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? Existing research highlights the ability of bureaucrats to influence governments through campaign contributions, endorsements, and election turnout. I explore a more fundamental mechanism: bureaucrats strategically shirking responsibilities to leverage voters. Politicians rely on bureaucrats to achieve policy goals, which grants bureaucrats considerable leverage. When bureaucrats deviate in preferences from politicians and are politically organized and independent, they can reduce effort to exert political pressure. I use data on New York Police Department (NYPD) 911 response times and 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 misaligned 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

In 2020, millions marched to demand police reform and lawmakers across the political spectrum initially pledged support. Yet, despite this momentum, reform efforts have largely stalled nationwide (Pearson, 2022). In this article, I highlight one particular explanation for this reversal: when local elected officials challenge police interests, they face systematic bureaucratic resistance that extends far beyond conventional political opposition. Reform-minded city officials have experienced service infractions in their districts, smear campaigns, and personal threats (Blumgart, 2020; Bauer, 2020). These tactics represent a strategy openly discussed in law enforcement circles, with one police publication advising departments to "get dirty and fight to win" by making reformers' "lives a living hell" (Bauer, 2020).

In this paper, I examine how bureaucrats target misaligned political principals through strategic service provision. While politicians formally control 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. This dependency creates a fundamental tension in democratic governance that bureaucrats can exploit to resist unwanted reforms. 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 strategic 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 and their central role in government to exert political influence on misaligned incumbents.

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 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 pro-reform elected officials. In so doing, the police can affect voters' perceptions of public safety and their evaluations of reforming incumbents and their policies.

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 (see Figure A3). More importantly, testing my argument requires variation in the alignment of politicians and bureaucrats, holding other factors influencing bureaucrats' behavior fixed. The unprecedented disagreement among council members over the 2021 budget vote allows me to analyze the impact of political misalignment on bureaucratic resistance in the same jurisdiction. Thus, the unique nature of this policy shock is an asset to this study, rather than a limitation.

A natural threat to inference is that police behavior might diverge across aligned and misaligned districts after the budget cut due to other trends (e.g., differences in traffic levels or migration following COVID outbreaks). To overcome this, I employ a triple difference-in-differences design where I compare response times across misaligned and aligned districts, before and after the budget vote and across agencies. I use response times of firefighters to 911 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) did not generate controversy and actually increased relative to previous fiscal years. Since firefighters had little reason to organize politically to exert pressure on 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 misaligned 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 of 13.1 minutes prior to the budget vote. This treatment effect does not appear to result from differences in available budgets across police precincts, 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 misaligned politicians by leveraging their influence on voters.

This research makes several contributions. First, this article speaks to scholarship on bureaucrats as interest groups. Extant research has highlighted a variety of ways for bureaucrats to exert political influence, including collective bargaining (Moe, 2011; Paglayan, 2019; Zoorob, 2019), union endorsements (Moe, 2006; Hartney and Flavin, 2011; Hartney, 2022), electoral mobilization (Flavin and Hartney, 2015), political contributions (Moe, 2011; DiSalvo, 2015), or direct lobbying (Anzia, 2022). Yet, scholars risk underestimating bureaucrats' full political power if they primarily considered these formal channels. 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 explicitly engaging in formal political activities.

Second, this study expands the literature on the politics of policing. While recent studies have taken more interest in the political nature of 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, 2023). 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.

Lastly, this research contributes to the scholarship on police effort and "de-policing." A growing literature examines whether police pull back in response to police protests (Shi, 2009; Shjarback et al., 2017; Cheng and Long, 2022; Rivera and Ba, 2023; Roman et al., 2025), legal investigations (Chanin and Sheats, 2018), or unfavorable wage negotiations (Levi, 1977; Mas, 2006). My theoretical claim and analysis differ from this existing work in several ways. First, I examine selective targeting of misaligned politicians rather than broad de-policing. Second, I measure police effort through response times to service calls rather than arrest rates, stops, or crime rates, which more directly captures variation in deliberate effort. Third, I address identification challenges by leveraging within-jurisdiction variation in political alignment rather than before-after comparisons (Shi, 2009; Chanin and Sheats, 2018; Cheng and Long, 2022; Roman et al., 2025). This approach helps separate strategic behavior from concurrent policy or resource changes—precisely the ambiguity that police exploit to shift blame toward policymakers.

2 Shirking for Political Leverage

Bureaucrats occupy a unique position as interest groups within democratic government. Moe (2006) argued that bureaucrats leverage politicians' electoral vulnerability to influence their principals and policy choices. Extant research has documented formal mechanisms of this influence. For example, teacher union endorsements and campaign contributions significantly boost election prospects of candidates in school board elections (Moe, 2006; DiSalvo, 2015; Zoorob, 2019; Hartney, 2022) and public sector unions often gain favorable work rules and compensation through collective bargaining or direct lobbying (Moe, 2011; Anzia, 2022). Yet, one fundamental source of bureaucratic power remains largely overlooked: their ability to selectively shirk everyday responsibilities to shape how voters perceive incumbents and their policies. This mechanism operates at the core of democratic accountability relationships, where bureaucrats serve as the crucial intermediaries between politicians and citizens.

Motivated by re-election incentives, political representatives use public policy to cater to their voters and donors. However, the success of these policies depends not only on the decisions of elected officials but also on how bureaucrats deliver services to voters. This government coproduction makes it difficult for voters to hold politicians accountable. Voters cannot easily determine whether poor service delivery stems from politicians' choices or bureaucratic behavior. This informational asymmetry opens the door for bureaucrats to strategically undermine public service outcomes for political ends.[1] When incumbents enact policies that bureaucrats dislike, bureaucrats may shirk their duties in the constituencies of such misaligned politicians. This could include delaying service provision, overlooking service infractions, or misusing their authority to sabotage the policy goals of their principals (Brehm and Gates, 1997). In doing so, bureaucrats can obtain their main objective (i.e., ensure favorable policy) in one of two ways: by damaging the reputation of unsupportive incumbents and jeopardizing their re-election chances, or by provoking public pressure that forces misaligned politicians to revise contested policies.

This notion of *leverage shirking* differs from the usual understanding of bureaucratic

[1] If voters could perfectly attribute poor service to bureaucrats rather than politicians, such resistance would not be viable in equilibrium. Fully informed voters would either (a) never punish politicians for anti-bureaucratic policies—making bureaucratic sabotage ineffective—or (b) reliably punish them, which would deter politicians from pursuing such policies in the first place.

shirking and agency loss in important ways. Traditional principal-agent models of bureaucracy operate within a dyadic relationship between politicians and bureaucrats as policymakers, thus disregarding the role of voters (e.g., Brehm and Gates (1997); Epstein and O'Halloran (1999); Huber and Shipan (2002)). In these models, shirking arises because bureaucrats have idiosyncratic preferences and abilities to implement the principals' policies (i.e., standard problems of moral hazard and adverse selection), not because it allows bureaucrats to leverage their influence on voters. By instead conceptualizing shirking as a tool to leverage electoral accountability and focusing on bureaucrats as service providers, this framework captures the triadic relationship among politicians, bureaucrats, and voters.[2]

Unlike public sector strikes, which involve the open withholding of labor to demonstrate bureaucrats' indispensability, leverage shirking operates through subtle service manipulation that exploits information asymmetries between voters and politicians. Whereas strikes are visible, easily attributable actions that require significant public support to succeed (Levi, 1977; Hertel-Fernandez et al., 2021), leverage shirking deliberately maintains plausible deniability. Bureaucrats engage in targeted service degradation—selectively delaying responses, applying regulations inconsistently, or performing minimal compliance—while blaming these failures on resource constraints or policy changes. By strategically undermining service quality in ways that voters may attribute to politicians' choices rather than bureaucratic resistance, leverage shirking provides bureaucrats a more flexible, lower-risk strategy to exert influence, particularly when public opinion does not strongly favor their causes.

This is not to say that all groups of bureaucrats act politically or are equally powerful across different political systems. Indeed, research on US federal bureaucrats reveals rela-

[2]While I focus on politically motivated shirking, my claim is not that it is the *only* or the most important source of agency loss in the public sector. Following related work (Forand et al., 2023), I assume other forms of shirking arise from differences in bureaucrats' public service motivation and are largely unrelated to political alignment. Empirically, I address alternative explanations related to morale effects in Section 7.

tively limited militancy across presidential administrations (Brehm and Gates, 1997; Golden, 2000). Several key institutional factors determine bureaucrats' capacity and incentives to engage in leverage shirking.

First, the structure of employment protections and autonomy shapes bureaucrats' incentives to exert political pressure. In patronage systems, where politicians control appointments, promotions, and transfers, bureaucrats' career advancements depend directly on their political principals' support and electoral success. This dependency aligns bureaucrats' incentives with politicians' goals (Ujhelyi, 2014), and minimizes motivation for resistance. Conversely, civil service systems—characterized by competitive examinations and tenure protections—insulate bureaucrats from political control over their careers and job security. Additionally, many public sector unions in the US have managed to negotiate strict work rules in their contracts that increase bureaucrats' discretion in their day-to-day work and make it harder for public employees to be disciplined or let go (Moe, 2011; DiSalvo, 2015; Anzia, 2022). These provisions facilitate resistance when policy disagreements arise.

Second, bureaucrats' ability to effectively leverage shirk requires mechanisms to overcome collective action problems in their strategic behavior. Public sector unions often serve this purpose by pooling resources and establishing shared political goals. Unions create private communication channels through which bureaucrats can align their resistance strategies, such as coordinated "work-to-rule" campaigns.[3]

Third, the mechanism depends on the combination of high service visibility but ambiguous accountability. Bureaucrats can only take advantage of politicians' electoral vulnerability if voters are well aware of the quality of public services (e.g., slower emergency responses, reduced maintenance, inconsistent enforcement) but are unsure who is to blame. Given these conditions, bureaucrats are likely better able to exert political pressure on the sub-national

[3] In principle, the mechanism allows for bureaucrats to shirk *in isolation*. However, individual bureaucrats likely do not have sufficient efficacy in moving voters' beliefs and might have incentives to free-ride on other bureaucrats' shirking.

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, which makes service quality observable to citizens while maintaining attribution ambiguity.

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 (Brehm and Gates, 1997; Forand et al., 2023). Similarly, better public services often facilitate bureaucrats' jobs. For instance, as lower crime rates reduce the need for constant policing, police 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 Leverage Shirking in US Policing

These scope conditions apply well to US municipal police. Police forces have strong policy preferences that often clash with those of their elected superiors. Unlike most unions, police unions have gravitated towards right-wing policies, often resisting criminal justice reforms proposed by reform-minded officials (Zoorob, 2019). Additionally, like other public sector employees, police have strong vested interests in maintaining the material benefits from government work, including large budgets, generous benefits, and operational autonomy (Moe, 2011). The majority of police contracts include provisions that increase officer discretion, create favorable working conditions and make it difficult to hold police officers accountable (Anzia, 2022), all of which creates institutional protections that enable officers to resist policies they oppose without facing immediate career consequences.

Rank-and-file employees of law enforcement agencies are generally well organized in powerful unions. In 2020, 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).[4] Law enforcement and its unions is known for a cohesive "police culture," marked by strong in-group solidarity and a norm of mutual protection, which fosters collective action (Zoorob, 2019).

Police services also satisfy the third scope condition. Citizens are sensitive to changes in public safety, including police response times. Local city council meetings regularly feature discussions about response times.[5] Additionally, local newspapers, community apps like Nextdoor, and local social media groups frequently contain discussions about police presence and responsiveness.[6] Yet, the causes of deteriorating police services remain difficult to attribute. For example, when response times increase, it could reflect resource constraints, increased call volumes, or deliberate slowdowns. Police officers and their unions can leverage this uncertainty to influence voter perceptions and blame policymakers for service infractions.

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 face a unique kind of militancy from police unions (Blumgart, 2020). Besides lobbying or litigating, 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 budget cuts, police forces employed billboards

[4]Four states (Georgia, North Carolina, South Carolina, and Tennessee) forbid police collective bargaining, while in four others (Alabama, Colorado, Mississippi, and Wyoming), the legality of collective bargaining depends on local laws (Sanes and Schmitt, 2014).

[5]For example, 889 (2.3%) of 37,936 council meetings in the 2020 and 2021 LocalView data (Barari and Simko, 2023) specifically discuss "response time," with these discussions frequently linked to citizens' public safety concerns.

[6]For instance, rising response times in NYC became a point of discussion in newspaper articles and social media following the George Floyd protests (Gross, 2020).

reading, “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).

While strikes are generally illegal across US states (Sanes and Schmitt, 2014), anecdotal evidence also suggests that police officers use targeted work slowdowns for political ends. 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. [...] 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 suggest that officers believe this behavior occurs for various reasons, including new laws or civil litigation (Nix et al., 2018), though quantitative evidence is mixed. Some studies find declines in the number of stops or arrests after protests or investigations (Shi, 2009; Shjarback et al., 2017; Roman et al., 2025), while others find little evidence at an aggregate level (Chanin and Sheats, 2018). These mixed findings likely stem from methodological challenges and differences in scope. Citywide analyses might mask targeted strategic behavior, where officers selectively reduce effort in specific areas rather than uniformly across jurisdictions (Shi, 2009; Cheng and Long, 2022; Chalfin et al., 2024). Additionally, studies relying on arrest rates or stops as outcome measures face difficulties isolating police effort since these metrics are further downstream in the policing process and influenced by multiple factors beyond officer effort alone. Further, before-after designs and cross-city comparisons often struggle to account for crucial variation in institutional conditions, e.g., union strength and political autonomy, and lack a valid counterfactual. Major events, such as protests or investigations that affect the whole department, often coincide with other policy changes that influence police behavior (Rivera and Ba, 2023). 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 are thus inherently limited in capturing bureaucrats' strategic behavior. By examining spatially targeted resistance through response times within a single jurisdiction with variation in political alignment, this study offers a more nuanced perspective on when and why police might strategically reduce their effort for political ends.

4 Empirical Case, Data, and Research Design

4.1 NYPD's 2021 Budget Cut

For the empirical analysis, I focus on NYPD officer behavior following the significant FY2021 budget cuts. On June 30, 2020, the New York City Council approved a new budget 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. To address demands for police reform, council members cut overtime pay by 67%, eliminated the July 2020 police academy class of roughly 1,160 officers, canceled hiring plans for traffic enforcement agents and civilian positions, and shifted several responsibilities from the police department to other city agencies (including school safety and monitoring of illegal vending) (City of New York, 2020).[7] Yet, since the latter component was not officially part of the FY2021 adopted budget, the final cut amounted to $415 million, mainly from reducing overtime ($328 million) (Citizens Budget Commission, 2020).

[7]See Figure A4 for more details.

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 police budget and the hefty reduction in police funding became the decisive feature of council members' voting behavior. The final vote on the budget was unusually divided, with 32 council members in favor and 17 members voting against the police funding cuts. 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. 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. The Police Benevolent Association (PBA), the NYPD's largest police union, promptly voiced dissent against the proposal, threatening that

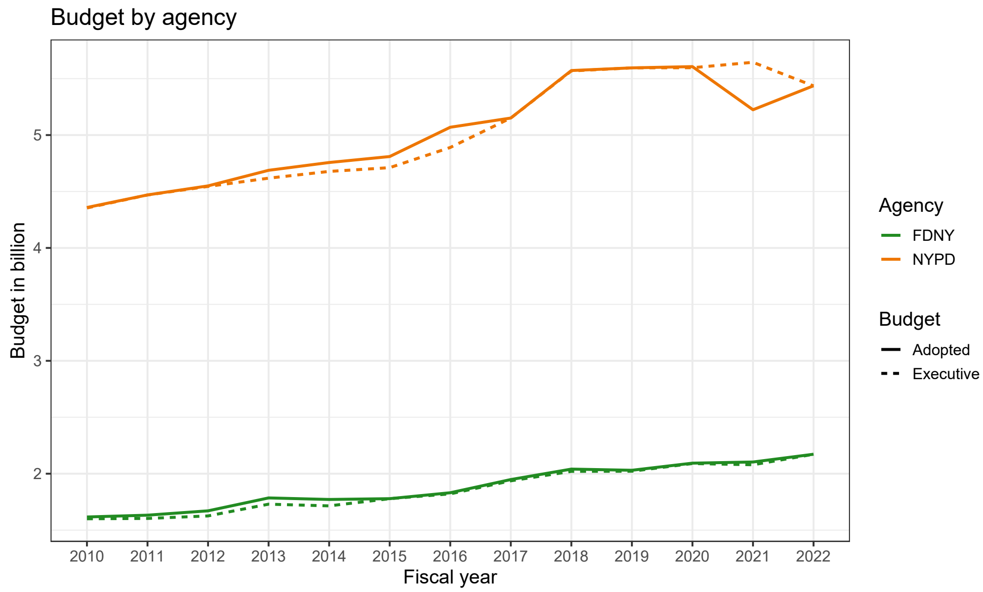
> "For decades, every time a city agency failed at its task, the city's answer was to take the job away and give it to the NYPD. If the City Council wants to give responsibility back to those failing agencies, that's their choice. But they will bear the blame for every victim, for every New Yorker in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

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

[8] PBA President Patrick Lynch on Twitter, June 12, 2020.

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.

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, e.g., 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, while conventional metrics of depolicing, including the number of arrests or searches (Chanin and Sheats, 2018; Shjarback et al., 2017), are heavily influenced by factors like criminal behavior or departmental policies, response times more directly reflect officers' decisions about prioritization and resource allocation. Third, and most importantly, officers' response times to calls correlate with public perceptions of policing quality. Using 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). I geolocate each call using coordinates to assign it to its corresponding political district.

4.3 Council Members' Voting Behavior

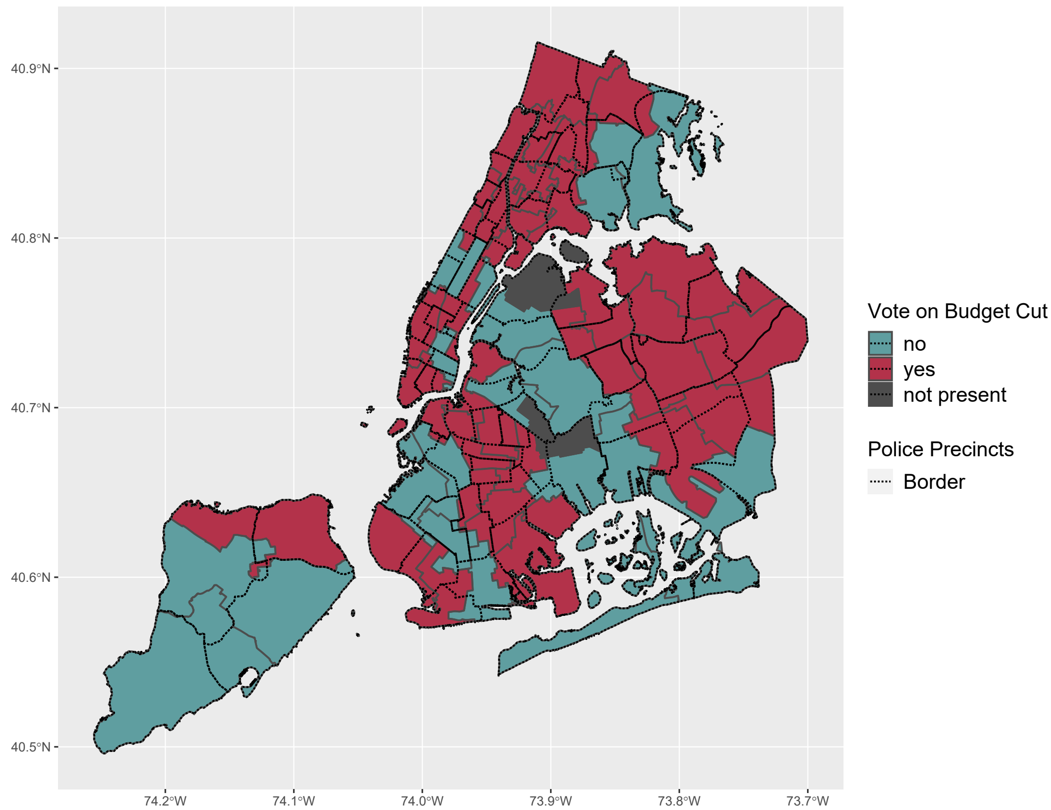
Figure 2 shows the distribution of council members' voting behaviors on the budget proposal across NYC's 51 council districts.[9] The map illustrates that both "yes" and "no" votes are distributed across the city, and districts with opposite voting patterns share a border in several instances. Additionally, these district borders cut across NYPD precinct boundaries, which allows me to analyze differences in response times across and within police management units in my empirical design.

To provide some information on possible factors influencing a council member's voting behavior, Table A3 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 A5 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.

[9] 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 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 misaligned 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 for fires and medical emergencies as my third control dimension.[10] Firefighters are largely comparable to police 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 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 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.[11] 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

[10] 86% (14%) of FDNY calls are medical emergencies (fire incidents and utility emergencies) in my sample.

[11] For the main analysis, I remove calls between May 30 - June 15, when large protests took place in NYC across several locations in response to George Floyd's killing. Consequently, response times were on average almost three minutes (22%) longer than in previous months. Table A5 presents estimation results including these strong outliers, showing that the main results largely hold with the full sample of calls.

account for differences in district characteristics (see Table A3). To the extent that these characteristics and their influence on NYPD response times stay constant across my sample period, my treatment effect estimates remain unbiased. Finally, date fixed effects capture the high variability of response times across time. I cluster standard errors ε_{icpda} on the district level.

Figure 3: Visual Representation of DiD Identification, Hypothetical

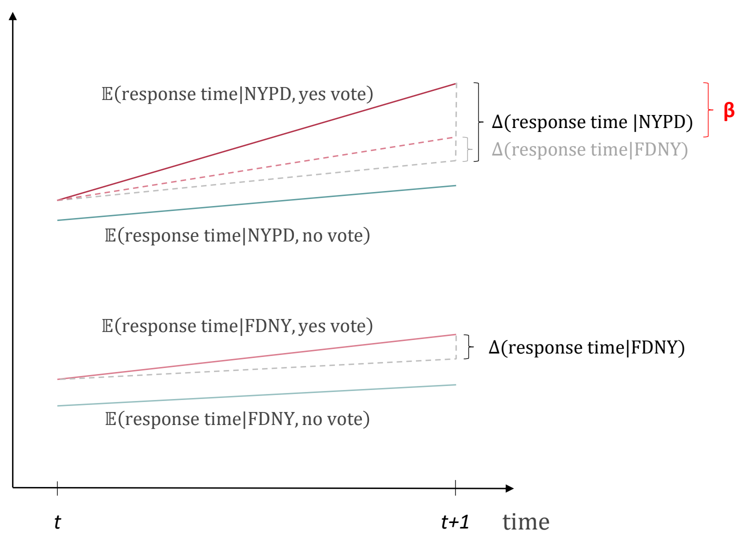


Figure 3 provides a graphical representation of the triple DiD identification strategy. While a simple DiD design would only rely on the divergent trends in NYPD response times within yes-voting districts vis-à-vis no-voting districts over time (i.e. Δ(response time|NYPD)), the triple DiD design incorporates 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 design is that *differences* in response times between NYPD and FDNY officers across treatment and control districts would have followed similar trends in the counterfactual absence of the budget vote. This assumption is bolstered by the fact that the FDNY was unaffected by the budget cut, but the two agencies operate within the same emergency response dispatch system, are governed by similar municipal oversight, serve identical geographic areas, and are similarly exposed to the same external shocks (e.g., construction, traffic patterns).[12]

5 Misalignment Increases Response Times

Figure 4 depicts the raw trends in average 911 response times across different types of districts over time and for different agencies.[13] 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 misaligned council districts and relative to FDNY calls. The figure also highlights time trends in response times (e.g., due to COVID19 waves), which my within-jurisdiction design accounts for.[14]

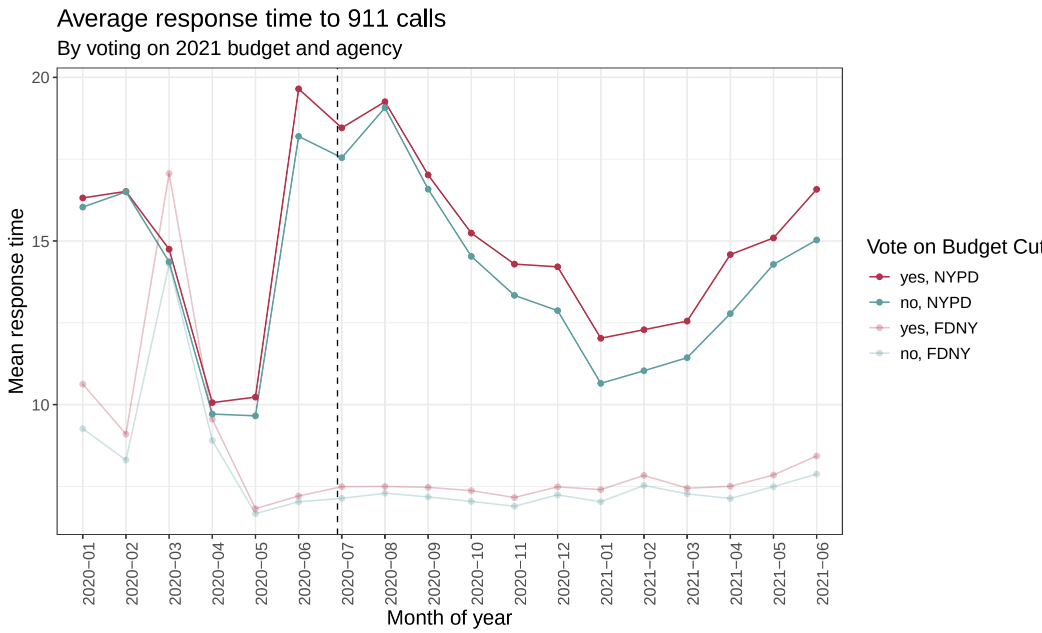
Table 1 evaluates trends in police 911 response times using the triple DiD model in Equation (1). We find that NYPD on average took about 5 minutes longer in their response times in aligned districts than FDNY before the budget vote (NYPD). After the vote, response times went up by about 2.3 minutes in aligned districts (after vote × NYPD). Most importantly, in line with the theory, the triple DiD estimate is positive, suggesting that response times in misaligned 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 meaningful. Similarly, an additional minute in police response times is large enough to

[12]I also estimate simple DiD models, separately for the NYPD and FDNY. Reassuringly, the results in Table A6 indicate that there is a positive ATT estimate for NYPD 911 calls, while the estimate for FDNY is smaller, negative and statistically insignificant.

[13]Figure A6 shows the difference in response times across districts instead of levels.

[14]Given the unusually high response times for the FDNY in March 2020 due to the COVID outbreak, I additionally estimate results excluding March 2020 in Table A7. Reassuringly, while the estimated treatment effects are smaller, the qualitative results remain unchanged.

Figure 4: Trends in 911 Response Time across Districts



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.

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

$$\text{response time}_{icpda} = \sum_{\tau \in [-6,11]} \beta_\tau \text{yes vote}_c \times \text{NYPD}_a + \delta_c + \eta_p + \gamma_d + \nu_a + \varepsilon_{icpda} \qquad (2)$$

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).

Figure A7 shows the respective treatment effects by month. While the estimates are imprecise, the overall ATT estimate does not seem to be driven by a specific month or immediately post-treatment. A few factors could explain this temporal diffusion of increased response times. First, some of the policy changes only materialized a few months into fiscal year 2021. For example, the budget forced cancelling the police academy class and the cadet program for July 2020, reducing the prospective number of officers by 1,381. The implications of this reduction in NYPD's headcount only unfolded once the new recruits would have been sworn in as NYPD officers six months later. Additionally, the issue of "defunding the police" gained salience as the 2020 federal elections and the 2021 NYC elections approached. In terms of pre-treatment trends, 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, and many council members published their vote intentions then.[15]

Given these challenges to identification, particularly the validity of the parallel trends across treated and control districts in the entire city, I employ a supplementary spatial difference-in-discontinuities design in Appendix D, where I compare response times in similar neighborhoods around the council district borders before and after the budget vote. Reassuringly, in line with the DiD results, the model suggests that for calls in close proximity to the district borders, the NYPD slowed down by an average of 1.1 minutes per call in yes-voting districts relative to no-voting districts after the budget cut.

6 Mechanisms and Additional Results

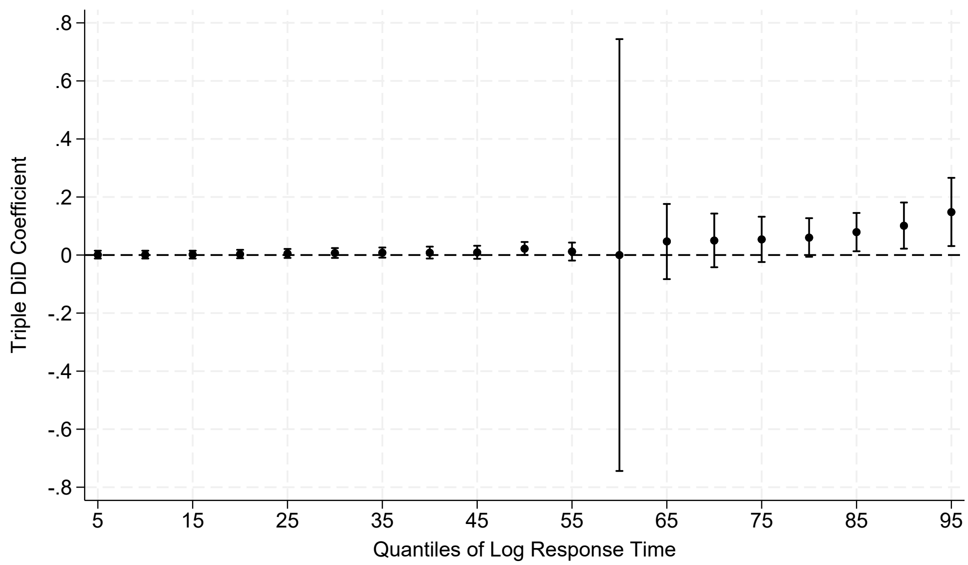
What type of calls do officers use for leverage shirking? If political motivation drives delays, increases in response times in misaligned districts are likely a result of late arrivals and "no shows" of officers to calls where police have sufficient discretion in how they address the incident and face fewer costs for shirking. Figure A8 indicates that these discretionary calls, such as crimes not in progress, disputes and vehicle accidents, take longer at baseline. To evaluate this mechanism, Figure 5 depicts quantile treatment effects.[16] The estimated treat-

[15] `https://council.nyc.gov/press/2020/06/12/1983/`; `https://docs.google.com/spreadsheets/d/1DAan2yEhaO8Mt9VmADAxNbCwhX8usfsSL51Pw9m4Fh0/edit#gid=2032235041`.

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

ment effects are largest at the upper end of the response time distribution, while remaining small and insignificant for other calls. Misaligned 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).[17]

Figure 5: Quantile Treatment Effects



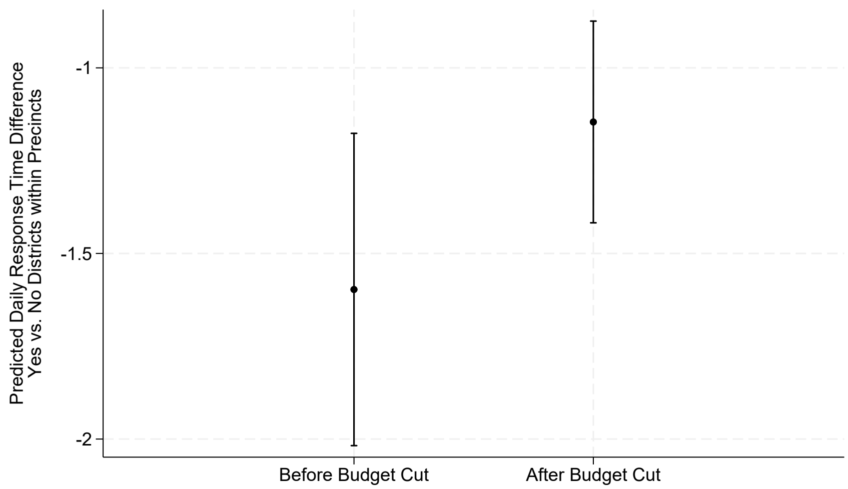
Note: Depicted are quantile treatment effects, estimated using recentered influence functions with 90% confidence intervals.

The DiD results so far leverage variation in police effort both across and within precincts. While this is commensurate with the theory, a more stringent test compares police behavior *within the same precincts*. Resource allocation and dispatch decisions fall primarily under precinct commanders' discretion, and police officers have been shown to be highly responsive to managerial directives (Mummolo, 2018). Fortunately, several NYPD precincts (60 out of 77) span city council districts with contrasting voting patterns. I analyze daily differences in average response times between treated and control areas of the same precincts over time. As Figure 6 indicates, following the budget vote, response times disproportionately increased

[17]To alleviate concerns that these effects are driven by a few outliers in response times, Table A8 shows the robustness of my DiD results using winsorized response times.

in misaligned areas compared to aligned areas of the same precincts. Prior to the vote, the NYPD responded on average 1.6 minutes faster in misaligned areas compared to aligned areas within the same precincts. After the vote, this advantage for misaligned areas decreased by 31%, with response times now only 1.1 minutes faster in misaligned areas.[18] While these results further bolster the theory of leverage shirking, I cannot distinguish between two potential mechanisms: either fewer officers were dispatched to misaligned areas, or officers deployed to these areas exerted less effort when responding to calls.[19]

Figure 6: Response Time Differentials within Precincts



Note: Depicted are predicted daily response time differences within precinct based on Column 2 of Table A4, with 90% confidence intervals.

My argument also highlights the role of police unions as a crucial bureaucratic actor in

[18]The treatment effect estimates are also geographically heterogeneous across districts. Figure A9 shows that response time increases were particularly large in precincts in the Bronx, especially in precinct 43, where pro-reform council member Rafael Salamanca (district 17) clashed with council member Ruben Dìaz (district 18), a staunch support of the NYPD who called himself "the opposite of AOC" and was endorsed by the PBA.

[19]Unfortunately, a direct test of the dispatch mechanism is impossible since dispatch information is unavailable for the NYPD.

targeting misaligned officials. There is ample qualitative evidence supporting this notion. In the months following the budget vote, NYPD's police unions engaged in various smear campaigns against council members who had supported the budget cut. A common tactic was to leverage crime incidents in their districts and connect these to council members' support of the budget on social media.[20] 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 of his constituents, he cares about bowing to a hashtag!"[21] What is more, as concerns about rising response times arose in the public discourse, police unions attributed the blame to the city council and the mayor.[22] 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.[23]

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[24], 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"[25] or "keep voting Democrat and you'll have war zones. just ask

[20] https://twitter.com/NYCPBA/status/1288122515898822657.

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

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

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

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

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

Chicago, Detroit, Baltimore!"[26] In sum, this qualitative evidence substantiates how police unions targeted misaligned politicians in the wake of the budget cut, leveraged their influence on voters in their political messages, and intended to affect the 2021 NYC elections.

How did citizens react to the strategic shirking of police officers? While systematic data on citizens' complaints about NYPD behavior is unavailable, 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.[27] 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. Changes in citizens' interactions with police post-policy could alter the data-generating process, raising sample selection problems and phantom counterfactuals (Slough, 2023). Specifically, if misaligned districts had differing call behaviors, such as calling only for minor

[26] `https://twitter.com/SBANYPD_Archive/status/1277424114249146374`.

[27] I use crime concerns as a proxy for overall public safety perceptions because direct questions about non-crime emergency services are limited in surveys across time and districts. While the shirking I document primarily affects non-crime calls, increased response times likely contribute to broader feelings of insecurity that manifest as crime concerns in surveys.

incidents or if crime rates evolved differently in these areas, this might explain increased response times. Yet, my data suggests that this is unlikely. 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 A10; Table A9). Call-type distributions across treatment and control districts also remain largely unchanged (Figure A11), and there's minimal evidence of differential crime incidents and citizens' reporting of them (Table A10). Similarly, Table A11 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.

Relatedly, disproportionately rising response times in treated districts could indicate a redistribution of policing priorities from emergency responses to fighting crime, which may even be electorally beneficial for misaligned incumbents. This is unlikely to be the case. Such re-prioritization would suggest a relative reduction of response times in treated districts for high-priority crime calls, which Figure 5 does not support. Additionally, there is no evidence that crime clearance rates improved in treated compared to control district following the budget cut (see Table A12). These findings suggest that police resources were not strategically redirected toward crime-fighting activities in these areas.

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 disproportionately dropped in precincts covering larger shares of yes-voting districts.[28] To test this, I matched NYPD officers in 2021 and 2020 to their precincts based on assignment data for both active and inactive personnel. Figure A12 illustrates the relationship between changes in resources—measured by staff count, overtime hours, and overtime expenses—and the proportion of calls from "yes" districts by precinct.

[28]The within-precinct DiD and the spatial RDD design already account for potential precinct-specific trends.

There is 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 officers' motivation to retaliate against public criticism. To assuage these concerns, I geocoded all 1,989 protests that occurred between January 2020 and June 2021 in NYC.[29] While Figure A13 shows some clustering in misaligned districts (particularly outside Manhattan), protests occurred citywide. Table A13 confirms that DiD estimates remain robust when controlling for the daily number of anti-police protests, indicating that protest patterns do not explain the observed differences in 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 A14 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 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 shirking in misaligned areas. First, police forces in yes-voting districts might have lower morale after the budget cut, which could drive down their incentives to improve 911 response times. Yet, in my within-jurisdiction design, this alternative explanation would require *differential* changes in moral across districts and even within precincts. Second, offi-

[29]The raw data comes from the Crowd Counting Consortium Dataset (`https://github.com/nonviolent-action-lab/crowd-counting-consortium`).

cers in misaligned 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., 2025). 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 response, they can log calls themselves. These officer-initiated calls are characterized by response times close to zero in my data. As Table A15 indicates, I find no evidence that officer-initiated calls decreased disproportionately in misaligned districts, thus alleviating concerns that morale or avoidance effects drive the results.[30]

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 shirking their duties in certain areas, bureaucratic agents can protest unwanted policy choices and exert pressure on political authorities. Bureaucrats' willingness and capacity to exercise such political power largely depend on the degree of preference misalignment with their political principals, their political autonomy and tenure protections as well as plausible deniability of responsibility for observable poor service provision. Focusing on municipal police and using NYPD 911 response times as a

[30] 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 A16.

case study, I find empirical evidence that largely supports this view.

By raising questions about who is controlling whom in politician-bureaucrat relationships, this study has important implications for our understanding of electoral accountability and principal-agent dependencies between elected authorities and their bureaucratic agents. I incorporate voters into the principal-agent framework to illustrate how bureaucrats can exploit politicians' accountability and voter uncertainty for political ends. To the best of my knowledge, this is the first study to examine bureaucrats' instrumental shirking for voter leverage and to analyze how preference misalignment of bureaucrats and politicians mobilizes bureaucrats' latent political power.

Additionally, this research provides new insights into issues of political representation and the role of bureaucrats as interest groups within government. Particularly, the findings of this study imply that bureaucrats can wield political power and affect voter welfare (e.g., waiting times for public services) independently of their more visible impact on electoral outcomes through campaign contributions, voter turnout, or lobbying.

Lastly, this study informs debates on police accountability and reform. Strong autonomy of local law enforcement allows the police to flex their muscle vis-à-vis misaligned elected superiors to push back against unwanted police reforms. If well-organized officers manage to exert sufficient pressure on reform-oriented incumbents through work slowdowns, lobbying activities, or recall campaigns, meaningful police reform may remain elusive—despite public support for such measures.

While the study focuses on a single city employing the largest US police force, similar dynamics likely apply in several other US cities. Appendix A.1 replicates the analysis with the contentious budget cut in Minneapolis in December 2020 and finds results consistent with the theory. Arrests disproportionately decreased and response times increased in the six districts that voted for police funding and staffing reductions compared to the seven districts that did not. 45% of each state's largest cities reduced the share of their police budget for fiscal year 2021, with reductions as high as 12.1% in Albuquerque and 9.7% in

Seattle (see Figure A3). Given the strong police unions in these major cities and their open opposition to these budgetary changes, the bureaucratic resistance and service reduction shown in this study likely reflect broader trends across US jurisdictions.

Several important questions remain beyond the scope of this study. First, the conditions discussed in Section 2—political autonomy, unionization, and service quality that is observable yet imperfectly attributable—are fixed in my empirical case. Future work should use variation in these factors to directly examine the necessary conditions for leverage shirking. For example, scholars could exploit localized changes in bureaucratic unionization through union certification elections to systematically compare the political strategies employed by unionized versus non-unionized bureaucrats.

Second, the voter side of the theory remains an important avenue for future research. How does leverage shirking affect voters' perceptions of political incumbents and their policies? How does information about service infractions spread across communities? What are the electoral consequences of bureaucratic power? Are these effects heterogeneous across voter groups, for instance, depending on their trust in police or preexisting views on the necessity of police reform? While sections E and F offer suggestive evidence on how voters might have responded to the NYPD's leverage shirking, experimental studies could provide more definitive answers.

Third, 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 losses might be offset by benefits for individuals subject to police interventions. If work slowdowns are clustered in overpoliced areas, the net impact of police shirking might not be negative overall.

Finally, the broader applicability of my argument across different policies and types of bureaucracies warrants further examination. Based on the scope conditions identified in this study, we might expect similar patterns of bureaucratic resistance in other politically autonomous and well-organized local bureaucracies. For instance, future research could investigate how progressive teachers and their unions respond to restrictive policies from local school boards—such as book bans or educational gag orders—or whether housing bureaucracies strategically delay building permits under certain circumstances.

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Appendix: Supporting Information for

Political Power of Bureaucratic Agents

Table of Contents

A Evidence Beyond NYC and Policing

A.1 Minneapolis: Budget Cut and Police Shirking

To evaluate the broader applicability of my theory, I replicate my main analysis using a similar cut to the police budget in December 2020 in Minneapolis. Following George Floyd's murder, the city council decided to slash \$8 million (7.5%) from the Minneapolis Police Department (MPD). There was considerable controversy in the council about plans to hire more officers in future years. The City Council had initially planned to drop the force's authorized size to 750 officers starting in 2022, but reversed course by a narrow 7-6 vote to maintain the staffing level at 888.

To measure police effort, I rely on the number of officer-initiated stops made by MPD by day-district-precinct and response times for all 911 calls in 2020-2021.[1] Using a DiD strategy, I compare police behavior on day d and precinct p between misaligned and aligned districts c, i.e., districts where council members supported both the cut to staffing and funding (i.e., misaligned) and those where council members voted against the cut in staffing levels (i.e.,

[1] Stops data is taken from `https://opendata.minneapolismn.gov/`; response time data was obtained through an open records request (DR25_001925).

aligned):

$$\log(\text{stops} + 1)_{cdp} = \alpha_c + \beta \text{misaligned}_c \times \text{after vote}_d + \gamma_d + \delta_p + \varepsilon_{cdp} \tag{3}$$

$$\text{response time}_{icdp} = \alpha_c + \beta \text{misaligned}_c \times \text{after vote}_d + \gamma_d + \delta_p + \varepsilon_{icdp} \tag{4}$$

The limited number of council districts (6 treated and 7 control districts) make inference challenging. Simple power analyses illustrate this. Without accounting for the clustered data structure, the minimal detectable effects with 80% power are 3% for the number of stops (0.05 standard deviations) and 1.35 minutes (0.02 standard deviations). However, accurately accounting for clustering in the data and treatment assignment significantly reduces power and increases the minimal detectable DiD effects substantially: 40% for the number of stops (0.64 standard deviations) and 11.4 minutes (0.11 standard deviations) for response times on average. This means I may be unable to detect more subtle but still policy-relevant effects that fall below these thresholds.[2] Given these power concerns, inferences need to be treated with caution in this analysis. I focus on the sign and size of the estimated DiD estimates and present robust standard errors without clustering as well as wild cluster bootstrap p-values following Roodman et al. (2019).

Overall, the estimates support the theory of leverage shirking and bolster the findings of the NYC case. As Table A1 shows, MPD officers reduced the number of stops by an additional 4% in misaligned districts compared to districts aligned with their preferences following the budget vote. The effect size is very similar when accounting for anticipation effects (i.e., excluding all days between George Floyd's murder and the budget vote on December 10, 2020) or excluding all arrests in the Powderhorn Park neighborhood where George Floyd died. Additionally, the effect seems largely driven by the intensive margin: Officers reduced the number of stops, conditional on making some stops in the district and precinct per day, by an additional 11% in treated districts relative to control districts. There is little evidence that they disproportionately reduced the probability of making *any* stop in the area. Conversely, the results in Table A2 suggest that response times disproportionately increased in misaligned districts after the budget vote by about 2 minutes. Again, the effect size is robust to excluding months right before the vote but after George Floyd's death and differentiating civilian-initiated from police-initiated calls.[3]

[2] I calculate the minimal detectable effect with 80% power as $(z_{1-alpha/2} + z_{1-beta}) \times \sqrt{V(\beta_{DiD})}$. I incorporate clustering in the variance of the DiD estimator: $V(\beta_{DiD}) = \sigma^2 \times \left(\frac{DE_{11}}{N_{11}} + \frac{DE_{10}}{N_{10}} + \frac{DE_{01}}{N_{01}} + \frac{DE_{00}}{N_{00}}\right)$, where $DE_{ij} = 1 + (m_{ij} - 1) \times ICC$ is the design effect adjustment using the intra-cluster correlation and average number of observations per cluster for each period-alignment-cell m_{ij} and N_{ij} are the number of observations in each cell.

[3] Figures A1 and A2 show event study estimates to evaluate pretreatment trends and the timing of treatment effects.

Table A1: Treatment Effect Estimates for Number of Stops

	Base (1)	Restricted (2)	Excluding Powderhorn (3)	Intensive Margin (4)	Extensive Margin (5)
misalignment × after vote	-0.0406	-0.0511	-0.0395	-0.1093	-0.0002
	(0.0164)	(0.0264)	(0.0163)	(0.0217)	(0.0127)
Date FE	✓	✓	✓	✓	✓
District FE	✓	✓	✓	✓	✓
Police Precinct FE	✓	✓	✓	✓	✓
Wild cluster bootstrap p-value	0.6550	0.7190	0.8790	0.5160	0.9940
Observations	20,468	15,008	20,468	7,351	20,468
Mean of DV	0.415	0.433	0.409	1.154	0.359
R^2	0.23489	0.24932	0.23755	0.42702	0.14373

Heteroskedasticity-robust standard-errors in parentheses.

Table A2: Treatment Effect Estimates for Response Times

	Base (1)	Severity (2)	Civilian initiated (3)	Restricted (4)
misalignment × after vote	1.789	1.959	2.072	1.982
	(0.4813)	(0.4797)	(0.6347)	(0.4978)
Date FE	✓	✓	✓	✓
District FE	✓	✓	✓	✓
Police Precinct FE	✓	✓	✓	✓
Call Importance FE		✓		
Wild cluster bootstrap p-value	0.2000	0.2360	0.1700	0.2940
Observations	1,144,584	1,144,584	817,709	953,466
Mean of DV	21.119	21.119	27.440	21.119
R^2	0.01348	0.02256	0.01212	0.01061

Heteroskedasticity-robust standard-errors in parentheses.

In addition to this quantitative evidence, there is ample qualitative evidence that the MPD targeted misaligned council members in the wake of the defund movement in Minneapolis. Following the protests, many residents began reporting delayed or nonexistent responses to emergency calls, prompting City Council members to publicly question whether police were engaging in deliberate slowdowns (Winter, 2020). Police Chief Medaria Arradondo attributed these delays to staffing shortages from officer departures and increased violent incidents that required longer officer engagement. However, some council members suspected their districts were specifically targeted because of their support for police defunding or restructuring initiatives. Council member Phillipe Cunningham, a fierce supporter of police reforms, reported that numerous constituents in his district received no police response to gunfire reports. When residents inquired about these delays, they were allegedly directed to contact Cunningham himself. Cunningham argued that police painted him as the villain: "It's my fault that they are not responding in a timely manner or at all" (Winter, 2020).

Figure A1: Monthly Effects for Number of Stops

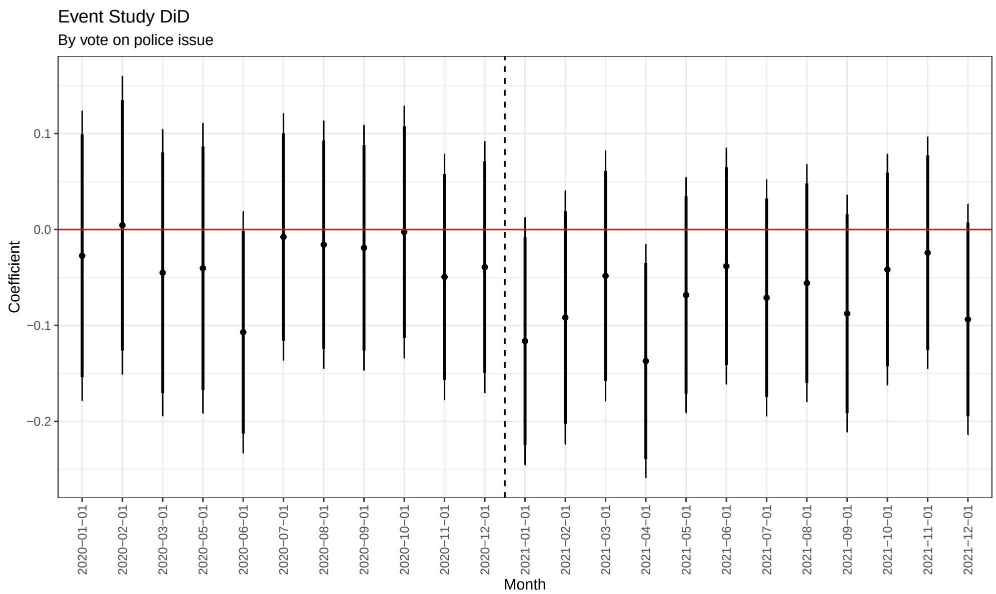
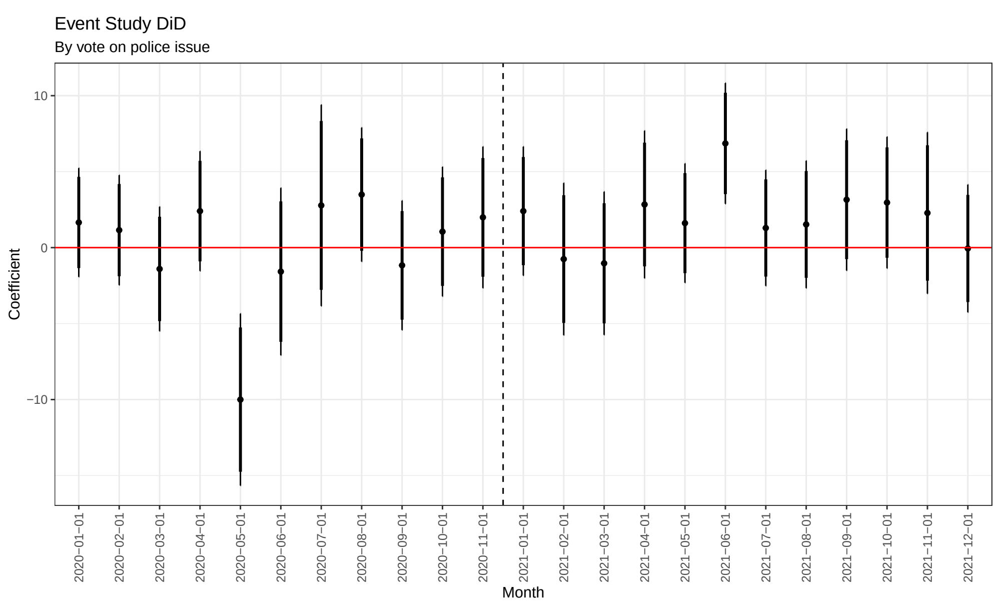


Figure A2: Monthly Effects for Response Times



A.2 Other Bureaucracies

While a test of the theory of leverage shirking beyond police is beyond the scope of this paper and should be carefully evaluated in future research, the theoretical scope conditions of the argument travel to other street-level bureaucracies where service quality is observable but difficult to attribute to the bureaucrats or political decisions. For example, municipal water utility workers could adjust repair schedules based on council members' support for utility privatization; bus drivers and transit workers might leverage their direct interaction with passengers to shape perceptions of service cuts or route changes; and planning department staff could delay permitting to resist budget policies. Several case studies demonstrate how bureaucrats beyond law enforcement have strategically manipulated service provision to exert electoral pressure on elected officials.

A.2.1 Fire fighters in Washington, DC

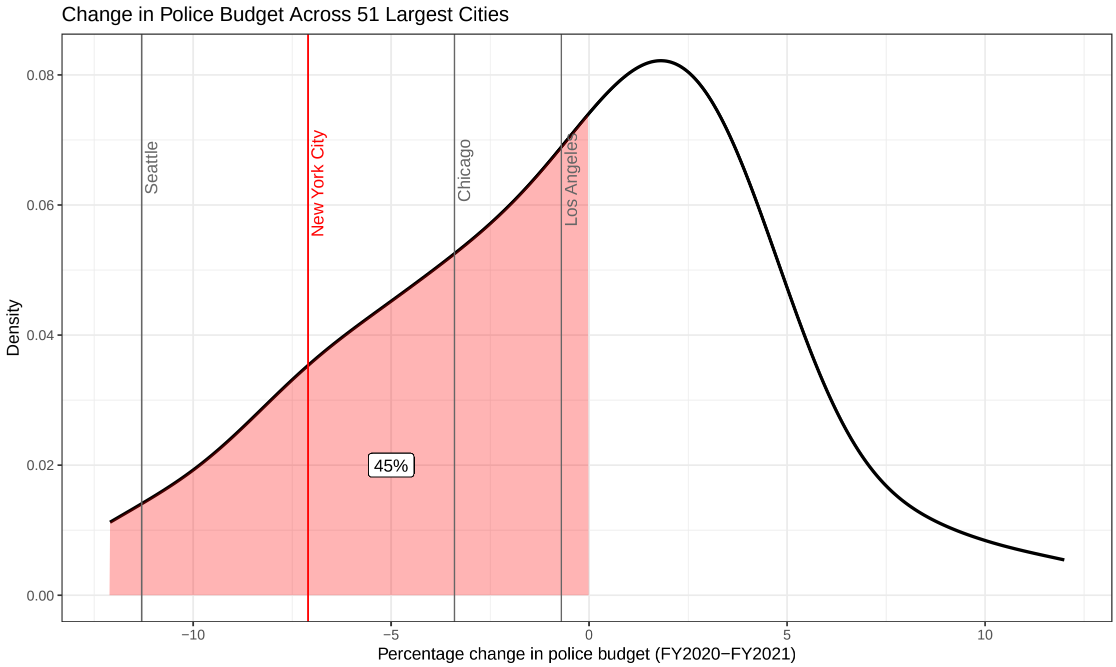
In November 2011, Fire Chief Kenneth Ellerbe proposed replacing the traditional scheduling system of 24-hour shifts followed by three days off with a "3-3-3" model—comprising three 12-hour day shifts, three 12-hour night shifts, and three days off. The union, Local 36 of the International Association of Fire Fighters (IAFF), opposed this plan, citing concerns about increased fatigue and potential negative impacts on family life. As tensions escalated between the union and Fire Chief over policy differences, firefighters organized coordinated sick-outs in 2013. While typical weekly sick calls averaged between 20-30 firefighters, on August 18 alone, 83 firefighters reported illness (Hermann, 2013). The absences caused the department to require mandatory overtime of 67 firefighters, requiring them to work for 36 consecutive hours. DCFEMS officials called the illnesses suspicious, while the union said the illnesses showed the department had too few firefighters to cover the schedule due to Ellerbe's policies (NBC, 2013).

A.2.2 Sanitation workers in Staten Island

In 2010, sanitation workers in NYC repeatedly clashed with Mayor Bloomberg over budget cuts and cost-saving measures. Following a snow blizzard in December 2010, sanitation workers dragged their feet in snow removal efforts around the city to create a policy failure for Bloomberg. In Staten Island, for example, residents complained to representatives about abandoned or stuck plows and salt spreaders (Staten Island Live, 2010). In conversations with City Councilman Dan Halloran, sanitation workers revealed that "they were told [by supervisors] to take off routes [and] not do the plowing of some of the major arteries in a timely manner. They were told to make the mayor pay for the layoffs, the reductions in rank for the supervisors, shrinking the rolls of the rank-and-file." (Goldenberg, 2010). While the Bloomberg administration blamed residents for shoveling snow into streets that had already been plowed and for tying up 911 with non-emergency calls, the mayor soon became the public face of the failed handling of the storm.

B Background on Case: NYPD 2021 Budget Cut

Figure A3: Distribution of Police Budget Cuts Across Major US Cities in 2020



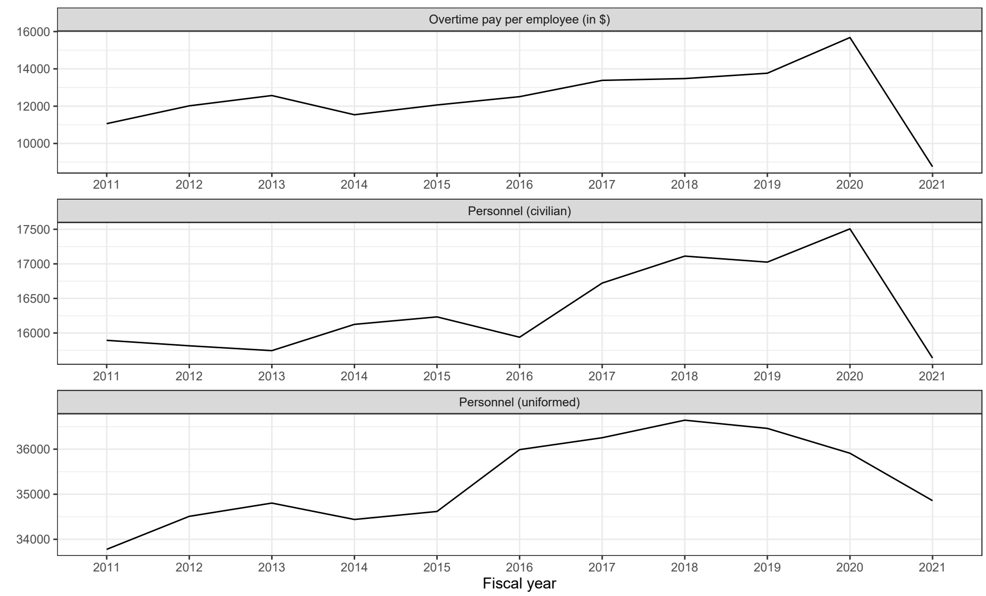
Note: The figure depicts changes in police budgets across all US state's largest cities, between fiscal years 2020 and 2021 (in percentages). Source: `https://www.smartcitiesdive.com/news/calls-to-defund-the-police-are-upending-fy21-budgets-heres-how/581163/`

Table A3: Summary Statistics - Covariates by Voting Behavior

	Vote on Budget Cut		difference	
	yes	no		
	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	

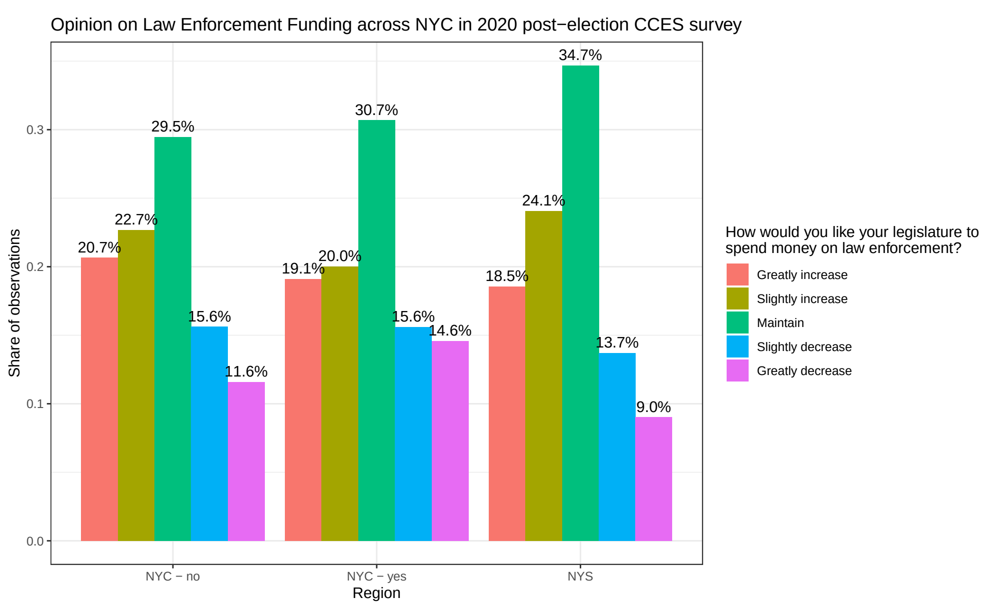
Sources: [a] Official Electoral Results, [b] Census Demographics, [c] Crowd Counting Consortium, [d] NYPD Complaint Data, [e] NYPD Shooting Incident Data.

Figure A4: Development of Personnel at NYPD



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

Figure A5: 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 Results and Robustness

Figure A6: Weekly Average Differences between Districts by Agency

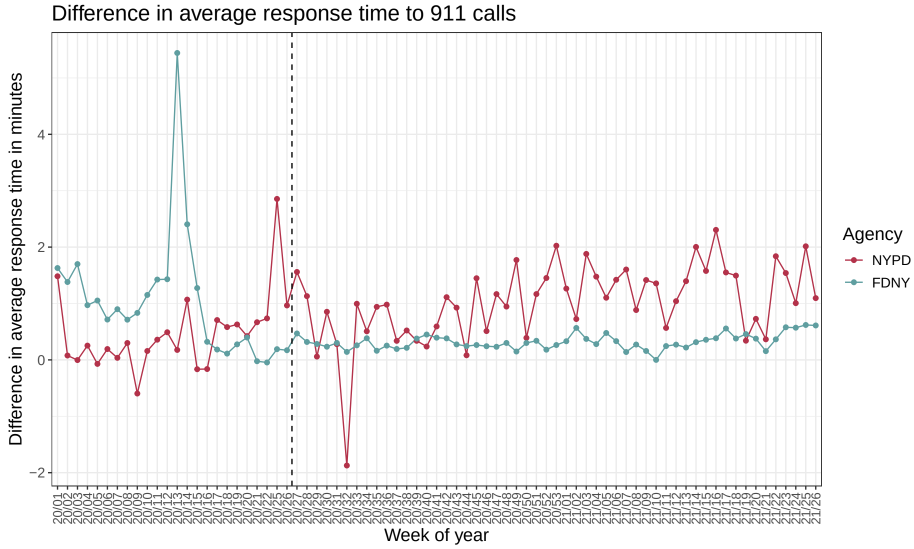
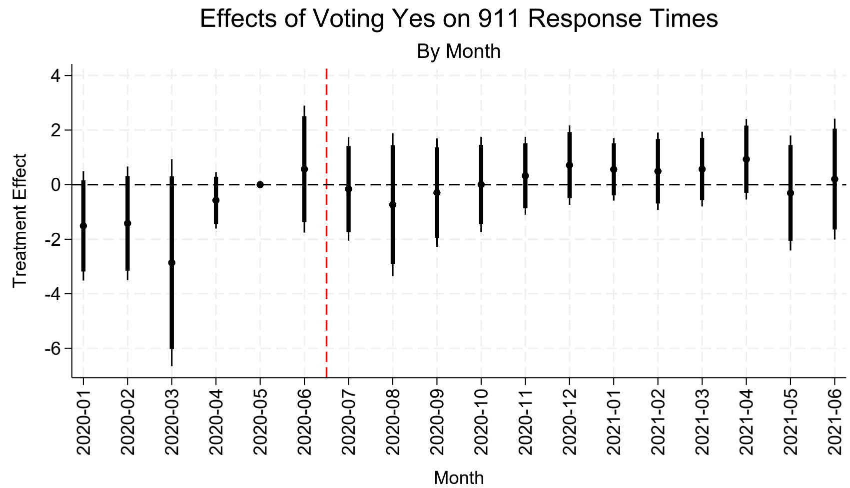


Table A4: Response Time Differentials within Precincts

	(1) NYPD & FDNY	(2) NYPD only
after vote × NYPD	0.427	
	(0.269)	
after vote	0.008	0.451*
	(0.086)	(0.254)
NYPD	-1.423***	
	(0.220)	
Police Precinct FE	✓	✓
Observations	41252	22077
Mean of DV	-0.578	-1.287
Adj. R^2	0.117	0.165

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Dependent variable: Daily differentials in average response times between misaligned and aligned areas within precincts. Heteroskedasticity-robust standard errors in parentheses.

Figure A7: Monthly Treatment Effects



Note: Depicted are month-specific treatment effects, based on estimations of Equation (2) with 90% and 95% confidence intervals.

Figure A8: Call Length by Call Type

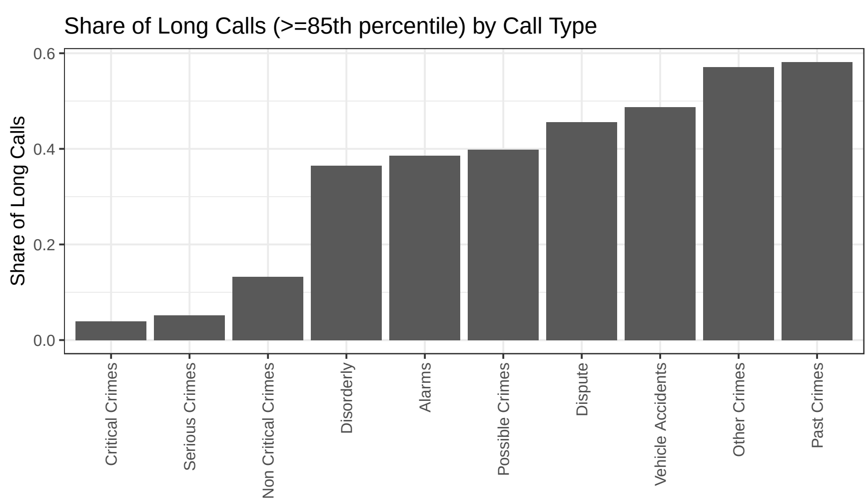


Table A5: Effect of Approving 2021 Budget on 911 Response Times, Including May 30 - June 15

	(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 A6: 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 A7: 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 A8: 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 A9: Difference in Number of Calls by 2021 Budget Vote and Time

	Simple DiD (1)	Triple DiD (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 A10: Difference in Crime Incidents by 2021 Budget Vote and Time

	Crime calls	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 in progress, log number of calls for serious crimes, log number of shootings, and log number of valid felony, misdemeanor, and violation complaints by date, precinct and council district. Cluster robust standard errors in parentheses, by district (49).

Table A11: 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 A12: Difference in Crime Clearance Rates by 2021 Budget Vote and Time

	All (1)	Felony (2)	Misdemeanor (3)	Violation (4)
yes vote × after vote	-0.0056	-0.0109	-0.0100	-0.0001
	(0.0106)	(0.0078)	(0.0120)	(0.0068)
District FE	✓	✓	✓	✓
Police Precinct	✓	✓	✓	✓
Date	✓	✓	✓	✓
Observations	135,656	135,656	135,656	135,656
Mean of DV	-0.588	-0.276	-0.434	-0.370
R^2	0.15941	0.09391	0.11533	0.13933

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Dependent variable: Difference in log number of arrests and log number of crime complaints, i.e., log clearance rate. Level of observation: Date-district-precinct. Cluster robust standard errors in parentheses, by district (49).

Table A13: 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).

Table A14: 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 A15: 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

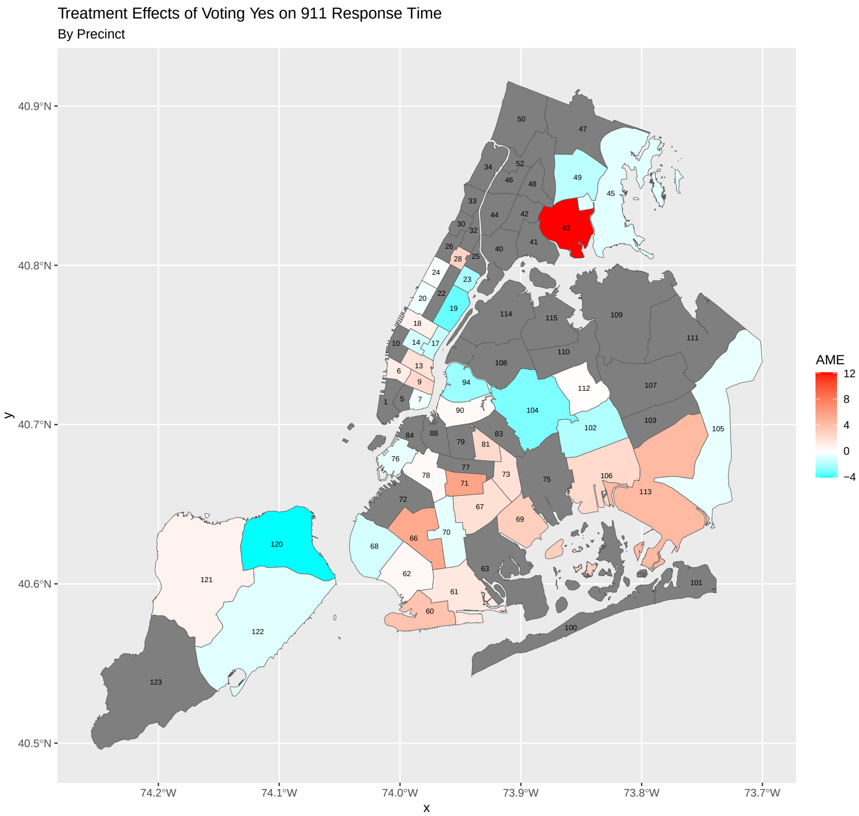
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Dependent variable: Dummy for zero or < 0.15 response time. Columns (1) and (3) only include NYPD calls. Coefficients for yes vote$_c$ and after vote$_d$ absorbed by district and day fixed effects, respectively. Cluster robust standard errors in parentheses,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

Table A16: 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).

Figure A9: Average Marginal Effects of Yes Vote by Precincts



Note: Depicted are ATT estimates from regressions within each precinct separately. Consequently, only precincts that straddle council districts with opposing budget votes are included.

Figure A10: Trends in Amount of 911 NYPD Calls across Districts

Figure A11: Distribution of 911 Call Types, by Period and District

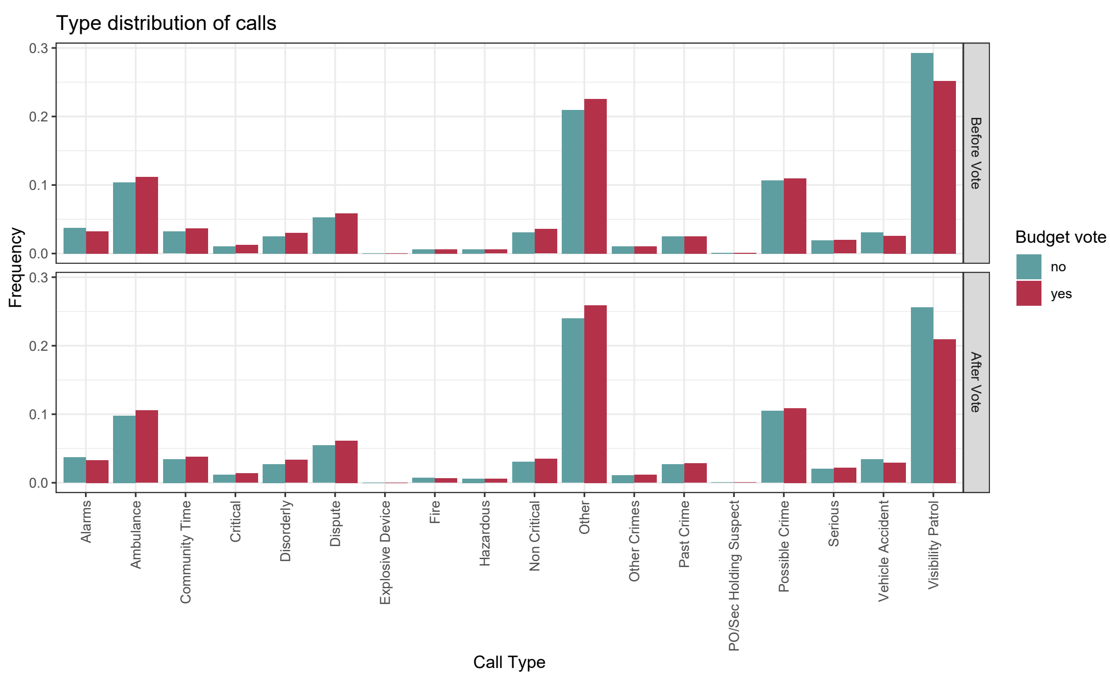


Figure A12: Correlation of Treatment and Change in Resources by Precinct

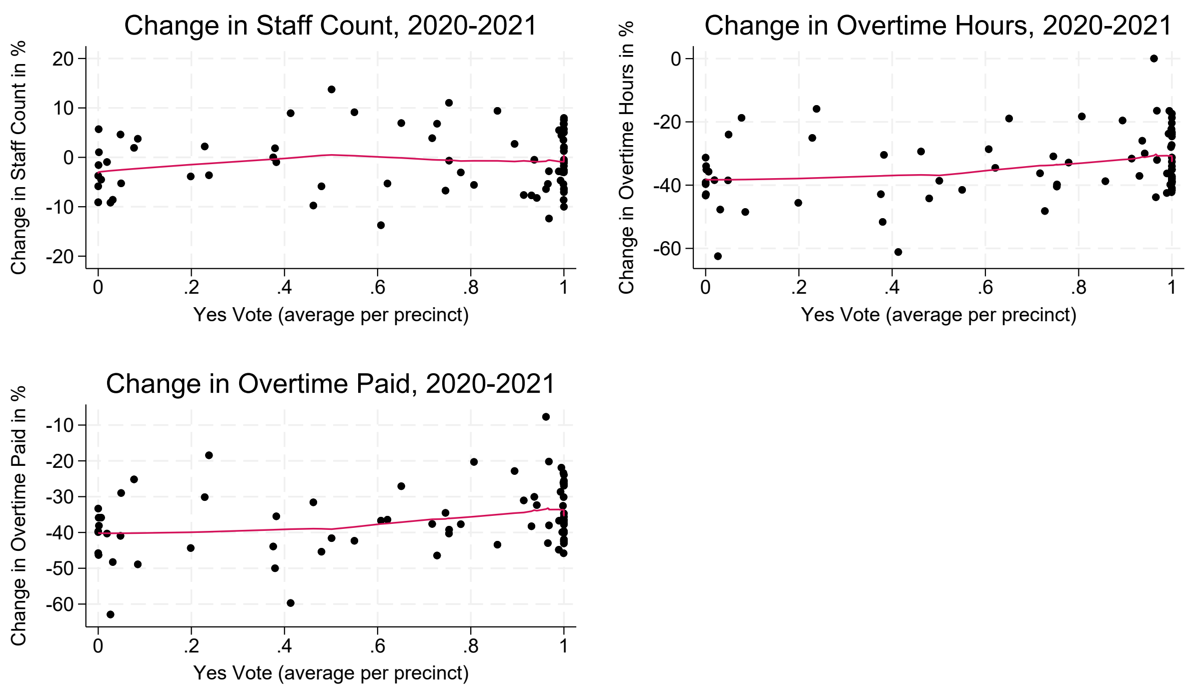
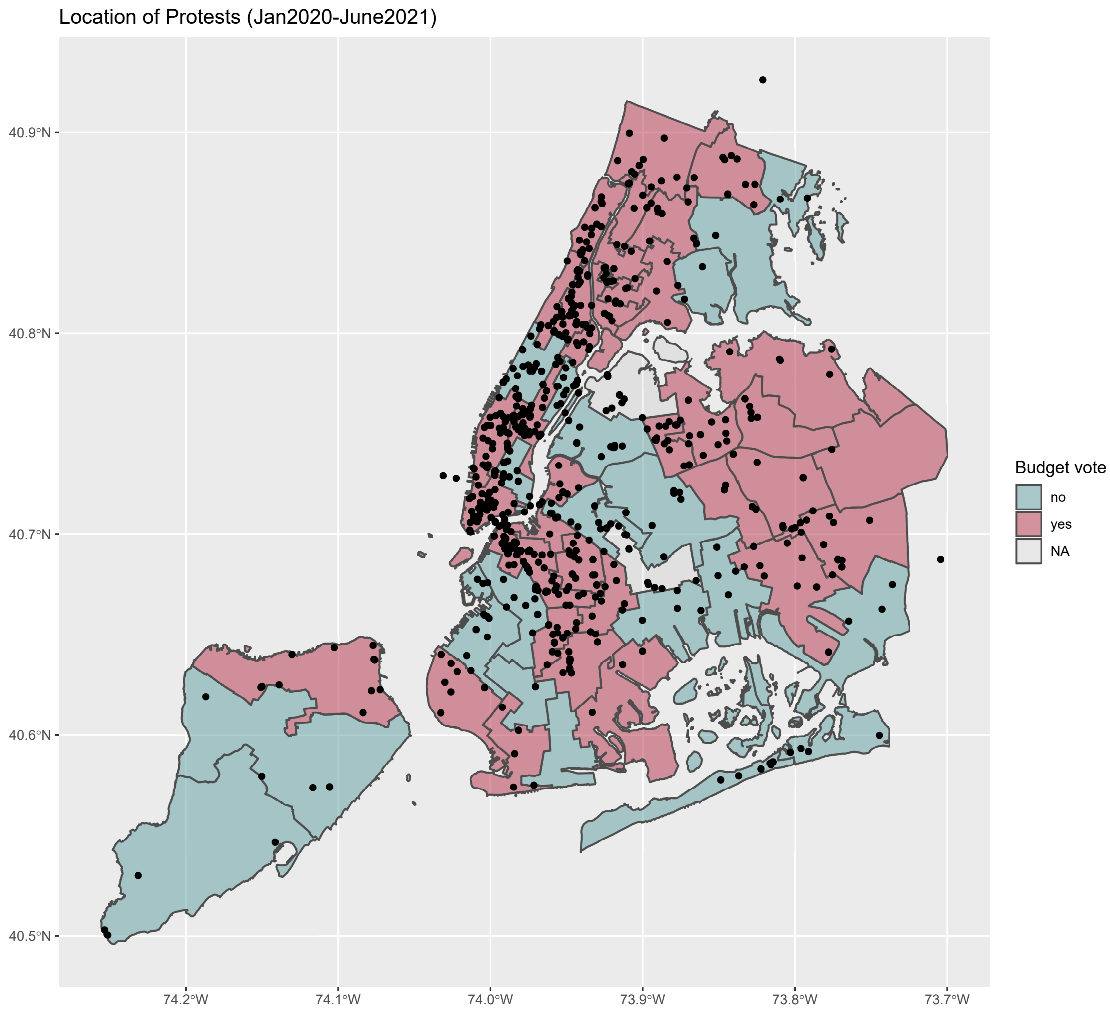


Figure A13: Location of Police-Related Protests



D Spatial Difference-in-Discontinuities Design

The DiD design crucially hinges on the comparability of treated and control districts in the entire city, across agencies and over time, i.e., the validity of the parallel trends assumption. As the main results indicate, this might be violated and complicated by the fact that police could shirk shortly *before* the vote in the hope to influence council members' voting behavior. To leverage more local variation in a-priori similar neighborhoods, I therefore supplement the analysis with a spatial difference-in-discontinuities design. 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 (see Figure A14). 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.[4] For both time periods, the resulting model is estimated as follows:

$$\begin{aligned}\text{response time}_{icpd} = \alpha + \tau \text{yes vote}_c + \beta_{-}\text{distance}_{icpd} + \beta_{+}\text{yes vote}_c \times \text{distance}_{icpd} \\ + \eta_p + \varepsilon_{icpd} \qquad (5)\end{aligned}$$

where response time_{icpd} is the response time of call i in district c and day d, yes vote_c is an indicator equal to 1 if council member of district c voted in favor of the budget cut. distance_{icpd} represents the 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

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

per day on the zip code level. Figure A15 and Figure A16 show the resulting balance in these covariates after matching.

Figure A14: RDD Sample

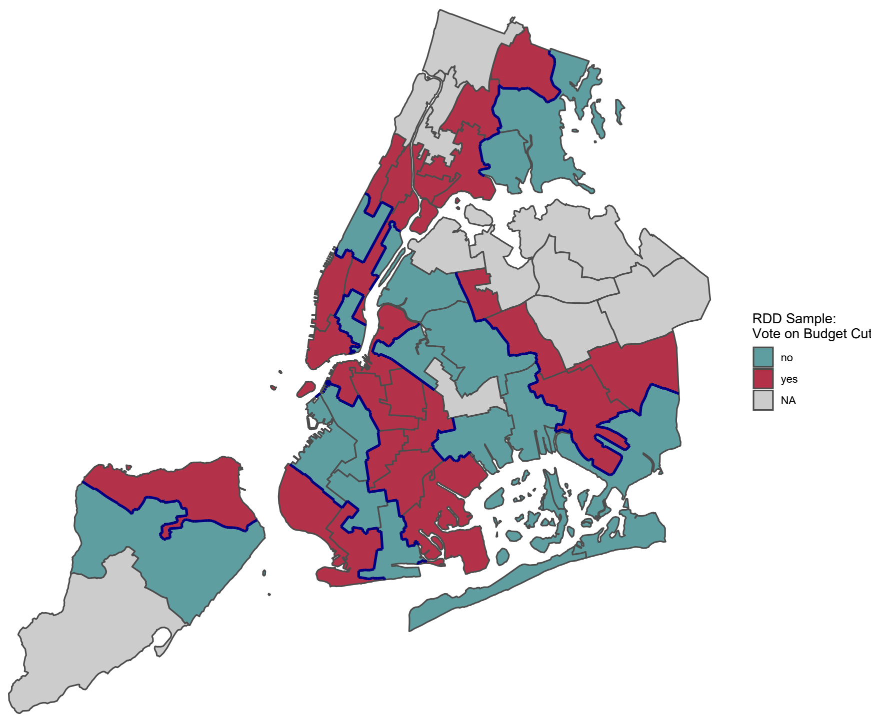


Figure A15: Balance of Matched RDD Sample - Major Call Types

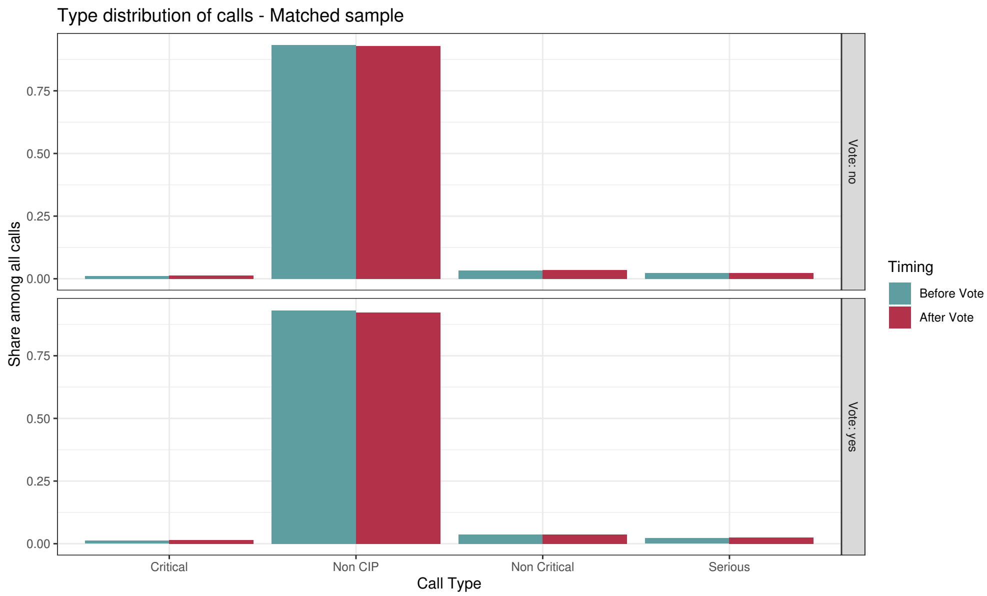


Figure A16: Balance of Matched RDD Sample - Daily Call Volume by Zip Code

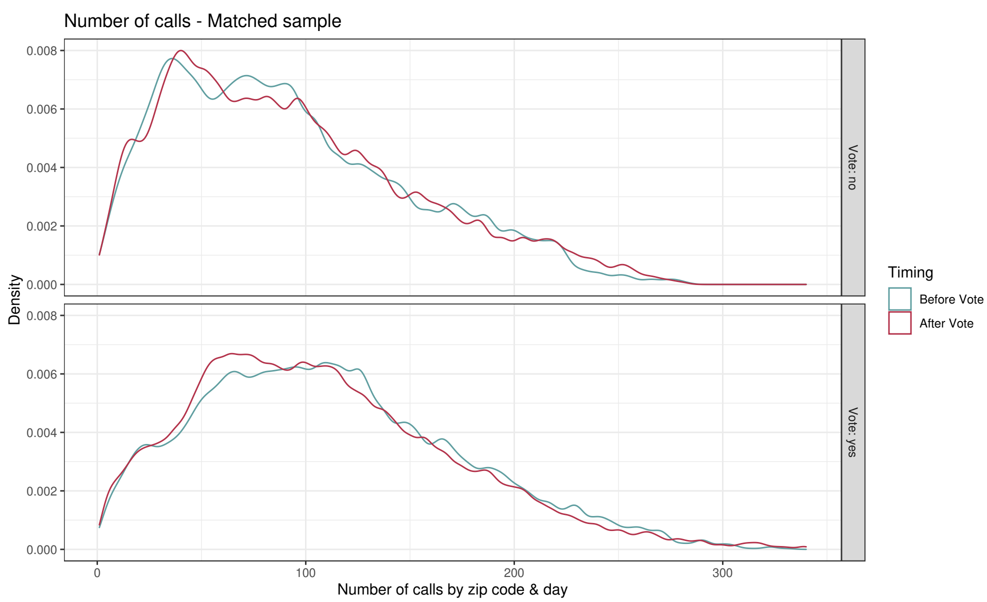


Table A17 shows the results. Interestingly, the negative RDD estimates in both periods suggest that NYPD officers respond faster to calls in treatment districts (yes votes) compared to neighboring control districts (no votes), both before and after the vote. This might be attributed to systematic differences in these neighborhoods that determine response times, including traffic, road quality etc.[5] More importantly for my argument, the difference in the RDD estimates is positive and significant. In line with previous results, the model suggests that for neighborhoods in close proximity to the district borders NYPD slowed down by about 68 seconds per call in yes voting districts relative to no voting districts after the budget vote.

Table A17: 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.

Table A18: 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).

[5] Table A18 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.

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.[6] 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:

$$MIP(crime)_{ijt} = \alpha + \beta \text{yes vote}_j \times \text{post vote}_t + \delta_j + \gamma_t + \mathbf{X}'_{\mathbf{ijt}}\rho + \varepsilon_{ijt} \tag{6}$$

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 A19: 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

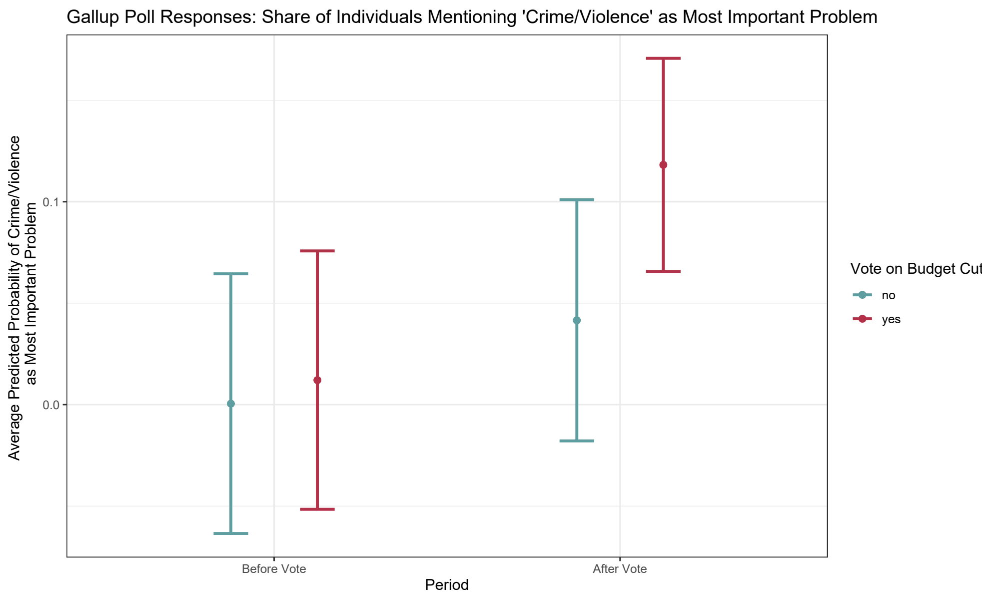
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Dependent variable: Dummy for indicating 'Crime/Violence' as MIP. Standard errors clustered by council districts. Individual controls include partisanship and race.

Table A19 presents the results of the difference-in-differences design, and Figure A17 depicts average predicted probabilities based on column (2) of Table A19. The results suggest that citizens in NYC were disproportionately more concerned about crime after the budget

[6]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.

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.

Figure A17: 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.[7] 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

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

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:

$$\Delta voteshare_{ie} = \alpha + \beta \text{yes vote}_i + \varepsilon_{ie} \tag{7}$$

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).

Table A20: 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).

The results in Table A20 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.

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