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 results 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 migration). To overcome this, I employ a triple difference-in-differences design where I compare response times across non-aligned and aligned, 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, FDNY 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 20 seconds more for NYPD calls compared to FDNY calls after the budget vote—a substantial increase relative to the average 911 response time

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

of 13.1 minutes prior to the budget vote. This treatment effect does not appear to be driven by differences in available budgets across each police precinct, demand for police presence or 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 emphasizing 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 speaking 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 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 ac-

count understates 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. 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

[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.

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] 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 resistance.[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 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

[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 resistance 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 shirk.

[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.

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 bureaucrats, 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,

[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.

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 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

[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).

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, rather than framing effort shirking as a blunt tool police use to oppose civilian and official criticism across an entire city, I focus on how police selectively target specific non-aligned politicians. Second, previous studies examine de-policing at the aggregate level by comparing police behavior before and after major events, such as protests or investigations that affect the whole department. This approach poses challenges for causal identification, as these events often coincide with other policy changes that influence police behavior. For instance, public protests often lead to changes in police management or resources, making it difficult to distinguish between workforce issues and strategic shirking. In fact, this very uncertainty allows police to shift blame towards policymakers, and before-after

designs used in prior research are thus inherently limited in capturing bureaucrats' strategic behavior. 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 police behavior.

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.

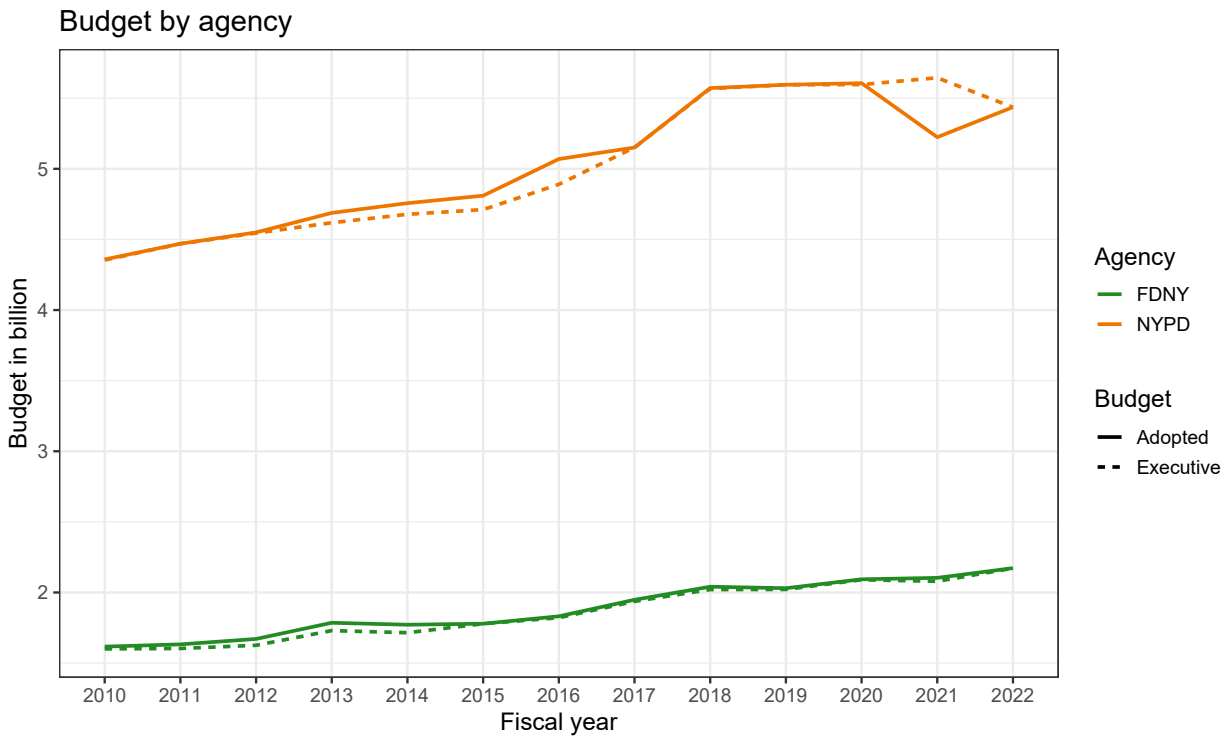
The scope of the budget adjustment was unprecedented and largely unexpected. As Figure 1 illustrates, NYPD's operating budget increased in almost all years prior to FY2021. Additionally, former NYC mayor Bill De Blasio's executive budget proposal in April 2020 included a minimal cut of only $24 million, 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]

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

[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.

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.

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. Overtime pay per NYPD employee—important source of officers' income—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

[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).

the FY21 academy and cadet classes (Citizens Budget Commission, 2020).[10]

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

[10] See Figure A2 for more details.

specific political district.[11]

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.[12] 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, which allows me to account for differences in response times across police management units using precinct fixed effects in my empirical design.

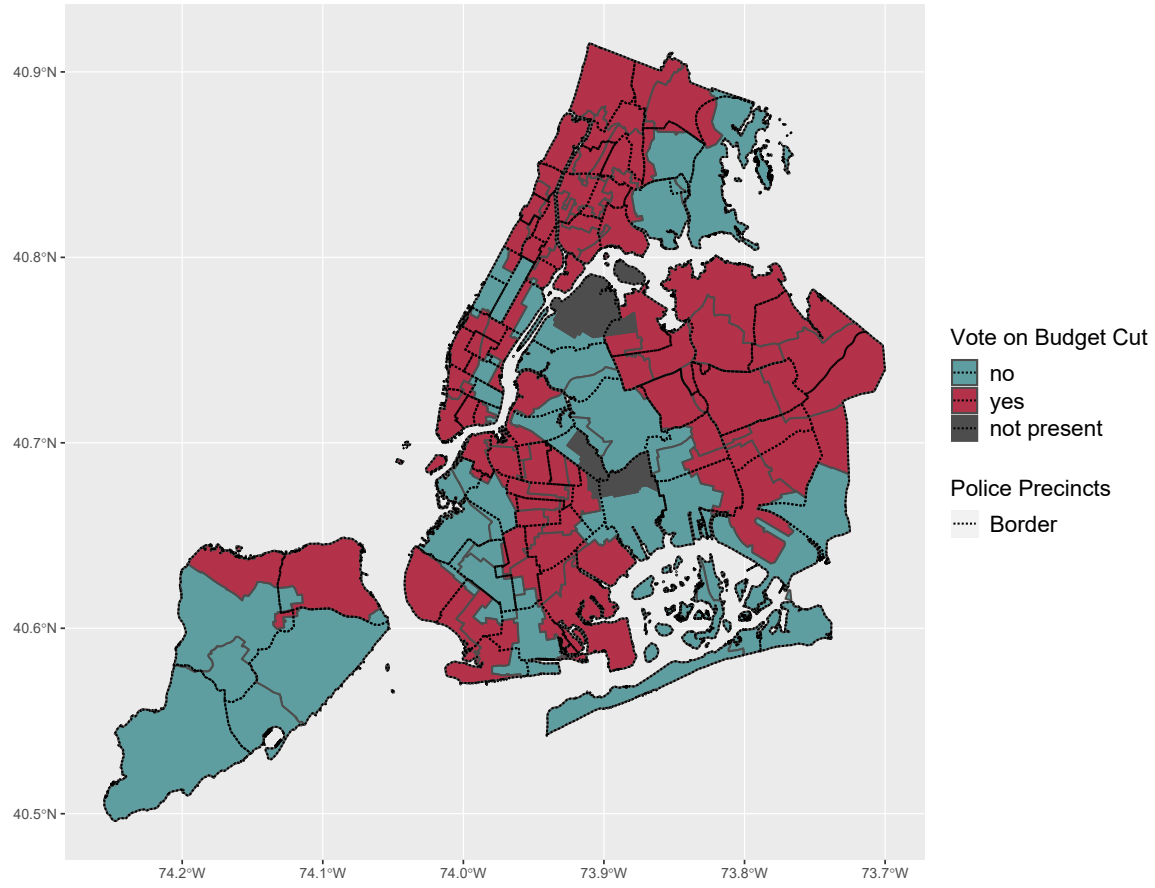
To provide some information on possible factors influencing a council member's voting behavior, Table A2 shows summary statistics of district characteristics. Unsurprisingly, districts in favor of the budget cut are somewhat more progressive and more crime-ridden. These areas had significantly larger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

These patterns raise concerns that shirking may be ineffective in progressive yes-voting districts, where voters might consistently blame the police for poor service and strongly support funding reductions for police. However, a majority of citizens in these districts did *not* necessarily support reducing the law enforcement budget. Figure A3 illustrates the distribution of preferences from the 2020 post-election CCES survey, showing how respondents across districts felt about law enforcement spending. Evidently, the differences in preferences between "yes" and "no" voting districts with respect to police funding remained marginal, and a majority of respondents supported increasing or maintaining law enforcement resources across both types of districts.

[11] Besides 911 call data, I collected various additional data for supplementary analyses presented below (seeAppendix A).

[12] 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



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 traffic or migration patterns due to COVID). To account for time-specific trends in response times, I additionally use response times of firefighters to 911 calls to fires and medical emergencies as my third control dimension.[13] 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, FDNY response times can serve as a credible counterfactual in bureaucrats' reactions to 911 calls absent electorally motivated behavior.

Thus, I estimate the following model:

$$\text{response time}_{icpda} = \beta_1 \text{yes vote}_c \times \text{after vote}_d \times \text{NYPD}_a + \delta_c + \eta_p + \gamma_d + \nu_a + \varepsilon_{icpda} \tag{1}$$

where $\text{response time}_{icpda}$ 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. δ_c, η_p, γ_d, and ν_a are district, police precinct, date, and agency fixed effects, respectively. Police precinct fixed effects capture key organizational distinctions within the NYPD and account for variations in management practices that can influence response times. Additionally, district fixed effects account for differences in district characteristics (see Table A2). To the extent that these

[13] 86% (14%) of FDNY calls are medical emergencies (fire incidents and utility emergencies) in my sample.

characteristics and their influence on NYPD response times stay constant across my sample period, my treatment effect estimates remain unbiased. Finally, date fixed effects capture the high variability of response times across time. I cluster standard errors ε_{icpda} on the district level.[14]

Figure 3: Visual Representation of DiD Identification, Hypothetical

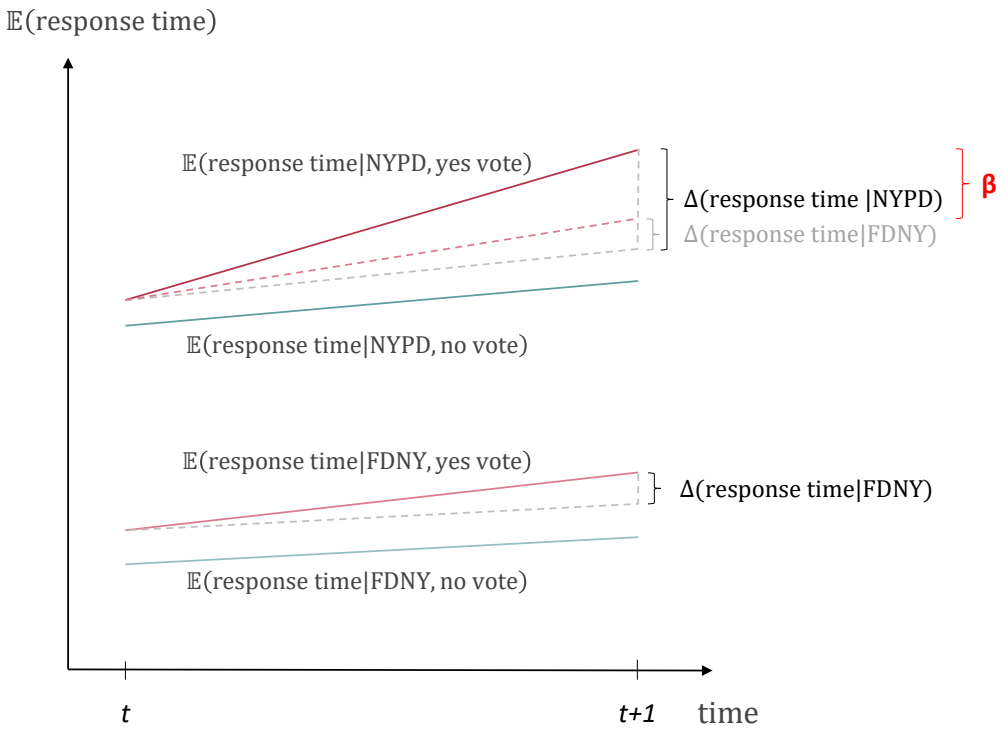


Figure 3 provides a graphical representation of the triple DiD identification strategy. While a simple DiD design would only rely on the divergent trends in NYPD response times within yes-voting districts vis-à-vis no-voting districts over time (i.e. Δ(response time|NYPD)), the triple DiD design incorporates the corresponding trends in FDNY response times in order to estimate the causal effect of the budget vote on bureaucrats' behavior (ATT = Δ(response time|NYPD) $-$ Δ(response time|FDNY)). The identifying assumption of this

[14]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.

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 vote.[15]

4.5 Spatial Difference-in-Discontinuities Design

The 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 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.[16] 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.

[15] 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.

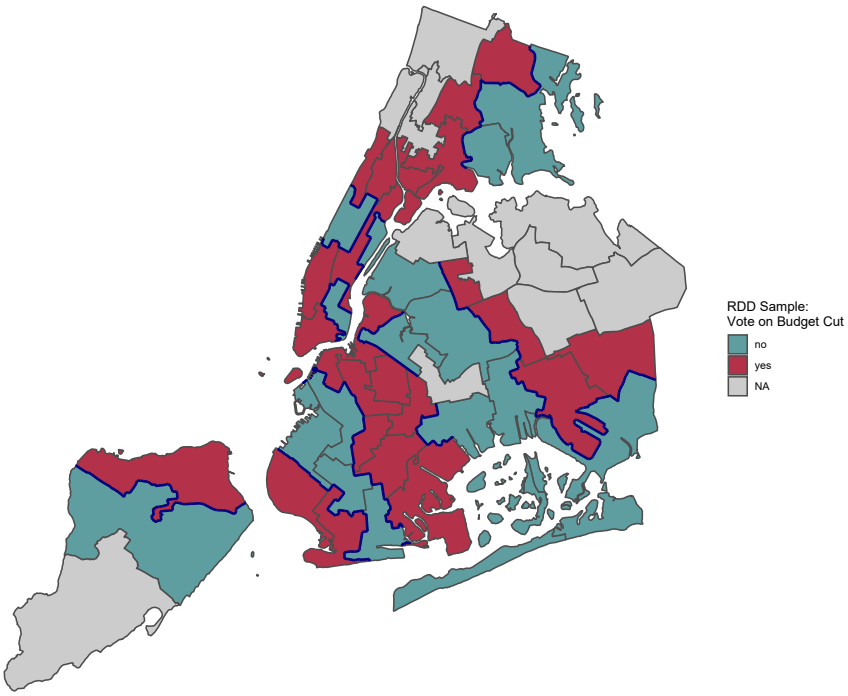
[16] As before I exclude dates affected by the George Floyd protest (May 29 - June 15, 2020). Additionally, to avoid concerns about anticipatory police behavior right before the vote, I also exclude calls between June 16 - June 30, 2020.

distance_{icpd} represents the minimum distance of call i to the border distinguishing these two categories of districts, and contains only units $\text{distance}_{icpd} \in [-h; h]$, where $-h$ and h denote the MSE-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 account for systematic differences in response times across police management units. 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 covariates, including call type and the number of calls per day on the zip code level.[17]

[17]Figure A5 and Figure A6 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 calls. The figure also highlights cyclical trends in response times (e.g., due to COVID19 waves), which my DiD design accounts for.[18]

[18]Given the unusually high response times for the FDNY in March 2020 due to the COVID outbreak and its potential threat for the parallel trends assumption, I additionally estimate results excluding March 2020 in Table A5. Reassuringly, while the estimated treatment effects are slightly smaller, the qualitative results remain unchanged.

Figure 5: Trends in 911 Response Time across Districts

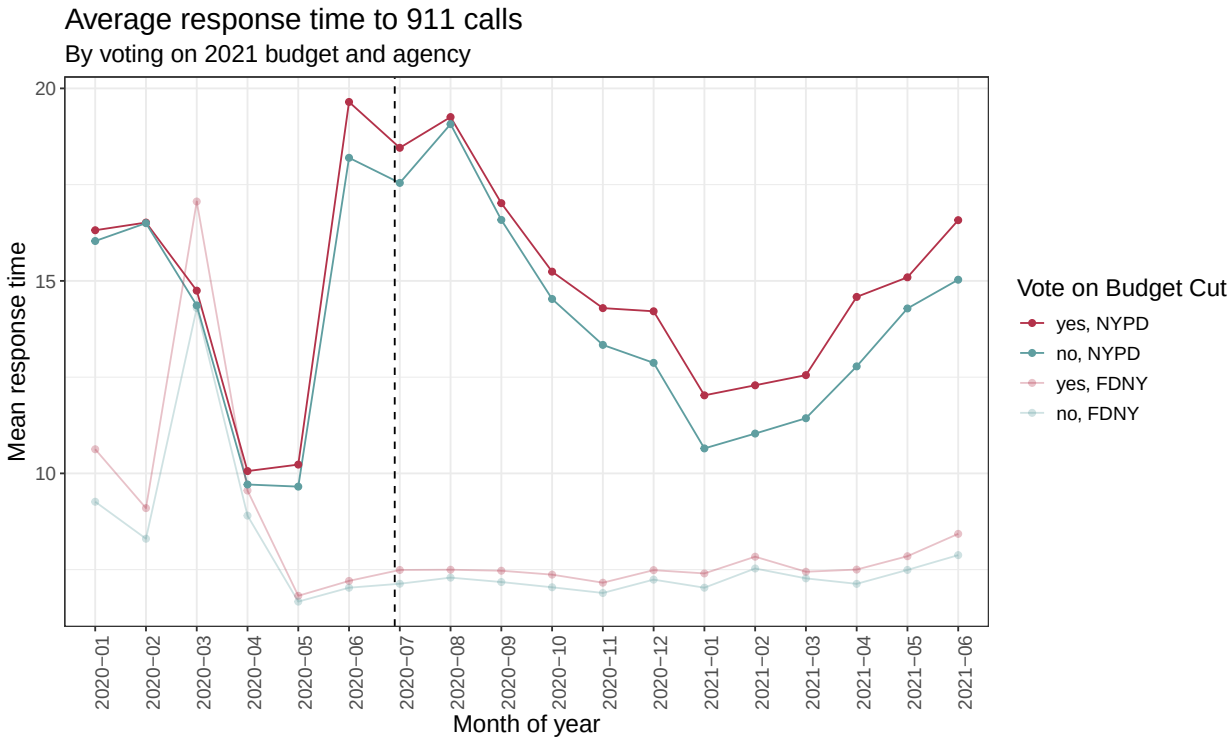


Table 1 evaluates trends in police 911 response times using the triple DiD model in Equation (1). We find that NYPD on average took about 5 minutes longer in their response times in aligned districts than FDNY before the budget vote (NYPD). After the vote, response times went up by about 2.3 minutes 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 20 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 13.1 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 robust to further controlling for the demand

for police presence (in Models (3)-(4)). Model (3) accounts for the total number of calls in districts and precincts per day by agency, and Model (4) 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 events plus serious fires from non-life-threatening emergencies and lower priority incidents for FDNY.[19]

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

	(1)	(2)	(3)	(4)
yes vote × after vote × NYPD	1.415**	1.433**	1.371**	1.443**
	(0.667)	(0.680)	(0.673)	(0.692)
NYPD	4.810***	5.257***	10.131***	-1.145
	(0.725)	(0.699)	(2.708)	(0.895)
yes vote × NYPD	-0.056	-0.250	-0.177	-0.213
	(1.129)	(1.108)	(1.057)	(1.108)
after vote × NYPD	2.286***	2.274***	2.303***	2.343***
	(0.430)	(0.439)	(0.434)	(0.449)
yes vote × after vote	-0.741	-0.756	-0.788	-0.752
	(0.561)	(0.562)	(0.565)	(0.569)
District FE	✓	✓	✓	✓
Date FE	✓	✓	✓	✓
Police Precinct FE		✓	✓	✓
Daily call volume (log) × Agency			✓	
Call Importance FE × Agency				✓
Observations	9,590,245	9,590,227	9,590,227	9,590,227
Mean of DV	13.095	13.095	13.095	13.095
Adj. R^2	0.025	0.032	0.033	0.034

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. 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, life-threatening medical emergencies, and serious fires, (2) Non-critical crimes, non-crime incidents, non-life threatening medical emergencies, and low priority fire incidents. Cluster robust standard errors in parentheses, by district (49).

[19]For more information on the official severity classification by agencies, see https://www.nyc.gov/site/911reporting/reports/definitions.page; https://www.nyc.gov/assets/fdny/downloads/pdf/about/citywide-stat-2015-08.pdf.

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

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

Figure A4 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.[20]

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 neighboring 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.[21] 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.

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

[21] Table A13, 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.

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 and Additional Results

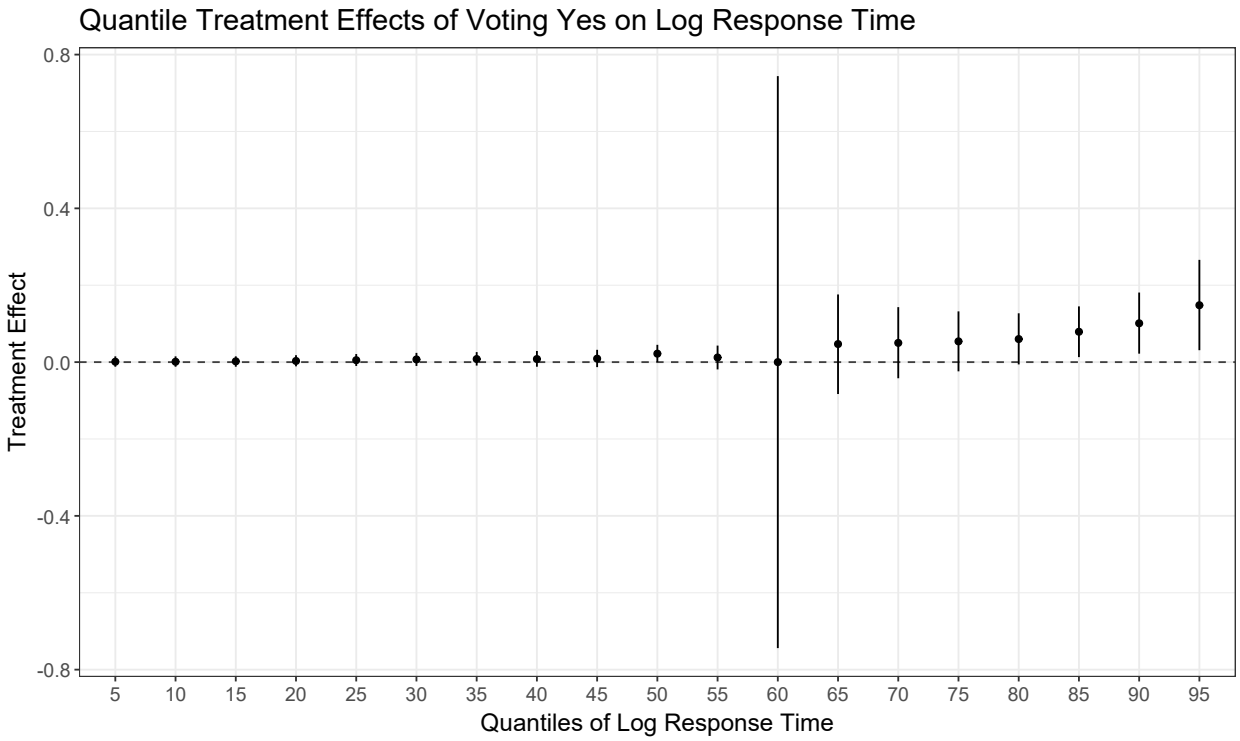
What type of calls do officers use for leverage shirking? If political motivation drives delays, increases in response times in non-aligned districts are likely a result of late arrivals and "no shows" of officers to calls where police have sufficient discretion in how they address the incident and face fewer costs for shirking. Figure A7 indicates that these discretionary calls, such as crimes not in progress, disputes and vehicle accidents, take longer at baseline. To evaluate this mechanism, Figure 6 depicts quantile treatment effects.[22] 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).[23]

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

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

[23]To alleviate concerns that these effects are driven by a few outliers in response times, Table A6 shows the robustness of my DiD results using winsorized response times.

Figure 6: Quantile Treatment Effects



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

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.[24] 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!"[25] What is more, as concerns about rising response times

[24] 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.

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

arose in the public discourse, police unions attributed the blame for reduced public service quality to the city council and the mayor.[26] When then-council member Ritchie Torres and then-borough president Eric Adams called for an 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.[27]

Police unions also weren't shy to call 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[28], 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"[29] or "keep voting Democrat and you'll have war zones.. just ask Chicago, Detroit, Baltimore!"[30] 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

[26] https://twitter.com/NYCPBA/status/1277671870205169665; https://twitter.com/NYCPBA/status/1300206634279620611.
[27] https://twitter.com/RitchieTorres/status/1303400519302631431.
[28] https://twitter.com/NYCPDDEA/status/1407332800345346054.
[29] https://twitter.com/NYCPBA/status/1277671870205169665.
[30] https://twitter.com/SBANYPD_Archive/status/1277424114249146374.

the opinion of the patrol supervisor happens to be."[31] 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.[32]

How did citizens react to the strategic shirking of police officers? While systemic data on citizens' complaints about NYPD behavior is unavailable[33], I illustrate possible downstream consequences of the budget vote and presumably the police resistance in Appendix E and F. To estimate how the budget vote correlated with citizens' perceptions of crime, I leverage responses to the "Most Important Problem" question in the monthly Gallup Social Series before and after the budget cut. Appendix E suggests that concerns about crime disproportionately increased among citizens in yes-voting districts after the budget cut compared to no-voting districts. Analyzing changes in vote shares of incumbent council members between 2017 and 2021 elections in Appendix F, I also 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 and underlying crime conditions. 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

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

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

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

individuals in non-aligned districts had different likelihoods to call the police, only called for minor incidents that take longer to respond a priori, or crime rates evolved differently in these council districts, this could explain increases in response times. Yet, my data suggests that this is unlikely to occur. The number of daily calls moves in tandem in treatment and control districts before and after the budget vote, with an insignificant 2% decrease in "no" districts (Figure A8; Table A10). Additionally, the distribution of different call types across treatment and control districts remains largely unchanged before and after the budget vote (Figure A9). There is also little evidence that the number of crime incidents and citizens' reporting of them changed differentially (see Table A11). Similarly, Table A12 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 explains response time differences.

Another concern is that the increase in response times in treatment districts may be driven by differential reductions in the number of available patrol officers due to staffing cuts, overtime limitations, or voluntary retirements. This would only affect my within-jurisdiction design if resources and staffing levels disproportionately dropped in precincts covering larger shares of yes-voting districts.[34] To test this alternative explanation, I matched NYPD officers in 2021 and 2020 to their precincts based on assignment data for both active and inactive personnel.[35] Figure A10 illustrates the relationship between changes in resources—measured by staff count, overtime hours, and overtime expenses—and the proportion of calls from yes-voting districts by precinct. The data shows little evidence that treatment areas experienced more significant resource declines. If anything, the treatment is correlated with a slightly *smaller* drop in staffing and overtime.

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 offi-

[34]The spatial RDD design already accounts for potential precinct-specific trends using fixed effects in split-sample regressions.

[35]See Appendix A for detailed information on the matching procedure and data sources.

cers' 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.[36] This data provides an imperfect, yet valuable fine-grained measure of citizens' outcry about policing in NYC over time. Figure A11 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 A14, 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.

Similarly, supporting the budget cut may capture other aspects of a council district that might affect police behavior post George Floyd. To address these concerns about a compound treatment, Table A7 presents results from "horse race" regressions where I estimate DiD designs with councilors' race and the 2020 Biden vote share on the district level, respectively. There is little evidence that these alternative district covariates affected police behavior after the budget cut. Importantly, the treatment effects of voting "yes" are largely robust to accounting for these additional predictors.

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

[36]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.

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 (Roman et al., 2023). If these alternative explanations are true, we should observe officers to reduce pro-active policing by minimizing the number of officer-initiated calls. 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 A8 indicates, I find no evidence that NYPD officer-initiated calls decreased disproportionately in non-aligned districts, thus alleviating concerns that pure morale or avoidance effects drive the results.[37]

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, their tenure protections as well as plausible deniability of responsibility for observable poor service provision. Focusing

[37] Given that officer-initiated calls in my sample likely follow a different data generating process than other calls, I also estimate my main results without officer-initiated calls in Table A9.

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.

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.[38] 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 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, or whether housing bureaucracies strategically delay building permits under certain circumstances.

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

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

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 June 30, 2021 ($N = 1,972,635$).	https://data.cityofnewyork.us/Public-Safety/EMS-Incident-Dispatch-Data/76xm-jjuj
Fire 911 call data (FDNY administered)	The Fire Incident Dispatch Data file contains data on fire and utility emergency service calls in NYC, generated by the Starfire Aided Computer 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 June 30, 2021 ($N = 313,620$). I removed all medical emergencies to ensure no duplicates of incidents with the EMS data.	https://data.cityofnewyork.us/Public-Safety/Fire-Incident-Dispatch-Data/8m42-w767/about_data
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

Continued on next page

Data Set	Description	Source
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
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

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Data Set	Description	Source
Staffing and overtime by police precinct	To measure the resources by precinct in fiscal years 2020 and 2021, I use city-wide payroll data. This lists salary of each city employee by agency at the end of each fiscal year. Each record represents the following statistics for every city employee: Agency, Last Name, First Name, Middle Initial, Agency Start Date, Work Location Borough, Job Title Description, Leave Status as of the close of the FY (June 30th), Base Salary, Pay Basis, Regular Hours Paid, Regular Gross Paid, Overtime Hours worked, Total Overtime Paid, and Total Other Compensation (i.e. lump sum and/or retro payments). I keep the last observation per employee in the payroll data (for those working in 2019 and 2020). To identify individual employees, I use appointment date, lastname, firstname, and initials. I then match these records to the list of active NYPD officers from NYPD online (as of 10/20/2021) in three steps: (1) Merge on appointment date, lastname, firstname, middle initial; (2) Merge again on appointment date, lastname and firstname, i.e. no initial; (3) Merge again on appointment date, lastname, and check all matches manually; (4) Merge again on appointment date plus fuzzy name merge for entire name, and check again manually. Finally, I add non-matched officers by matching them with the list of active and inactive officers from CCRB records (as of 10/30/2021). I am able to match 96% of all officers.	https://nypdonline.org/link/officer-profile; https://www.nyc.gov/site/ccrb/policy/MOS-records.page
Election results	I use administrative data on election results at the election district level for the 2017 and 2021 city council elections from the NYC Board of Elections. For each electoral district and election I calculate the vote share for council members voting on the 2021 budget. See Appendix F for further discussion and results.	https://vote.nyc/page/election-results-summary
Gallup Poll Social Series	I use micro-level data from the monthly Gallup Poll Social Series between January 2019 and January 2023 in supplementary analyses. I rely on the "Most Important Issue" question to assess trends in citizens' concern about crime across zip codes of NYC before and after the budget cut. See Appendix E for further discussion and results.	https://www.gallup.com/175307/gallup-poll-social-series-methodology.aspx

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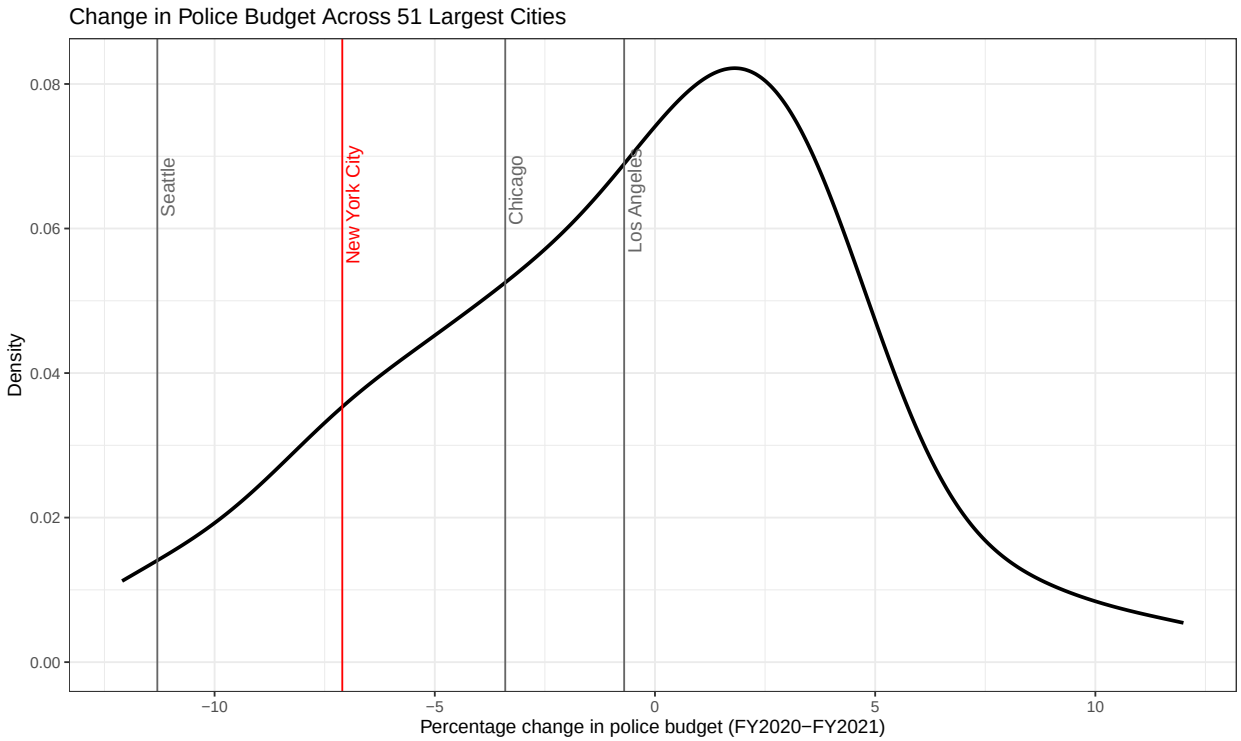
Data Set	Description	Source
CCES 2020 post-election survey	I use micro-level data from the 2020 post-election CCES survey in supplementary analyses. I use response to the following question: "State legislatures must make choices when making spending decisions on important state programs. How would you like your legislature to spend money on law enforcement?" to assess differences in citizens' preferences on police funding across zip codes of NYC before and after the budget cut. I match zip codes to council districts based on what district covers the largest area of each zip code.	https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi%3A10.7910/DVN/E9N6PH

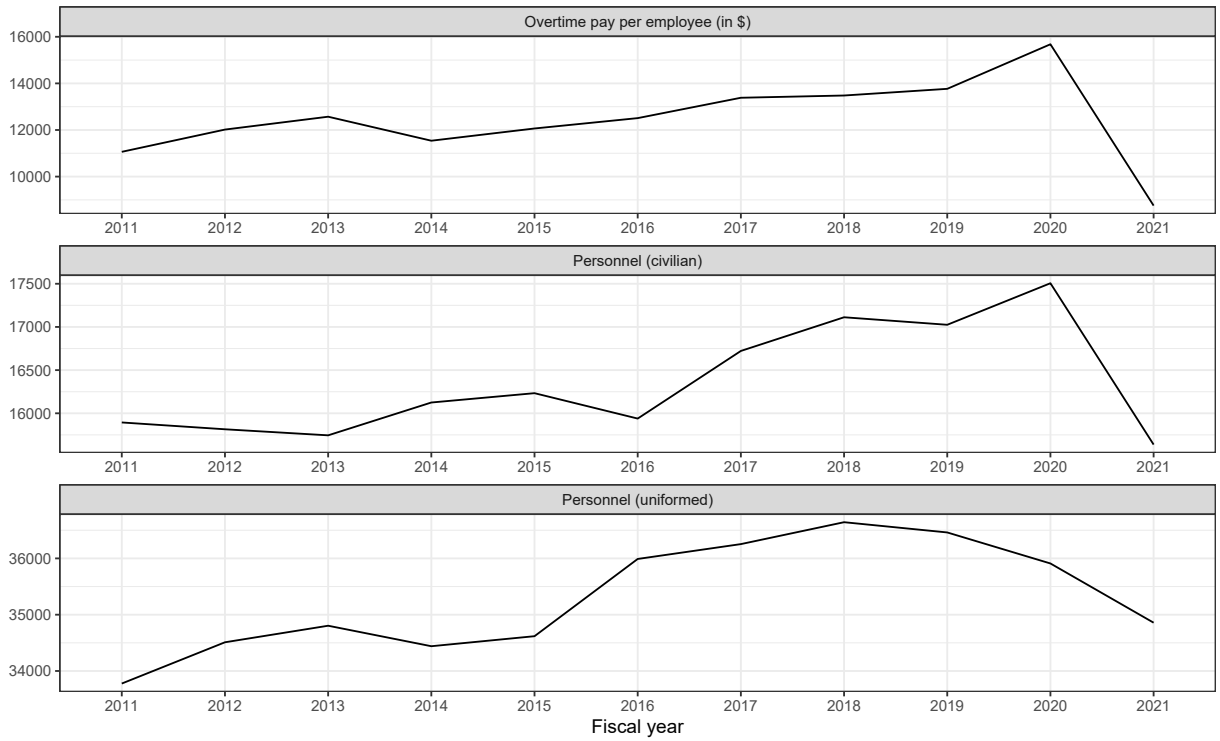
B Background on Case: NYPD 2021 Budget Cut

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



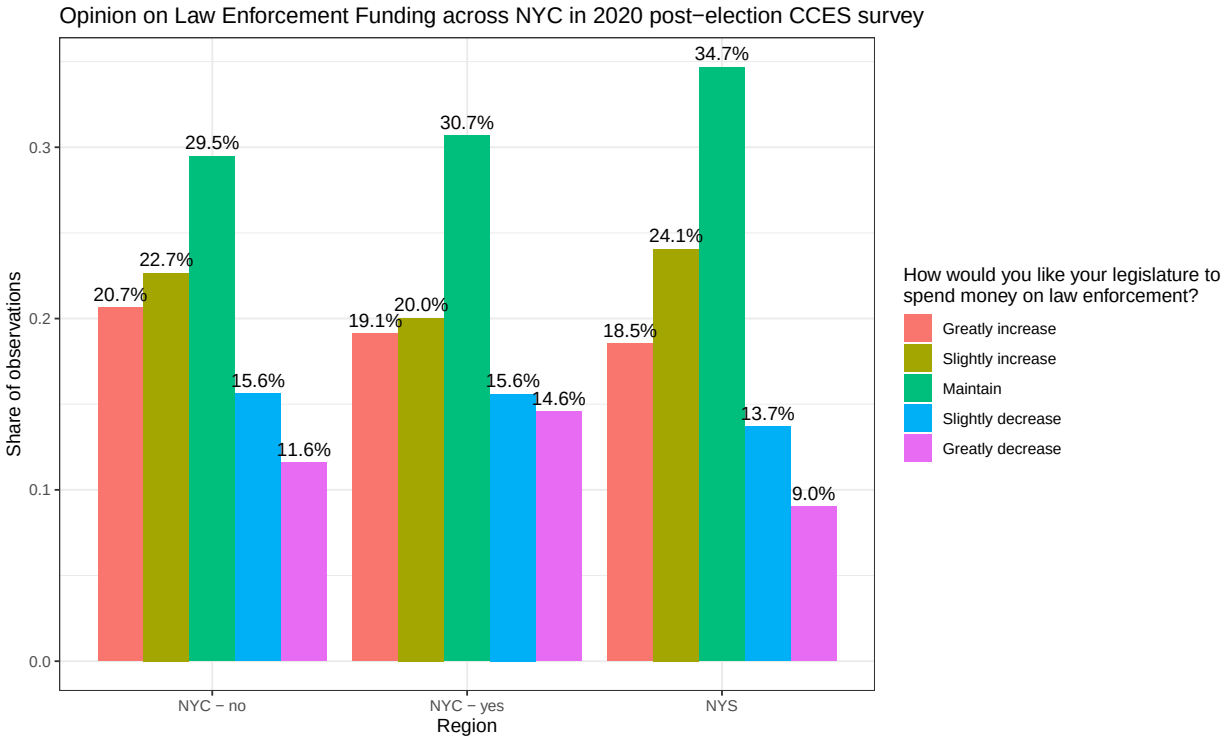
Note: The figure depicts NYPD resources from the FY2015, FY2020 and FY2021 Mayor's Management Reports (MMR), including paid overtime per employee, civilian personnel and uniformed personnel.

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.

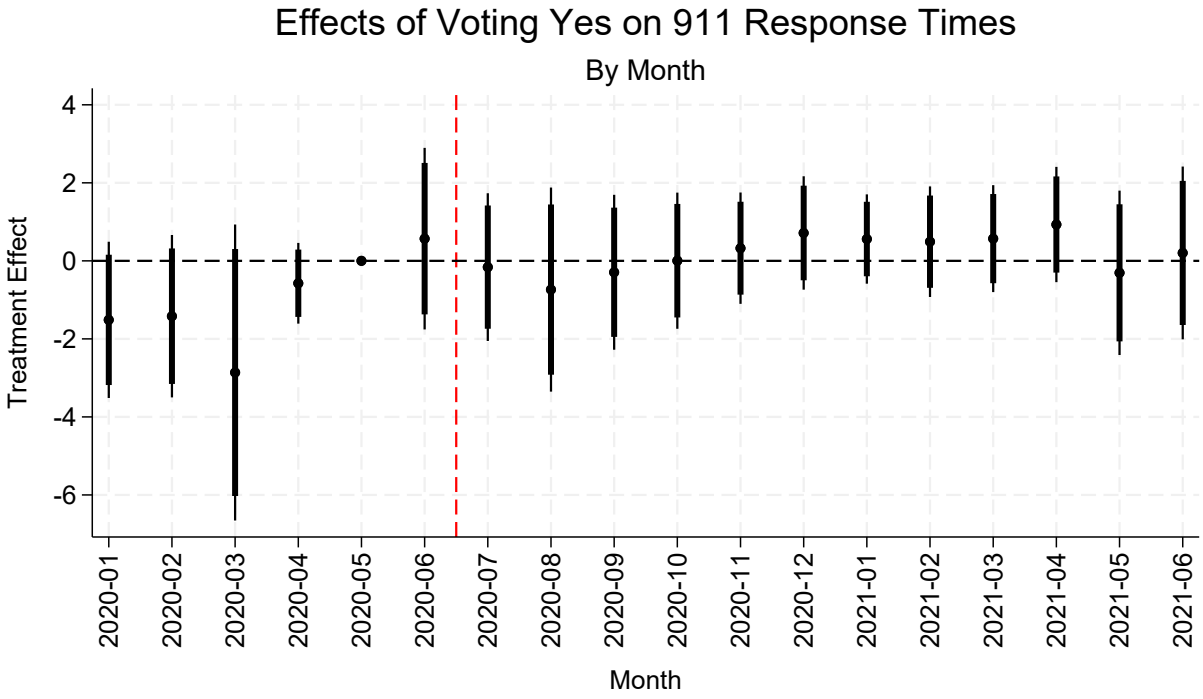
Figure A3: Preferences on Police Funding in 2020



Note: The figure depicts the distribution of survey responses regarding law enforcement funding from the 2020 post-election CCES survey, by type of NYC council districts and compared to citizens of New York State outside NYC. Missings are removed.

C Additional Figures for Main Results

Figure A4: Monthly Treatment Effects



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

Figure A5: Balance of Matched RDD Sample - Major Call Types

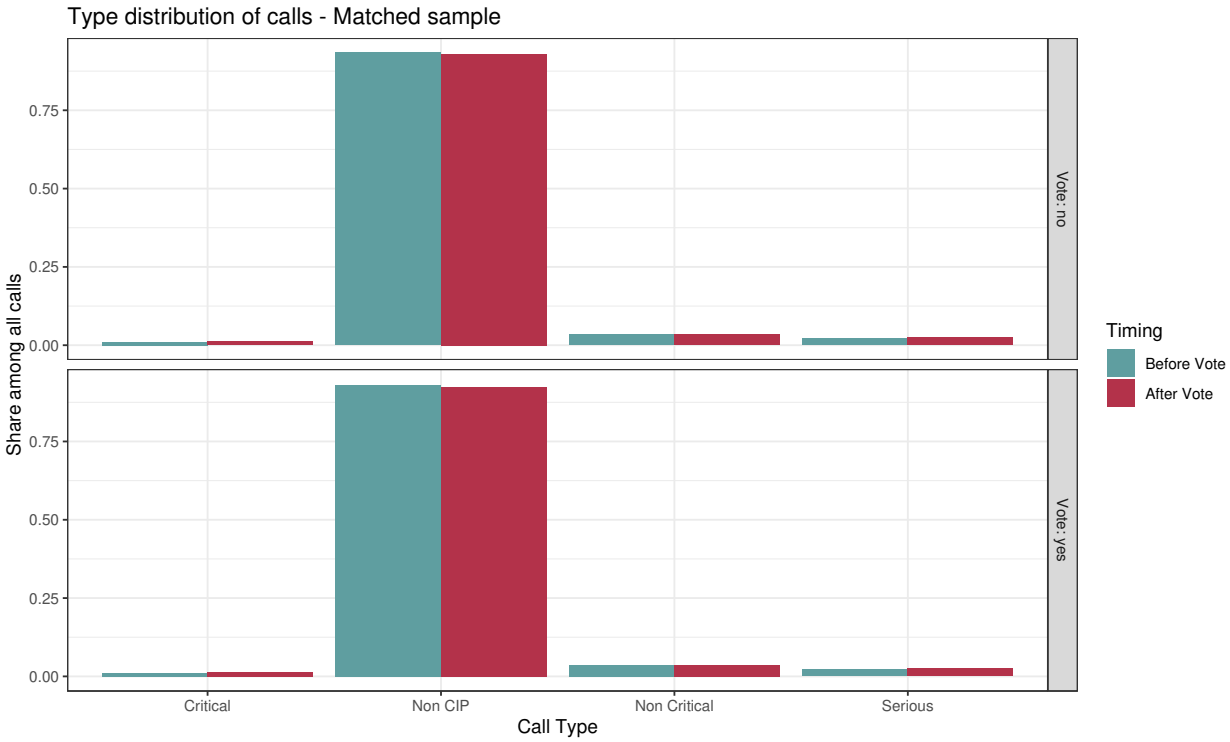


Figure A6: Balance of Matched RDD Sample - Daily Call Volume by Zip Code

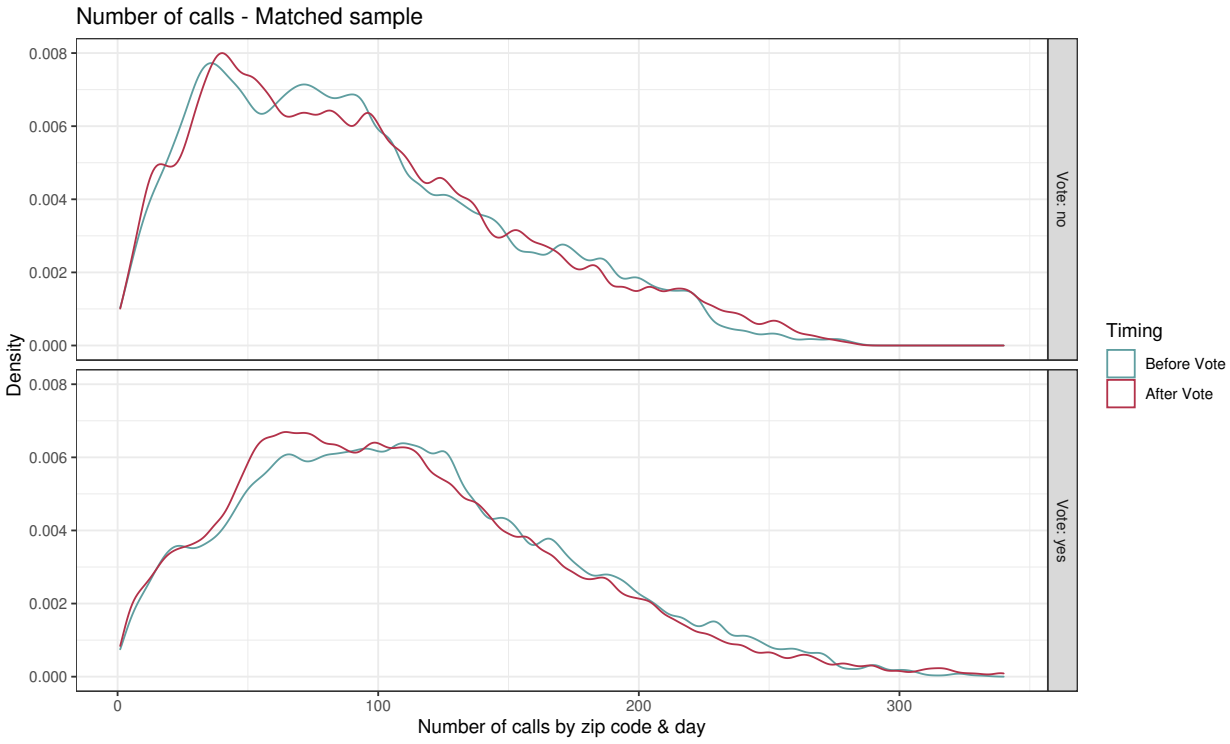
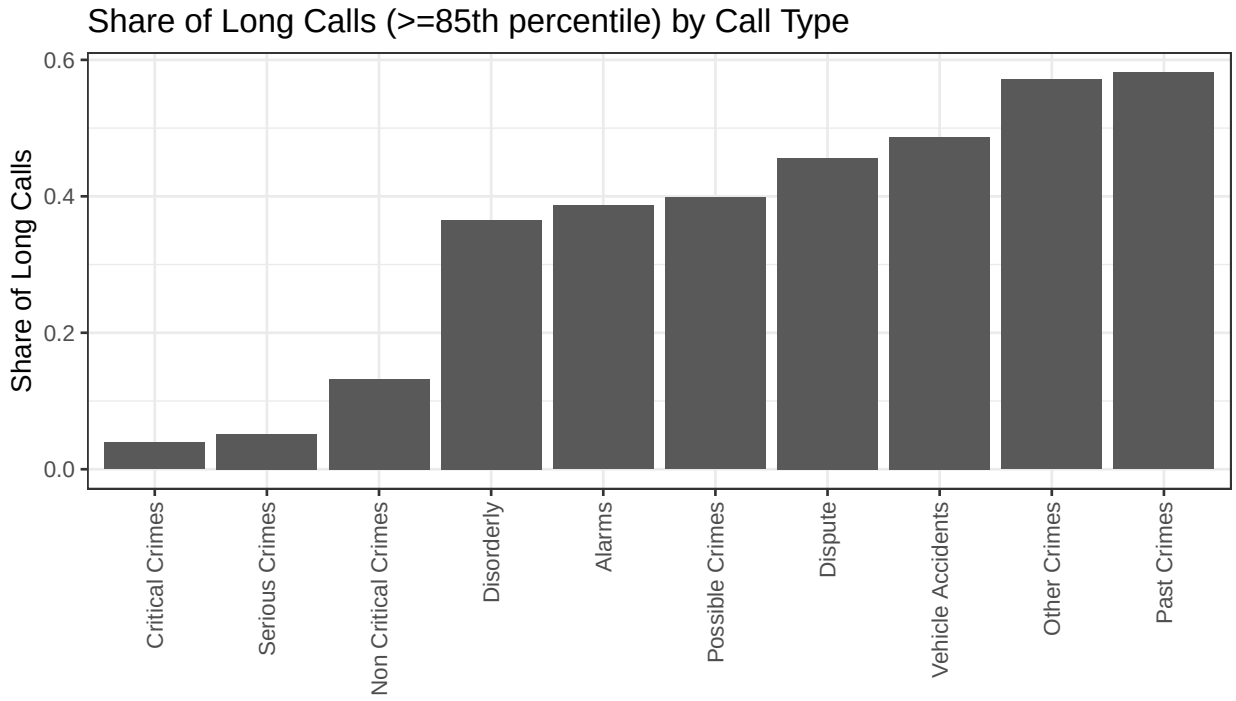


Figure A7: Call Length by Call Type



D Robustness

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

	(1)	(2)	(3)	(4)
yesvote × postvote × NYPD	1.187*	1.208*	1.152*	1.218*
	(0.623)	(0.633)	(0.626)	(0.646)
NYPD	5.299***	5.749***	10.504***	-0.582
	(0.731)	(0.698)	(2.722)	(0.912)
yesvote × NYPD	0.170	-0.024	0.044	0.014
	(1.164)	(1.140)	(1.091)	(1.142)
postvote × NYPD	1.799***	1.785***	1.825***	1.839***
	(0.407)	(0.414)	(0.407)	(0.424)
yesvote × postvote	-0.667	-0.677	-0.707	-0.674
	(0.518)	(0.519)	(0.523)	(0.525)
District FE	✓	✓	✓	✓
Date FE	✓	✓	✓	✓
Police Precinct FE		✓	✓	✓
Daily call volume (log) × Agency			✓	
Call Importance FE × Agency				✓
Observations	9853758	9853736	9853736	9853736
Mean of DV	13.169	13.169	13.169	13.169
Adj. R^2	0.025	0.032	0.033	0.034

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. 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*	-0.811	-0.807	-0.806
	(0.393)	(0.389)	(0.398)	(0.520)	(0.520)	(0.525)
daily call volume (log)		-1.748***			0.225***	
		(0.281)			(0.072)	
District FE	✓	✓	✓	✓	✓	✓
Police Precinct FE	✓	✓	✓	✓	✓	✓
Date FE	✓	✓	✓	✓	✓	✓
Call Importance FE			✓			✓
Observations	7369246	7369246	7369246	2220981	2220981	2220981
Mean of DV	14.508	14.508	14.508	8.409	8.409	8.409
Adj. R^2	0.034	0.034	0.036	0.099	0.099	0.106

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. 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 Excluding March 2020

	(1)	(2)	(3)	(4)
yes vote × after vote × NYPD	0.993**	0.998**	0.930**	1.010**
	(0.452)	(0.466)	(0.458)	(0.474)
NYPD	5.922***	6.339***	10.080***	-0.396
	(0.814)	(0.800)	(2.841)	(0.984)
yes vote × NYPD	0.349	0.171	0.228	0.203
	(1.313)	(1.301)	(1.249)	(1.305)
after vote × NYPD	1.178***	1.195***	1.170***	1.254***
	(0.300)	(0.316)	(0.307)	(0.320)
yes vote × after vote	-0.339	-0.346	-0.375	-0.344
	(0.249)	(0.250)	(0.257)	(0.253)
District FE	✓	✓	✓	✓
Date FE	✓	✓	✓	✓
Police Precinct FE		✓	✓	✓
Daily call volume (log) × Agency			✓	
Call Importance FE × Agency				✓
Observations	9007257	9007240	9007240	9007240
Mean of DV	12.968	12.968	12.968	12.968
Adj. R^2	0.025	0.032	0.033	0.034

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. 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, life-threatening medical emergencies, and serious fires, (2) Non-critical crimes, non-crime incidents, non-life threatening medical emergencies, and low priority fire incidents. Cluster robust standard errors in parentheses, by district (49).

Table A6: 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	1.145**	1.203**
	(0.544)	(0.585)
NYPD	4.595***	4.582***
	(0.629)	(0.633)
yes vote × NYPD	-0.413	-0.481
	(0.971)	(0.961)
after vote × NYPD	2.067***	2.166***
	(0.368)	(0.396)
yes vote × after vote	-0.642	-0.685
	(0.473)	(0.513)
District FE	✓	✓
Police Precinct FE	✓	✓
Date FE	✓	✓
Observations	9590227	9590227
Mean of DV	12.294	12.339
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 A7: Effect of Approving 2021 Budget on 911 Response Times
Robustness to Compound Treatments

	(1)	(2)	(3)	(4)
	Triple DiD		Simple DiD	
yes vote × after vote × NYPD	1.16*	1.19*		
	(0.67)	(0.68)		
yes vote × after vote	-0.51	-0.40	0.66	0.79**
	(0.52)	(0.55)	(0.40)	(0.39)
NYPD	6.06***	7.39***		
	(0.96)	(2.54)		
yes vote × NYPD	-0.44	0.04		
	(1.11)	(1.01)		
after vote × NYPD	3.28***	0.57		
	(0.67)	(1.18)		
white councilor × after vote × NYPD	-1.88***			
	(0.67)			
white councilor × after vote	1.64***		-0.30	
	(0.48)		(0.38)	
white councilor × NYPD	-1.53			
	(1.19)			
Biden vote share × after vote × NYPD		2.34		
		(1.66)		
Biden vote share × after vote		-3.59***		-1.12
		(1.09)		(1.26)
Biden vote share × NYPD		-2.92		
		(3.28)		
District FE	✓	✓	✓	✓
Date FE	✓	✓	✓	✓
Police Precinct FE	✓	✓	✓	✓
Observations	9590227	9590227	7369246	7369246
Mean of DV	13.095	13.095	14.508	14.508
Adj. R^2	0.032	0.032	0.034	0.034

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. 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 A8: Effect of Approving 2021 Budget on Probability of Officer-Initiated Calls

	(1)	(2)	(3)	(4)
	Response Time = 0		Response Time < 0.15	
yes vote × after vote × NYPD		0.013		0.002
		(0.014)		(0.009)
NYPD		-0.011***		0.553***
		(0.002)		(0.011)
yes vote × NYPD		-0.003		-0.007
		(0.002)		(0.017)
after vote × NYPD		0.354***		-0.006
		(0.008)		(0.007)
yes vote × after vote	0.011	-0.003***	0.001	-0.001
	(0.014)	(0.001)	(0.009)	(0.001)
District FE	✓	✓	✓	✓
Police Precinct FE	✓	✓	✓	✓
Date FE	✓	✓	✓	✓
Observations	7369246	9590227	7369246	9590227
Mean of DV	0.250	0.194	0.569	0.439
Adj. R^2	0.315	0.323	0.039	0.251

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. 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 A9: Effect of Approving 2021 Budget on 911 Response Times, Excluding Zero Response Time Calls

	Simple DiD	Triple DiD		
	(1)	(2)	(3)	(4)
yes vote × after vote × NYPD		2.396*	2.350*	2.468*
		(1.317)	(1.318)	(1.344)
NYPD		5.249***	7.332**	-7.494***
		(0.702)	(3.097)	(1.099)
yes vote × NYPD		-0.237	-0.213	-0.184
		(1.113)	(1.063)	(1.118)
after vote × NYPD		10.543***	10.549***	10.947***
		(0.834)	(0.838)	(0.859)
yes vote × after vote	1.548*	-0.823	-0.856	-0.821
	(0.895)	(0.569)	(0.574)	(0.576)
District FE	✓	✓	✓	✓
Police Precinct FE	✓	✓	✓	✓
Date FE	✓	✓	✓	✓
Daily call volume (log) × Agency			✓	
Call Importance FE × Agency				✓
Observations	5523493	7727607	7727607	7727607
Mean of DV	19.355	16.252	16.252	16.252
Adj. R^2	0.052	0.056	0.057	0.061

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. 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: Difference in Number of Calls by 2021 Budget Vote and Time

	Simple DiD	Triple DiD
	(1)	(2)
yes vote × after vote × NYPD		-0.014
		(0.033)
NYPD		1.128***
		(0.102)
yes vote × NYPD		-0.039
		(0.134)
after vote × NYPD		0.014
		(0.023)
yes vote × after vote	-0.024	-0.018
	(0.028)	(0.015)
District FE	✓	✓
Police Precinct FE	✓	✓
Date FE	✓	✓
Observations	113700	212626
Mean of DV	3.625	3.154
Mean of untransformed DV	80.565	53.527
Adj. R^2	0.265	0.326

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Dependent variable: Log number of calls by date, precinct and council district. Column (1) only includes NYPD calls. Cluster robust standard errors in parentheses, by district (49).

Table A11: Difference in Crime Incidents by 2021 Budget Vote and Time

	Calls for crimes	Serious crime calls	Shootings	Complaints
yesvote × postvote	-0.014	0.011	0.004*	-0.011
	(0.013)	(0.012)	(0.002)	(0.015)
District FE	✓	✓	✓	✓
Police Precinct FE	✓	✓	✓	✓
Date FE	✓	✓	✓	✓
Observations	114661	114661	114661	114661
Mean of DV	1.382	0.707	0.006	1.342
Adj. R^2	0.261	0.194	0.019	0.230

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Dependent variable: Log number of calls for crimes, log number of shootings, log number of complaints, or log number of calls for serious crimes by date, precinct and council district. Cluster robust standard errors in parentheses, by district (49).

Table A12: 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 A13: 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 A14: Effect of Approving 2021 Budget on 911 Response Times Accounting for Protests

	(1) Simple DiD	(2)	(3) Triple DiD	(4)
yes vote × after vote × NYPD		1.432**	1.403**	1.379**
		(0.680)	(0.673)	(0.671)
NYPD		5.257***	5.213***	5.372***
		(0.699)	(0.694)	(0.709)
yes vote × NYPD		-0.250	-0.255	-0.299
		(1.108)	(1.110)	(1.101)
after vote × NYPD		2.274***	2.404***	2.426***
		(0.439)	(0.446)	(0.446)
yes vote × after vote	0.684*	-0.755	-0.724	-0.707
	(0.393)	(0.563)	(0.553)	(0.552)
# of protests (log)	0.298	0.090	-5.511***	
	(0.761)	(0.445)	(1.187)	
after vote × # of protests (log)	-0.314		7.820***	
	(0.970)		(1.786)	
NYPD × # of protests (log)			7.558***	
			(1.919)	
after vote × NYPD × # of protests (log)			-10.448***	
			(2.875)	
# of protests (log) (June 2020)				0.490
				(0.920)
after vote × # of protests (log) (June 2020)				1.102**
				(0.541)
NYPD × # of protests (log) (June 2020)				-1.240
				(2.051)
after vote × NYPD × # of protests (log) (June 2020)				-1.628
				(1.043)
District FE	✓	✓	✓	✓
Police Precinct FE	✓	✓	✓	✓
Date FE	✓	✓	✓	✓
Observations	7369246	9590227	9590227	9590227
Mean of DV	14.508	13.095	13.095	13.095
Adj. R^2	0.034	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).

Figure A8: Trends in Amount of 911 NYPD Calls across Districts

Figure A9: Distribution of 911 Call Types, by Period and District

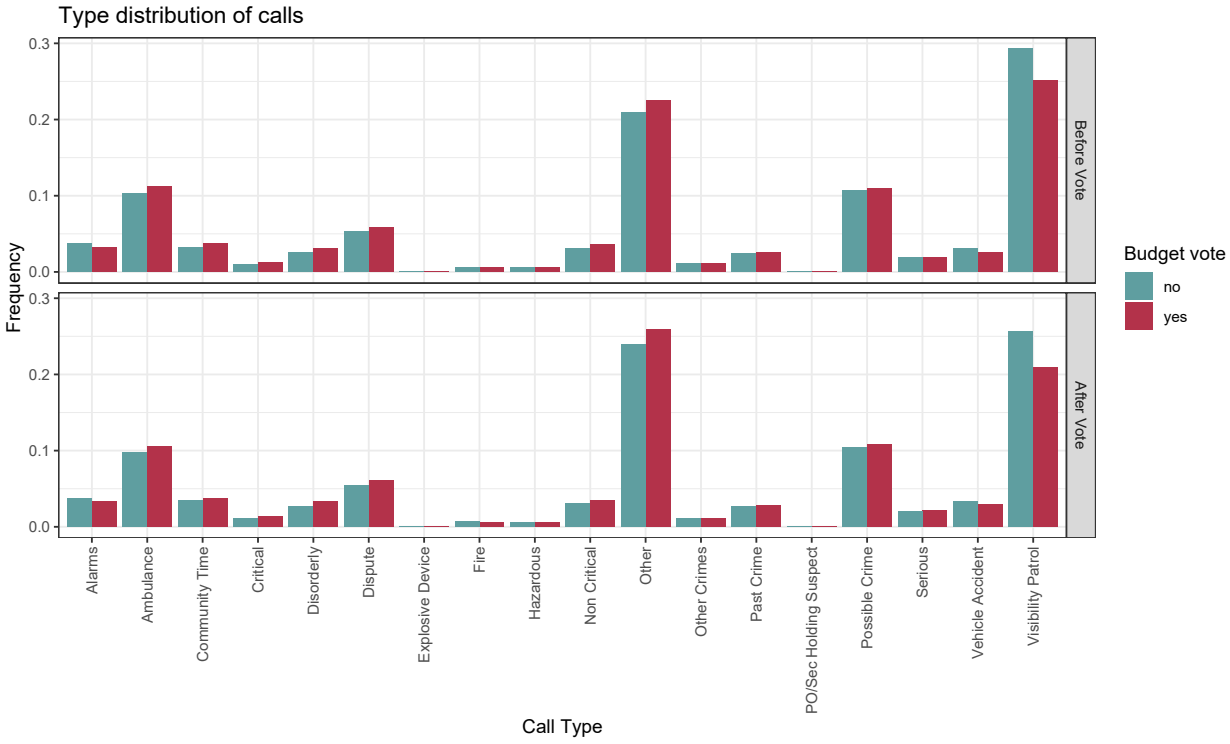


Figure A10: Correlation of Treatment and Change in Resources by Precinct

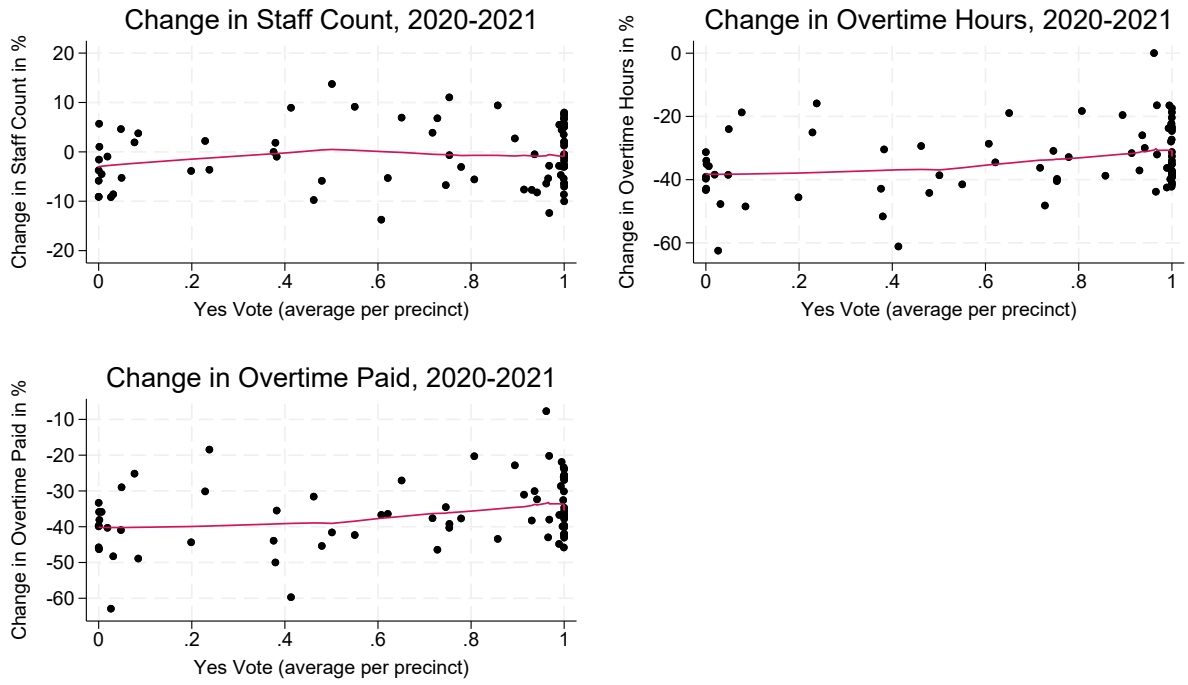
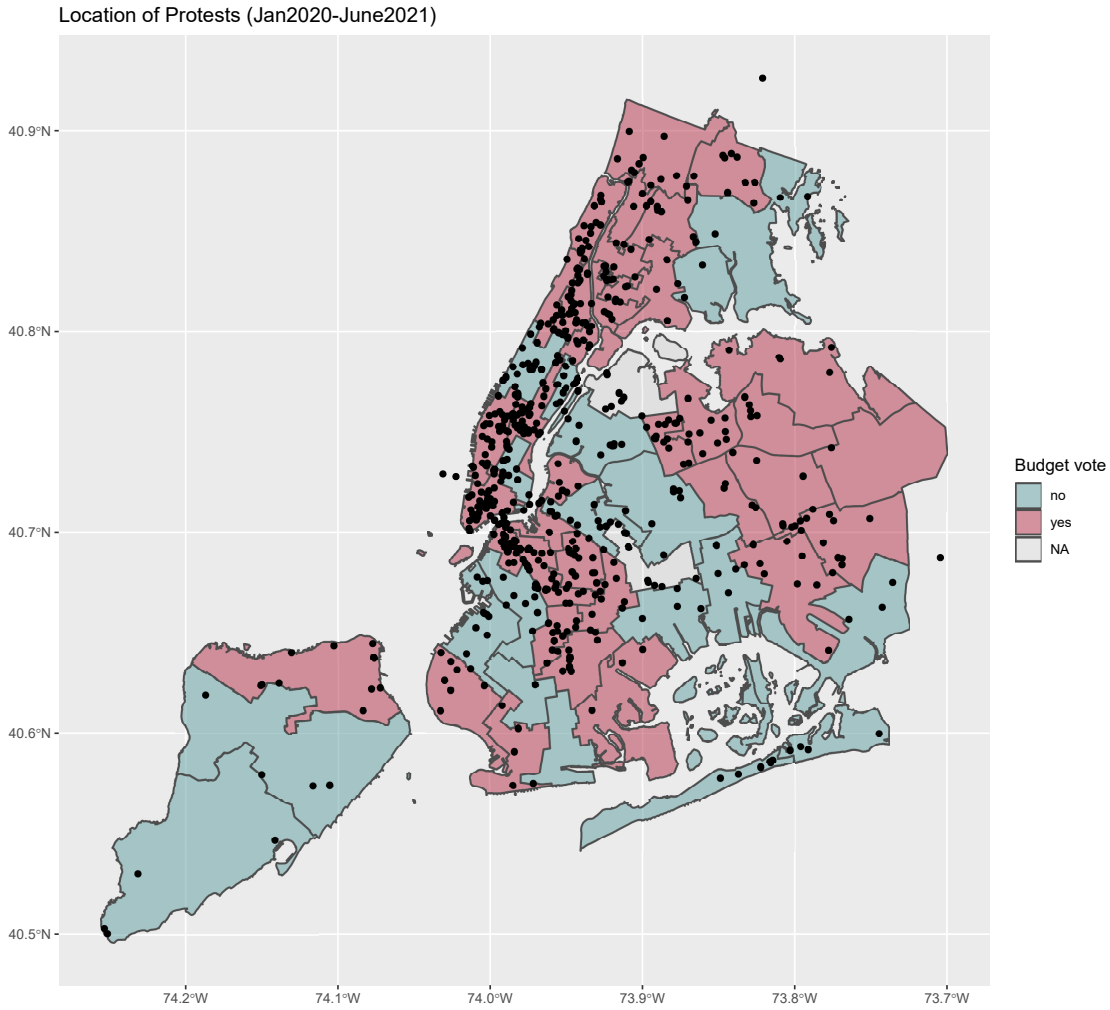


Figure A11: Location of Police-Related Protests



E Changes in Public Safety Concerns

In this section, I study how citizens' concerns about crime diverged across types of council districts after the budget cut. I use micro-level data from the monthly Gallup Social Series (2019-2023), which includes a question on what issue respondents perceive to be the most important problem facing the country today. Information about the zip code of respondents allows me to match respondents in New York City to council districts.[1] Any interpretation of the following results requires considerable caution since restricting the Gallup data to only observations in the relevant neighborhoods of New York City yields a small number of observations and these survey data are by no means representative on the level of the council district. I estimate a simple difference-in-differences model:

$$MIP(crime)_{ijt} = \alpha + \beta \text{yes vote}_j \times \text{post vote}_t + \delta_j + \gamma_t + \mathbf{X'_{ijt}}\rho + \varepsilon_{ijt} \tag{4}$$

where $MIP(crime)_{it}$ is a dummy for whether respondent i in district j and month t mentions that crime is one of the top three most important issues in the country at the time. δ_j and γ_t are council and month fixed effects, respectively. $\mathbf{X_{ijt}}$ are respondent-level controls for partisanship and race.

Table A15 presents the results of the difference-in-differences design, and Figure A12 depicts average predicted probabilities based on column (2) of Table A15. The results suggest that citizens in NYC were disproportionately more concerned about crime after the budget cut in yes-voting than in no-voting districts. While these patterns are only descriptive and may be driven by a more general shift in the political environment, they are in line with the idea that police may play with the citizens perceptions of public safety as a result of their day-to-day service provision.

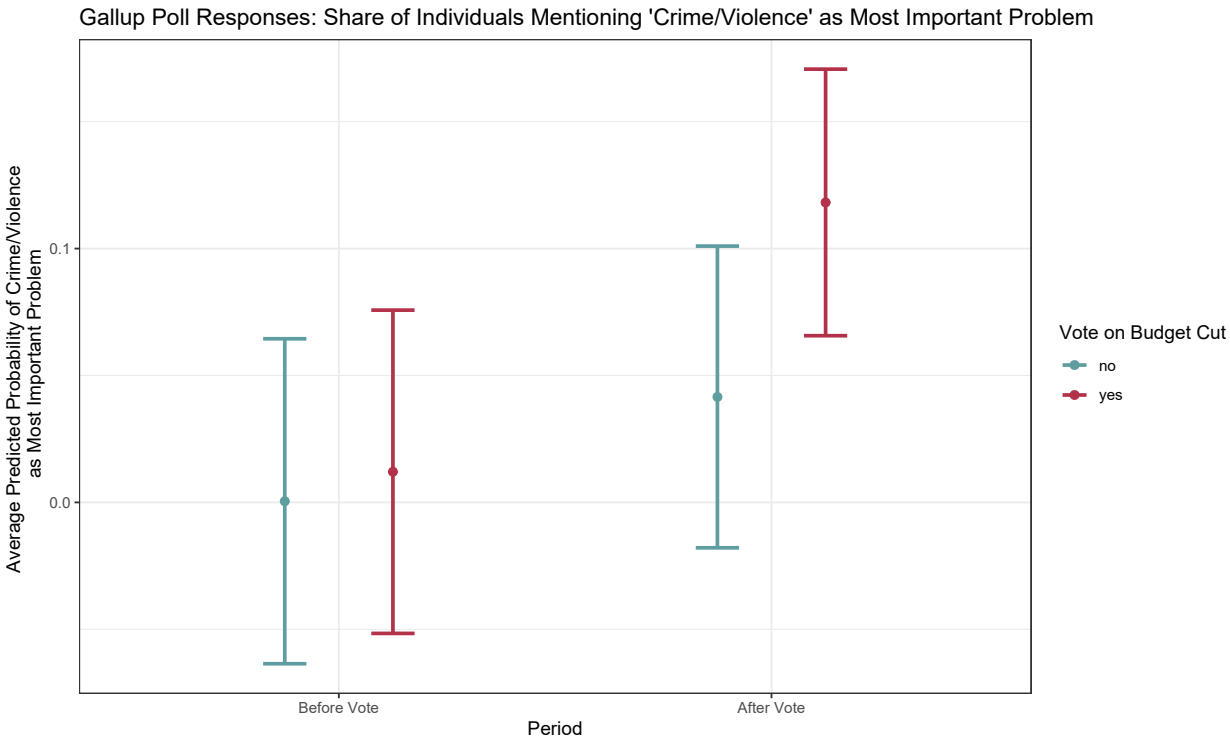
[1] Since zip codes are not perfectly subsumed in council districts, I match each zip code to the council district that accounts for the majority of its geographic area.

Table A15: Effect of Approving 2021 Budget on Crime Concerns

	(1)	(2)	(3)
yesvote × postvote	0.06*	0.07*	0.07*
	(0.03)	(0.03)	(0.03)
postvote	0.02		
	(0.03)		
yesvote	−0.03		
	(0.02)		
Council districts FE		✓	✓
Month FE		✓	✓
Individual controls			✓
Num. obs.	808	808	808
N Clusters	49	49	49

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Dependent variable: Dummy for indicating 'Crime/Violence' as MIP. Standard errors clustered by council districts. Individual controls include partisanship and race.

Figure A12: Predicted Probabilities of Indicating Crime as 'Most Important Problem'



F 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.[2] 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:

$$\Delta voteshare_{ie} = \alpha + \beta \text{yes vote}_i + \varepsilon_{ie} \tag{5}$$

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 A16 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.

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

Table A16: 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).