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

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Abstract

Research on bureaucratic politics generally describes the link between politicians and bureaucrats as a top-down relationship of principals and agents where politicians attempt to control their bureaucratic subordinates. In contrast, I theorize how politicians' dependence on bureaucratic effort for re-election allows bureaucrats to exert political power over their elected principals. If bureaucrats are organized in cohesive unions with strong tenure protections and deviate in their preferences from politicians, they can collectively reduce effort to exert political pressure. I use data on New York Police Department (NYPD) 911 response times together with council members' preferences on the FY2021 $1 billion cut to the NYPD's budget. Employing difference-in-differences and spatial difference-in-discontinuities designs, I find that police reduced effort in districts of non-aligned politicians by slowing response times. This study informs the theoretical debate on principal-agent relationships in government and highlights the importance of organized political interests to explain policing in US cities.

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1 Introduction

During the pandemic, governments in many cities across the US found themselves in contentious public clashes with law enforcement unions over requirements that officers receive COVID19 vaccines. Although the coronavirus has been the most common cause of duty-related deaths since 2020 with four times as many officers dying from COVID19 than from gunfire (Medina, 2021), many police officers and their unions have resisted vaccinations, threatening work stoppages and lawsuits. While city leaders state that they are trying to keep the public safe, law enforcement officers and their union representatives claim that vaccine mandates violate their rights. For instance, in October 2021 the head of Chicago's largest police union, John Catanzara, called on its 11,000 union members to ignore the city's requirement to report their vaccination status stating, "it is the city's clear attempt to force officers to 'Chicken Little, the sky is falling' into compliance. Do not fall for it. Hold the line." (Honderich, 2021). Expecting that officers would refuse to submit to the mandate, he added that "it's safe to say the city of Chicago will have a police force at 50 percent or less for this weekend coming up. [...] Whatever happens because of the manpower issue, that falls at the mayor's doorstep." (Bosman, 2021).

In this paper, I examine how divergent police preferences of bureaucrats and their political principals incentivize bureaucratic agents to protest against unwanted policy change. I argue that misaligned preferences along with strong tenure protections for bureaucratic careers induce bureaucrats to put pressure on political authorities. In systems with high levels of political control, bureaucrats are found to act as electoral brokers for politicians by working harder and actively boosting politicians' chances of re-election (Iyer and Mani, 2012; Pierskalla and Sacks, 2019; Brierley, 2020). I theorize that the converse can hold in professionalized bureaucracies where bureaucrats' careers are independent of political influence. If bureaucrats differ in their collective preferences from elected politicians and are shielded from political control, they can leverage politicians' dependence on bureaucratic effort for

re-election to exercise power over elected superiors (Moe, 2006, 2011; Ujhelyi, 2014). Hence, my theory models the triad relationship of bureaucrats, politicians, and voters to understand how poor public service provision is used as a way of political influence for bureaucrats, thus turning the usual principal-agent relationship on its head.

To empirically test this argument, I focus on the behavior of US municipal police. Local law enforcement in US cities is well organized in dense, cohesive unions that have long opposed police reforms. Anecdotal evidence suggests that police unions influence local and national politics through lobbying, litigation, or participating in electoral campaigns (Blumgart, 2020; Zoorob, 2019). Yet, little is known about how police officers adjust their day-to-day activities to affect their elected principals and the policy choices they make in office. Applying my theoretical argument, I expect that the police reduce their effort to exert political pressure on non-aligned local elected officials. In so doing, the police can exploit the public's fear of crime and affect voters' evaluations of incumbents to maintain public safety. Hence, by branding certain incumbents as incompetent and "soft on crime," the police aim to punish their elected principals and affect public policy.

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.

For causal identification I employ a triple difference-in-differences design where I compare response times across non-aligned and aligned districts, before and after the budget vote and across agencies. I use response times of firefighters to 911 medical emergency calls to account

for time-specific trends in response times across districts (e.g., due to differences in traffic levels). Firefighters are largely comparable to police officers in their unionization rates and local government structures. Yet, unlike funding for the NYPD, the adopted budget of the Fire Department of the City of New York (FDNY) increased relative to previous fiscal years. Hence, since firefighters had little reason to organize politically to exert pressure on city council members, emergency medical services (EMS) response times can serve as a credible counterfactual in bureaucrats' reactions to 911 calls absent electorally motivated behavior. As 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.

In line with my theory, I find that response times in non-aligned districts increased by about one minute and 30 seconds for NYPD calls compared to FDNY calls after the budget vote – a substantial increase relative to the average 911 response time of 13.4 minutes prior to the budget vote. The size and precision of this treatment effect is robust to accounting for the available budget in each police precinct, demand for police presence and police-related protests. Additionally, supplementary analyses suggest that the effect is driven by a reduction in officer-initiated calls and by delays for longer calls where police have more discretion, including past crimes, disputes and vehicle accidents. I further provide qualitative evidence from official statements and social media posts of NYPD police unions to substantiate how police organizations targeted non-aligned politicians in the wake of the budget cut by leveraging their influence on voters.

This research makes three main contributions. First, it addresses possible trade-offs of bureaucratic independence, thus contributing to the literature on politization of bureaucracies. If politicians can influence the recruitment, promotion, and assignment of civil servants, they can exploit the induced loyalty of bureaucrats for their own electioneering and clientelist practices (Iyer and Mani, 2012; Pierskalla and Sacks, 2019; Brierley, 2020). Indeed, politicians' power to exploit their control over civil servants has often been found to cause

electoral cycles in bureaucratic staffing (Iyer and Mani, 2012; Colonnelli et al., 2020) and bureaucrats' efforts to implement policy (Pierskalla and Sacks, 2019; Brierley, 2020). To overcome such problems, scholars and practitioners in public administration generally advocate for bureaucracies to be strongly independent from political authorities (Rauch and Evans, 2000). Yet, this study raises questions about whether a strict political insulation of civil servants necessarily prevents electorally motivated behavior of bureaucrats, thus speaking to recent research on the strategic politicking of bureaucrats (Potter, 2019) and the political preferences of career executives (Bolton et al., 2020).

Second, this study contributes to the growing literature on local interest groups, especially with respect to the organization and influence of public sector unions (Anzia and Moe, 2019; Anzia, 2022). The modern interest group literature contains numerous studies on how groups attempt to influence bureaucracy from the outside through lobbying (Yackee and Yackee, 2006; Gordon and Rashin, 2021) or dense personal and professional networks (Adolph, 2013). Yet, little work – with the exception of scholarship by Sarah Anzia and Terry Moe (Anzia and Moe, 2019) – has considered how interest groups influence public policy from *inside* government as official participants in bureaucratic decision-making. Additionally, the role of *local* interest groups on public policy is largely understudied, even though local venues are often more important avenues for interest group representation than platforms in Washington (Anzia, 2022).

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

2 Preference Alignment, Tenure Protection and Resistance of Bureaucracies

An extant literature on bureaucratic politics has studied the intricate relationship between political authorities, generally pictured as the principals, and non-elected bureaucrats, who serve in the role of executive agents. Standard principal-agent models show that the level of policy conflict between politicians and bureaucrats is a central predictor of these actors' behavior (McCubbins and Schwartz, 1984; Epstein and O'Halloran, 1999; Huber and Shipan, 2002). As policy preferences of politicians and bureaucrats diverge, intended and implemented policies can increasingly deviate, thus resulting in agency loss. Most existing theories acknowledge that the degree of agency loss depends on politicians' ability to control the appointment and behavior of bureaucratic agents. However, little work recognizes the converse mechanism, i.e. how *bureaucrats* may exert pressure on politicians' careers and actions. I argue that these interdependencies are shaped by the political organization of bureaucracies and have important implications for public service provision.

In systems of political patronage, politicians have substantial influence on the appointments, transfers and dismissals of bureaucrats at their discretion. This automatically aligns the incentives of bureaucrats and politicians (Ujhelyi, 2014). If politicians can affect bureaucrats' career progression, bureaucratic agents depend on the re-election and continuous support of their political principals. As a result, bureaucrats are often found to implement policies that ensure an incumbent's electoral success and signal their loyalty – a fact politicians have exploited for their own electoral gains (Brierley, 2020).

The converse can hold in systems of professionalized, meritocratic bureaucracies. If bureaucrats are selected through competitive examinations and enjoy civil service protections including job tenure, collective bargaining and standardized pay scales, politicians lose most of their direct influence on bureaucrats' careers and actions. This makes it easier for bureaucrats who disagree with politicians' preferences to diverge from intended policy. Most

existing principal-agent models center on how political authorities can devise institutional arrangements in these settings, including ex ante monitoring and ex post control (McCubbins and Schwartz, 1984) of bureaucrats, to minimize such agency loss. Yet, these top-down theoretical accounts paint bureaucrats as passive agents and disregard the fact that political principals in a democratic setting are *elected* and thus vulnerable to the behavior of bureaucrats (Moe, 2006, 2011). I argue that in professionalized systems bureaucrats can attempt to lower the re-election chances of incumbents and thus put pressure on political principals without worrying about retaining their jobs.

Bureaucrats have various tools to exert such political pressure. First, similar to other interest groups, bureaucrats can influence policy-makers by donating to political campaigns (Zoorob, 2019), endorsing political candidates (Moe, 2006, 2011), and actively participating in policymaking venues or collective bargaining (Anzia and Moe, 2019). Additionally, unlike interest groups outside government, bureaucrats can adjust their work effort as another way to exercise their political power. Motivated by incentives of re-election, political representatives use public policy to cater to their voters and donors. Yet, since voters rarely observe politicians' performance directly, they generally base their evaluations of elected representatives on public policy as implemented by bureaucrats (Ujhelyi, 2014). In the absence of perfect information about the inner workings of government, voters face difficulties in attributing changes in the quality of public policy to bureaucrats vis-à-vis politicians. This imperfect information allows bureaucrats to sabotage the public payoff by reducing their effort to damage the reputation of non-aligned politicians and thus push back against unwanted policies.[1] For instance, bureaucrats can protest unwanted political objectives by delaying the execution of policy as opposed to working diligently. Even more extreme, bureaucrats may actively sabotage the political agenda of their principals, for instance, by obstructing policy implementation with the attempt to wreck a policy or prevent possible

[1]Note that if voters can perfectly attribute poor service quality to shirking bureaucrats, they would never misattribute it to politicians' behavior. Hence, in equilibrium politically motivated shirking is ineffective and thus unobserved.

reforms (Brehm and Gates, 1997). Thus, due to the inherent dependence of elected officials on bureaucratic agents and voters' imperfect information about political responsibilities, electoral accountability can under certain conditions deteriorate public service provision.[2]

This is not to say that all groups of bureaucrats act politically or that they are equally powerful across different political systems. In fact, existing research on US federal bureaucrats' resistance suggests that bureaucratic militancy has been limited across different presidencies (Brehm and Gates, 1997; Golden, 2000). I therefore highlight several scope conditions for my theory.[3] First, bureaucrats need to develop mechanisms to overcome collective action problems in their strategic behavior. Public sector unions often serve this purpose, as they pool employees' resources, streamline political goals and reduce the possibility for selective punishment of individual bureaucrats. A strong union, therefore, enables bureaucrats to *collectively* resist the agenda of their political principals, while shielding them from detrimental consequences, such as job loss. This highlights a second important scope condition for my theory: Since local public sector unions often form smaller, more cohesive interest groups than their larger federal counterparts (Moe, 2006; Anzia, 2022), bureaucrats are likely better able to exert political pressure on the subnational level. Third, the theory depends on the observability of public goods provision. Bureaucrats can only take advantage of politicians' electoral vulnerability if voters are well aware of the quality level of public services, while facing uncertainty about who is to blame for any deterioration. For example, while voters might be less aware about the output of employees in a city's office of labor relations, they are often more immediately affected by and informed about the behavior of street-level bureaucrats, such as police officers or social workers. Lastly, I argue that reducing effort is inherently costly for bureaucrats, which creates limits to the optimal level of politically mo-

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

[3]Since these conditions remain fixed in my empirical setting, I am unable to test their importance for the theoretical mechanism. I leave this to future research.

tivated shirking. Public sector employees are often found to exert effort without significant monetary incentives, because they tend to be intrinsically motivated to perform (Brehm and Gates, 1997; Forand et al., 2022). Similarly, higher quality of public services often facilitates bureaucrats' jobs. For instance, as lower crime rates reduce the inherent need for constant policing, police officers benefit from a sufficient level of effort. Unsurprisingly, recent research finds that officers strongly prefer working in lower-crime neighborhoods (Ba et al., 2021). Additionally, if bureaucrats engaged in constant shirking, this strategy would lose its valuable signaling effect, bureaucrats would risk alienating voters and politicians, and could thus trigger more anti-bureaucracy policies instead of advancing their causes.

3 Bureaucratic Resistance of US Municipal Police

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

Rank-and-file employees of law enforcement agencies are generally well organized in powerful unions with strong tenure protections. In 2020, for instance, 56% of the 764,141 police officers in the US were unionized, compared to only 25% of employees in the public sector overall and 6% in the private sector (Hirsch and Macpherson, 2021).[4] Additionally, police unions tend to be characterized by a cohesive "police culture" with high levels of in-group solidarity, often manifested in a norm of mutual protection and cover-ups of bureaucratic

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

transgressions (Zoorob, 2019). These dense and cohesive unions make police networks particularly conducive to collective action.

Additionally, police forces are found to have strong policy preferences. Unlike most unions, police unions have gravitated towards right-wing policies throughout American history, often resisting criminal justice reform initiatives (Zoorob, 2019). The major Fraternal Order of Police (FOP), for instance, has supported legislation that turns the killing of police officers into a hate crime and has backed the "Police Bill of Rights," which protects officers accused of misconduct in several states (Zoorob, 2019).

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

Police officers have also used work slowdowns and strategic de-policing by avoiding certain areas or activities (such as traffic stops) to voice discontent and intensify their pressure on local politicians. For instance, while strikes by law enforcement is not permissible in virtually all US states (Sanes and Schmitt, 2014), police officers have been found to influence contract negotiations and avoid increased oversight following federal investigations by reducing effort, mirrored in lower arrest rates (Mas, 2006; Shi, 2009; Shjarback et al., 2017; Devi and Fryer, 2020). When proposing a budget cut to the local police department in 2018, Minneapolis

City Council member Steve Fletcher, for example, received complaints from business owners and constituents, indicating that officers were delaying response times to calls for service in his district (Blumgart, 2020). As the politician put it:

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

By reducing their effort in policing districts of non-aligned politicians, the police can intensify the salience of public safety issues for voters and thus increase the importance of their own agendas in electoral campaigns. Additionally, they evoke perceptions of deteriorating safety and increasing crime rates among the public. By playing with the public's fear of crime, the police can paint incumbents in these areas as "soft on crime" and anti-police – labels that elected officials try to avoid and for which they tend to be punished (Huber and Gordon, 2004; Drago et al., 2019). Thus, I expect that the police aim to punish politicians and affect public policy by exerting lower effort in areas where incumbents run on police-reform agendas.[5]

[5]Note that while electoral vulnerability of politicians is key for my theory, I do not argue that police aim to influence elections *directly* through shirking. Rather, police intend to exert latent political pressure on politicians through bureaucratic sabotage, e.g., by causing public safety concerns and triggering complaints by constituents. Given the high importance of public safety issues and low turnout in US local elections (Anzia, 2014), shirking can be an effective strategy to exert political pressure.

4 Empirical Case, Data, and Research Design

4.1 NYPD's 2021 Budget Cut

For the empirical analysis I focus on the behavior of NYPD officers in response to the significant cuts to the NYPD budget in FY2021. On June 30, 2020, the New York City Council agreed to a very grim budget for the following fiscal year that sharply reduced municipal services. The NYPD experienced the most significant cut in its funding, as the City Council reduced its budget by about $1 billion and imposed hiring freezes for police officers (Rubinstein and Mays, 2020). In particular, in an attempt to reform the NYPD organization and placate calls to defund the police, City Council members reduced overtime payments by 67%, eliminated the July 2020 police academy class of roughly 1,160 officers, cancelled hiring plans for traffic enforcement agents and civilian positions, and transferred several responsibilities from the police department to other city agencies (including school safety and monitoring of illegal vending) (City of New York, 2020; Rubinstein and Mays, 2020; O'Brien and Rosenberg, 2020). Yet, since the latter component was not officially part of the City Council's FY2021 adopted budget, the final cut amounted to $415 million, with most of the savings due to reductions in both civilian and uniformed overtime ($328 million) (Citizens Budget Commission, 2020).

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

three years, the City Council had approved the budget unanimously.[6]

The scope of the budget adjustment was unprecedented and largely unexpected. As Figure 1 illustrates, NYPD's operating budget increased in almost all years prior to FY2021. Additionally, former NYC mayor Bill De Blasio's executive budget proposal in April 2020 included a minimal cut of only $24 million, and although the mayor promised on June 7 to shift some of the NYPD's budget to social services and youth programs, he declined to specify the amount of cuts (Coltin, 2020). Just weeks before the budget deadline, city council leaders agreed on June 12 to set a goal of $1 billion in cuts to the NYPD budget and De Blasio eventually approved their proposal on June 23 (Coltin, 2020; New York City Council, 2020). The Police Benevolent Association (PBA), the NYPD's largest police union, promptly voiced dissent against the proposal, threatening that

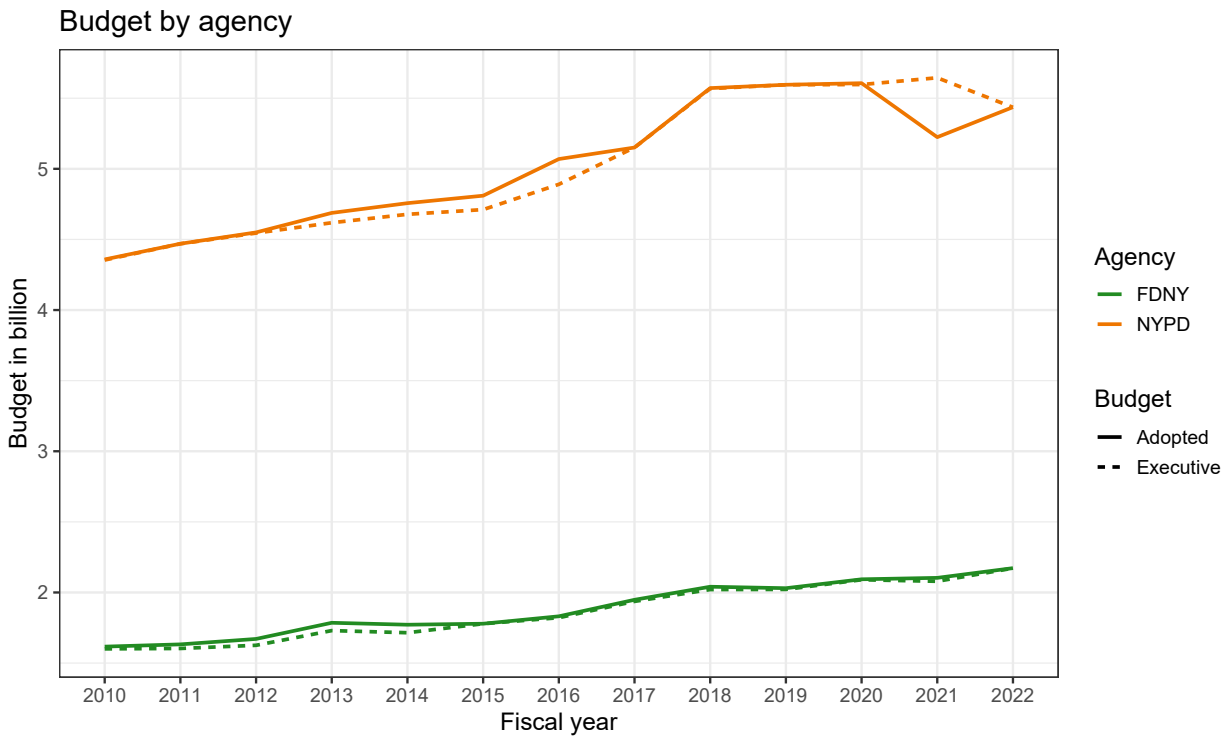
> "For decades, every time a city agency failed at its task, the city's answer was to take the job away and give it to the NYPD. If the City Council wants to give responsibility back to those failing agencies, that's their choice. But they will bear the blame for every victim, for every New Yorker in need of help who falls through the cracks. They won't be able to throw cops under the bus anymore."[7]

Police unions play an important role in NYC politics and the operations of the NYPD. In addition to the PBA, which represents all sworn NYPD officers (about 24,000), there are four major police unions representing various ranks of NYPD employees (the Detectives' Endowment Association, the Sergeants Benevolent Association, the Lieutenants Benevolent Association, and the Captains' Endowment Association). These organizations function as private corporations supported by their members' dues, are responsible for negotiating NYPD contracts, provide legal services, and administer health and welfare benefits to their

[6] https://legistar.council.nyc.gov/MeetingDetail.aspx?ID=706279&GUID=01DA50FD-50E7-4CC4-8BFF-9D4AE78E20DE&Search=, https://legistar.council.nyc.gov/MeetingDetail.aspx?ID=611179&GUID=CD813688-DB0A-4257-BBA4-729DFE70539E&Search=, https://legistar.council.nyc.gov/MeetingDetail.aspx?ID=552057&GUID=B2B17436-DA1F-4C53-AC72-AE5697067133&Search=.

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

Figure 1: Operating Budget of NYPD and FDNY Over Time



Note: The executive budget is based on the mayor's submission of a proposed budget in April each year. The adopted budget is the finalized budget in each fiscal year that the mayor and City Council vote on. Source: NYC Mayor's Office of Management and Budget (2021)

members. Additionally, they are publicly known for their inflammatory media presence and their lobbying activities to influence NYC legislation and local elections.[8]

The funding changes in the NYPD had significant implications for rank-and-file employees at the agency. The NYPD spends the vast majority of its annual budget on personnel. For instance, in FY2020, 92% of the operating budget was for personnel services, while the remainder was assigned to purchase supplies, materials, and other services for the agency's operations.[9] Additionally, overtime spending is an important source of officers' income. In FY2020, overtime spending totaled $635 million, 44% of all citywide overtime expenses (Citizens Budget Commission, 2020). In the same year, the median share of overtime pay

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

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

out of total pay for NYPD employees amounted to 12%.[10]

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

4.2 Measuring Police Behavior: Calls for Service

To measure police behavior and effort, I use fine-grained data on 911 calls for service. In particular, I use officer response times to calls (i.e., the time between when the call was logged in the dispatch system and when officers arrived at the scene). These data are suitable to test my theory for several reasons. First, officers spend a substantial amount of their time responding to 911 calls (Neusteter et al., 2020). Most of the incidents are noncriminal in nature—citizens make calls to complain or request that an officer perform a welfare check. As a result, police officers have a considerable amount of discretion in when and how they respond to these calls for service, which is often reflected in a large variation in dispatcher and officer response times to calls across departments and incidents (Neusteter et al., 2020). Second, earlier studies indicate that neighborhood characteristics, including the economic wealth and demographics of residents in an area, affect call patterns and police officers' response times (Cihan et al., 2012; Lee et al., 2017). Yet, no previous work has considered the effect of the *political* characteristics of neighborhoods on officer behavior in response to calls for service. Third, 911 calls, especially officers' response times to calls, are related to people's perceptions of the quality of policing. Using different response time surveys

[10] Calculated from FY2020 NYC payroll data.
[11] See Figure A1 for more details.

across various US cities, several studies have found negative correlations between response times and respondents' evaluations of police performance (Pate et al., 1976; Parks, 1984). Additionally, some work suggests that shorter response times are associated with higher arrest rates (Cihan et al., 2012; Lee et al., 2017; Blanes i Vidal and Kirchmaier, 2017). There are further technical advantages to using calls for service data to measure police effort. In addition to the timing and chronology of each call, the data includes detailed information on the location of the incidence and classifications for the call type and priority level. This allows me to geocode each call and assign it to a specific political district.[12]

4.3 Council Members' Voting Behavior

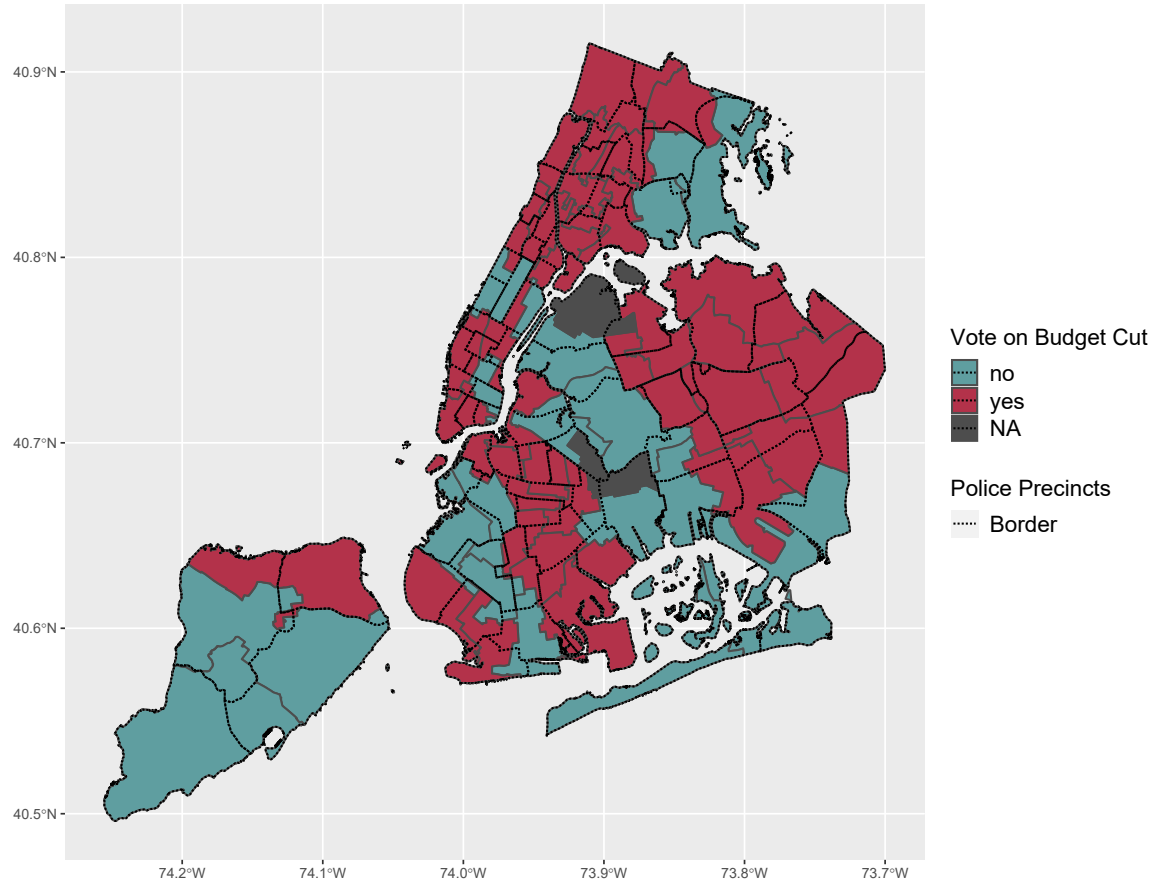
Figure 2 shows the distribution of council members' voting behaviors on the budget proposal across NYC's 51 council districts.[13] The map illustrates that both "yes" and "no" votes are distributed across the city, and districts with opposite voting patterns share a border in several instances. Additionally, these district borders cut across NYPD precinct boundaries. As I discuss in greater detail in Section 4.4, this allows me to compare changes in 911 response times across districts *within* police precincts using precinct fixed effects in my empirical designs.

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

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

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

Figure 2: NY City Council Votes on 2021 Budget



4.4 Triple Difference-in-Differences Design

To identify the effect of preference alignment between the NYPD and New York City Council members on police behavior my main specification leverages the fine-grained geographic information on 911 calls in a difference-in-differences (DiD) model. I compare response times in districts of council members in favor of the budget cut to response times in districts of council members who voted against the budget reduction, before and after the vote on June

30. This implies that the 32 council members who supported the significant cut to the NYPD's funding are deemed to be non-aligned with police preferences, while the 17 representatives who opposed the policy remained aligned with the NYPD's general interests. Yet, in a simple DiD model, it is inherently difficult to distinguish politically motivated shirking of police officers from general time-specific dynamics across districts (e.g., differences in traffic). To account for time-specific trends in response times, I additionally use response times of firefighters to 911 medical emergency calls as my third control dimension. Firefighters are largely comparable to police officers in their unionization rates and local government structure. Yet, unlike funding for the NYPD, Figure 1 shows that the adopted budget of the FDNY increased relative to the planned budget in April 2020 and the operating budget in previous fiscal years. Hence, since firefighters had little reason to organize politically to exert pressure on City Council members, EMS response times can serve as a credible counterfactual in bureaucrats' reactions to 911 calls absent electorally motivated behavior.

Thus, I estimate the following econometric model:

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

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

Importantly, police precinct fixed effects alleviate concerns that the estimated treatment effect is driven by mechanical changes in the number of available patrol officers due to reduc-

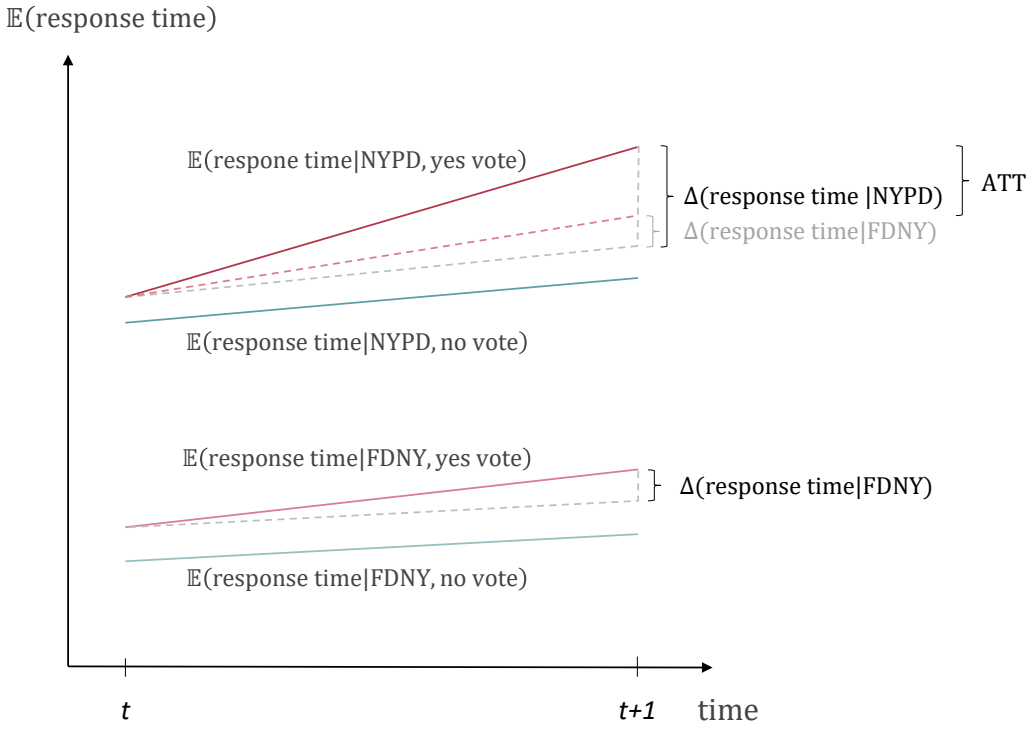
tions in staffing, overtime, or voluntary retirements. Since such police patrol management is organized on the police precinct level, the within-precinct DiD setup ensures that mechanical changes in response times for a given precinct are subsumed by delays among portions of the precinct that voted against the budget cut. *Differential* increases in response times in yes-voting regions of the precinct can thus help identify delays resulting from politically motivated behavior of police. This design implies that only precincts with variation in the vote patterns *within* the precinct boundaries contribute to the estimated treatment effect. 62 of the 77 NYPD precincts in my sample respond to both treatment and control districts, thus ensuring that the effective sample is close to the overall sample. Additionally, district fixed effects account for differences in district characteristics (see Table A2). To the extent that these characteristics and their influence on NYPD response times stay constant across my sample period, my treatment effect estimates remain unbiased. I cluster standard errors ε_{icpda} on the district level.[14]

Figure 3 provides a graphical representation of the triple DiD identification strategy. While a simple DiD design would only rely on the divergent trends in NYPD response times within yes-voting districts vis-à-vis no-voting districts over time (i.e. Δ(response time|NYPD)), the triple DiD design incorporates the corresponding trends in FDNY response times in order to estimate the causal effect of the budget vote on bureaucrats' behavior ($ATT = \Delta(\text{response time}|\text{NYPD}) - \Delta(\text{response time}|\text{FDNY})$). The identifying assumption of this design is that *differences* in response times between NYPD and FDNY officers across treatment and control districts would have followed similar trends in the absence of the budget vote.[15]

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

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

Figure 3: Visual Representation of DiD Identification



4.5 Spatial Difference-in-Discontinuities Design

The triple DiD design crucially hinges on the validity of the parallel trends assumption. This might be complicated by the fact that my theory is agnostic about the exact timing of the effect of preference misalignment on bureaucrats' behavior. While the triple DiD design estimates the effect *after* the budget vote took place, the theory also allows for police to shirk shortly *before* the vote in an effort to influence council members' voting behavior. Although this dynamic might be alleviated by the heightened public attention to the issue of policing before the vote, which reduced the ability of police to shift politicians' positions on the issue, it can pose challenges to my triple DiD identification strategy. To leverage more cross-sectional variation, I therefore supplement the analysis with a spatial difference-in-discontinuities design. As shown in Figure 4, I use a spatial RDD design to compare NYPD response times in close proximity to the council district borders that separated yes

and no voting members. For each 911 call I calculate the minimum distance to a separating border to construct the running variable. To provide estimates for the changes in these RDD estimates before and after the vote, I split my sample along the date of the budget vote.[16] For both time periods, the resulting model is estimated as follows:

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

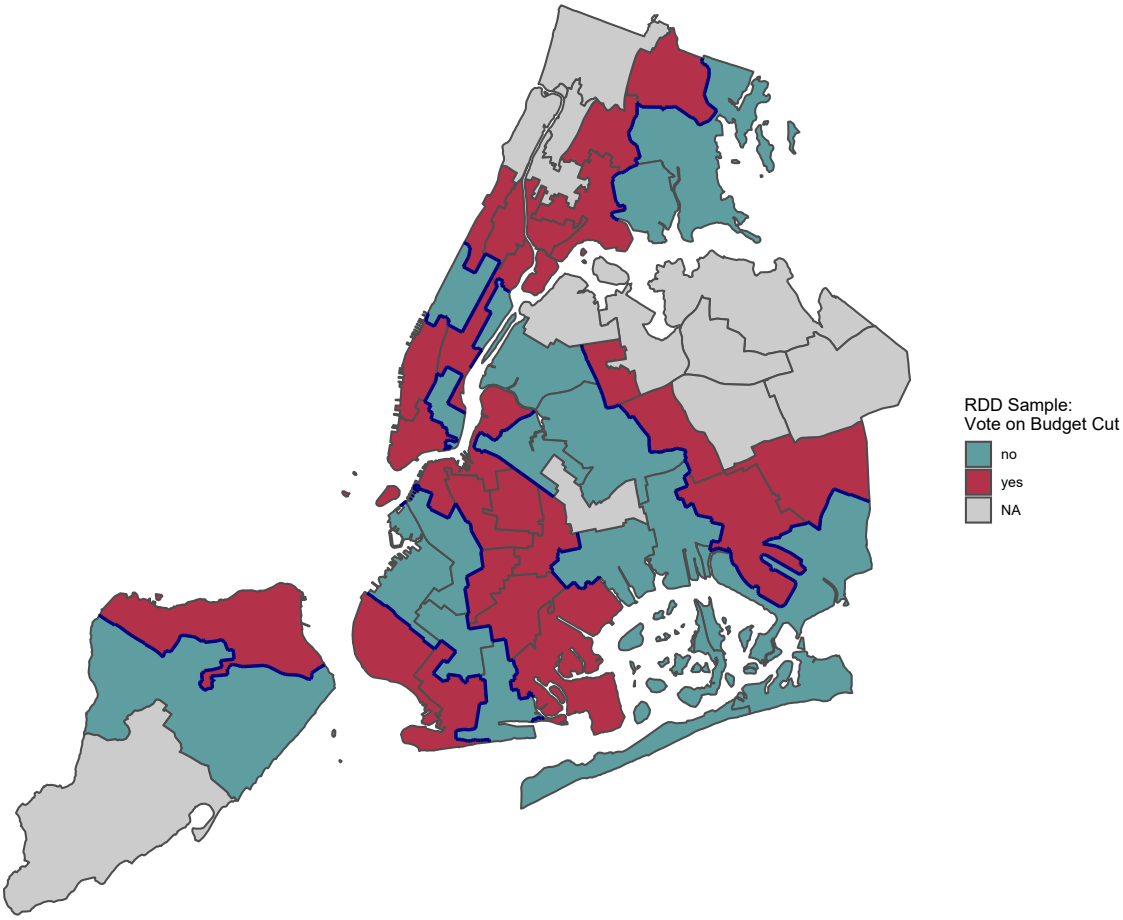
where $\text{response time}_{icpd}$ is the response time of call i in district c and day d, yes vote_c is an indicator equal to 1 if council member of district c voted in favor of the budget cut. distance_{icpd} represents the distance of call i to the border distinguishing these two categories of districts, and contains only units $\text{distance}_{icpd} \in [-h; h]$, 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 ensure comparisons of calls within the same police administration. I use Monte Carlo simulations to provide confidence intervals of the difference in RDD estimates (King et al., 2000).

A few clarifying comments are warranted. Like all spatial RDD settings that rely on administrative borders, estimates of τ likely suffer from compound treatment problems, since many characteristics beyond a council member's vote change discontinuously along district borders, such as road quality or demographics. Yet, this is less problematic in a *difference*-in-discontinuities design. To the extent that these characteristics and their effect on NYPD response times stay constant across the periods before and after the vote, the *difference* in the RDD treatment effects remains unbiased. Yet, if other determinants of NYPD response times change pre and post budget vote along the separating border, the difference in RDD

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

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 time-variant covariates that might be relevant for response times, including call type and the number of calls per day on the zip code level.[17]

Figure 4: RDD Sample



[17]Figure A2 and Figure A3 show the resulting balance in these covariates after matching.

5 Results

5.1 Triple Difference-in-Differences Design

Figure 5 depicts the raw trends in average 911 response times across different types of districts over time and for different agencies. The dynamics in police behavior seem to corroborate the general theory. The figure provides some graphical evidence that NYPD average response times were elevated after and in the two weeks before the budget vote, and more so in non-aligned council districts and relative to FDNY-EMS calls. The figure also highlights cyclical trends in response times (e.g., due to COVID19 waves), which my triple DiD design accounts for.

Figure 5: Trends in 911 Response Time across Districts

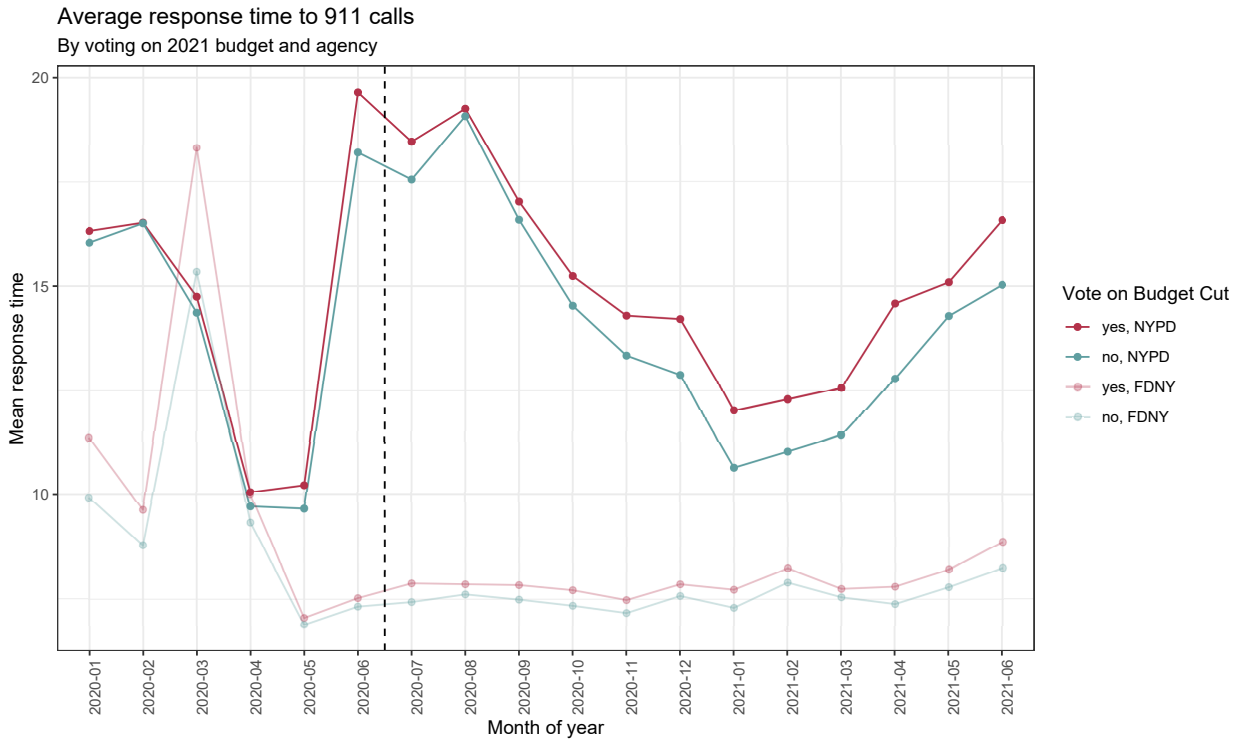


Table 1 evaluates trends in police 911 response times using the triple DiD model in Equation (1). We find that NYPD on average took about 5 minutes longer in their response times in aligned districts than FDNY before the budget vote (NYPD). After the vote, response

times went up by about 2.5 minutes in aligned districts (after vote × NYPD). Most importantly, in line with the theory the triple DiD estimate is positive, suggesting that response times in non-aligned districts increased by about one minute and 30 seconds more for NYPD calls than for FDNY calls after the budget vote (yes vote × after vote × NYPD). With an average response time of about 13.4 minutes throughout the sample period, this increase is substantial. Similarly, an additional minute in police response times is large enough to elicit public concern. For instance, after examining data of overall NYPD response times in 2020, then-Brooklyn Borough President Eric Adams said that "[a] minute in policing is a lifetime, when you are wrestling with someone, when you are being robbed, that extra 60 seconds is the difference between an apprehension or even a person's life" (Gross, 2020). The size and precision of this treatment effect is largely robust to further controlling for the demand for police presence (in Models (2)-(5)). Model (2) accounts for the total number of calls in districts and precincts per day. Similarly, Model (3) and (4) proxy demand for police presence using the total number of shootings in districts and precincts each day and the number of valid felony, misdemeanor, and violation crimes reported to the NYPD. Model (5) incorporates fixed effects for the official importance level of NYPD and FDNY calls. This separates critical and serious crime incidents from non-critical crimes and non-crime calls for NYPD, and life-threatening from non-life-threatening emergencies for FDNY.

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

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

Figure A4 shows the respective treatment effects by month. While the estimates are imprecise, there is some indication of pre-treatment divergence in 911 response times, particularly in June 2020. This could be the result of ramifications from the George Floyd protests and police anticipating politicians' positions on the budget leading up to the official

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

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

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

vote on June 30. In fact, on June 12 council speaker Johnson together with the leaders of city council caucuses and the chairs of the committees on finance, capital budget, and public safety published a joint statement to announce the $1 billion cut to NYPD spending (New York City Council, 2020), and many council members published their vote intentions around

that time.[18]

5.2 Spatial Difference-in-Discontinuities Design

To alleviate concerns about anticipatory behavior and the validity of the parallel trends assumption, I turn to the spatial difference-in-discontinuity as a secondary analysis. Table 2 shows the results. Interestingly, the negative RDD estimates in both periods suggest that NYPD officers respond faster to calls in treatment districts (yes votes) compared to neighboring control districts (no votes), both before and after the vote. This might be attributed to systematic differences in these neighborhoods that determine response times, including traffic, road quality etc.[19] More importantly for my theory, the difference in the RDD estimates is positive and significant. In line with previous results, the model suggests that for neighborhoods in close proximity to the district borders NYPD slowed down by about 68 seconds per call in yes voting districts relative to no voting districts after the budget vote.

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

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

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

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

[19] Table A7, for instance, indicates that calls in yes-voting parts of the RDD sample are slightly closer to the precinct headquarter, thus presumably shortening the amount of travel necessary.

6 Mechanisms

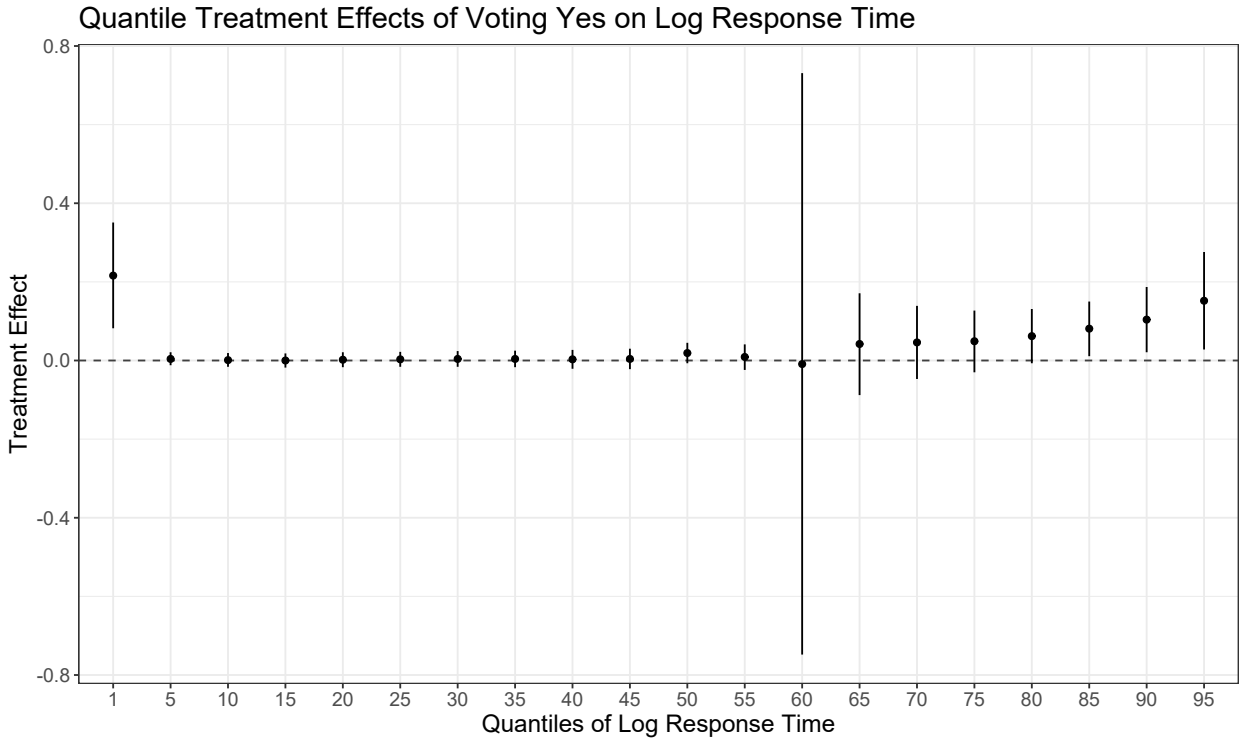
My theory predicts that police officers slowed their response times in non-aligned districts to protest against the budget cut and put pressure on reformist politicians. Police can rely on two particularly effective ways to drive up response times. On the one hand, officers can reduce pro-active policing by minimizing the number of officer-initiated calls. If police encounter events that warrant a police response, they can log calls themselves. Since these calls are characterized by close to 0 response times, the estimated treatment effect may result from increases in the lower tail of NYPD response times. On the other hand, increases in response times in non-aligned districts might be driven by "late arrivals" of officers, thus driving up response times at the upper tail of the distribution. To evaluate these mechanisms, Figure 6 depicts quantile treatment effects.[20] In line with these expectations, the estimated treatment effects are largest at the lower and upper end of the response time distribution, while remaining small and insignificant for other calls. Non-aligned districts not only experienced decreases in officer-initiated calls (response times increased by 22% or 26 seconds at the 1st quantile), but also 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).[21] Average response times by call-type, location and period in Figure A5 further indicate that these long response times are predominantly clustered among calls where police have broad discretion in how they address the incident, such as past crimes, disputes and vehicle accidents.

My theory also highlights the role of police unions as the crucial bureaucratic actor in targeting non-aligned officials and organizing politically motivated shirking. There is ample qualitative evidence supporting this notion. In the months following the budget vote, NYPD's police unions engaged in various smear campaigns against council members who

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

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

Figure 6: Quantile Treatment Effects



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

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.[22] Similarly, police unions publicly defamed council members and their decisions on public safety policies in their districts. For instance, the Lieutenants Benevolent Associated used a video installation outside of a council member's office, shaming him for "anti-cop laws" and proclaiming that the council member "voted to defund the police among other anti-police, and anti-public safety bills. He doesn't care about the well being [sic] of his constituents, he cares about bowing to a hashtag!"[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

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

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

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 Chicago, Detroit, Baltimore!"[26] What is more, as concerns about rising response times arose in the public discourse, police unions attributed the blame for reduced public service quality to the city council and the mayor.[27] When then-council member Ritchie Torres and then-borough president Eric Adams called for an investigation into longer response times and a possible NYPD slowdown in September 2020, police unions reacted with personal insults, leading council members to call for resignations among union officials.[28] In sum, this qualitative evidence substantiates how police unions targeted non-aligned politicians in their campaigning in the wake of the budget cut, leveraged their influence on voters in their political messages and intended to affect the 2021 NYC elections.

Police unions can pass on their political strategy to the relevant police forces in at least two ways. First, they may coordinate with their rank-and-file members directly. Anecdotal evidence suggests that NYPD's police unions regularly communicate with their members, giving them instructions on how to vote and contribute in local elections and how to adjust their work effort politically. For instance, following mayor DeBlasio's police reform endeavours in the wake of Eric Garner's death in 2014, the PBA circulated a bulletin instructing its members that "Starting IMMEDIATELY: At least two units are to respond to EVERY call, no matter the condition or severity, no matter what type of job is pending, or what the opinion of the patrol supervisor happens to be."[29] Second, to put pressure on cer-

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

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

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

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

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

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

tain council members, police unions could urge precinct management to adjust deployment within precincts. Precinct chiefs have considerable discretion in how to allocate resources across their precincts and police officers have been shown to be highly responsive to managerial directives (Mummolo, 2018). Unfortunately, precinct-level deployment information is unavailable for the NYPD, thus precluding a direct test of this conjecture for my case.[30]

How did citizens react to the strategic shirking of police officers? While systemic data on citizens' complaints about NYPD behavior is unavailable[31], I illustrate possible downstream electoral consequences of the budget vote and presumably the police resistance in Appendix D. Analyzing changes in vote shares of incumbent council members between 2017 and 2021 elections, I provide some suggestive evidence that council members approving the budget cut lost more votes in their electoral districts than council members voting no.

7 Alternative Explanations and Robustness

In this section, I address several alternative explanations for my findings. First, I consider whether the results are an artifact of citizens' call patterns. If citizens interacted differently with the police after the policy change in certain areas, one may suspect that the number of calls and the distribution of call types changed, giving rise to sample selection problems and phantom counterfactuals (Slough, 2023). Particularly, if individuals in non-aligned districts had different likelihoods to call the police or only call for minor incidents that take longer to respond a priori, this could explain increases in response times in these council districts. Yet, the NYPD call data suggests that this is unlikely to occur. Figure A6 shows the average daily number of calls per district for treatment and control areas. Evidently, the daily number of calls moves almost in tandem in the treatment and control districts, both before and after the budget vote. The gap between the call volume in "yes" vs. "no" voting districts decreases slightly after the $1 billion budget cut, thus making it presumably

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

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

harder to find longer response times in treatment districts following the policy change.[32] To further evaluate whether citizens' reporting behavior differed along the treatment dimension, Figure A7 depicts the type distribution of 911 calls across districts and periods. The frequency of different types of calls as well as the difference in the occurrence of call types across treatment and control districts remains largely unchanged before and after the budget vote. This alleviates concerns that the estimated increase in response times is driven by differences in citizens' propensity to call the police for specific types of incidents. Similarly, Table A6 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.

Additionally, it is possible that public outrage following George Floyd's death in May 2020 gave rise to differences in policing, either due to changes in civilian behavior or officers' motivation to retaliate against public criticism. Hence, if politicians' voting behavior actually captured changes in citizens' opinions about the police, the estimates may be the result of simultaneity bias, where policing is a function of public outrage, civil disorder, and mistrust of officers rather than politicians' votes. To assuage these concerns, I geocoded all police-related protests in NYC since January 2020.[33] This data provides an imperfect, yet valuable fine-grained measure of citizens' outcry about policing in NYC over time. Figure A8 depicts the location of the 1,989 protests that occurred between January 2020 and June 2021. While there seems to be some clustering of protests among non-aligned districts, especially outside of Manhattan, the figure also indicates that protests took place across the entire city. Table A8, in turn, shows estimates of the DiD model after accounting for the daily number of anti-police protests in a district and precinct in various ways. Reassuringly, the treatment effect estimates remain robust to this alternative explanation of increases in

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

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

911 response times.

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

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

To distinguish these accounts, I leverage councillors' different reasons to vote "no" on the budget. While eight council members opposed the budget cut because they perceived it to be too much, nine councillors voted against the budget because they thought the reductions to the NYPD's funding were not enough. Incumbents and voters in "no, not enough" areas tend to be more progressive than in "no, too much" and "yes" voting districts; they have a majority non-white population and more than 85% of them supported Biden in the 2020 pres-

idential election on average (see Table A9). Figure A9 shows the treatment effect estimates from the triple DiD design after disaggregating different types of "no" votes. The estimates suggest a non-monotonicity in the treatment effects along the intensity of non-alignment: While there is a positive, albeit small and insignificant treatment effect for "yes" vs. "no, too much" districts, the differences in response time trends between "no, not enough" and "no, too much" districts is (imprecisely) estimated to be *negative*. The difference in these treatment effect estimates is significant at conventional levels, suggesting that the main results in Table 1 are mainly driven by differences in response times between "yes" and "no, not enough" districts. Taken together, while this non-monotonicity is difficult to square with the morale and avoidance mechanisms, it is consistent with politically motivated shirking and the fact that shirking may have divergent implications for law enforcement depending on voters' latent preferences towards police and officers' ability to use politicians as scape goats.

8 Conclusion

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

This study explains why and when police officers in cities like Seattle reduce their effort in responding to citizens' calls for service. I have argued that bureaucrats can – under certain conditions – leverage their influence on public policy to exercise power over the political authorities to whom they answer. By strategically and collectively shirking their duties in certain areas, bureaucratic agents can protest unwanted policy choices, exert pressure on political authorities, and affect the policies they make while in office. As I have argued, bureaucrats' willingness and capacity to exercise such political power largely depend on the degree of preference misalignment with their political principals as well as their unionization

status and tenure protection. Focusing on municipal police and using data on 911 response times of NYPD officers as a case study, I find empirical evidence that largely supports this view. Relative to the FDNY response times and compared to calls in FY2020, NYPD officers took about 90 seconds longer to respond to calls in city council districts that had voted for the $1 billion cut to the NYPD budget – a policy that police unions in NYC heavily condemned.

This research provides new insights into issues of political representation and interest groups within local government. To the best of my knowledge, this is the first study to exploit exogenous variation in the preference alignment of bureaucrats and politicians to study bureaucrats' role as organized interest groups within government. Moving beyond the anecdotal evidence on police unions' involvement in local politics, this study highlights the importance of political interests and power to explain police behavior in US cities.

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

Lastly, by raising questions about who is controlling whom in politician-bureaucrat relationships, this study also has important implications for our understanding of principal-agent dependencies between elected authorities and their bureaucratic agents. To be sure, this study does not provide evidence for the claim that elected government is run by a "deep state" of embedded, biased bureaucrats who work to thwart legitimate political agendas. Prior research has repeatedly shown that civil servants in the American bureaucracy are,

for the most part, individuals dedicated to serving the public and tend to be more responsive than resistant to changes in political administrations (Brehm and Gates, 1997; Golden, 2000). Rather, the argument and analysis presented here aim to characterize the dynamics that *can* give rise to bureaucratic resistance.

While the study focuses on a single city employing the largest police force in the US, similar dynamics of police resistance likely apply in many other US cities and municipalities. Beyond NYC, 45% of each state's largest cities reduced the share of their police budget for fiscal year 2021, with cuts to the police budget ranging up to 12.1% and 9.7% in Albuquerque and Seattle, respectively.[34] In light of the strong police unions in these major cities and their open resistance to these budgetary changes, the bureaucratic power play and reduced public service of police forces shown in this study likely represents a broader phenomenon across US cities. Using data from a larger set of US cities, future work may address this conjecture explicitly. It is also worth testing the broader applicability of my theory across types of bureaucracies. For instance, subsequent work might study whether and how progressive teachers and their unions resisted recent restrictive policies of local school boards, such as book bans and educational gag orders.

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

Second, this study remains agnostic about the broader welfare implications of politically motivated behavior of police. Although work slowdowns and worse public service delivery

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

reduce the public utility of citizens calling for help, these reductions might be offset by utility increases for individuals subject to police interventions. If work slowdowns are clustered in overpoliced areas, the net impact of police shirking might not be negative overall. I leave these considerations for future research.

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

A Data Description

Table A1: Description of Data Sources

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

Continued on next page

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

B Tables

Table A2: Summary Statistics - Covariates by Voting Behavior

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

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

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

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

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

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

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

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

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

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

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

Table A6: 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 A7: Call Distance to NYPD Precinct Headquarters
RDD Sample (within 200 meter bandwidth)

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

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

Table A8: Effect of Approving 2021 Budget on 911 Response Times
Accounting for Protests

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

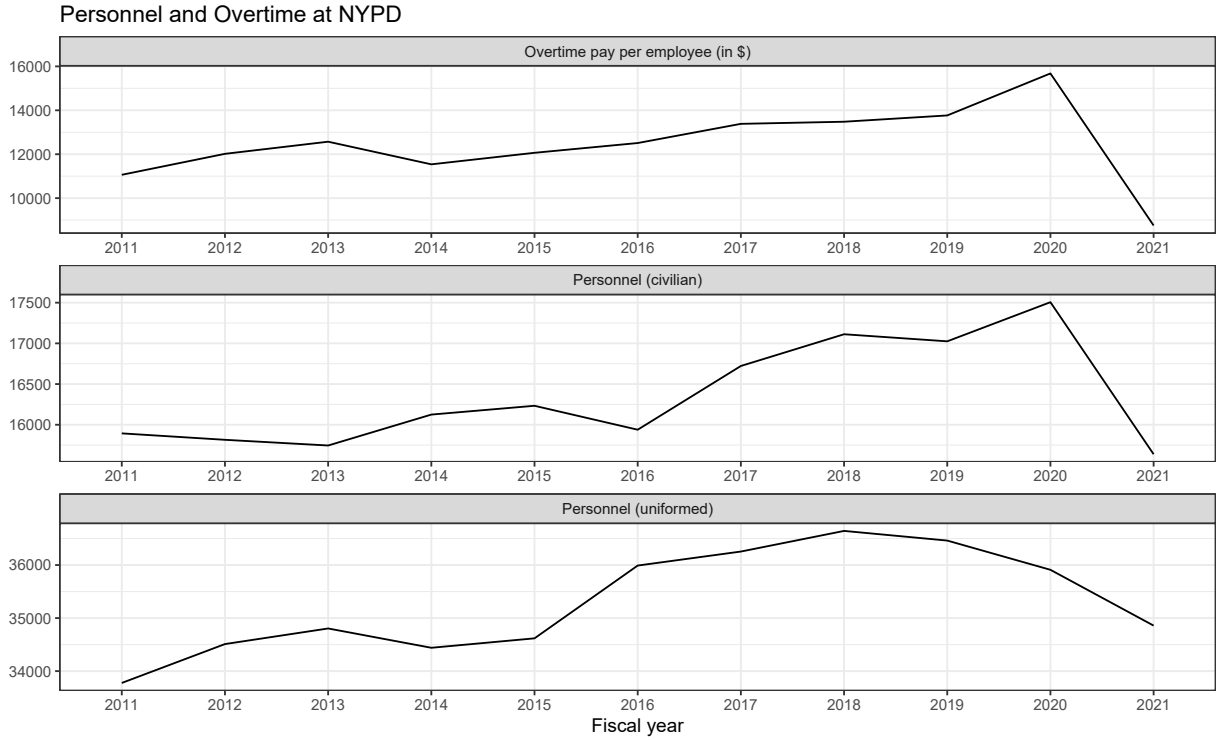
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Dependent variable: Response time in minutes. Coefficients for yes vote_c and after vote_d absorbed by district and day fixed effects, respectively. Cluster robust standard errors in parentheses, by district (49).

Table A9: Summary Statistics - Covariates by Voting Behavior

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

C Figures

Figure A1: 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. Figures for FY2021 are not yet final.

Figure A2: Balance of Matched RDD Sample - Major Call Types

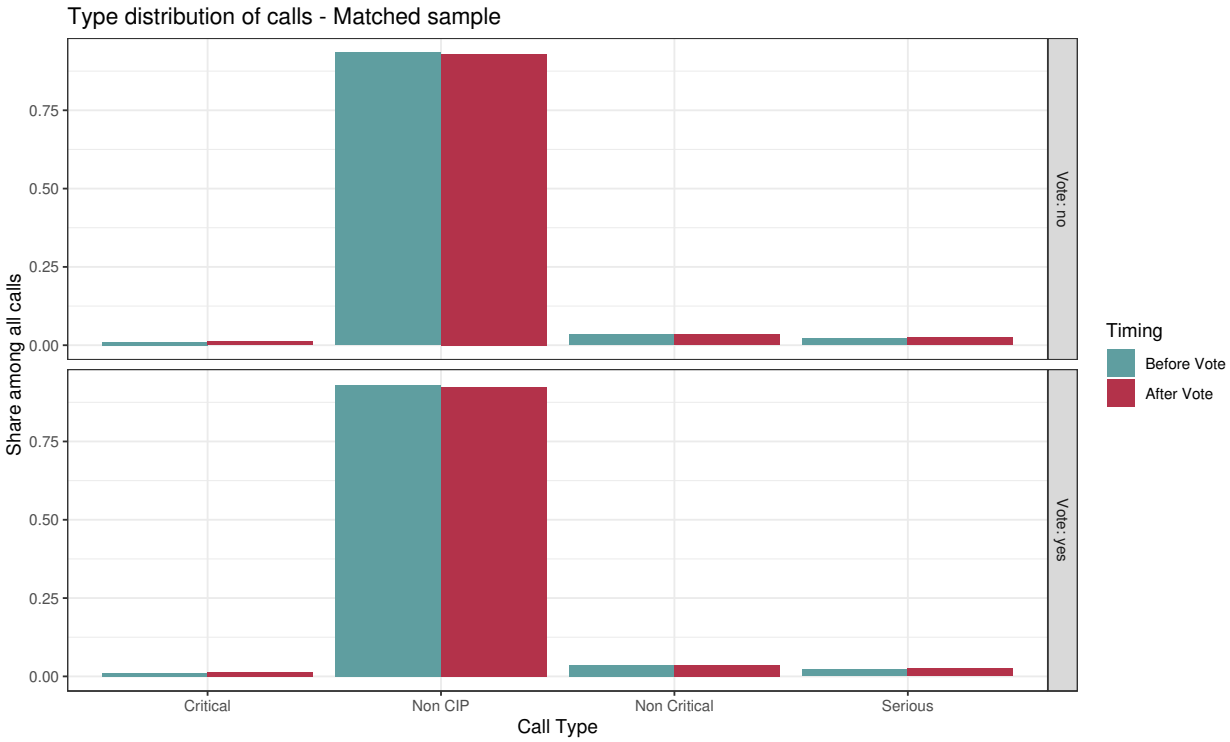


Figure A3: Balance of Matched RDD Sample - Daily Call Volume by Zip Code

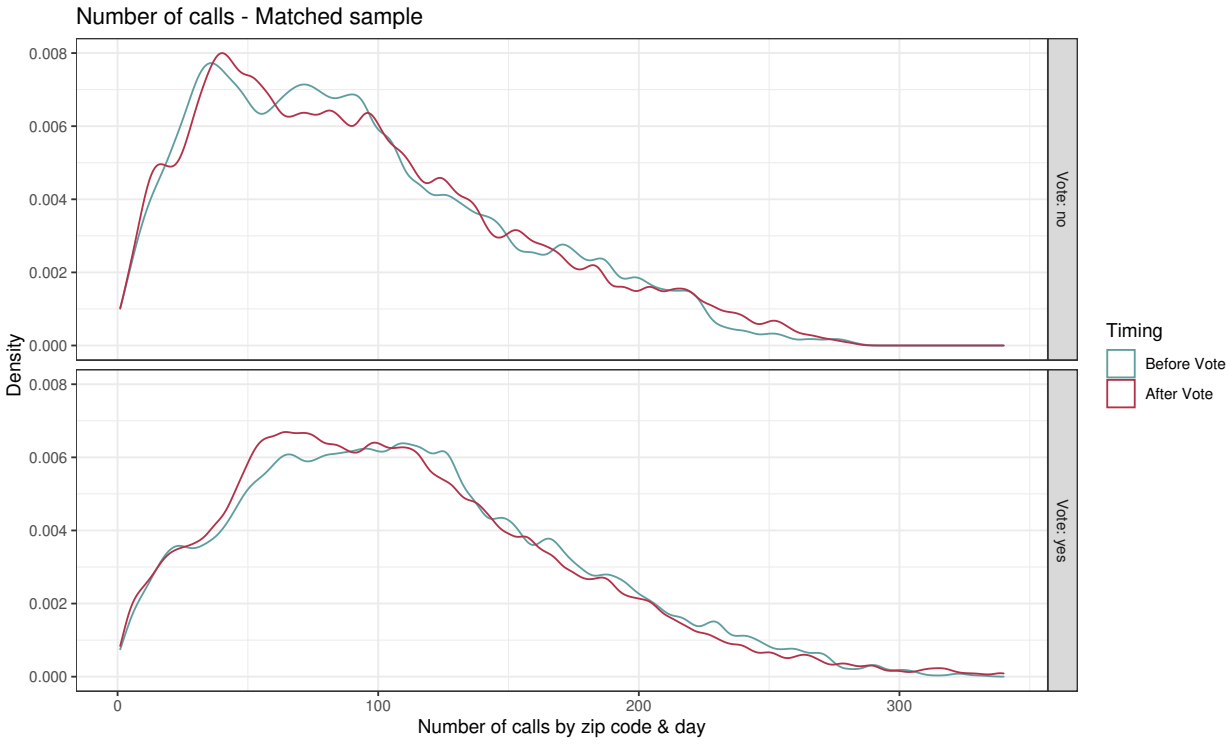
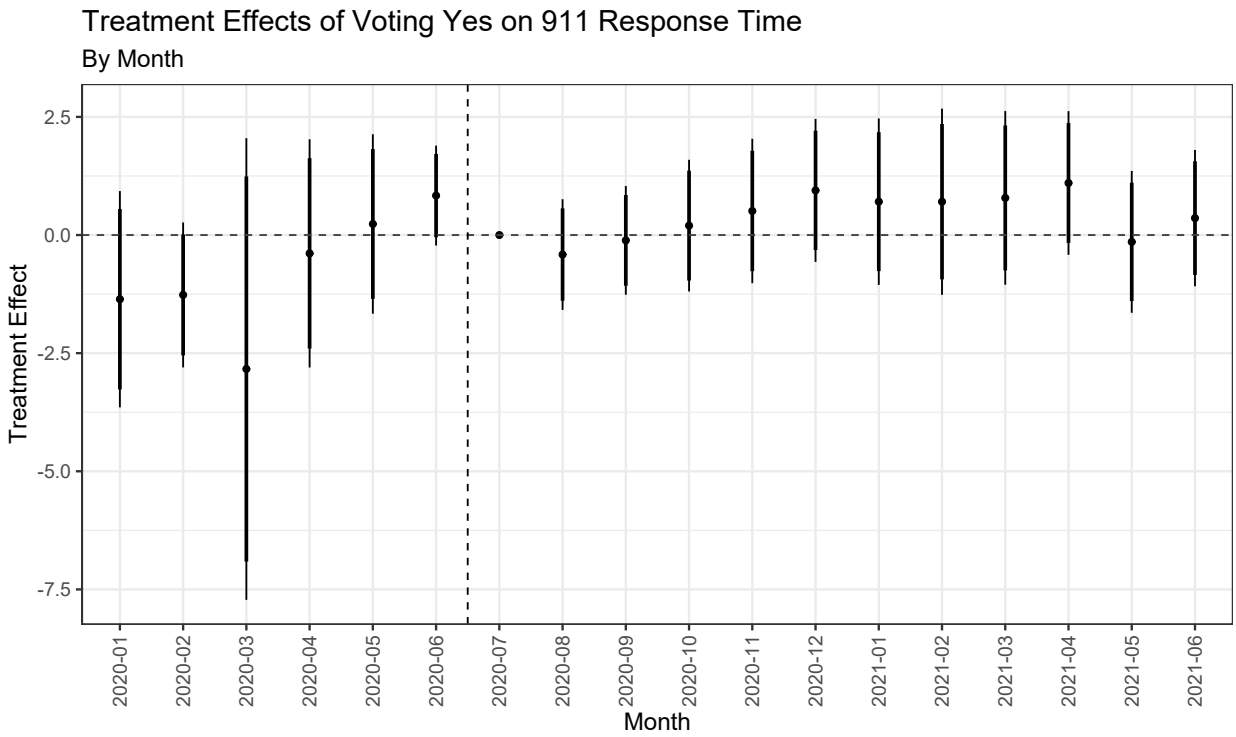


Figure A4: Monthly Treatment Effects



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

Figure A5: Average Response Times across Call Types, Districts and Periods

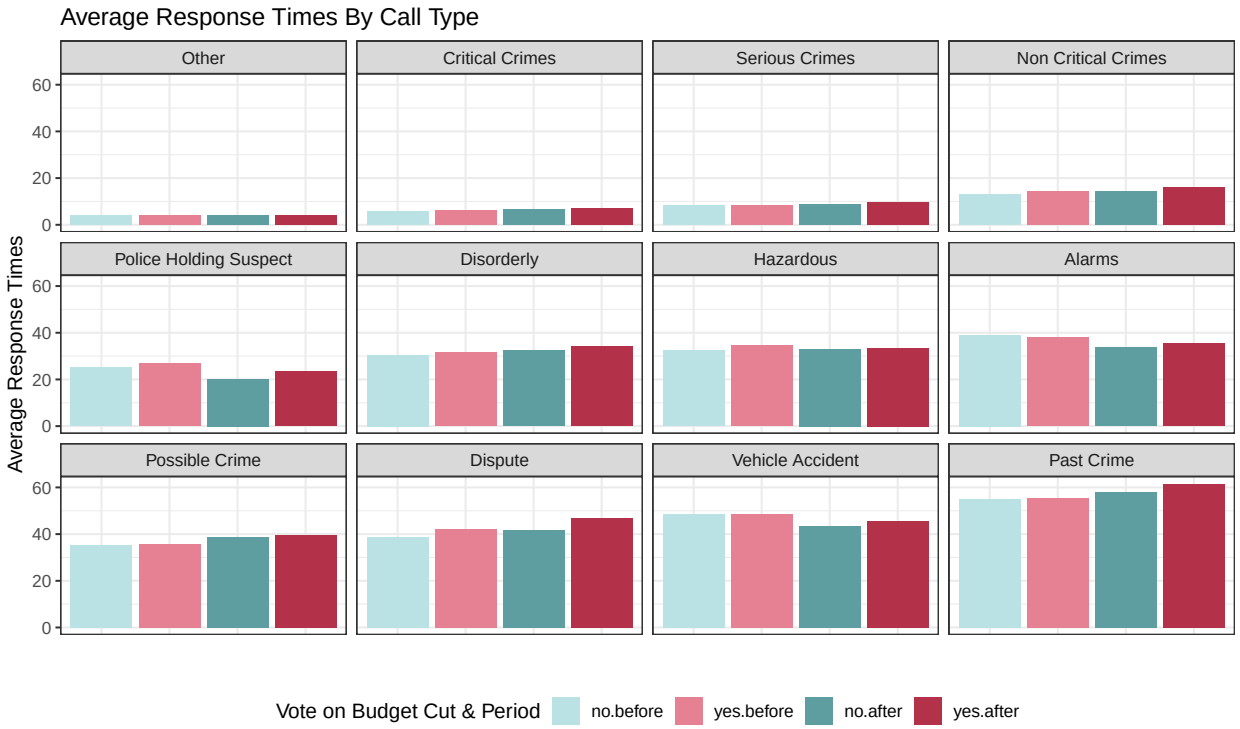


Figure A6: Trends in Amount of 911 NYPD Calls across Districts

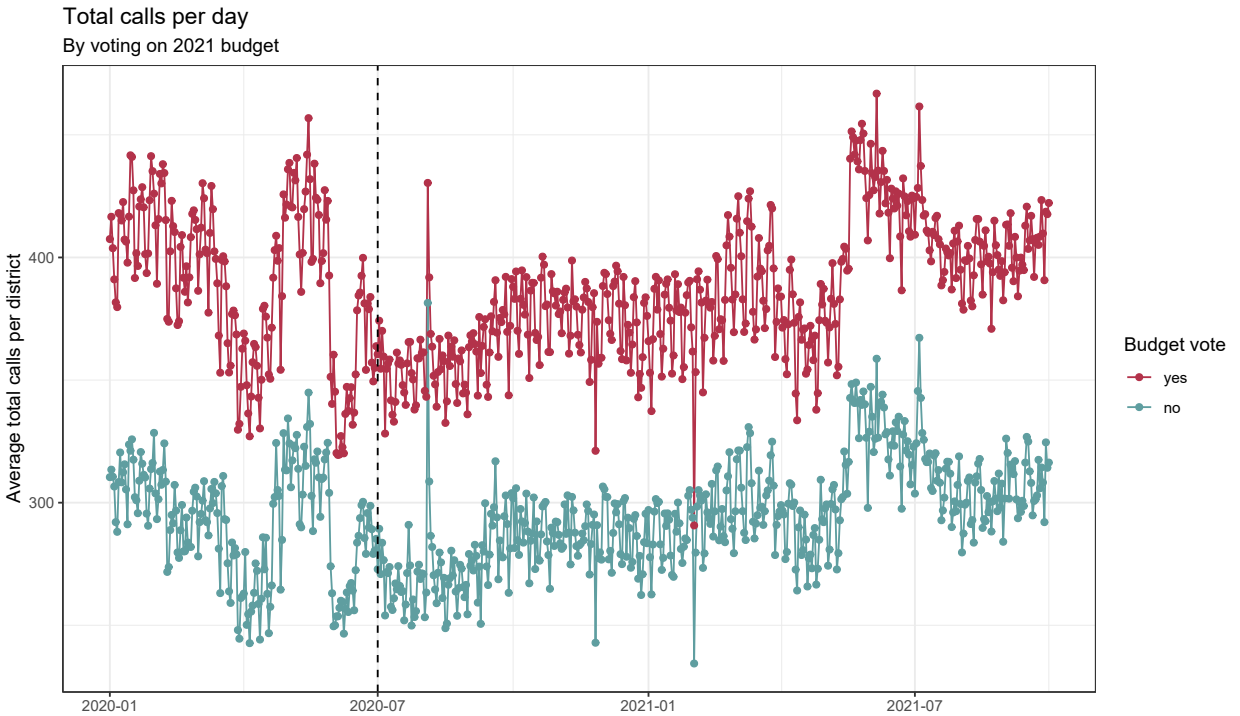


Figure A7: Distribution of 911 Call Types, by Period and District

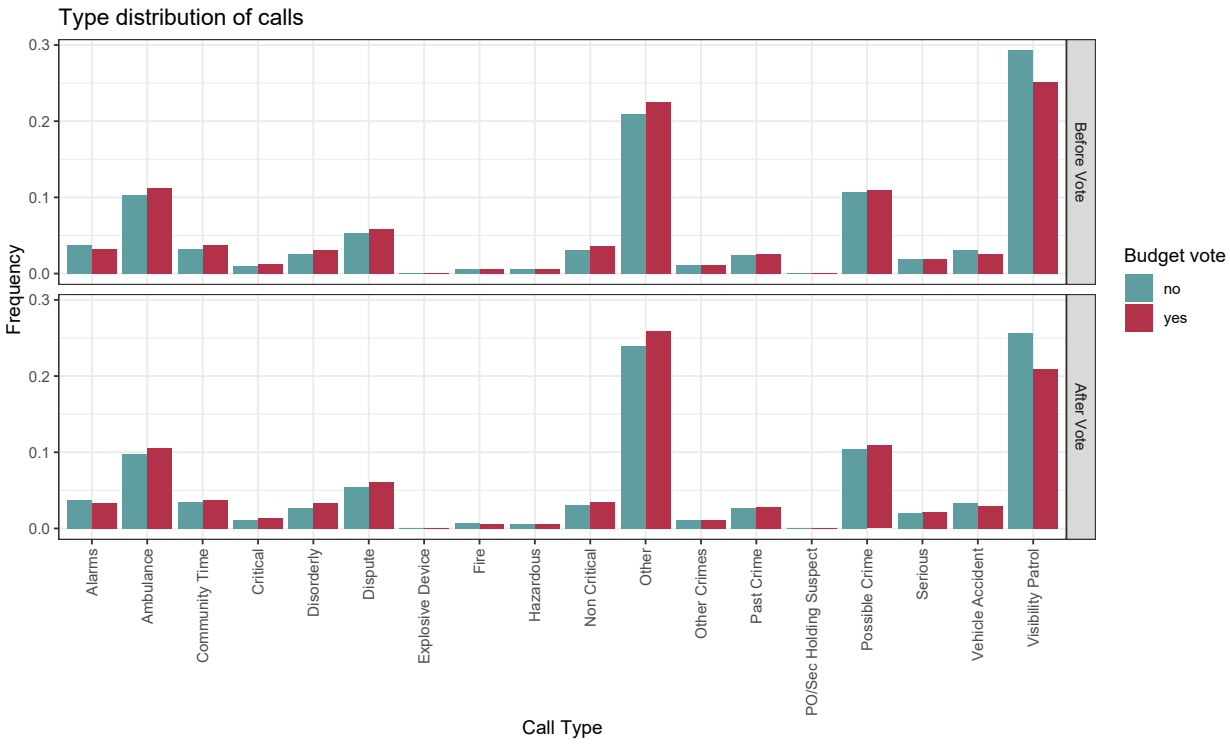


Figure A8: Location of Police-Related Protests

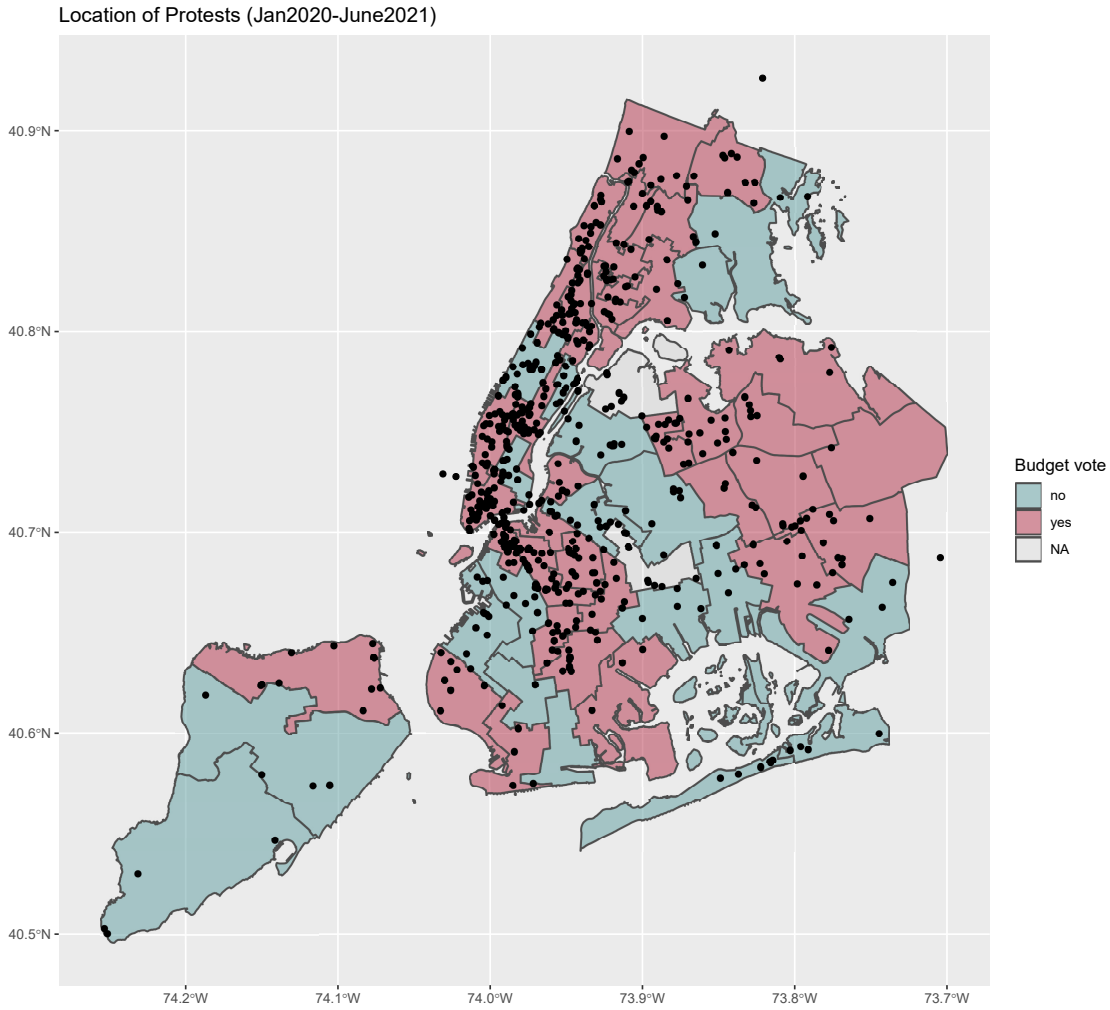
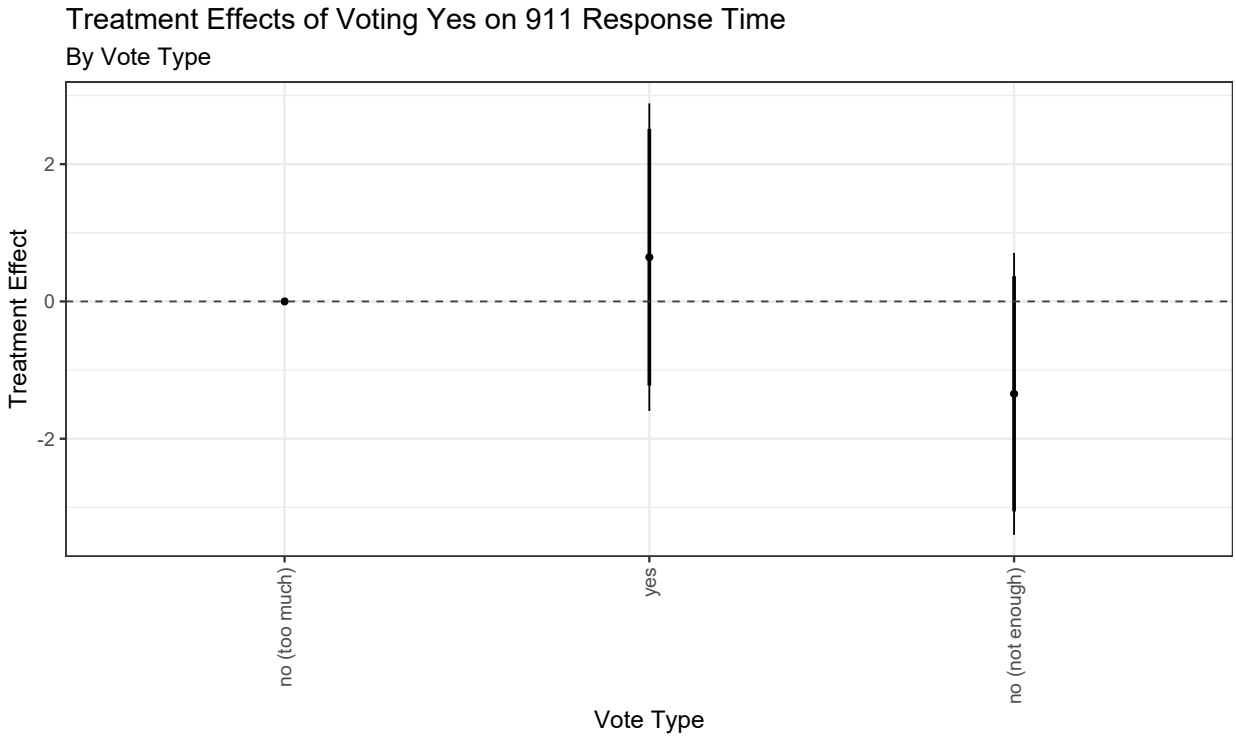


Figure A9: Disaggregating Treatment Effects by No Votes



D Impact on Candidate Vote Share

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

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

I then estimate the following first-difference model:

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

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

The results in Table A10 suggest that approving the 2021 budget cut was indeed associated with a reduction in council member's vote shares. In the Democratic primary elections, where most of the electoral competition takes place in NYC, incumbents who supported the budget cut lost 33 percentage points more than council members opposing the substantial cut. In fact, two of the seven council members in favor of the budget cut in this sample lost their primary elections all together – a rare event for incumbents in NYC's Democratic primaries. Given the important caveats of this analysis, these estimates do not allow for causal inferences. Yet, they provide some correlational evidence that council members who acted contrary to police interests during the 2021 budget vote might have incurred some electoral costs in the upcoming city elections.

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

Table A10: 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).