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

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September 2022

Abstract

Research on public organizations highlights how political control of bureaucracies induces bureaucrats to act as electoral brokers for politicians. In contrast, I study the converse mechanism in professionalized bureaucracies. I argue that politicians' dependence on bureaucratic effort for re-election purposes allows politically insulated 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 their principals, they can collectively shirk effort to affect the electoral fortunes of politicians. To test this theory, 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 municipal police reduced effort in districts of non-aligned politicians. This study informs the theoretical debate on principal-agent relationships in government and sheds light on the importance of organized political interests to explain policing in US cities.

*I am particularly grateful to Sanford Gordon, Anna Harvey, Julia Payson, Tara Slough and Hye Young You for their continuous guidance and support throughout this project. I also thank Jennifer Gaudette, Byeonggeun Heo, Kyuwon Lee, Porey Lin, Alexis Palmer, Carolina Torreblanca as well as participants of the WINNING summer workshop, the NYU Graduate PE workshop, the NYU Pre-dissertation seminar, the NYU Methods workshop and EPSA 2022 for their helpful comments and suggestions. Paige Reddick provided excellent research assistance.

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1 Introduction

During the pandemic, local governments in many cities across the US found themselves in contentious public clashes with law enforcement unions over requirements that officers receive COVID19 vaccines or prove their vaccination status. 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 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).

What explains such bureaucratic resistance to their elected superiors' policies? In this paper, I examine how bureaucrats' and their political principals' divergent policy preferences incentivize bureaucratic agents to protest against unwanted policy changes. I argue that misaligned preferences along with strong tenure protections induce bureaucrats to take collective action to put pressure on political authorities. In systems with high levels of political control bureaucrats can be incentivized to act as electoral brokers for politicians by working harder and actively boosting politicians' chances of re-election (Golden, 2003; Iyer and Mani, 2012; Berenschot, 2018; Pierskalla and Sacks, 2019; Brierley, 2020). In the case of strong tenure protections for bureaucrats, in contrast, bureaucrats can try to pressure political actors by shirking duties. If bureaucrats differ in their collective preferences from elected

politicians and are insulated from political control, they can leverage politicians' dependence on bureaucratic effort to exercise political power over their elected principals (Moe, 2006, 2011; Ujhelyi, 2014). Several studies have examined the conditions under which bureaucrats resist the intentions of political authorities through shirking or sabotaging policy (Brehm and Gates, 1997; Golden, 2000; Shinar, 2012; Nou, 2019). Yet, to the best of my knowledge, little work has considered how shirking is used as a way to exert political pressure on elected principals, thus turning the 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 generally well organized in dense, cohesive unions that tend to be strong advocates of law-and-order policies and have long opposed police reforms. Anecdotal evidence suggests that police unions aim to influence local 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, affect public policy, and influence local elections.

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 councillors 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 rely on response times of fire fighters to 911 medical emergency calls to account for time-specific trends in response times across districts (e.g., due to differences in traffic levels). Fire fighters 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 fire fighters 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 a spatial difference-in-discontinuities setup, 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 than for FDNY calls after the budget vote. The size and precision of this treatment effect is largely robust to further accounting for the demand for police presence, police-related protests, as well as net migration to and from NYC. Additionally, supplementary analyses suggest that the effect is driven by calls where police have more discretion, including alarms, previous crimes, and vehicle accidents; and that increases in response times are clustered among councillors unaffected by term limits.

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 (Golden, 2003; Iyer and Mani, 2012; Berenschot, 2018; 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; Akhtari et al., 2020) and bureaucrats' efforts to implement policy (Nath, 2018; 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; Dahlström et al., 2012; Charron et al., 2017). 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 (Chen and Johnson, 2015; Doherty et al., 2018, 2019; 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, 2014; Anzia and Moe, 2015, 2019). The modern interest group literature contains numerous studies on how groups attempt to influence bureaucracy from the outside through lobbying (Yackee and Yackee, 2006; You, 2017; Gordon and Rashin, nd) or dense personal and professional networks (Adolph, 2013; Carpenter and Moss, 2013). Yet, little work – with the exception of scholarship by Sarah Anzia and Terry Moe (Anzia and Moe, 2015, 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; Soss and Weaver, 2017; Ba et al., 2021), few scholars study police as a political institution within government, accountable to and incentivized by other governmental actors (Soss and Weaver, 2017; Cook and Fortunato, nd). 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 (Weingast, 1984; McCubbins and Schwartz, 1984; McCubbins et al., 1987; 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 (Weingast, 1984; McCubbins and Schwartz, 1984; Ujhelyi, 2014). 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 a system 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 incumbents' electoral success and signal their loyalty – a fact politicians have exploited for their own electoral gains (Golden, 2003; Ujhelyi, 2014; Berenschot, 2018; Nath, 2018; Brierley, 2020).

In systems of professionalized, meritocratic bureaucracies, in contrast, the incentives of politicians and bureaucrats may diverge. 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 when implementing political decisions. Most existing principal-agent models center on how political authorities can devise institutional arrangements in these settings, including ex ante monitoring (McCubbins et al., 1987, 1989) and ex post control (Weingast, 1984; McCubbins and Schwartz, 1984) of bureaucrats, to minimize such agency losses and limit bureaucratic discretion. Yet, these top-down theoretical accounts largely ignore features of democratic government that make delegation relationships in professionalized bureaucracies highly distinctive (Moe, 2006). In particular, they disregard the fact that political principals in a democratic setting are *elected* and thus vulnerable to the behavior of bureaucrats (Moe, 2006, 2011). Hence, while bureaucrats might work harder to get a politician re-elected in systems of political patronage, I argue that the converse can hold in professionalized bureaucracies: If tenure protections for government employees are high, bureaucrats can attempt to lower the re-election chances of incumbents and thus actively put pressure on political principals without worrying about retaining their jobs (Ujhelyi, 2014).

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 (Anzia and Moe, 2019) or collective bargaining (Anzia and Moe, 2015). Additionally, unlike interest groups outside government, bureaucrats can adjust their work effort as a cost-effective 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' behavior vis-à-vis politicians' actions. This imperfect information allows bureaucrats to sabotage the public payoff by reducing their effort to damage the reputation of non-aligned politicians and thus jeopardize their re-election intentions.[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 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.

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.[2] First, bureaucrats need to develop mechanisms to overcome collective action problems in their strategic behavior. Public sector unions often serve this purpose. Public employees organized in unions can more easily coordinate their behaviors and resources, and often share common political interests, both of which help them pursue their political goals (Moe, 2006). Additionally, it reduces 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 conse-

[1] Note that if voters can perfectly attribute poor service quality to shirking bureaucrats, they would never elect politicians who are non-aligned with bureaucrats. Hence, in equilibrium no policy conflict arises and shirking becomes unnecessary.

[2] 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.

quences, such as job loss. This highlights another 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 and influence elections on the subnational level. Additionally, the theory depends on the observability of bureaucratic performance. 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 (Lipsky, 1980), such as police officers or social workers. Lastly, strategic shirking is only observable if slacking – as opposed to investing costly effort – is not the default behavior of bureaucrats. Existing research offers three main reasons for this. First, public sector employees are often found to exert effort without significant monetary incentives, because they tend to be intrinsically motivated to perform (Wilson, 1989; Brehm and Gates, 1997; Gailmard and Patty, 2007; Prendergast, 2007). More importantly, 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). Finally, if bureaucrats engaged in constant shirking, this behavior would lose its valuable signaling effect.

One might challenge this argument on the grounds that it views local bureaucrats as relatively sophisticated political actors. Yet, a growing literature on local interest groups shows that bureaucratic unions are among the most active interest groups and wield substantial influence on city budgets and municipal elections (Moe, 2011; Anzia, 2022). Nevertheless, one could propose a simpler version of my argument in which local bureaucrats shirk in response to unwanted policy change simply to retaliate against responsible politicians rather than to exert political pressure. Yet, as long as politicians care about service provision for their

constituencies in part because of re-election concerns, electoral dynamics are inherenlty part of the logic – even in the face of bureaucrats' less strategic behavior. Similarly, one might contend that shirking street-level bureaucrats more likely push back against unwanted views and behavior of their *clients* rather than indirectly target non-aligned politicians. However, citizens have been shown to attribute insufficient public services to their elected representatives (Ujhelyi, 2014; Arnold and Carnes, 2012), which in turn incentivizes politicians to ensure high quality public service for their constituents. Hence, even if local bureaucrats have less strategic intentions vis-à-vis their elected officials than I argue, politicians necessarily become a crucial player of the game.

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. While initially controlled by local partisan machines, 20^{th}-century reformers increasingly aimed to establish departments as expert-led bureaucracies independent from political pressures (McCall, 2021). Besides this professionalization and formal independence, the nature of policing further reduces 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 turbulent with situations requiring officers' individual choices (Worden and McLean, 2017).

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).[3] 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 transgressions (Worden and McLean, 2017; Zoorob, 2019). These dense and cohesive unions make police networks particularly conducive to collective action (Zoorob, 2019).

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; Levin, 2020). 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, picketing, or asserting rights in the face of perceived discriminatory treatment (Bies, 2017), 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

[3]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; Kullgren and Iafolla, 2020)

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)

Building on my general theory and in light of their strong tenure protections, I expect the police to push back against unwanted policies and exert political pressure on non-aligned local elected officials by shirking their duties. 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 may evoke perceptions of deteriorating safety and increasing crime rates among the public. By playing with the public's fear of crime, the police can more easily 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 (Levitt, 1997; Huber and Gordon, 2004; Drago et al., 2019). Thus, I expect that the police aim to affect public policy and influence local elections by exerting lower effort in areas where incumbents run on police-reform agendas.

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 incoming 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, the FY2021 budget became a highly contentious policy issue in the New York City 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.[4]

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

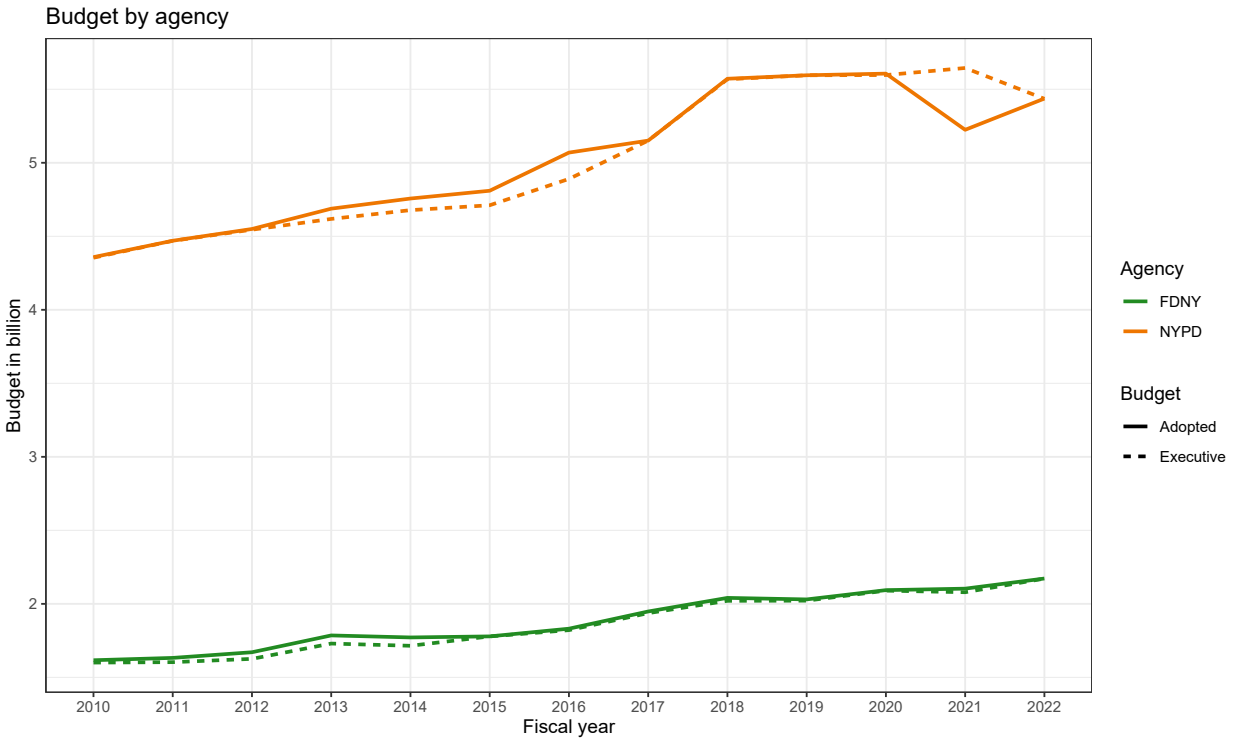
> "For decades, every time a city agency failed at its task, the city's answer was to take the job away and give it to the NYPD. If the City Council wants to give responsibility back to those failing agencies, that's their choice. But they will bear the blame for every victim, for every New Yorker in need of help who falls through the cracks. They won't be able to throw cops under the bus anymore."[5]

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 members. Additionally, they are publicly known for their inflammatory media presence and

[4] 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=

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

Figure 1: Operating Budget of NYPD and FDNY Over Time



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

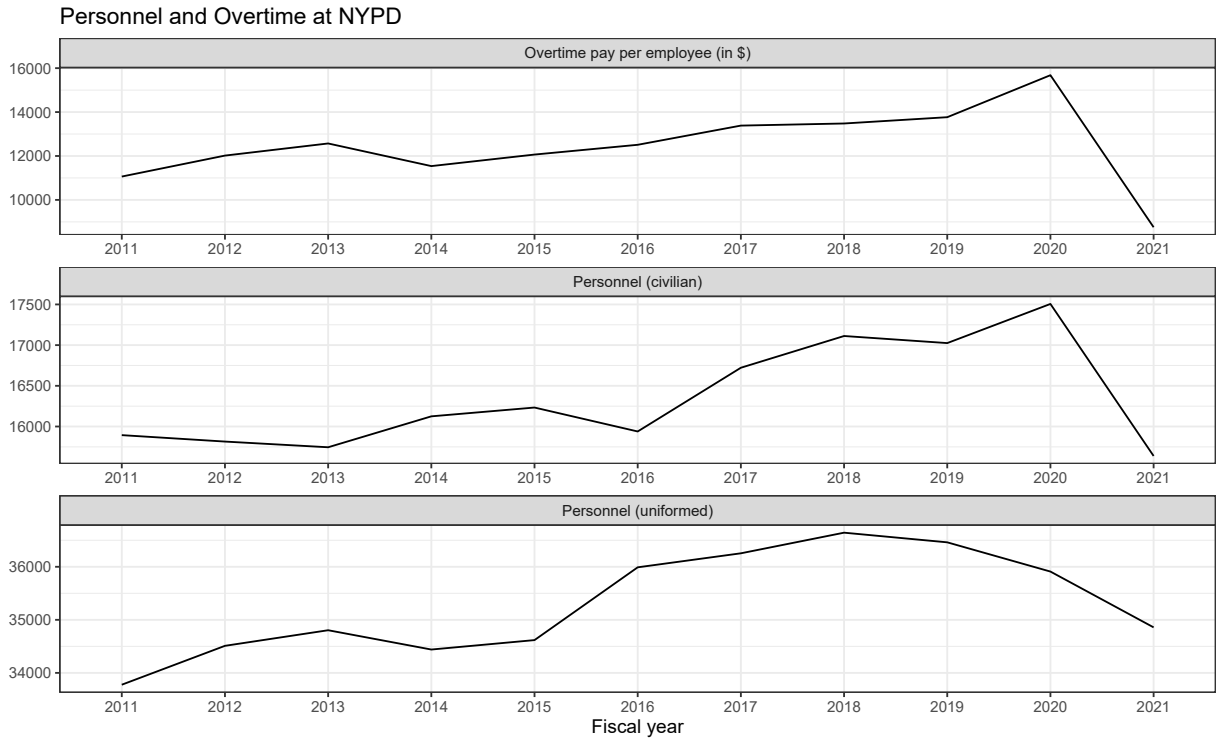
their lobbying activities to influence NYC legislation and local elections.[6]

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.[7]. Additionally, overtime spending is an important source of officers' income. In FY2020, overtime spending totaled $635 million, 44% of all citywide overtime expenses (Citizens Budget Commission, 2020) In the same year, the median share of overtime pay out

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

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

Figure 2: 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.

of total pay for NYPD employees amounted to 12%.[8] As Figure 2 illustrates, overtime pay per employee dropped by 44% 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).

[8]Calculated from FY2020 NYC payroll data, https://data.cityofnewyork.us/City-Government/Citywide-Payroll-Data-Fiscal-Year-/k397-673e

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 made 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 (Neusteter et al., 2020). 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). For instance, a 2007 study on calls for service at the Baltimore Police Department in 2000 found that officers' responses to 911 calls were often affected by their assumptions about the legitimacy of those calls based on the relatively sparse information they received from dispatchers (Moskos, 2007). Furthermore, 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; Cihan, 2014; Lee et al., 2017). Yet, no previous work has considered the effect of the *political* characteristics of neighborhoods on officers' behavior in response to calls for service. Lastly, 911 calls, especially officers' response times to calls, are related to people's perceptions of the quality of policing. Using different response time surveys across various US cities, several studies have found negative correlations between response times and respondents' evaluations of police performance (Pate et al., 1976; Parks, 1984; Priest and Carter, 1999). Additionally, some work suggests that shorter response times are associated with higher arrest rates (Cihan et al., 2012; Cihan, 2014; Lee et al., 2017; Blanes i Vidal and Kirchmaier, 2017). Besides these more substantive reasons, there are some 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.[9]

4.3 Council Members' Voting Behavior

Figure 3 shows the distribution of council members' voting behaviors on the budget proposal across New York City's 51 council districts.[10] The map illustrates that both "yes" and "no" votes are fairly evenly distributed across the city, and districts with opposite voting patterns share a border in several instances. This allows me to compare changes in 911 response times across districts *within* police precincts. Since police precincts and their commanding officers differ in management practices, it is important to move beyond this administrative level to study the behavior of police officers.

To provide some information on possible factors influencing a council member's voting behavior, Table 1 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.

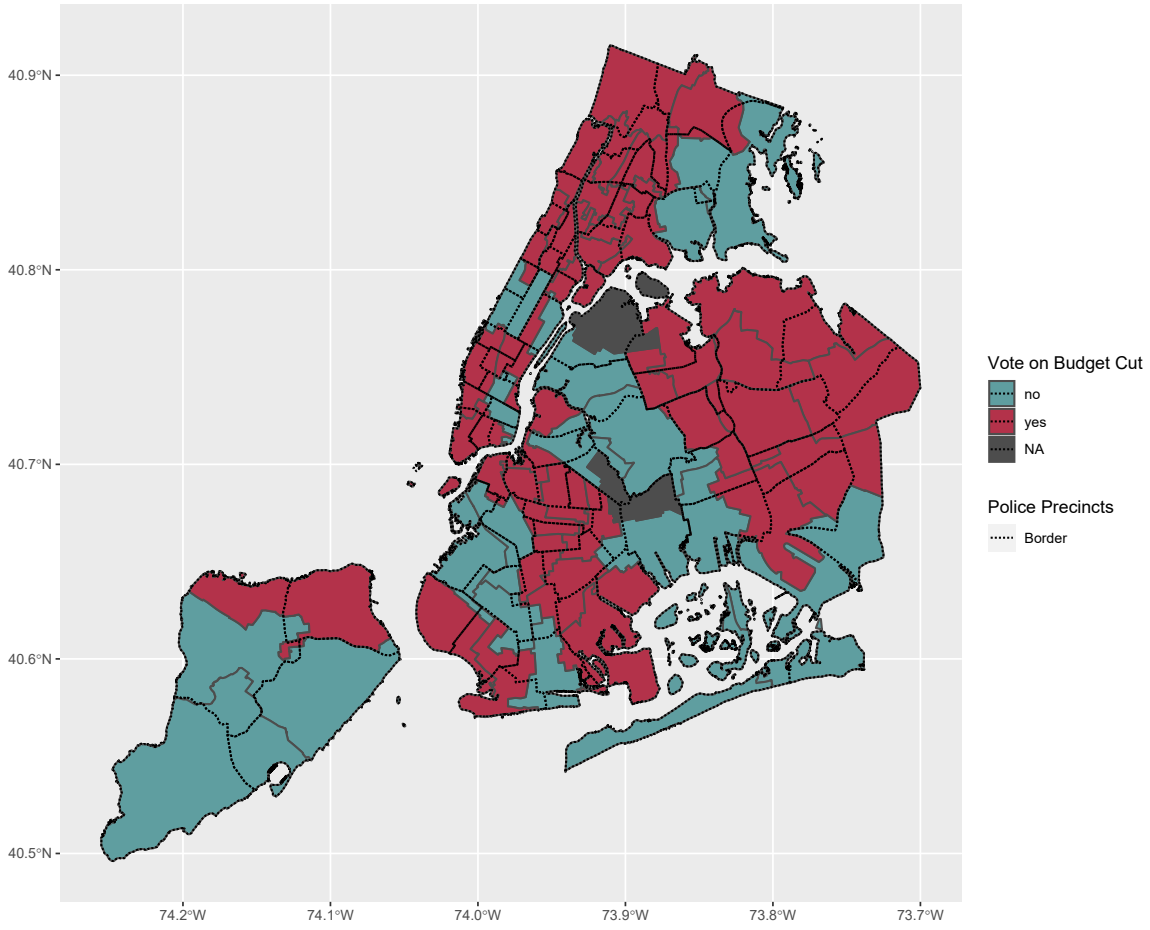
4.4 Triple Difference-in-Differences Design

To causally 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

[9]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.

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

Figure 3: NY City Council Votes on 2021 Budget



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 fire fighters to 911 medical emergency calls as my third control dimension.

Table 1: 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.

Fire fighters 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 fire fighters 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:

$$rtime_{icpda} = \beta_1 yesvote_c \times postvote_d \times NYPD_a + \mathbf{X'_{icpda}}\rho + \delta_c + \eta_p + \gamma_d + \nu_a + \varepsilon_{icpda} \tag{1}$$

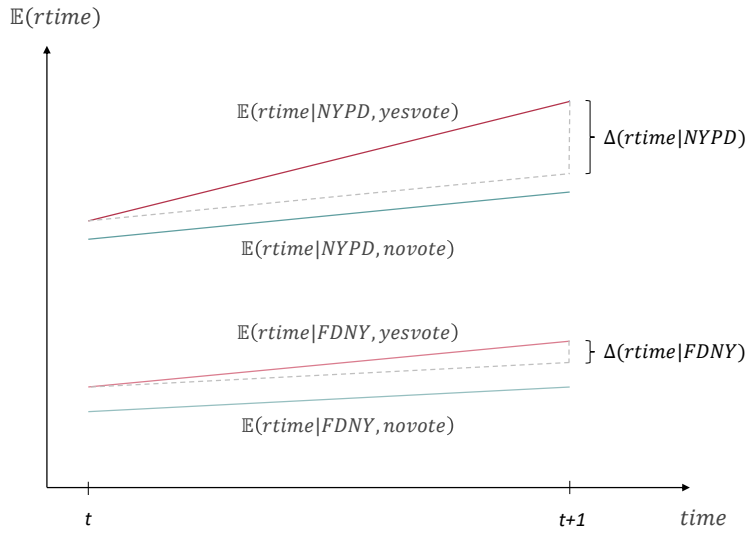
where $rtime_{icpda}$ is the response time of call i in district c, day d and agency a, $yesvote_c$ is an indicator equal to 1 if council member of district c voted in favor of the budget cut, $postvote_d$ indicates whether a call happened after June 30, 2020 and $NYPD_a$ indicates whether the NYPD or the FDNY responded to the 911 call. $\mathbf{X_{icpda}}$ is a vector of covariates, including the total number of calls per day and other proxies for crime rates and demand for police presence. δ_c, η_p, γ_d and ν_a are district, police precinct, date and agency fixed effects, respectively. Importantly, police precinct fixed effects alleviate concerns that the estimated treatment effect is driven by mechanical changes in the number of available patrol officers due to reductions in staffing or voluntary retirements. Since such police patrol management is organized on the police precinct level, the fixed effects account for possible level changes in staffing and remuneration after the budget vote. Additionally, district fixed effects account for differences in district characteristics (see Table 1). To the extent that these characteristics and their influence on NYPD response times remain constant across my sample period, my treatment effect estimates remain unbiased. Yet, these traits may provide important alternative explanations for my results, if a district's budget vote is compounded with these additional characteristics. I specifically address such concerns in Section 7. I cluster standard errors ε_{icmd} on the district level. 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. Figure A1 and Table A2 present trends and estimation results including these strong outliers, showing that the main results largely hold with the full sample of calls.

Figure 4 provides a graphical representation of the DiD identification strategy. Figure (a) represents the logic of the triple DiD design. 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. $\Delta(rtime|NYPD)$), 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 (i.e. $\Delta(rtime|NYPD) - \Delta(rtime|FDNY)$). 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. Figure (b), in turn, illustrates how the precinct fixed effects account for changes in response times due to reductions in resources (e.g. number of precinct staff, overtime, etc.). Crucially, precinct fixed effects ensure 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. Using the 19th NYPD precinct as an example, the figure makes clear that 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.

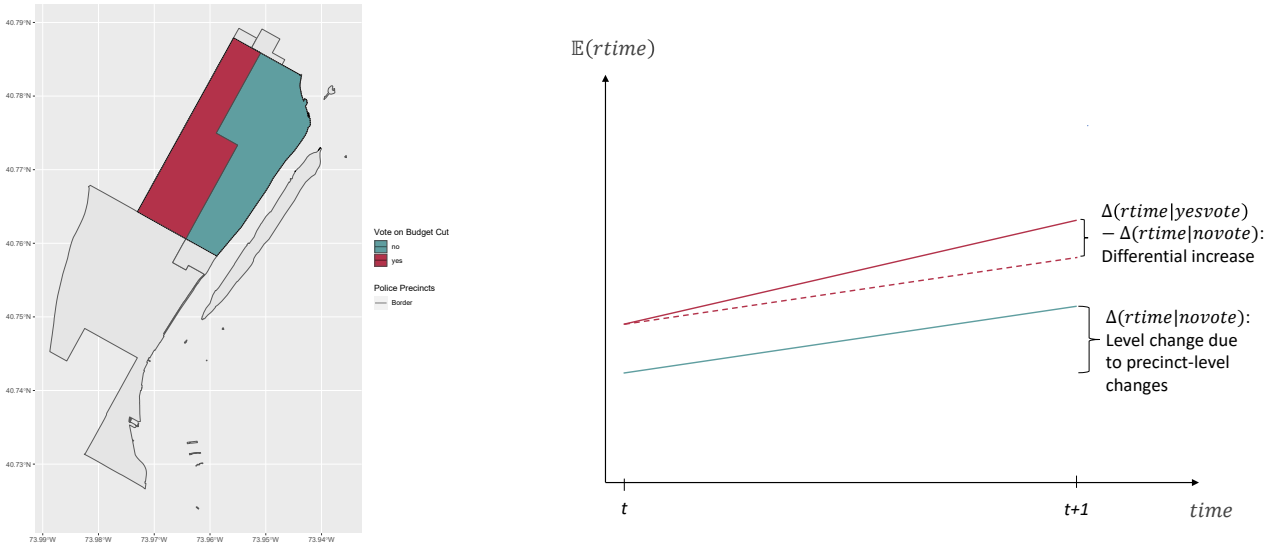
4.5 Spatial Difference-in-Discontinuities Design

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 is likely alleviated by the hightened public attention to the issue of

Figure 4: Visual Representation of DiD Identification



(a) Triple DiD strategy



(b) Within-precinct design

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 5, 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.[11] For both time periods, the resulting model is estimated as follows:

$$rtime_{icpd} = \alpha + \tau yesvote_c + \beta_- dist_{icpd} + \beta_+ yesvote_c \times dist_{icpd} + \eta_p + \varepsilon_{icpda} \tag{2}$$

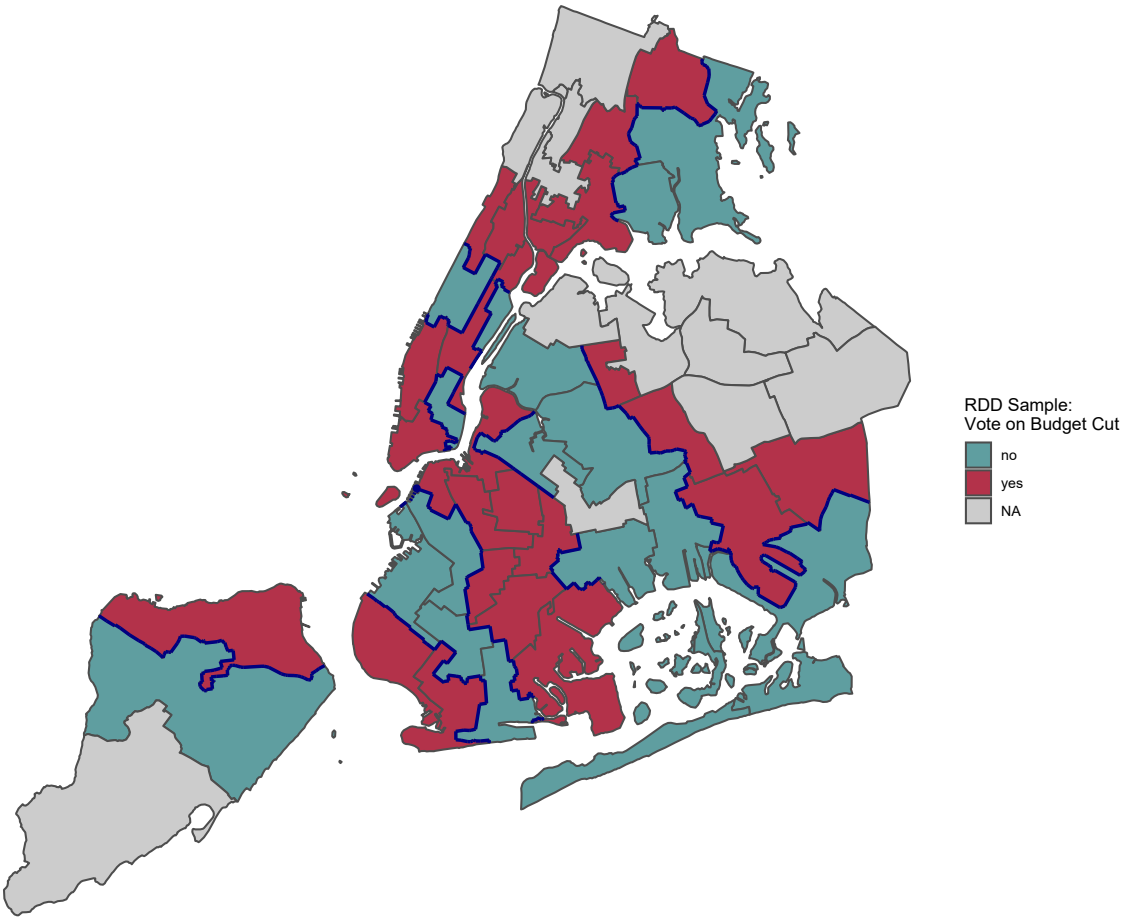
where $rtime_{icpda}$ is the response time of call i in district c, day d and agency a, $yesvote_c$ is an indicator equal to 1 if council member of district c voted in favor of the budget cut. $dist_{icpd}$ represents the distance of call i to the border distinguishing these two categories of districts, and contains only units $dist_{icpd} \in [h; h]$, 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.

A few clarifying comments are warranted. Similar to 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 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 also estimate RDD estimates where I match observations across periods using coarsened exact

[11] 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.

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.[12]

Figure 5: RDD Sample



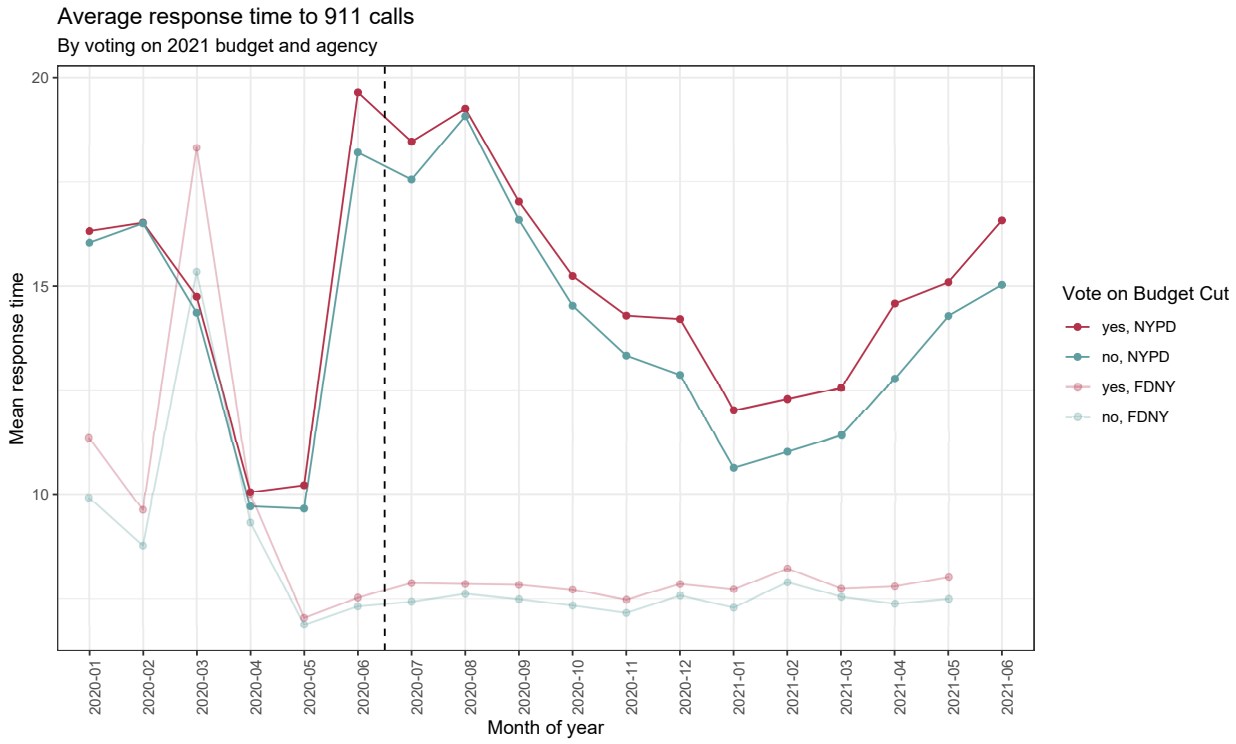
[12]Figure A4 and Figure A5 show the resulting balance in these covariates after matching.

5 Results

5.1 Triple Difference-in-Differences Design

Figure 6 depicts the 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. While NYPD response times across treatment and control districts followed largely parallel trends before the budget cut, response times started to diverge between the two types of districts following the council vote for the NYPD budget. After the FY2021 budget was proposed, districts with council members in favor of the budget cut exhibited consistently longer response times than districts whose councillors opposed the policy.

Figure 6: Trends in 911 Response Time across Districts



To assuage concerns that these divergent trends may be due to differences in non-strategic determinants of response times, such as traffic in different districts or the availability of 911 dispatchers, Figure 6 also shows the 911 response times for EMS calls. The FDNY is

responsible for these incidents and dispatches FDNY ambulances. Although response times to medical emergencies also follow similar trends across different types of districts, these differences did not seem to increase after the vote on the 2021 budget. Hence, the raw data provides some suggestive evidence that response times in non-aligned districts increased relative to aligned districts after the budget vote and more so for NYPD calls than FDNY emergencies.

Table 2: Effect of Approving 2021 Budget on 911 Response Times

	(1)	(2)	(3)	(4)	(5)
yesvote × postvote × NYPD	1.514**	1.471*	1.515**	1.508**	1.518**
	(0.735)	(0.733)	(0.736)	(0.734)	(0.737)
NYPD	4.700***	7.303***	4.702***	4.698***	4.706***
	(0.696)	(0.739)	(0.697)	(0.697)	(0.697)
yesvote × NYPD	-0.259	-0.261	-0.261	-0.259	-0.263
	(1.109)	(1.067)	(1.109)	(1.109)	(1.108)
postvote × NYPD	2.483***	2.573***	2.482***	2.491***	2.477***
	(0.473)	(0.469)	(0.473)	(0.473)	(0.473)
yesvote × postvote	-0.836	-0.877	-0.834	-0.838	-0.837
	(0.633)	(0.640)	(0.633)	(0.641)	(0.631)
total calls (log)		-1.784***			
		(0.228)			
total crime calls (log)			0.124		
			(0.173)		
# of shootings (log)				2.526***	
				(0.471)	
# of complaints (log)					0.334**
					(0.165)
District FE	✓	✓	✓	✓	✓
Police Precinct FE	✓	✓	✓	✓	✓
Date FE	✓	✓	✓	✓	✓
Observations	9,068,936	9,068,936	9,068,936	9,068,936	9,068,936
Mean of DV	13.463	13.463	13.463	13.463	13.463
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 $yesvote_c$ and $postvote_d$ absorbed by district and day fixed effects, respectively. Cluster robust standard errors in parentheses, by district (49).

Table 2 evaluates the divergent trends in police 911 response times using the triple DiD model in Equation (1). We can see 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 ($postvote \times NYPD$). 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. With an average response time of about 13.5 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 ($p = 0.045$) of this treatment effect is largely robust to further controlling for the demand for police presence (in Models (2)-(5)). Model (2) and (3) account for the total number of calls in districts and precincts per day, generally and for calls related to ongoing crimes. Similarly, Model (4) and (5) proxy demand for police presence using the total number of shootings in districts and precincts each day[13] and the number of valid felony, misdemeanor, and violation crimes reported to the NYPD.[14]

To identify the timing of the treatment effect and evaluate pre-treatment trends, I re-estimate Equation (1) in an event study setup:

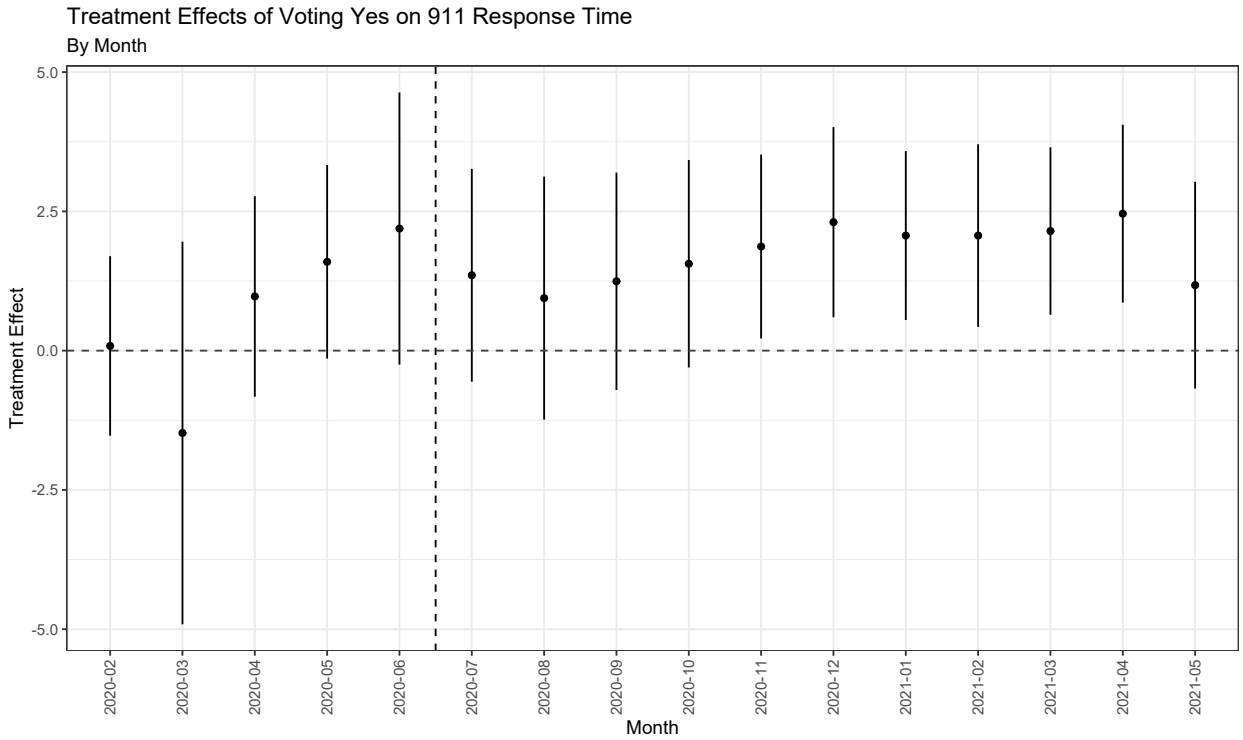
$$rtime_{icpda} = \alpha + \sum_{\tau \in [-5,11]} \beta_\tau yesvote_c \times NYPD_a + \mathbf{X'_{icpda}}\rho + \delta_c + \eta_p + \gamma_d + \nu_a + \varepsilon_{icpda} \tag{3}$$

[13] https://data.cityofnewyork.us/Public-Safety/NYPD-Shooting-Incident-Data-Year-To-Date-/5ucz-vwe8

[14] https://data.cityofnewyork.us/Public-Safety/NYPD-Complaint-Data-Current-Year-To-Date-/5uac-w243

Figure 7 shows the respective treatment effects by month. The estimates suggest that police shirking increased gradually across months after the budget vote. This time dynamic may be explained by various factors. First, some of the policy changes only materialized a few months into fiscal year 2021. For example, the budget forced cancelling the police academy class and the cadet program for July 2020, reducing the prospective number of officers by 1,381. The implications of this reduction in NYPD's headcount only unfolded once the new recruits would have been sworn in as NYPD officers six months later. Additionally, the issue of "defunding the police" gained substantial salience as the 2020 federal elections and the 2021 NYC elections approached, thus heightening officers' impetus to push back against politicians who supported such policies.[15]

Figure 7: Monthly Treatment Effects



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

In terms of pre-treatment trends, Figure 7 indicates that response times largely followed

[15]Day-specific treatment effects shown in Figure A2 depict a similar picture. There is no immediate increase in response times in non-aligned districts right after the vote on the budget cut.

parallel trends in treatment and control districts in the months prior to the budget vote, thus providing support for the identification assumption of my DiD design.

5.2 Spatial Difference-in-Discontinuities

To bolster the estimates of my main specification, Table 3 shows results for the spatial difference-in-discontinuity design. Interestingly, the negative RDD estimates in both periods suggest that NYPD officers respond faster to calls in treatment vs. control districts, 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. More importantly for my theory, the difference in the RDD estimates is positive and significant. In line with previous results, all models suggest that for neighborhoods in close proximity to the district borders NYPD slowed down by 51-56 seconds per call in yes voting districts relative to no voting districts after the budget vote.

Table 3: Effect of Approving 2021 Budget on 911 Response Times
Spatial Difference-in-Discontinuities

	Before Vote	After Vote	Difference	Before Vote	After Vote	Difference
Conventional	-2.280	-1.345	+0.935	-2.280	-1.386	+0.894
	(-2.569; -1.992)	(-1.599; -1.09)		(-2.569; -1.992)	(-1.639; -1.133)	
Bias-Corrected	-2.300	-1.410	+0.890	-2.300	-1.453	+0.847
	(-2.589; -2.011)	(-1.665; -1.155)		(-2.589; -2.011)	(-1.706; -1.2)	
Robust	-2.300	-1.410	+0.890	-2.300	-1.453	+0.847
	(-2.631; -1.969)	(-1.675; -1.145)		(-2.631; -1.969)	(-1.719; -1.187)	
Precinct FE	✓	✓		✓	✓	
Matched Sample				✓	✓	
Kernel	Triangular	Triangular		Triangular	Triangular	
Bandwidth	mserd	mserd		mserd	mserd	
BW_est	518.182	173.933		518.182	179.065	
Obs_left	599725	1412690		599725	1411730	
Obs_right	1254137	2845564		1254137	2844357	

Dependent variable: Response time in minutes. 95% confidence intervals shown in parentheses.

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. If the increase in response times is indeed a result of strategic behavior of NYPD personnel, one would expect that the effect was driven by calls where police have sufficient discretion in how they address the incident and face fewer costs for shirking. Using NYPD radio signal codes for each call, I classify non-crime incidents and non-critical calls as discretionary. These include alarms, past and possible crimes, disorderly behavior, disputes, vehicular accidents, ambulance assistance, response to complainants, traffic conditions, and visibility patrols. Non-discretionary calls, in contrast, are shooting incidents, assisting other police officers, crimes in progress, reports of suspicious substances, and roving bands. Table 4 shows split sample regressions for discretionary and non-discretionary calls.[16] There is some suggestive evidence that the overall treatment effect is mainly driven by discretionary calls. While the point estimate for discretionary calls is virtually identical to earlier results, the treatment effect is estimated to be somewhat smaller for non-discretionary calls. Yet, these differences do not reach significance at conventional levels.

Additionally, my theory delineates the re-election possibility of politicians as an important scope condition. According to my argument, the main way police officers can exert pressure on non-aligned politicians is by affecting voters' perceptions of public safety and hence their performance evaluations of policymakers. Thus, if incumbents are uninterested in or restricted from running in the upcoming election, they are much less vulnerable to the strategic shirking of law enforcement agents. To test this theoretical implication I estimate the triple DiD model separately for term-limited or retiring incumbents and for unrestricted candidates in Table 5. Twenty-seven council members were term-limited, and three councillors retired prior to the 2021 municipal elections in NYC. In line with the expectations,

[16] Figure A3 shows results for individual call types separately.

Table 4: Effect of Approving 2021 Budget on 911 Response Times
By Call Type

	(1) discretionary	(2) nondiscretionary
yesvote × postvote × NYPD	1.529**	1.319*
	(0.748)	(0.697)
NYPD	5.009***	1.926***
	(0.753)	(0.330)
yesvote × NYPD	-0.162	-0.539
	(1.207)	(0.454)
postvote × NYPD	2.473***	2.268***
	(0.480)	(0.470)
yesvote × postvote	-0.831	-0.891
	(0.635)	(0.594)
District FE	✓	✓
Police Precinct FE	✓	✓
Date FE	✓	✓
Observations	8,492,706	2,275,920
Mean of DV	13.528	9.842
Adj. R^2	0.034	0.064

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

the results suggest that the main treatment effect is primarily driven by council districts whose members could run in the 2021 elections.[17] While the effect estimate of approving the 2021 budget on 911 response times is small and fails to reach statistical significance for term-limited and retiring council members, it is substantially larger and significant for non-term-limited councillors. Yet, it is important to note that part of this heterogeneity also results from trends in FDNY response times across districts, as the strong negative coefficient for *yesvote* × *postvote* among non-term-limited council members indicates.

[17]To illustrate the electoral consequences of the budget vote and presumably the police resistance, I further provide some suggestive evidence in Appendix D that council members approving the budget cut lost votes in their electoral districts between the 2017 and 2021 elections relative to council members voting no.

Table 5: Effect of Approving 2021 Budget on 911 Response Times
By Term Limits of Council Members

	(1) non-term-limited	(2) term-limited & retiring
yesvote × postvote × NYPD	3.154***	0.498
	(0.857)	(0.983)
NYPD	5.779***	4.338***
	(1.445)	(0.773)
yesvote × NYPD	-0.308	-0.647
	(2.139)	(1.170)
postvote × NYPD	1.396***	2.990***
	(0.345)	(0.601)
yesvote × postvote	-2.653***	0.333
	(0.604)	(0.819)
District FE	✓	✓
Police Precinct FE	✓	✓
Date FE	✓	✓
Observations	3,656,398	5,412,537
Mean of DV	14.642	12.667
Adj. R^2	0.040	0.027

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

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. Particularly, if individuals in non-aligned districts were more likely 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, this pattern is unlikely to occur for several reasons. If citizens anticipate police shirking in "yes" voting districts, they are likely to call

police *less* in treatment areas than in control areas or restrict calls to very serious incidents that allow for little discretion in police responses. Similarly, recent research suggests that high-profile acts of police brutality, such as the killing of George Floyd, reduce individuals' willingness to cooperate with law enforcement, including their propensity to call 911 (Ang et al., 2021; Desmond et al., 2016; Zoorob, 2020; Desmond et al., 2020). If councillors' voting behaviors are correlated with citizens' opinions on and trust in law enforcement, individuals in treatment districts are likely more skeptical of police, thus subduing their inclinations to call 911. This logic is borne out by the NYPD call data. Figure 8 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. In fact, the gap between the call volume in "yes" vs. "no" voting districts decreases slightly after the $1 billion budget cut, thus making it harder to find longer response times in treatment districts following the policy change.[18] To further evaluate whether citizens' reporting behavior differed along the treatment dimension, Figure 9 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.

Yet, even in the absence of divergent call patterns by citizens, police may still *perceive* differences in the necessity to promptly respond to emergency calls across districts. For example, in the wake of the COVID19 pandemic, many New Yorkers left the city, particularly from densely populated and affluent neighborhoods in Manhattan and Brooklyn (NYC Office of the Comptroller, 2021). If police officers felt less pressured to rush to calls in these areas due to shrinking population density, the treatment indicator may pick up a spurious relationship since these liberal neighborhoods were also more likely to vote in favor of the

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

Figure 8: Trends in Amount of 911 NYPD Calls across Districts

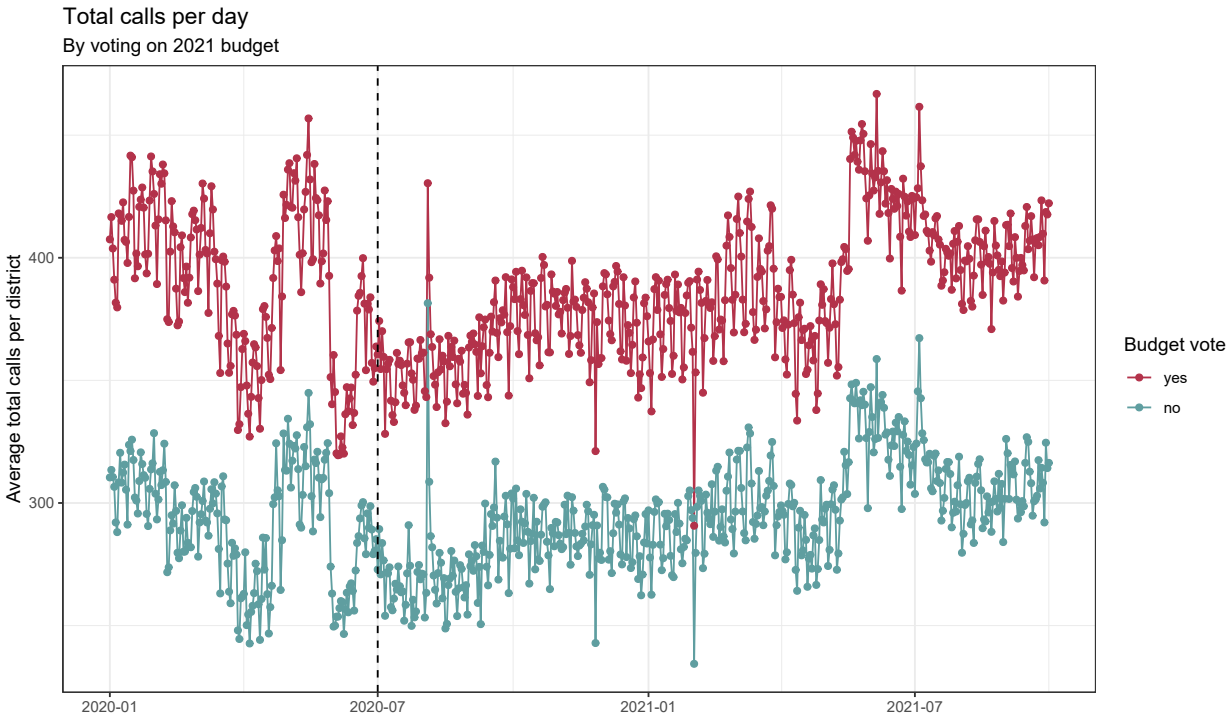
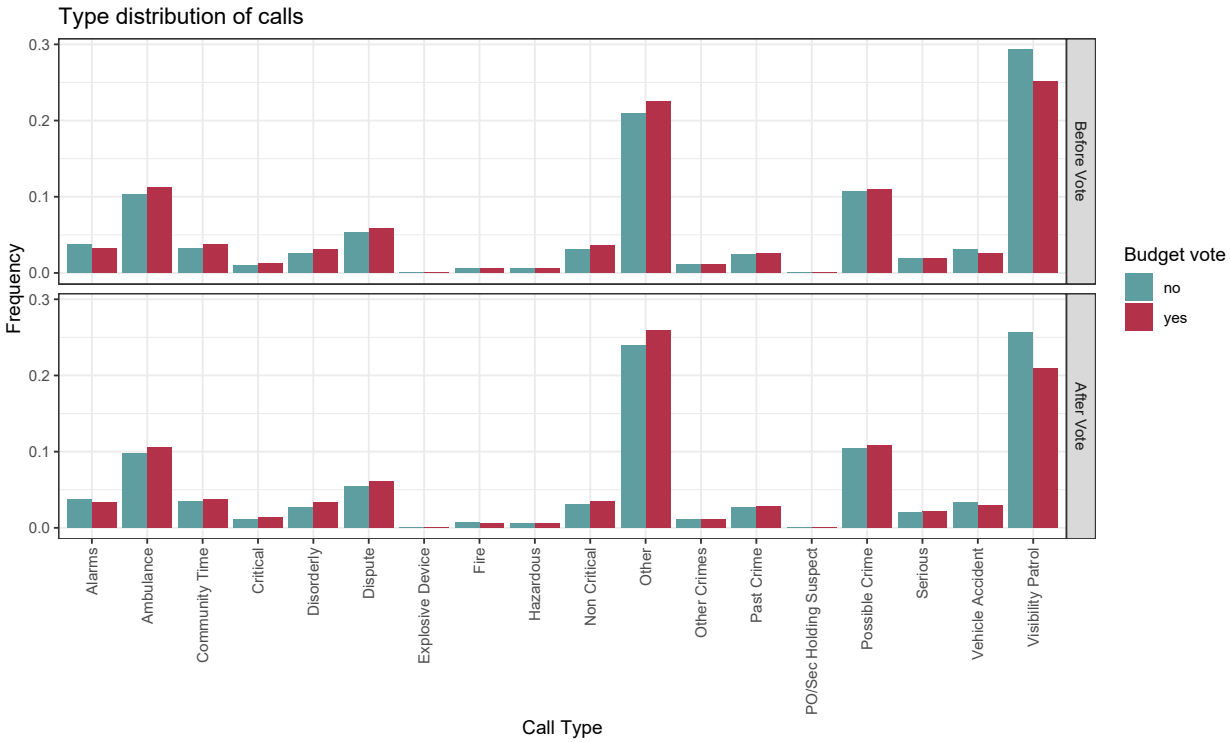


Figure 9: Distribution of 911 Call Types, by Period and District



police reform. To account for this alternative explanation, I use monthly data on change of address requests published by the United States Postal Service.[19] In Table A3 I control for monthly net migration in each district as a share of total district population. Since the change of address data is on the ZIP Code level, I assign each district to the ZIP Code that accounts for the largest share of the district area (in Column (1) and (2)) or to the average of net migration across ZIP Codes, weighted by their share of the district area (in Column (3) and (4)). Compellingly, the estimates of the treatment effect remain largely unchanged in size and significance compared to the main analysis.

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" (Rivera and Ba, 2019), 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.[20] 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.[21] Figure 10 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 A4, 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.

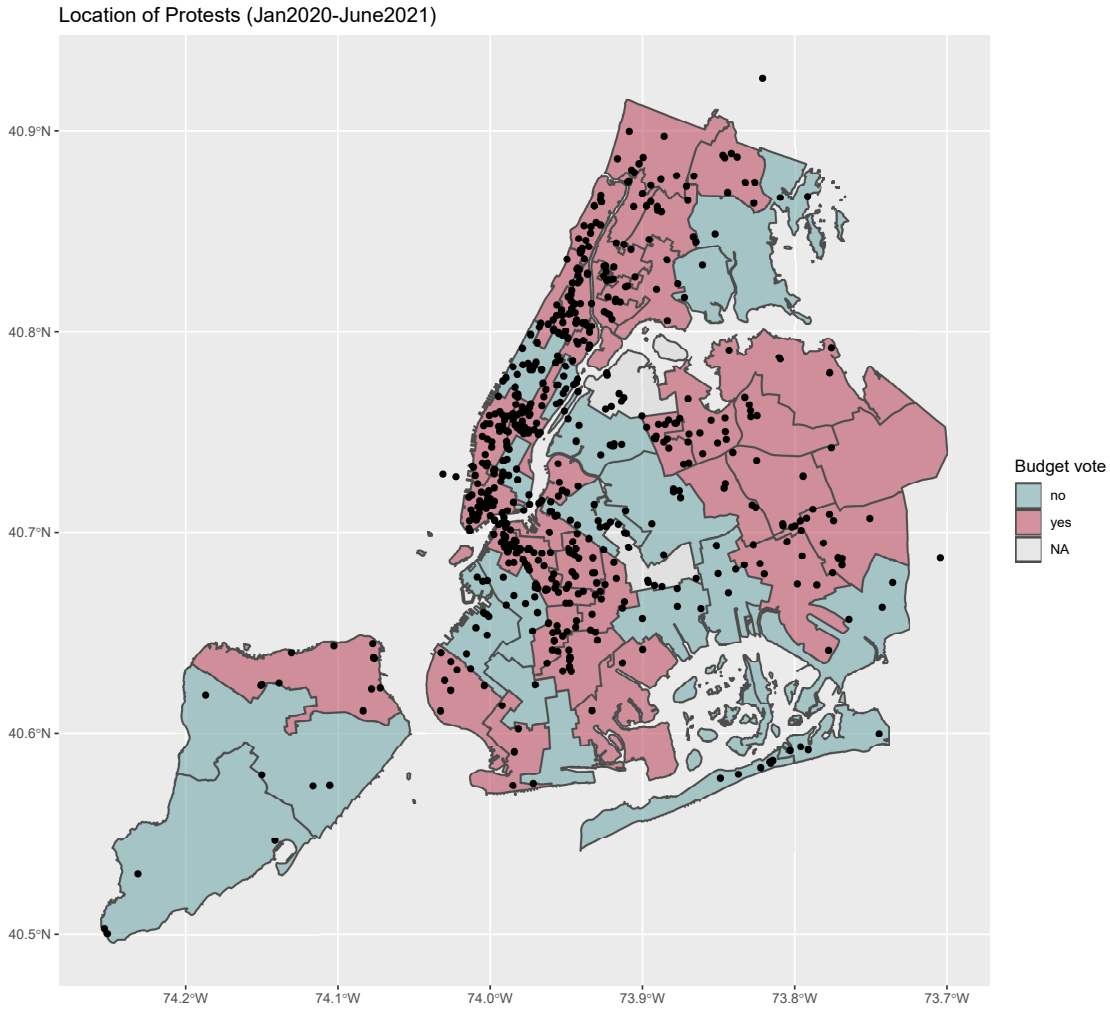
[19] https://about.usps.com/who/legal/foia/library.htm

[20] Alternatively, one could use high-frequency survey data to directly capture individuals' opinions about the police. Yet, the only available poll that offers such fine-grained data is the Nationscape Survey that was designed to collect weekly snapshots of the American electorate throughout the 2019-2020 primary and general elections. Unfortunately, the survey ends in January 2021, thus spanning only part of the sample period in this study.

[21] https://github.com/nonviolent-action-lab/crowd-counting-consortium.

Reassuringly, the treatment effect estimates remain robust to this alternative explanation of increases in 911 response times.

Figure 10: Location of Police-Related Protests



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 the behavior of bureaucratic agents and their role as organized interest groups. 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 (Blumgart, 2020; Anzia, 2022), 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, hard-working 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; Lewis, 2017). Rather, the argument and analysis presented here aim to characterize the dynamics that *can* give rise to bureaucratic power plays and resistance.

Yet, there remain several open questions this study cannot address. First, the study focuses on a single city employing the largest police force in the US. Yet, given the scope conditions of my theory similar dynamics of police resistance likely apply in many other US cities and municipalities. Beyond New York City, 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.[22] 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

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

represents a broader phenomenon across US cities. Using data from a larger set of US cities, future work may address this conjecture explicitly.

Second, the analysis does not provide evidence for the "Fergusson Effect." According to this narrative, there is a direct relationship between strategic police disengagement and rising crime levels. Evaluating this claim is inherently difficult, however, since one would need to isolate crime developments from the multitude of confounding factors influencing crime, such as changes in citizen behavior. Consequently, there is conflicting evidence on the effect of police pullbacks – some research suggests that de-policing can increase crime (Mas, 2006; Shi, 2009; Premkumar, 2020) and gun violence (Devi and Fryer, 2020), while others find that police disengagement does not significantly compromise public safety (Shjarback et al., 2017; Rosenfeld and Wallman, 2019; Chalfin et al., 2021) or even claim that a reduction in proactive policing can reduce crime (Sullivan and O'Keeffe, 2017). Assessing how the NYPD's resistance to the 2021 budget cut affected crime rates is beyond the scope of this paper. However, given that the increase in response times is rather modest and concentrated among minor incidents, it is unlikely that the budget cut and the resulting police response substantially affected crime levels.

Additionally, my argument and analysis abstract away from internal hierarchies of local law enforcement. Some scholars suggest that police administrators have little influence on officers' management of individual calls (Wilson, 1978). Yet, it is possible that the disengagement of NYPD officers in certain districts is partly due to changes in how administrators assigned forces across neighborhoods 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.

Lastly, this study remains agnostic about the broader welfare implications of politically motivated behavior of police. Although work slowdowns and worse public service delivery 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: Effect of Approving 2021 Budget on 911 Response Times, Including May 30 - June 15

	(1)	(2)	(3)	(4)	(5)
yesvote × postvote × NYPD	1.286*	1.247*	1.289*	1.280*	1.291*
	(0.683)	(0.680)	(0.684)	(0.682)	(0.685)
NYPD	5.204***	7.760***	5.207***	5.201***	5.211***
	(0.695)	(0.742)	(0.696)	(0.696)	(0.696)
yesvote × NYPD	-0.028	-0.033	-0.033	-0.028	-0.032
	(1.141)	(1.099)	(1.141)	(1.142)	(1.141)
postvote × NYPD	1.981***	2.070***	1.978***	1.990***	1.974***
	(0.443)	(0.438)	(0.444)	(0.443)	(0.444)
yesvote × postvote	-0.752	-0.791	-0.749	-0.753	-0.753
	(0.586)	(0.593)	(0.585)	(0.594)	(0.582)
total calls (log)		-1.751***			
		(0.233)			
total crime calls (log)			0.269		
			(0.176)		
# of shootings (log)				2.674***	
				(0.513)	
# of complaints (log)					0.441**
					(0.170)
District FE	✓	✓	✓	✓	✓
Police Precinct FE	✓	✓	✓	✓	✓
Date FE	✓	✓	✓	✓	✓
Observations	9,322,968	9,322,968	9,322,968	9,322,968	9,322,968
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 $yesvote_c$ and $postvote_d$ absorbed by district and day fixed effects, respectively. Cluster robust standard errors in parentheses, by district (49).

Table A3: Effect of Approving 2021 Budget on 911 Response Times Accounting for Address Changes

	(1)	(2)	(3)	(4)
yesvote × postvote × NYPD	1.482*	1.706**	1.481*	1.752**
	(0.744)	(0.769)	(0.745)	(0.755)
NYPD	4.710***	5.638***	4.711***	5.624***
	(0.695)	(0.689)	(0.695)	(0.689)
yesvote × NYPD	-0.236	-0.043	-0.234	-0.051
	(1.105)	(1.005)	(1.105)	(1.009)
postvote × NYPD	2.475***	3.051***	2.474***	3.091***
	(0.474)	(0.552)	(0.474)	(0.541)
yesvote × postvote	-0.831	-1.012	-0.827	-1.037*
	(0.614)	(0.629)	(0.613)	(0.617)
netmigration	0.593***	-1.531***	0.640***	-1.491***
	(0.152)	(0.483)	(0.153)	(0.466)
postvote × netmigration		-1.818***		-1.970***
		(0.674)		(0.675)
NYPD × netmigration		2.540***		2.549***
		(0.529)		(0.488)
postvote × NYPD × netmigration		2.493***		2.753***
		(0.803)		(0.813)
District FE	✓	✓	✓	✓
Police Precinct FE	✓	✓	✓	✓
Date FE	✓	✓	✓	✓
Observations	9,068,936	9,068,936	9,068,936	9,068,936
Mean of DV	13.463	13.463	13.463	13.463
Adj. R^2	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 $yesvote_c$ and $postvote_d$ 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
Accounting for Protests

	(1)	(2)	(3)
yesvote × postvote × NYPD	1.514**	1.481**	1.457*
	(0.736)	(0.729)	(0.727)
NYPD	4.700***	4.656***	4.807***
	(0.696)	(0.692)	(0.709)
yesvote × NYPD	-0.259	-0.263	-0.308
	(1.108)	(1.110)	(1.102)
postvote × NYPD	2.483***	2.609***	2.639***
	(0.473)	(0.480)	(0.481)
yesvote × postvote	-0.836	-0.800	-0.784
	(0.634)	(0.626)	(0.623)
# of protests (log)	0.019	-5.960***	
	(0.514)	(1.230)	
postvote × # of protests (log)		8.203***	
		(1.991)	
NYPD × # of protests (log)		7.919***	
		(1.890)	
postvote × NYPD × # of protests (log)		-10.629***	
		(2.977)	
# of protests (log) (June 2020)			0.563
			(1.000)
postvote × # of protests (log) (June 2020)			1.135*
			(0.597)
NYPD × # of protests (log) (June 2020)			-1.157
			(2.058)
postvote × NYPD × # of protests (log) (June 2020)			-1.652
			(1.092)
District FE	✓	✓	✓
Police Precinct FE	✓	✓	✓
Date FE	✓	✓	✓
Observations	9,068,936	9,068,936	9,068,936
Mean of DV	13.463	13.463	13.463
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 $yesvote_c$ and $postvote_d$ absorbed by district and day fixed effects, respectively. Cluster robust standard errors in parentheses, by district (49).

C Figures

Figure A1: Trends in 911 Response Time across Districts, Including Outliers (May 29 - June 15 2020)

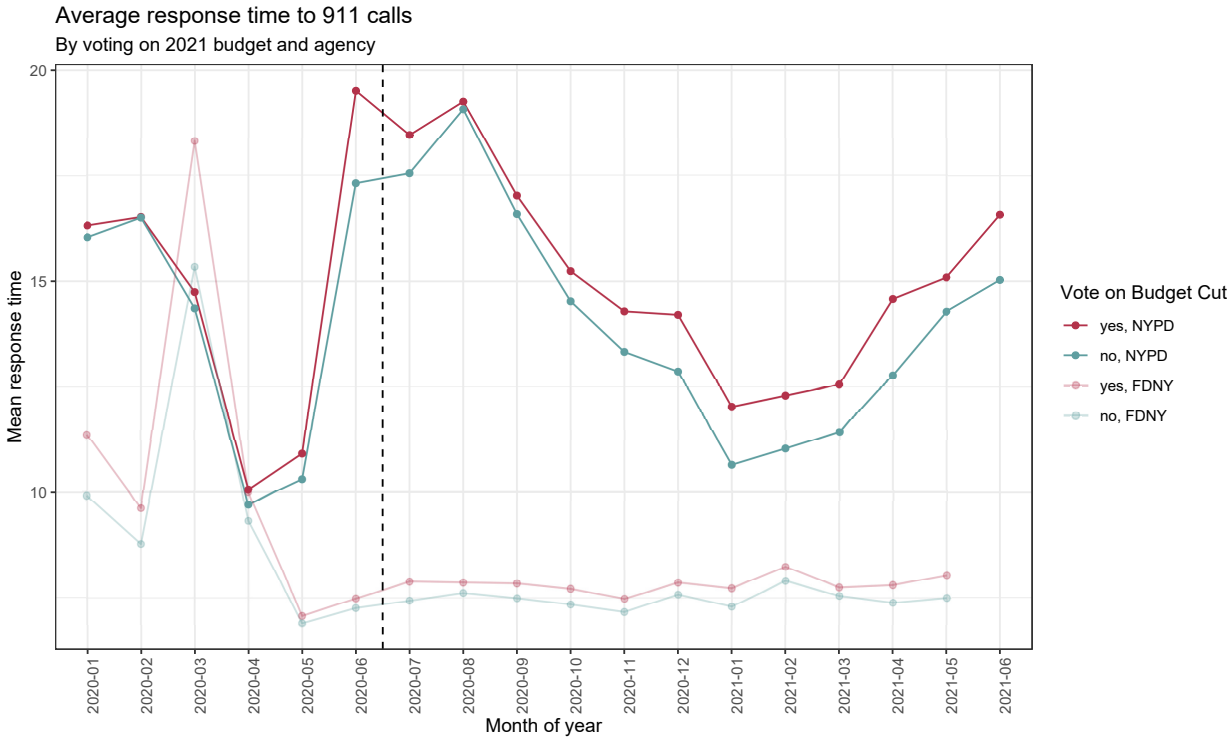
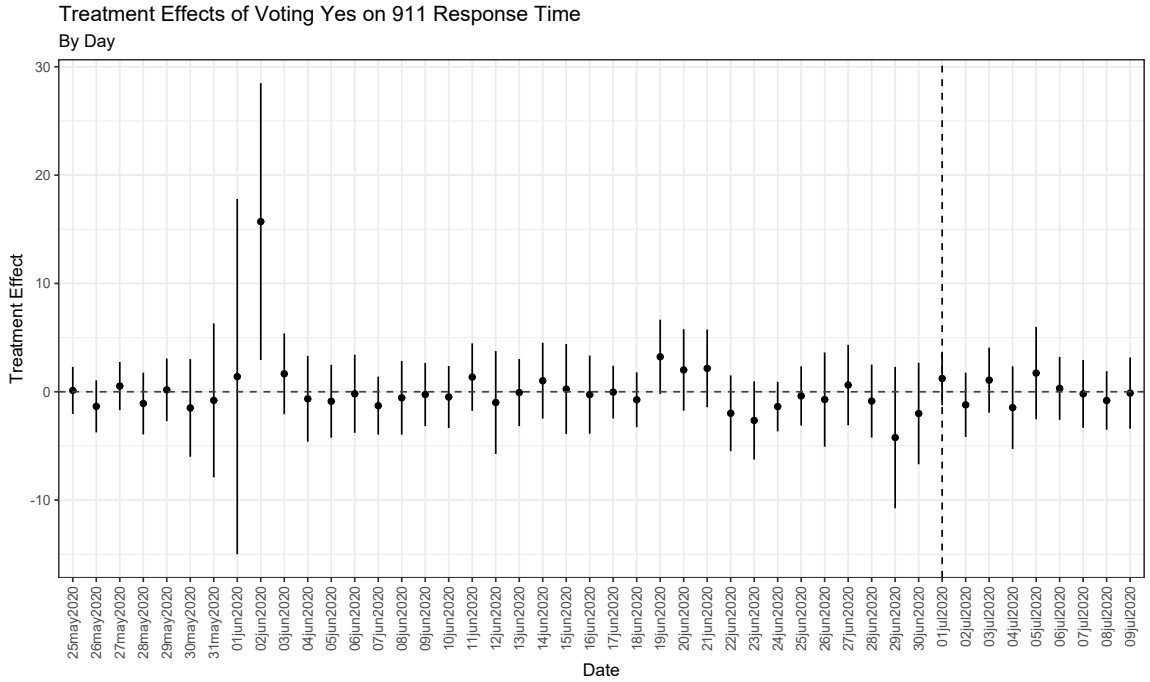


Figure A2: Daily Treatment Effects



Note: Depicted are date-specific treatment effect estimates in a short window around the policy change, together with 90% confidence intervals.

Figure A3: Treatment Effects by Call Type

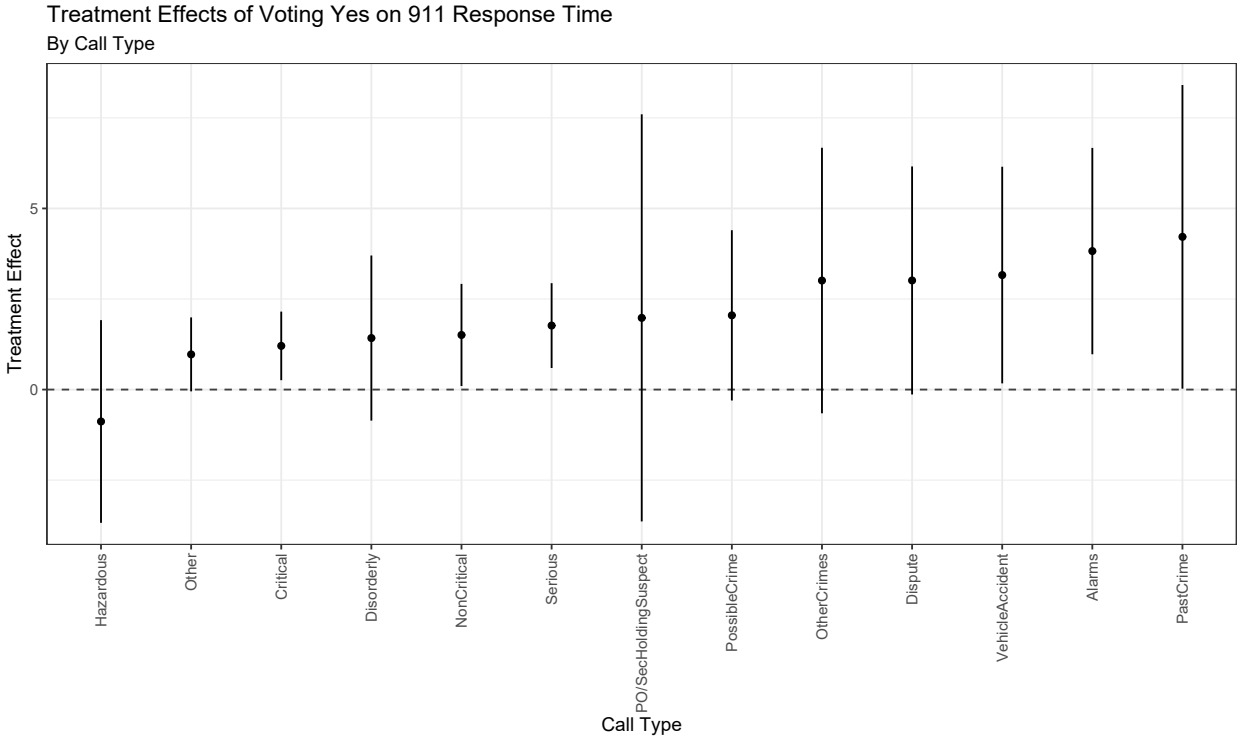


Figure A4: Balance of Matched RDD Sample - Major Call Types

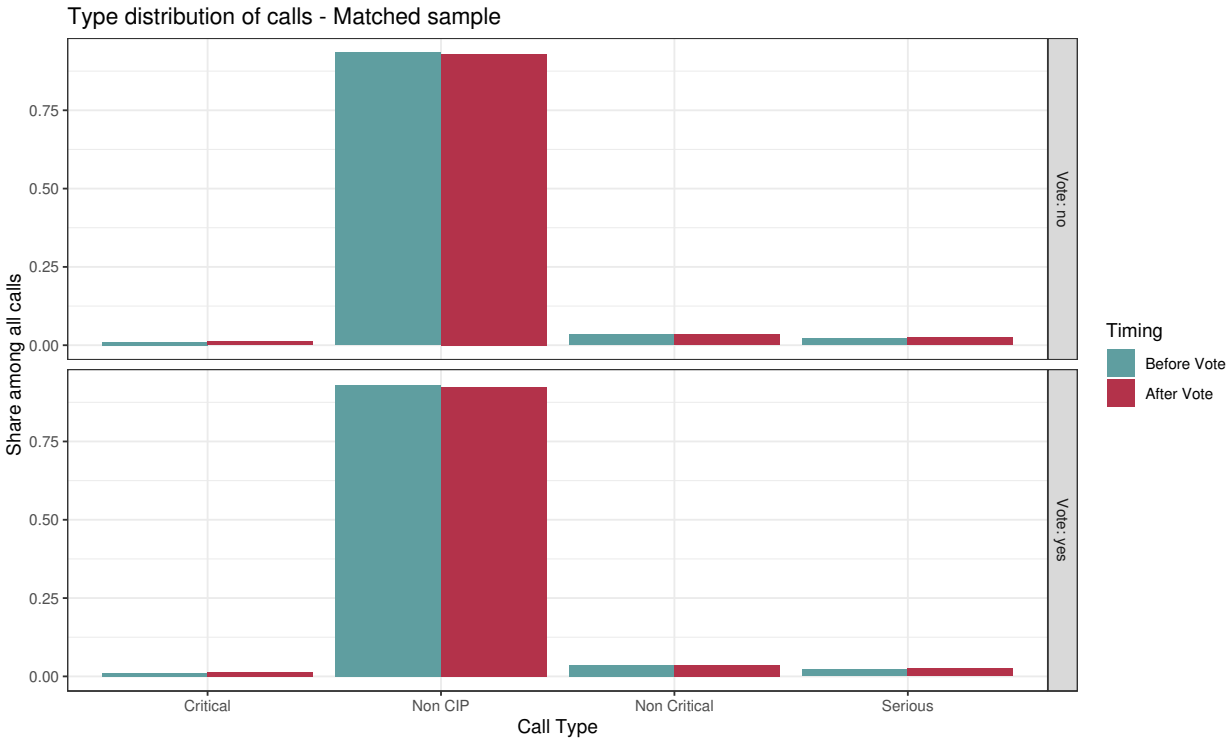
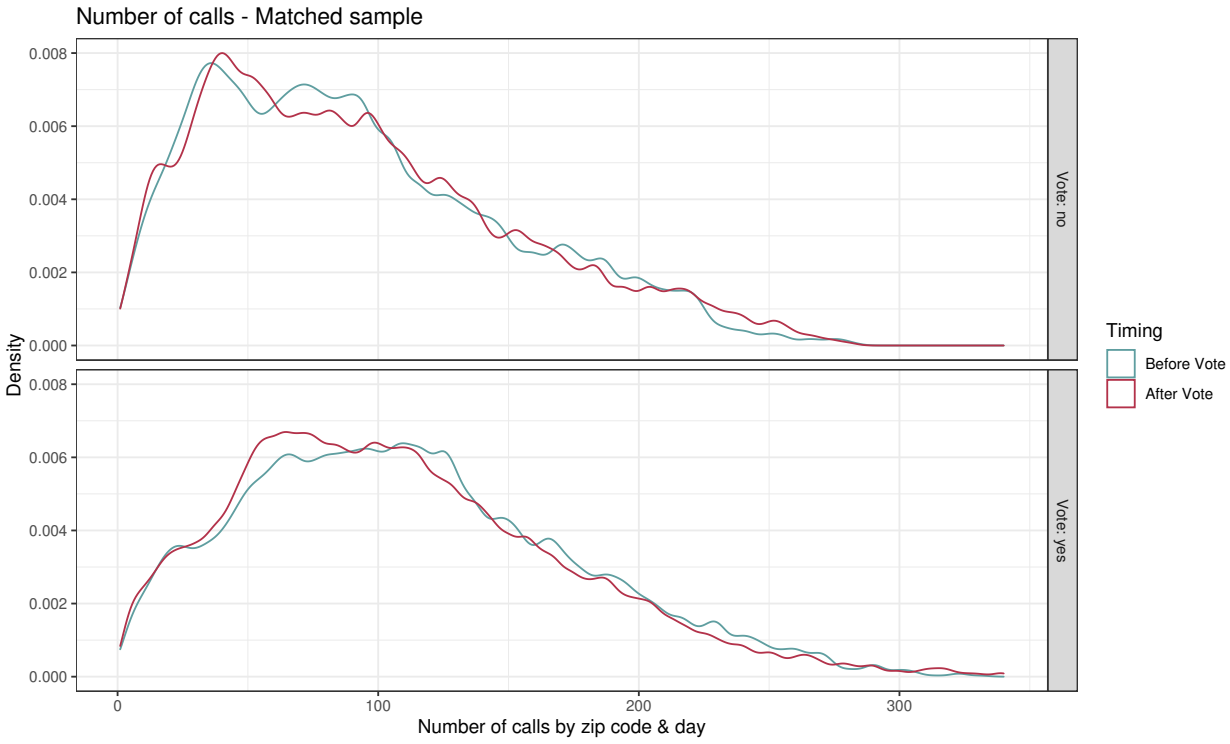


Figure A5: Balance of Matched RDD Sample - Daily Call Volume by Zip Code



D Impact on Candidate Vote Share

In this section I provide some correlational evidence suggesting that council members opposed to police interests incured 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:

$$\Delta voteshare_{ie} = \alpha + \beta yesvote_i + \varepsilon_{ie} \tag{4}$$

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 boostrap p-value following (Roodman et al., 2019).

The results in Table A5 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 memers who acted contrary to police interests during the 2021 budget vote might have incured some electoral costs in the upcoming city elections.

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

Table A5: Effect of Approving 2021 Budget on 2021 Election Vote Shares

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

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$; 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 *yesvote* and is computed using the cluster wild bootstrap procedure of Roodman et al. (2019).