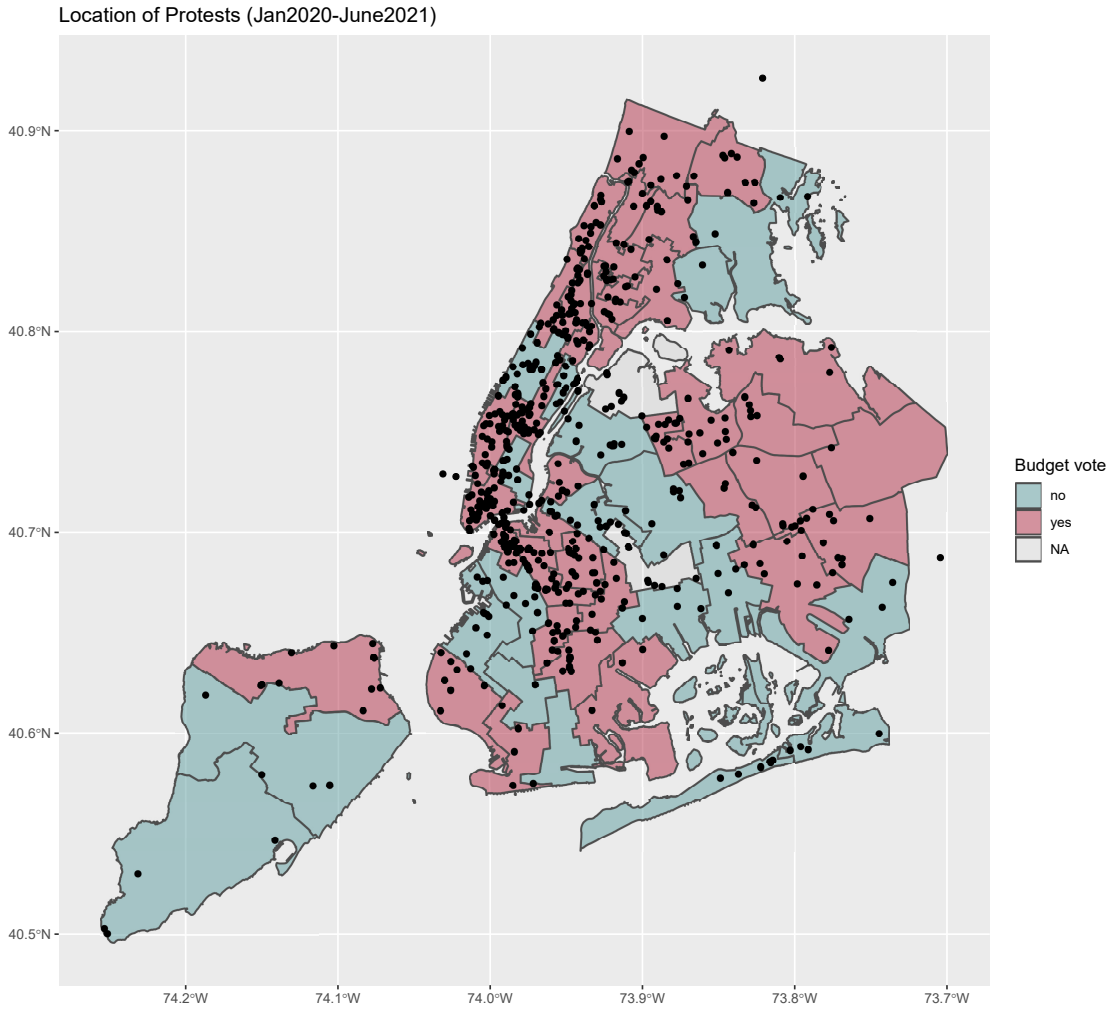
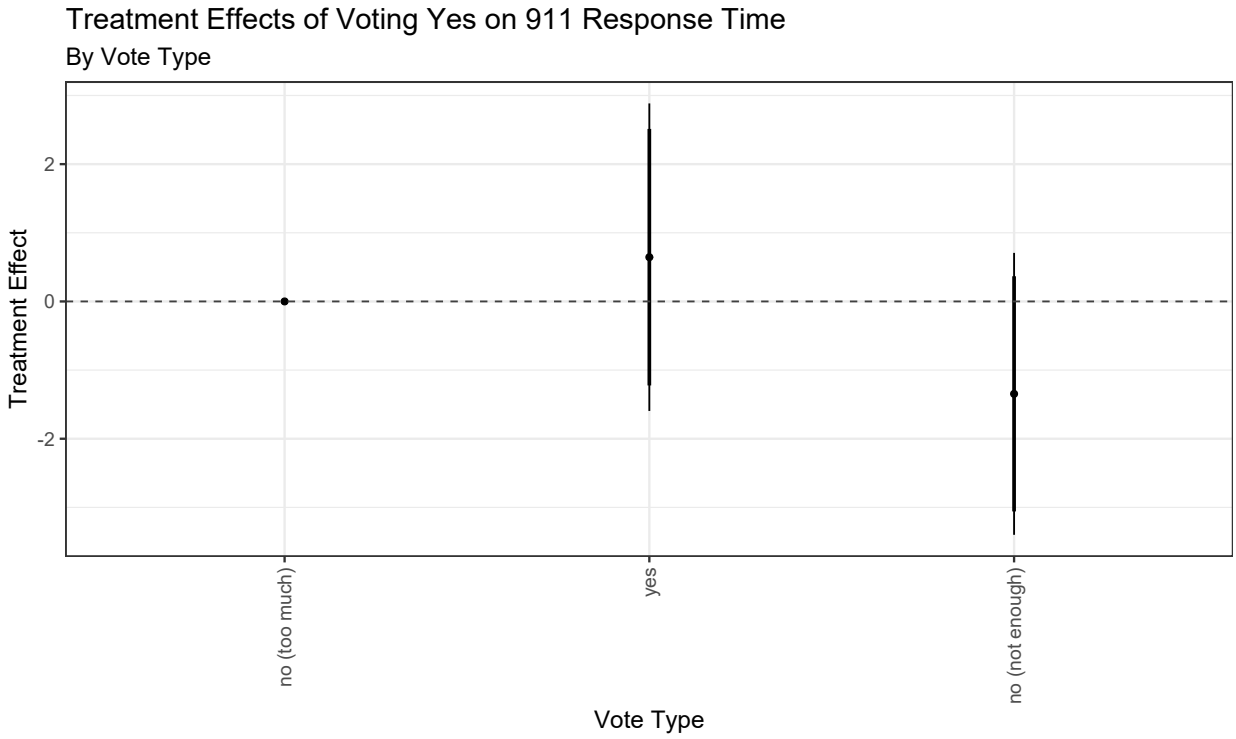


Figure A10: Disaggregating Treatment Effects by No Votes



D Impact on Candidate Vote Share

In this section I provide some correlational evidence suggesting that council members opposed to police interests incurred electoral costs in the 2021 municipal elections relative to aligned council members. For this exercise I collect administrative data on election results on the election district level (i.e. the smallest electoral unit within a council district) for the 2017 and 2021 city council elections from the NYC Board of Elections.[1] For each electoral district and election I then calculate the vote share for council members voting on the 2021 budget.

Several aspects complicate this analysis. First, since I am interested in whether incumbents lost votes due to their votes on the 2021 budget, my sample is restricted to council members who ran in both elections and to districts where general/primary elections took place in both years. Another caveat arises due to a change in NYC's electoral system in 2021. New York City switched to rank-choice voting (RCV) for primary elections, allowing voters to rank up to five candidates for each race. Earlier elections were conducted under a standard first-past-the-post format. This implies a slight modification of my outcome variable, since vote shares are no longer simple to estimate. To calculate an incumbent's vote share that is comparable to my measure for the 2017 elections, I use individual-level cast vote records to compute the share of voters within a precinct who ranks each candidate as their top choice. This measure is easy to grasp and relatively analogous to vote shares in a first-past-the-post system.

I then estimate the following first-difference model:

$$\Delta voteshare_{ie} = \alpha + \beta \text{yes vote}_i + \varepsilon_{ie} \tag{4}$$

where I regress a council member i's difference in their vote share in electoral district e between 2017 and 2021 on whether they voted yes as opposed to no on the 2021 budget. As before, I cluster standard errors on the council district level. However, since there is a very small number of clusters in this model, I also present wild cluster bootstrap p-value following (Roodman et al., 2019).

The results in Table A11 suggest that approving the 2021 budget cut was indeed associated with a reduction in council member's vote shares. In the Democratic primary elections, where most of the electoral competition takes place in NYC, incumbents who supported the budget cut lost 33 percentage points more than council members opposing the substantial cut. In fact, two of the seven council members in favor of the budget cut in this sample lost their primary elections all together – a rare event for incumbents in NYC's Democratic primaries. Given the important caveats of this analysis, these estimates do not allow for causal inferences. Yet, they provide some correlational evidence that council members who acted contrary to police interests during the 2021 budget vote might have incurred some electoral costs in the upcoming city elections.

[1] https://vote.nyc/page/election-results-summary

Table A11: Effect of Approving 2021 Budget on 2021 Election Vote Shares

	Primary	General
yes vote	-0.33^{**}	-0.09
	(0.13)	(0.14)
Mean of DV	-0.26	0.13
Adj. R^2	0.23	0.03
Num. obs.	871	1059
N Clusters	9	11
Wild cluster bootstrap p-value	0.09	0.56

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$; Dependent variable: Δ in vote share for incumbent on electoral district level. Standard errors clustered on the council district level in parentheses. Bootstrap p-value refers to the coefficient on yes vote and is computed using the cluster wild bootstrap procedure of Roodman et al. (2019).