Do Elections Improve Constituency Responsiveness? Evidence from U.S. Cities*

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Abstract

Do elections motivate incumbent politicians to serve their voters? In this paper we use millions of service requests placed by residents in U.S. cities to measure constituency responsiveness. We then test whether an unusual policy change in New York City, which enabled city councilors to run for three rather than two terms in office, improved constituency responsiveness in previously term-limited councilors' districts. Using difference-in-differences, we find robust evidence for this. Taking advantage of differential timing of local election races in New York City and San Francisco, we also find late-term improvements to responsiveness in districts represented by reelection seeking incumbents. Elections improve municipal services, but also create cycles in constituency responsiveness. These findings have implications for theories of representative democracy.

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A prominent conception of representative democracy, dating back to at least James Madison, holds that periodic voting promotes political accountability (Madison [1788] 1966). This "electoral connection" (Mayhew 1987) encourages representatives to serve their constituents for fear of being ousted on election day. Prior empirical research on whether such a connection exists falls into two categories. First, do representatives shirk in their final term in office (when the electoral connection is absent)? Second, do representatives shirk when elections are distant in time (when the electoral connection is weakened)?

In this paper, we shine new light on these questions using a new measure of constituency services as well as a new quasi-experimental research design. Addressing constituency requests is a central activity for most elected officials (Cain, Ferejohn and Fiorina 1987; Fiorina 1989, ch. 7; King 1991; Mayhew 1987).[1] Yet, relatively few studies assess whether electoral incentives improve constituency responsiveness — or, conversely, whether a weaker electoral connection causes politicians to shirk on this activity.[2] We therefore collect data on more than 15 million service requests placed by residents in New York City (NYC) and San Francisco (SF). We then link these data with the election districts of local city councilors to study how response times to service requests are shaped by councilors' electoral incentives.

To do this, we first take advantage of an unusual policy change in NYC, where city councilors voted in 2008 to extend their own term limits from two to three terms. This policy change allows us to implement a difference-in-differences strategy, comparing changes to response times in districts with newly eligible councilors to changes in districts represented by first-term councilors, who were always eligible for reelection. This strategy eliminates many confounders that could bias the relationship between electoral incentives and incumbent effort, such as cross-sectional quality differences between politicians (due to skill or experience) and time shocks that affect responsiveness among all representatives. The results from this analysis indicate that elections substantially improve constituency responsiveness.

To further assess the importance of elections, and to extend our analysis beyond NYC, we also analyze whether incumbents are less responsive earlier in their terms, when voters direct less attention to politics

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[1] For example, Mayhew (1987, 22) observes, "For the average congressman the staple way [to claim credit] is to traffic in what may be called 'particularized benefits,' [the bulk of which] come under the heading of 'casework' — the thousands of favors congressional offices perform for supplicants in ways that normally do not require legislative action. Each office has skilled professionals who can play the bureaucracy like an organ — pushing the right pedals to produce the desired effects."

[2] In Table A.1, we summarize the empirical literature on last-term shirking in the United States. Of the 26 empirical papers we surveyed, only two analyzed a measure of constituency services, and in both cases this measure was self-reported by state legislators (Carey, Niemi and Powell 1998; Carey et al. 2006).

(Lenz and Healy 2014; Huber, Hill and Lenz 2012). Term limits and staggered elections mean that incumbents within the same city run for reelection at different times. Thus, we compare changes to responsiveness among reelection-seeking incumbents to changes among incumbents who either have a reelection bid at a later time or are ineligible to seek reelection. We find that, while constituency responsiveness improves in all districts as elections approach, it improves much more rapidly in districts represented by reelection-seeking incumbents. The flip side of this finding, of course, is that incumbents exert relatively less effort earlier in their terms.

In addition to providing placebo tests to shore up the validity of our research design, we address two alternative interpretations of these results. First, we show that the results do not reflect *effort reallocation* from or to legislative activity (e.g., introducing or sponsoring ordinances or resolutions). We find that incumbents' efforts on legislation remained constant even as they were becoming more responsive to demands for constituency services. Second, we do not find that constituents submitted more (or fewer) requests in districts where councilors became eligible for a third term. Our results are driven by the supply of constituency service, not changes in demand for councilors' time.

Our findings support the conception of representative democracy articulated by Madison. They also bolster prominent political economy models on elections (Alt, Bueno de Mesquita and Rose 2011; Besley 2006; Dewan and Shepsle 2011; Nordhaus 1975; Rogoff 1990; Shepsle et al. 2009; Tufte 1978; Przeworski, Stokes and Manin 1999). Despite elegant predictions, these models have been refuted (e.g., Besley 2006; Carson et al. 2004; Lott and Bronars 1993; Poole and Romer 1993; Keele, Malhotra and McCubbins 2013) almost as many times as they have been supported (e.g., Alt, Bueno de Mesquita and Rose 2011; Besley and Case 1995; Cummins 2012; Figlio 1995; Rothenberg and Sanders 2000; Snyder and Ting 2003).[3] If we look more specifically at constituency services, however, this paper contributes to a growing consensus that elections meaningfully shape how politicians interact with voter requests (Carey, Niemi and Powell 1998; Carey et al. 2006), at least in a system in which the precise identity of the submitter of the request is unknown (see also Kalla and Broockman 2016). In short, our evidence from two major U.S. cities suggest that models of representative democracy are correct in predicting both that elections discipline politicians and that they create cycles in incumbent responsiveness.

[3] Franzese (2002, 378) reviews the literature on electoral cycles and concludes, "On balance, then, the empirical literature uncovers some possible, but inconsistent and weak, evidence for electoral cycles in macroeconomic outcomes, with evidence for cycles in real variables generally weakest (but not wholly absent)." See Canes-Wrone and Park (2012), Grier (2008), and Krause (2005) for more recent contributions to this literature.

Elections and Shirking: Theoretical and Empirical Background

A large literature in political science shows how the electoral connection — which normally forces politicians to exert costly efforts on behalf of constituents — can be severed or attenuated. First, incumbents entering their last terms in office no longer need to worry about voters punishing them at the polls for their (in)actions (e.g., Besley and Case 1995; Figlio 1995; Rothenberg and Sanders 2000). Second, when elections are distant in time, voters pay less attention to their representative's activities. It is in the immediate run up to elections that voters direct their attention to politics and, in so doing, discipline politicians (e.g., Huber and Gordon 2004; Nordhaus 1975; Shepsle et al. 2009; Tufte 1978).[4]

These ideas are consistent with a simple maximization problem, in which incumbents weigh the cost of effort (e.g., on bills or constituency service) against the electoral payoff. For ineligible incumbents, there is no electoral payoff, so they do not exert themselves to win over voters; by contrast, those seeking reelection expend effort to shore up their electoral prospects. This generates the first prediction we test in this paper — namely, *term limits reduce incumbent effort*.[5]

Now suppose that the returns to effort for those eligible to seek reelection increase as the next election approaches. Past research has offered two related reasons for this. First, if retrospective voters suffer from recency bias (e.g., Lenz and Healy 2014; Huber, Hill and Lenz 2012), incumbents concentrate efforts just before their reelection contests — the period that weighs most heavily on voters' minds when they cast their votes (Nordhaus 1975; Shepsle et al. 2009; Tufte 1978).[6] Second, even if voting is prospective rather than retrospective, reelection-seeking incumbents may ramp up their efforts as elections approach to signal their competence (e.g., Rogoff 1990).[7] In these models, voters try to infer their incumbent's quality, as this predicts how they will perform in their second term. If voters pay greater attention to signals sent during campaigns or if constituency service, as opposed to policy efforts, provide a certain and more immediate boost to voters' welfare, then incumbents should shift effort towards public service requests in the run-up to elections. These

[4] In addition to term limits and electoral proximity, simply having elections at all is frequently used to measure the impact of electoral incentives. In particular, past research has studied the impact of elections versus appointments, finding that elected representatives are more responsive (Grossman et al. 2014) and serve a broader set of constituents (Sances 2016) than appointed leaders.

[5] For a review of models making this and related claims, see Dewan and Shepsle (2011).

[6] The resolution of 311 requests — e.g., broken street lights, potholes, impassable sidewalks — affects more people than just those submitting complaints. This wider audience may be particularly attuned to the quality or maintenance of public services in the run-up to elections.

[7] See Besley (2006) and Canes-Wrone, Herron and Shotts (2001) for other signaling models in this tradition.

two strands of the literature both imply that the optimal level of effort for eligible incumbents increases as elections approach. Thus, *eligible incumbents should increase their effort levels over the course of their terms (while effort among ineligible incumbents should remain constant)*.[8]

Empirical Challenges

Despite the clarity of these two predictions, one can find empirical studies that claim to support and refute both of them. Table A.1 provides evidence for this. This table summarizes 26 studies of last-term shirking in the United States.[9] Of these studies, half find evidence of shirking while the other half find no or inconclusive evidence.[10]

Table A.1 highlights three ways in which empirical studies can be extended to potentially resolve or clarify these mixed results. First, past research has focused on "ideological shirking," analyzing politicians' voting records and policy outcomes while in office.[11] Constituency services — one of the most common activities in the daily lives of representatives (Cain, Ferejohn and Fiorina 1987; Fiorina 1989, ch. 7; Mayhew 1987) — have received less attention. Our review revealed two studies of this activity (Carey, Niemi and Powell 1998; Carey et al. 2006), both of which use measures of constituency services that were self-reported by state legislators.

Second, most studies of shirking focus on only one incumbent activity. Doing so makes it difficult to distinguish between two different outcomes: a shirking incumbent and an incumbent who is reallocating effort across activities. For example, if a last-term incumbent decides to devote ten fewer hours per week to legislation but allocates twenty additional hours per week to casework, a study focusing on legislation may wrongly conclude that the incumbent shirked in her last term. To reduce the possibility that such reallocation

[8] This aligns with a large literature on electoral cycles in incumbents' behavior (Schumpeter 1939; Nordhaus 1975; Tufte 1978; Rogoff 1990; Schultz 1995; Franzese 2002; Canes-Wrone and Park 2012).

[9] Outside the United States, Ferraz and Finan (2011) and Klasnja and Titiunik (2017) find that term-limited Brazilian mayors engage in more corruption, and that voters, recognizing the moral hazard generated by term limits, are less likely to re-elect incumbent mayors.

[10] Franzese (2002) and Canes-Wrone and Park (2012) review empirical results in the literature on electoral cycles in incumbent effort and reach the same conclusion.

[11] Studies of ideological shirking can in turn be divided into three categories, analyzing whether last-term incumbents (1) vote differently than they have previously (e.g., Lott 1987; Lott and Bronars 1993; Snyder and Ting 2003), (2) vote in opposition to their constituents' preferences (e.g., Besley 2006; Wright 2007; Tien 2001), or (3) favor a different set of fiscal policies (e.g., Erler 2007; Keele, Malhotra and McCubbins 2013).

could be driving our results, this paper analyzes legislative activity in addition to constituency services. We acknowledge that these activities do not capture all ways in which representatives serve constituents; as Lott (1990, 133) points out, there are "as many [potential measures of effort] as there are outputs that a politician produces." We present the analysis of legislative action as a suggestive test of (no) reallocation across two important duties.

Third, some research on shirking has relied on cross-sectional comparisons of legislators that are or are not in their last terms in office. Omitted variables that are difficult to measure, such as motivation or quality, may threaten causal inference in such studies. Our design extends more recent work that exploits panel data on officials' behavior (e.g., Alt, Bueno de Mesquita and Rose 2011; Bails and Tieslau 2000; Besley and Case 1995; 2003; Besley 2006; Erler 2007; Keele, Malhotra and McCubbins 2013; Snyder and Ting 2003). These studies compare changes to behavior over time, reducing confounding due to fixed incumbent characteristics. They also avoid selection concerns associated with retirement decisions by restricting comparisons to incumbents whose election eligibility is mandated by law rather than chosen.[12] We attempt to build on such studies by studying councilors who serve within the same city and deal with similar constituency requests.

Measuring Constituency Responsiveness in U.S. Cities

We collect detailed data on service requests in NYC and SF. These cities log information on each service request filed by residents via 3-1-1, a system recently implemented in many major U.S. cities to redirect non-emergency requests from 9-1-1 and to centralize hotlines maintained by individual city agencies. The NYC database has around 14 million observations going back to 2004; the SF database, around 1 million observations going back to 2008.[13]

Three aspects of these data allow us to measure local responsiveness to constituency concerns. First, we use the dates a request was opened and closed to measure *response times*.[14] We discuss what it means for a request to be opened and closed below. Second, we use the reported *location* of each request to match it with a council district boundary. Both NYC and SF have single-member districts (51 in NYC and 11 in

[12] One concern in our study may be that NYC councilors amended the law governing their election eligibility. We discuss the implication of this for our results below, noting that it likely biases against finding an effect of the term limit extension (see p. 12). We also show that our results are not driven by newly eligible councilors who supported the reform (SI 4).

[13] The NYC data are available at `https://nycopendata.socrata.com/`, and the SF data at `https://data.sfgov.org/City-Infrastructure/Case-Data-from-San-Francisco-311-SF311-/vw6y-z8j6/data` (as of winter 2016).

[14] We use the date a request was opened to determine when it occurred relative to either the term extension or election.

Table 1 Summary Statistics for the Most Frequent Request Types in NYC (2004-2013)

Complaint Type	Frequency (in millions)	Percent of all requests	Cumulative percent	Response times in days: Mean	Median	Trimmed mean†
Construction/Plumbing	2.3	16.4	16.4	29.3	12.0	24.0
Heating	2.1	14.7	31.1	5.5	4.0	5.2
Bridge/Highway/Street	1.2	8.7	39.8	4.4	1.0	3.1
Noise	1.2	8.2	48.0	4.3	0.0	4.1
Sanitation/Cleaning	1.0	6.8	54.8	3.2	1.0	3.1
Paint/Graffiti	0.8	5.7	60.5	31.4	13.0	27.2
Sidewalk/Sewer	0.7	4.9	65.4	44.7	1.0	11.1
Water	0.7	4.7	70.1	6.2	0.1	5.1
Construction-related	0.5	3.9	74.0	50.7	16.0	30.7
Street Light Condition	0.5	3.3	77.3	13.3	1.0	9.7
Other‡	3.2	22.9	100	39.3	5.0	21.1

† Excludes response times above the 99th percentile.
‡ Includes 88 complaint types, which we keep as separate categories in the analysis.

SF), meaning that requests and council members are uniquely matched. Third, the data contain information about *request type* (e.g., public housing request, pothole, abandoned vehicle), which allow us to account for different response times to different types of requests in our analysis. The most frequent request types in the NYC database (2004-2013), alongside response time statistics, are displayed in Table 1.[15]

How City Councilors Impact Service Responsiveness

We interviewed 3-1-1 representatives, heads of city agencies, and councilor staff to better understand how service requests are handled by city bureaucracies and to what extent councilors intervene to impact service responsiveness.[16] Based on these interviews, Table A.2 provides an outline of how requests are handled from the time they are submitted by a resident until they are closed by agency staff. A request has been *opened* when all the intake information about the request has been logged and it has been assigned to an agency.

[15] In the subsequent empirical analysis, we trim the top 0.1% (for NYC 2008) or 1% (other analyses) of observations in terms of response times to eliminate large, potentially influential outliers. These different trimming rules are based on the number of large outliers in each of the samples. These rules result in response time distributions that are quite similar across the samples used for analysis. In SI 6, we demonstrate that our results are robust to different decisions about whether and how much to trim the data. We also dichotomize response times (e.g., more or less than five days) and run linear probability models, confirming that our conclusions are not driven by outliers.

[16] The interviewees included NYC's 3-1-1 director of communication, representatives from the Departments of Housing and Transportation familiar with the 3-1-1 process, and staff members from nine city council offices.

An agency will *close* the request after resolving it (which may require rerouting it to a different agency) or after determining that no action is necessary. In all cases, agency workers will physically inspect the issue to determine what type of work is needed.[17] More than anything, Table A.2 highlights the important role individual agencies play in the 3-1-1 system: 3-1-1 provides a centralized and standardized way for requests to be submitted to various agencies. Once there, agencies are responsible for resolving requests and can prioritize across different requests as they see fit.

Given the central role agencies play in resolving requests, it is not surprising that council staff say they regularly turn to agencies to address concerns about service responsiveness in their district. This happens both at a small and a large scale. At a smaller scale, all city council offices we talked to help residents with individual service requests. (Well-staffed offices have a "constituency services" team devoted just to this.) Often, this involves helping the resident file a 3-1-1 request. The councilor office will then follow up with agency intergovernmental liaisons or other agency staff to make sure city workers respond to the request as quickly as possible. Council staff said they are in contact with agencies on a daily basis. They also spoke about the effectiveness of these efforts. For example, a council staff member said that contacting agencies about a specific constituency concern "really smooths things along." Another council member concerned about over-development regularly monitors and responds to constituency concerns filed via 3-1-1 or the Department of Buildings, and is known for his success in limiting new housing development in the district. This type of action shows that councilors are highly motivated and able to help residents with service issues.

Our interviews also revealed two ways in which council offices can impact response times at a larger scale. The first and most common way is to inform agencies about broader issues within the district. Several district offices said the council member or an office representative meets frequently with agency commissioners or intergovernmental liaisons. For example, a staff member, speaking of sanitation and transportation issues in the district, told us "[our district representative] discusses the specific issues that constituents have

[17] There is no information in the database about the actual action taken by the agency — that is, whether an agency closed the issue after actually resolving it (4a-b in Table A.2) or after determining that the request was "non-warranted" (4c). It is unlikely, however, that differences in non-warranted requests could impact our results. First, the 3-1-1 director of communication and agency representatives indicated that a very small proportion of requests are non-warranted (e.g., prank calls). Second, even if that were not true, difference-in-differences, which we use below, account for baseline cross-sectional differences in non-warranted requests as well as for over-time changes that affect both treatment and control districts. Our inferences would only be threatened in the unlikely scenario that *changes* to response times to non-warranted requests were different in treatment and control districts *and* these changes were such that they improved overall response times more in treatment districts.

so that they [i.e., the agencies] are aware of them and so that they can take appropriate measures." Office staff also send letters to agencies. For example, a staff member said her office often compiles issues their constituencies have and sends a letter addressed to the agency commissioner (e.g., they recently sent a letter to the Department of Transportation regarding potholes in the district).

The second way in which council districts affect constituency responsiveness at a larger scale is by working with other council members, forming task forces or taking legislative action. For example, a representative from a city council office said that, when they notice that an issue is prevalent, they have a meeting with other councilors, especially councilors that represent similar districts. Then if enough agree, they launch a task force that consists of central city council administrators, agency representatives, and city council members. The representative gave an example concerning a request regarding special education. Noticing that there were not enough resources for special education within the district, the council office formed a task force that implemented a program to better integrate special education children into the public school system.

Although we cannot directly quantify the effectiveness of these particular actions in terms of response times, the interviews highlight plausible mechanisms for how elections influence local responsiveness. When councilors are no longer eligible to seek reelection, or when elections are distant in time, councilors are less motivated — and they allocate less time and staff resources — to pressure agencies to impact response times in their district.

Lastly, this discussion may raise the question of why agency commissioners and staff heed the demands of city politicians in the first place. First, the monitoring problems found in many principal-agent relationships are limited in our case. This is because the data collected by the 3-1-1 system can be used to monitor response times at the council district level. City councilors told us they use these data, as well as reports released by the city, to track responsiveness in their district.[18] Second, council staff report building professional and personal relationship with agency staff. Whether because of social or quid pro quo benefits, such relationships could be used to get agencies to reallocate resources when necessary. Third, if agency commissioners care about their budgets, then they should strive to do well by elected officials, who approve the city budget, including funding for both the 3-1-1 program and city agencies. Allocations to these agencies are not guaranteed year-to-year. For example, between fiscal year 2007 and 2010, the Department of Public Works (DPW) in SF saw its annual general fund allocation drop from nearly $27.9 million to $13.4 million (Dept. of Public Works 2010).

[18] Local Law 47 in NYC requires the 3-1-1 service to make periodic public reports with call data aggregated by city council district. In SF, the 3-1-1 data include information on the supervisor district in which the service request is located.

Effects of Reelection Eligibility on Constituency Responsiveness

To estimate a causal effect of reelection eligibility on politicians' efforts, we take advantage of the term-limit extension instituted by Mayor Bloomberg and the New York City Council on October 23, 2008. The extension enabled the mayor and city councilors to run for three rather than two four-year terms in office. But it did not affect every city councilor equally.[19] A subset of councilors (14 of 51) were in their first term of office at the time of the decision, and would have been eligible for another term regardless. Another group of incumbents suddenly went from being term-limited to eligible for reelection in the next cycle of elections held in November, 2009.[20]

We implement a difference-in-differences (DiD) design. The treatment group consists of incumbents who were termed out before the October 23, 2008 decision but ran for a third term after the decision. This group has 29 incumbents, as not all of the 37 newly eligible councilors took advantage of the extension.[21] Our control group is incumbents who were allowed to seek reelection both before and after the decision. We estimate the DiD using the following model with councilor and period fixed effects:

$$y_{idt} = \alpha_d + \delta_t + \beta D_{dt} + \gamma_{type} + \varepsilon_{idt} \tag{1}$$

where i indexes complaint; d, city council district; and t, day. The outcome variable is the number of days it took to resolve the complaint. D_{dt} is an indicator equal to 1 for treated city council districts after the term-limit extension and 0 otherwise. The parameter associated with this variable, β, is of key interest. A negative estimate of β would indicate that response times dropped — improved — in treatment districts relative to control districts after the term limit extension. The model includes fixed effects for council district (α_d) and every day in the time-series (δ_t). Because response times vary by the type of request — as Table 1 makes clear — we also include fixed effects for complaint type (γ_{type}) and the day on which the complaint was lodged (δ_t). In all analyses, we cluster the standard errors on councilor.

Table 2 presents the results, showing that responsiveness in treated districts improved significantly relative to control areas after the term-limit extension. Using different time windows on either side of the

[19] We consider only city councilors, not the mayor, in our analyses.

[20] Term-limited councilors are not geographically clustered, but spread across the city's boroughs. Manhattan has the lowest proportion of term-limited councilors at 50 percent.

[21] The other eight incumbents left politics or ran for different positions (e.g., four ran for comptroller). We do not include these individuals in the main analysis, but show that our results hold including all incumbents in SI 3.

Table 2 Effect of Term Extension on Constituency Services
The average response time fell by more than 1 day in treated districts.

	Dependent variable:			
	Response Time			
Time frame†	2	3	4	12
$\hat{\beta}$‡	−1.194	−1.437	−1.241	−0.641
	(0.751)	(0.672)	(0.562)	(0.852)
	p = 0.112	p = 0.033	p = 0.028	p = 0.452
	$\mathbb{1}$(Response Time < 5 Days)			
Time frame†	2	3	4	12
$\hat{\beta}$‡	0.032	0.016	0.018	0.005
	(0.011)	(0.009)	(0.010)	(0.010)
	p = 0.005	p = 0.073	p = 0.065	p = 0.629
Observations	117,276	171,419	230,067	663,880

†Weeks on either side of the extension used to estimate Eq. 1
‡Difference-in-differences estimator (see Eq. 1)
Standard errors clustered on districts in parentheses

extension (2-4 weeks), we find that $\hat{\beta}$ is negative in all cases.[22] Response times decreased by 1.2-1.4 days, or 4%, in treated districts after the term-limit extension ($p < 0.05$ for the three or four week time windows; $p = 0.11$ for the two week window).[23] Compared to events known to severely hamper city services, these response time changes are meaningful. For example, the January 20-23, 2005 blizzard, which dropped over a foot of snow in NYC, resulted in a 7% increase in response times to service requests opened in the time window of the blizzard, and labor day weekends on average result in an 8% increase in response times.

These results are robust to an alternative modeling strategy. We transform the dependent variable, coding a new binary outcome equal to 1 if a complaint was resolved within five days and 0 otherwise. We then substitute this new outcome variable on the left-hand-side of Equation 1 and estimate linear probability models. The bottom-half of Table 2 includes the results from this specification. The probability that a complaint was resolved within five days increased by two to three percentage points in those districts affected by the term-limit extension, as compared to control areas. This should alleviate the concern that large outliers — requests resolved long after the policy change — are unduly influencing the results.

[22]Summary statistics for the key variables included in Equation 1 are shown in Table A.3.

[23]Our inferences are unchanged if we employ a version of the block bootstrap, in which we randomly draw 51 districts with replacement to form our bootstrap sample.

In the final column of Table 2, we include the results using a 12-week window around the policy change. The effect attenuates: the coefficient declines by a factor of between two and five. Two sources of attenuation are consistent with our argument. First, our later results on election timing suggest that our control group (first-term councilors) may have been improving more rapidly in the pre-treatment period. Second, in response to the policy change, treated councilors ramped up efforts and reduced response times relative to controls, which generated an initial dip in response times in the post-treatment period relative to control. Yet, as time passed, all councilors seeking reelection — both treated and control — converged to a similar level; these councilors did, after all, hold the same office and face the same electoral incentives.[24] Both dynamics are consistent with our account and would lead to an attenuation of our negative effect as we expand the window around the policy change.

Identification

To interpret the estimates presented above causally, the parallel trends assumption must hold. That is, in the absence of the term limit extension, treatment and control districts must have followed the same trend in responsiveness.[25]

Going into our analyses, we had two potential concerns about this assumption. First, treated and control councilors could have different pre-treatment trends due to the upcoming election, which took place in November 2009. For example, reelection eligible incumbents (the control group) could be ramping up their

[24] In SI 5, we show that there are no differential trends in responsiveness between these groups in the run up to the 2009 elections, as councilors in both groups face the same reelection incentives.

[25] We recognize that to recover the average treatment effect on the treated (ATT), two additional assumptions — the stable unit treatment value (SUTVA) and constant treatment effects — are required. Spillovers could result from city-wide improvements to response times, e.g., due to Mayor Bloomberg's reelection bid, in line with Levitt (1997). However, this would bias $\hat{\beta}$ toward 0. In addition, we note that ATT does not generalize to the control group (by definition). In our case, features of the treatment group — for example, their additional experience in office — may impact the size of the treatment effect. This does not violate the identifying assumption. ATT is a relevant quantity to the extent that most elected officials can stay in office for more than one term before term limits are imposed. Furthermore, note that this assumption does not imply that treatment and control districts must be balanced on levels. For example, the design accounts for the fact that second-term councilors may have better average response times due to their longer tenure in office. The unit fixed effects in Equation 1, represented by α_d, account for all fixed differences (whether observed or unobserved) across councilors and council districts.

efforts in anticipation of the election, relative to ineligible incumbents.[26] Second, given that city councilors approved the extension, it is possible that they could have anticipated that it would pass. If so, treated incumbents may have ramped up their efforts with constituency services before October 23, when the extension was formally approved in City Hall.[27]

We evaluate the plausibility of the parallel trends assumption in a set of "placebo" tests. We substitute the actual date of passage with a set of earlier, fake dates. We then re-estimate the DiD using these new time windows. If these estimates fail to statistically differentiate between treatment and control districts, then we have evidence of parallel *pre*-treatment trends, which in turn would make us more confident that treatment and control districts would have followed parallel trends in the absence of the term-limit extension.

Figure 1(a) displays the placebo estimates and their 95% confidence intervals. (They are based on 40 randomly drawn dates from January 1, 2008 to September 11, 2008, six weeks prior to the actual term-limit extension.) The estimate from the actual term-limit extension is the right-most, black point. As is apparent in Figure 1(b), the t-statistic of our actual estimate is more negative than all of the placebo estimates; our actual result is the only coefficient significant at the 5%-level. Based on these placebo tests, there is no evidence of diverging pre-treatment trends, shoring up the parallel trends assumption.

Effects of Election Timing on Constituency Responsiveness

Do elections also affect *when* incumbents exert effort? We use data from two elections to answer this question: the New York City Council Elections of 2005, and the analogous San Francisco Board of Supervisor Elections of 2010.

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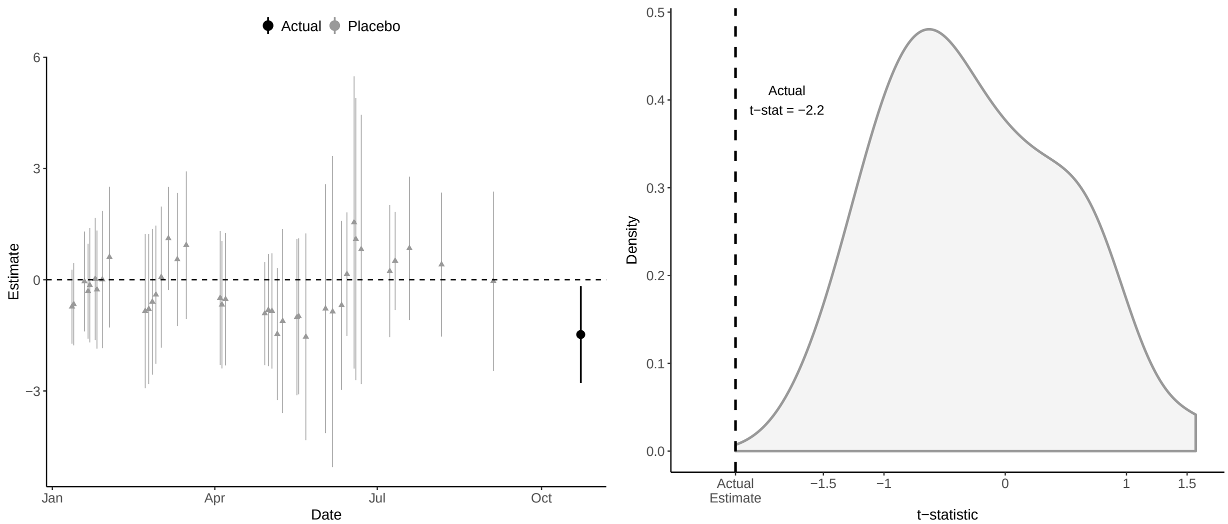
[26] This biases against our eventual finding. If the control group is improving more rapidly, then our counterfactual prediction overestimates the improvement we would have expected among term-limited councilors had their eligibility remained unchanged.

[27] Again, this would bias the estimate of β toward zero, making it more difficult to find an effect of the policy change. Moreover, evidence from the time the extension was debated suggests that anticipatory effects were limited. The term limit extension was catalyzed by Mayor Bloomberg positioning himself as the city's most capable leader in the face of the 2008 financial crisis. Given the uncertain economic climate and falling city revenues, Bloomberg was successful in convincing a majority of council members (and, in the 2009 election, voters) that his financial experience would be necessary in the tough times ahead (Honan 2008). The bill passed, 29-22, just two weeks after Bloomberg had decided that he wanted to run again. The final vote was preceded by 20 hours of public hearings and a full day of floor debate in what was described as a divided City Hall (Chan and Hicks 2008).

Figure 1 Placebo Estimates of Response Time by Treatment Status
Placebo tests indicate that divergent pre-treatment trends do not explain the effect.



(a) Estimates and 95% CIs

(b) Distribution of t-statistics

Estimates with 95% CIs from 40 placebo dates drawn at random from January 1, 2008 to September 11, 2008. The actual term-limit extension was on October 23 and is the right-most, black point. Each estimate uses three weeks of data on either side of each placebo date; thus, dates that fall in the six weeks between September 12 and October 22 are ineligible for inclusion (as these dates would necessitate the inclusion of post-October 23 data, contaminating the placebo tests). On the right, we show the distribution of t-statistics from these estimates; our actual result is indicated by the dashed line.

Empirical Strategy

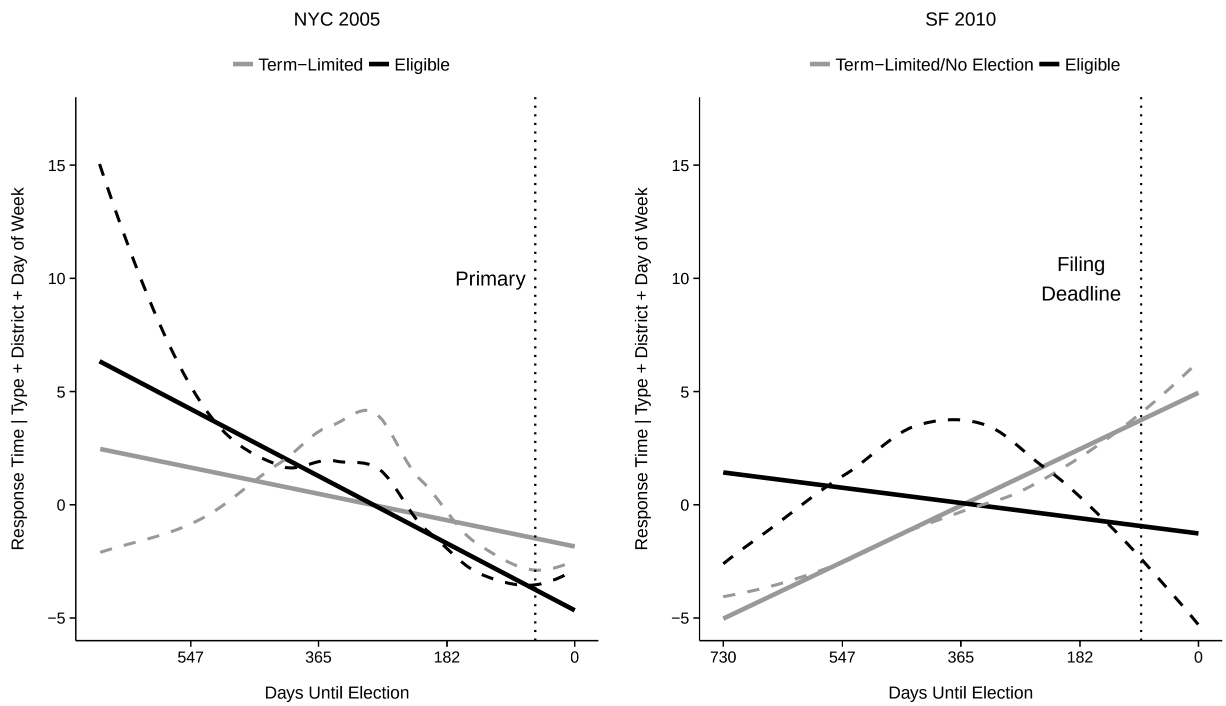
We compare incumbents who are seeking reelection (treated) with incumbents who cannot seek reelection due to term limits or staggered elections (control). In NYC, which has term-limits but no staggered elections, 44 of 51 incumbents ran for reelection in 2005. In SF, which has term-limits *and* staggered elections (half of the Board is elected every two years in alternating elections), one incumbent sought reelection in 2010. We pool data across the two elections; SI 8 presents the results for the two elections separately.

We evaluate whether response times fall more precipitously as elections approach in districts where incumbents are eligible to stand for reelection, relative to districts represented by an ineligible councilor. To do this, we estimate the trends in response times for both groups after accounting for level differences across districts, the nature of the complaint, and the day of the week on which a complaint was filed. Our empirical model is

$$y_{idt} = \alpha_d + \delta t + \beta(D_d \cdot t) + \gamma_{type} + \phi_{day} + \varepsilon_{idt} \tag{2}$$

Figure 2 Response Times prior to the Election by Treatment Status

Response times fall faster in districts with eligible incumbents up to primary election or filing deadline.



Note: Solid lines are linear trends; dashed lines are loess smoothers.

where i indexes complaints, d city council district, and t represents days before the relevant election date. D_d is an indicator for treated city council districts (i.e., those with an eligible incumbent). We include fixed effects for districts, request type, and the day of the week on which the complaint was made, and cluster the standard errors at the council district. To analyze legislative efforts, we use the same specification without the fixed effects for complaint type (γ_{type}) or the day of the week (ϕ_{day}).

We again label the quantity of interest as β.[28] The key identifying assumption is that eligible and ineligible incumbents would have followed parallel trends in response times absent elections. A negative estimate indicates more sharply declining response times among treated incumbents (i.e., increased effort) relative to control incumbents.

Results

In Figure 2, we explore whether the timing of elections affects responsiveness to 3-1-1 requests using a non-parametric approach that allows for non-linear trends. In both NYC and SF, it appears that response times to service requests declined more rapidly in treated districts than in control districts. (Before creating this plot, we first partial out the variation in response times explained by the complaint type, council district, and day of the week on which the complaint was made.) The SF figure (right) is particularly striking: roughly one year prior to the election, response times fell off sharply in the treated district, while they continued to increase in districts with ineligible incumbents. In NYC (left), response times appear to be declining almost monotonically in treated districts, while in control districts, response times continue to increase through the winter months of 2005. This figure demonstrates that our findings persist, even if we allow for flexibly estimated time-trends.[29]

In Table 3, we present the results from estimating Equation 2 using the primary and general election dates.[30] We prefer to use the date of the primary, as incumbents' electoral threats diminish sharply after these political events. In all but Staten Island (where Republicans dominate), Democratic primary winners in NYC went on to win their general election contests in landslides. In SF, the eligible incumbent was assured of running unopposed after the candidate filing deadline.

We also split the results by the number of days before the election we use to estimate Equation 2, corresponding to 2, 1.5, 1, and 0.5 years.[31] The estimates of β are negative in all models, indicating that response times declined more rapidly in districts with an eligible incumbent. The finding does attenuate when we use the period six months before the general election. At least thirty percent of this period occurs after primary elections, after which the electoral returns to amplified effort for eligible incumbents may sharply diminish.

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[28] There may be interesting level differences in responsiveness across treatment and control groups. However, this comparison is confounded by differences between councilors — for example, in their experience — so we do not devote attention to the intercepts, α_d.

[29] Interestingly, response times in NYC appear to increase slightly following the primary election. City Council elections in NYC are partisan, and — in all but a few districts — the Democratic nominee has an overwhelming advantage in the general election. The primary election, on the other hand, tends to be competitive. After weathering the primaries, incumbents may therefore be unconcerned that shirking will be punished by partisan voters. We estimate Equation 2 for both the primary and general elections.

[30] In San Francisco, supervisor elections are non-partisan, so there is no party primary. However, there is a filing deadline 88 days prior to the election, after which new candidates cannot enter the race.

[31] Our data from NYC go back only to January 1, 2004, so the two-year window corresponds to 677 rather than 730 days.

Table 3 Estimates of Differential Time-Trends in Constituency Services (β in Equation 2)
The linear trend in responsiveness falls significantly faster where incumbents can run for reelection.

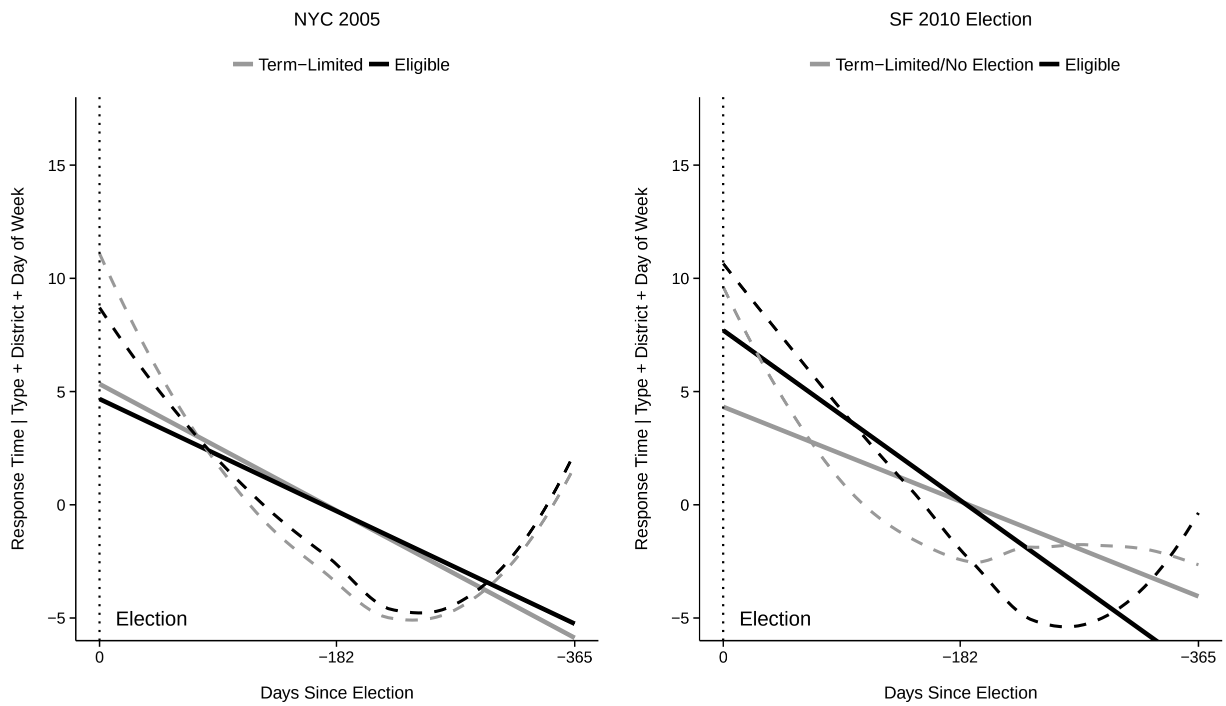
	Dependent variable:			
	Response Time			
Time frame[†]	730	547	365	182
	Days to Primary (NYC) or Filing Date (SF)			
$\hat{\beta}$	−0.025	−0.020	−0.016	−0.041
	(0.004)	(0.005)	(0.010)	(0.020)
	$p < 0.001$	$p < 0.001$	$p = 0.109$	$p = 0.035$
Observations	2,386,852	2,307,747	1,667,983	804,668
	Days to General Election			
$\hat{\beta}$	−0.023	−0.015	−0.012	−0.011
	(0.005)	(0.005)	(0.010)	(0.017)
	$p < 0.001$	$p = 0.006$	$p = 0.236$	$p = 0.513$
Observations	2,659,909	2,451,003	1,722,714	848,525

[†] Maximum number of days before election used to estimate Eq. 2.
Standard errors clustered on districts in parentheses.

To interpret the substantive effect of the estimates, note that they represent the implied effect for *one service request* as we move *one day* closer to the election. The estimates from column 1 imply that moving six months closer to the election corresponds to a four-day reduction in response times in treated districts relative to control. These effects are larger than the change in response times induced by the January 2005 blizzard in NYC or by labor day weekends, events that substantially affect service delivery.

We analyze the elections separately in SI 8, and one aspect of the SF election is worth highlighting. The estimates are more negative when the control group is term-limited incumbents rather than incumbents facing no election in 2010 (due to staggered elections). Theories of election cycles imply that incumbents facing reelection — even if that contest will not occur for two more years — should be more concerned about public service responsiveness than term-limited incumbents. Voters may be particularly attuned to the responsiveness of their elected officials during election times, whether or not their supervisor is seeking reelection. Thus, from the perspective of an incumbent seeking reelection in two years, improved performance during

Figure 3 Response Times by Treatment Status *After* the Election
After the election, response times do not fall faster in the districts of previously eligible councilors.



Note: Solid lines are linear trends; dashed lines are loess smoothers.

this time period may be an opportunity to persuade future supporters at a moment when supervisors' efforts are particularly salient.[32]

If these differences are driven by reelection incentives, then they should disappear after the election. As Figure 3 illustrates, the trends in our treated and control districts appear very similar in the year after the election.[33] In SI 7 we perform a series of placebo tests using data from the post-election period, and these results also suggest that trends in responsiveness do not diverge after the elections. When election contests are not imminent, response times follow similar trends in our treated and control districts.[34]

[32] An alternative interpretation of our results is that bureaucrats in city agencies are less responsive to lame-duck councilors. However, this interpretation cannot explain why the reelection seeking incumbent in SF outperforms her off-cycle colleagues, who are not lame ducks.

[33] The identities of the individuals in the treatment group do not meaningfully change post-election. Election eligible councilors won reelection in 45 out of 46 cases.

[34] This casts some doubt on a plausible alternative explanation related to rates of learning among first- and second-term officials. Rookie councilors may climb a steeper learning curve and, thus, improve more rapidly than term-limited councilors. Our design is vulnerable to this time-varying confounder, and we do not have a separate measure of learning capacity across councilors over time. However, this alternative explanation implies that divergent trends should be

As an additional falsification test, we look at the November 2009 NYC city council election — a contest in which all incumbents were eligible to run by virtue of the term-limit extension. With no variation in eligibility, we expect to find *similar* trends in responsiveness among first and second-term incumbents. Reassuringly, our estimates of β in this context are precisely estimated zeros (see Table SI.6). This provides additional evidence that variation in election eligibility explains our findings.

Reallocation from Legislative Activity?

To assess whether improved response times to constituency services come at the expense of legislative action, we also collected data on city councilors' legislative activity. The data for NYC come from the city's Legislative Research Center, which compiles all of the legislation introduced in each city council meeting, including information on which councilors sponsored or co-sponsored the actions.[35] We collect similar data for SF. In November 2009, the SF Board of Supervisors started to publish information about which supervisors sponsored particular ordinances, resolutions, and requests for hearings at each Supervisor meeting.[36]

These data sources allow us to generate panel data on legislative activity for every city councilor and supervisor in NYC and SF. For both cities, we code our outcome variable as the number of local laws and resolutions sponsored or co-sponsored by a council member at each meeting. As councilors are better able to control when a bill is introduced than when it is eventually passed, we use the date of the council meeting in which the legislation was introduced and not the date of its eventual passage or dismissal.

We find no consistent evidence that the term-limit extension or approaching elections change legislative effort. Using Equation (1) for column 1 and Equation (2) for columns 2-5 (omitting fixed effects for complaint type and the day of the week), the estimates are small in magnitude and typically indistinguishable from zero. The one significant coefficient in column 3 implies that eligible incumbents (co)sponsored three fewer actions in the six months prior to the primary date. During that period, the average eligible incumbent sponsored over 65 sixty actions (sd = 40); this effect represents a less than five percent change in their overall

apparent just after the election, which we do not observe. This alternative explanation would also suggest that legislative productivity should be increasing more rapidly among first-term incumbents, which we do not observe in Table 4.

[35] Available at `http://legistar.council.nyc.gov/`. To extract data from this site, we amended scripts from Legistar Scraper, a Python library from Gregg and Poe (2013).

[36] The legislation introduced in each supervisor meeting in 2009 can be found at `http://www.sfbos.org/index.aspx?page=1589`.

Table 4 Estimates of Changes in Legislative Activity
Eligible incumbents do not meaningfully reduce their legislative efforts.

	Dependent variable:				
	Total Legislative Actions Introduced				
	NYC 2008	NYC 2005 & SF 2010			
Time frame†	42	365	182	365	182
		Days to Primary		Days to Election	
$\hat{\beta}$	−0.393	−0.002	−0.018	−0.002	0.002
	(1.108)	(0.002)	(0.005)	(0.002)	(0.005)
	p = 0.724	p = 0.357	p = 0.001	p = 0.154	p = 0.679
Observations	160	1,339	803	1,675	876

†Days before election used to estimate model.
Standard errors clustered on districts in parentheses

activity. These findings suggest that incumbents were not cutting back on legislative effort as they ramped up their work on constituency services.[37]

Discussion and Conclusion

This paper considers a long-standing question in political science about how elections shape the work that representatives do while in office. Analyzing 15 million service requests from NYC and SF, we find that city councilors' electoral incentives are a robust predictor of responsiveness to constituents' concerns. Elections encourage overall improvements to constituency responsiveness, consistent with many models of representative democracy. Elections also induce cycles in responsiveness: incumbents ramp up their efforts as elections approach, suggesting increased effort to signal their competence to voters just before they head to the polls.

These findings also contribute to the empirical literature on U.S. local politics. Despite Progressive Era municipal reforms that limited the influence of party machines (Anzia 2012; Bernard and Rice 1975; Bridges 1997; Trounstine 2008), politics still fundamentally shapes local service provision. For example, polarized political preferences (Alesina, Baqir and Easterly 1999; Trounstine 2015), the racial identities of politicians and constituents (Hajnal and Trounstine 2005; Schumaker and Getter 1977), and variation in local institutions (Hajnal 2009; Hajnal and Trounstine 2010) all influence how local governments distribute services. We extend

[37] We have also tried specifications in which we split the outcome variable by type of legislative action, with substantively similar conclusions. We use only two time frames (1 and 0.5 years) when estimating these models due to the availability of these data.

this scholarship by showing how local elections shape the allocation of municipal services within cities over the course of campaigns.

While we are cognizant of the important differences between city and state or congressional offices, our findings also bolster past studies that suggest that state and congressional incumbents seeking reelection devote more effort to constituency services (e.g., Carey, Niemi and Powell 1998; Carey et al. 2006). Though evidence of ideological shirking among retiring or ineligible incumbents is mixed (see Table A.1), there is greater agreement among the small set of studies that focus on casework or constituency services as an outcome.

Lastly, what are the normative implications of our findings? In Madison's ([1788] 1966) propitious view of representative democracy, elections discipline politicians should they fail to serve voters' interests. However, our results could also be indicative of pandering (Canes-Wrone, Herron and Shotts 2001) or responsiveness to a subset of constituents (Sances 2016), rather than diligent effort to serve all residents. Two pieces of evidence leave us more hopeful. First, we do not find that incumbents reallocate effort away from legislative activity towards constituency service requests. Election incentives appear to increase overall effort; eligible incumbents are not obviously pandering by focusing only on voters' short-term interest in, for example, getting a street-light or sidewalk repaired (Mani and Mukand 2007). Second, we do not find that our effects are driven by increased responsiveness in neighborhoods with a particular racial composition (see SI 9); in fact, we find no consistent evidence that our effects are moderated by race.[38] Despite many disaffected voters, an electoral connection persists.

[38] This does not mean that race is inconsequential for service provision in NYC and SF, as we only look at heterogeneous effects and not level-differences in responsiveness.

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Appendix 1 Summary of Existing Literature on Last-Term Shirking

We conducted a survey of work on last-term shirking in the United States by collecting articles from 1990 and onward from Google Scholar. We started with results returned from key word searches (e.g., "shirking term limits" and "shirking retirement congress"), choosing articles that clearly studied the effect of term limits or retirement on an outcome that captures incumbent effort (or, conversely, shirking) in some way. We followed the current literature in defining incumbent effort broadly, e.g., including fiscal outcomes that may not be directly attributable to politicians. We identified additional studies by following up on citations in the articles that were initially returned in our search. This procedure resulted in a total of 26 articles on the topic.

Table A.1 summarizes these articles. As can be seen, they study governors, state legislators, and members of congress. Studies of governors and state legislators analyze the effect of term limits on shirking, while studies of members of congress analyze the effect of retirement. The studies in the table also differ with respect to their dependent variable and, hence, the type of shirking they consider. Most studies analyze ideological shirking — that is, whether the voting record or fiscal policies put in place by last-term incumbents differ from their previous records or their constituents' preferences. Six studies analyze legislative attendance rates, two studies consider casework/constituency services, and one study looks at agency oversight.

Table A.1 Recent empirical investigations of last-term shirking in the United States

Paper	Sample	Dependent variable(s)	Evidence of shirking? Ideo.	Attend.	Casework	Oversight
Term limits						
Alt et al. (2011)	Governors	Fiscal variables	✓			
Bails and Tieslau (2000)	State legislatures	Fiscal variables	×			
Besley (2006)	Governors	Fiscal variables; congruence	×			
Besley and Case (1995)	Governors	Fiscal variables	✓			
Besley and Case (2003)	Governors	Fiscal variables	✓			
Cain and Kousser (2004)	California	Vote deviation; oversight	×			✓
Carey et. al. (1998)	State legislatures	Legislation; casework	×		✓	
Carey et al. (2006)	State legislatures	Legislation; casework	×		✓	
Clark and Williams (2013)	State legislatures	Vote deviation; attendance	✓†	✓		
Crain and Oakley (1995)	Governors	Capital investments	✓			
Crain and Tollison (1993)	Governors	Fiscal volatility	✓			
Cummins (2012)	State legislatures	Budget balance	✓			
Erler (2007)	State legislatures	Fiscal variables	×			
Keele et al. (2013)	State legislatures	Fiscal variables	×			
Lewis (2012)	State legislatures	Fiscal variables	✓			
Wright (2007)	State legislatures	Congruence; attendance	×	✓		
Retirement						
Carson et al. (2004)	Congress	Vote deviation	×			
Figlio (1995)	Congress	Vote deviation; attendance	✓	✓		
Lott (1987)	Congress	Vote deviation; attendance	×	✓		
Lott (1990)	Congress	Attendance		✓†		
Lott and Bronars (1993)	Congress	Vote deviation	×			
Poole and Romer (1993)	Congress	Vote deviation	×			
Rothenberg and Sanders (2000)	Congress	Vote deviation; attendance	✓	✓		
Snyder and Ting (2003)	Congress	Vote deviation	✓†			
Tien (2001)	Congress	Congruence	✓			
Vanbeek (1991)	Congress	Vote deviation	×			

Notes: ✓ = results in study can be interpreted as evidence of shirking, × = no evidence of shirking, † = conclusion applies only to a subset of states or legislators. The four types of shirking are with respect to vote content (ideology), legislative attendance rates, constituency services (casework), and agency oversight. Cells are left blank if a study did not consider a given type of shirking. "Fiscal variables" include per capita state government expenditure and taxation (and sometimes borrowing costs and economic growth).

Appendix 2 Information about Service Protocol and Time Line

Table A.2 provides an overview of how 3-1-1 service requests are handled from when they are opened until they are closed. A request has been *opened* when all the intake information about the request has been logged and it has been assigned to an agency. The agency will *close* the request when the issue has been resolved (which may require rerouting it to a different agency) or when it has determined that no action is necessary. In all cases, agency workers will physically check on the issue to determine what type of work is needed. In the paper, we code our dependent variable as the number of days it took from the time a request was opened until it was closed.

Table A.2 Service Request Protocol and Timeline

1. A request is called in to a 3-1-1 response center or submitted online.

2. A 3-1-1 representative (or online system, if the request was submitted online) will ask for and determine the type of request, and an actioning agency will be assigned. Based on a pre-assigned workflow, the service representative will then do the intake of required information (as requested by the assigned agency) and submit the request. The request has been *opened*, and the date is recorded in the 3-1-1 database.

3. The request appears in the acting agency's queue for action, and will have a service level agreement (SLA) specifying a due date based on the request type. The agency may prioritize among different requests as they see fit.

4. Agency staff physically inspect the reported issue, with one of three potential outcomes, each of which results in the request being *closed*:
    a. Agency staff resolve the issue (may require revisits). Some issues are easily verified as resolved (e.g., graffiti removed), while others may require following up with the constituent (e.g., calling the next day to verify that heating works). Once the agency has ensured that the issue is resolved, the agency will report it as closed.
    b. The issue is rerouted to a different agency. This may happen if the agency determines the issue falls outside its jurisdiction. For example, NYC Department of Transportation may reroute a highway issue to NY State, or a misreported issue may be rerouted to a different city agency. In this case the issue will be marked as closed once it has been resolved by the new agency.
    c. Agency staff determine that no work is warranted (e.g., trash was already picked up, prank call) and mark it as closed.

Appendix 3 Summary Statistics

Table A.3 Summary Statistics for NYC Service Request Data, 10/23/2007 - 10/23/2009

Statistic	N	Mean	St. Dev.	Min	Max
y:Response Time (Days)	3,420,140	17.48	56.57	0	1,043
t:$\mathbb{1}$(Post-Ext.)	3,420,140	0.50	0.50	0	1
D:$\mathbb{1}$(Compliers)	2,742,258	0.73	0.44	0	1

Trimmed Top 0.1% of Response Times

Table A.4 Summary Statistics for NYC, 1/1/2004 - 11/08/2005

Statistic	N	Mean	St. Dev.	Min	Max
y:Response Time (Days)	2,378,172	22.65	60.10	0	785
t:Days Before Election	2,378,172	281.31	168.30	0	677
D:$\mathbb{1}$(Treated)	2,376,717	0.87	0.34	0	1

Trimmed Top 1% of Response Times

Table A.5 Summary Statistics for SF, 11/02/2008 - 11/02/2010

Statistic	N	Mean	St. Dev.	Min	Max
y:Response Time (Days)	612,338	24.08	57.86	0.00	524.97
t:Days Before Election	323,105	414.52	244.81	0	853
D:$\mathbb{1}$(Treated)	612,338	0.05	0.21	0	1

Trimmed Top 1% of Response Times

Supporting Information

Do Elections Improve Constituency Responsiveness? Evidence from U.S. Cities

Following text to be published online.

Contents

SI 1 Changes in Request Volume Following Term Extension (NYC 2008)

We explore whether changes in response times are driven by differential changes in the demand for constituency services. Concretely, we estimate the difference-in-differences specified in equation 1 using the number of requests as the dependent variable (omitting the fixed effects for request type). Figure SI.1 and table SI.1 do not suggest differential changes in request volume: newly eligible (treated) incumbents saw an increase of three to eight requests in their districts — small fluctuations relative to the average number of requests that cannot be distinguished from zero in two of three specifications.

Figure SI.1 Average Number of Requests around the Term Extension
No differential change in request volume around the term extension.

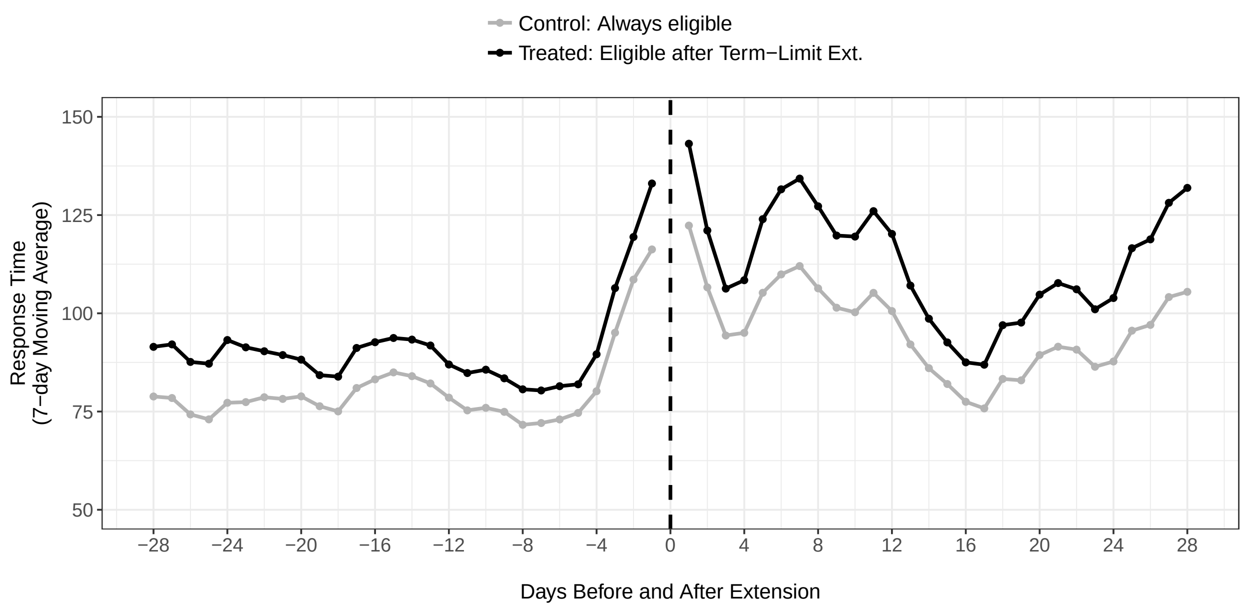


Table SI.1 Effect of Term Extension of Request Volume

	Dependent variable:		
	Number of Requests		
	2	3	4
$\hat{\beta}^{\ddagger}$	3.424	4.451	7.611
	(2.908)	(2.973)	(4.331)
	p = 0.240	p = 0.135	p = 0.080
Observations	1,160	1,720	2,280

‡Difference-in-differences estimator (see Eq. 1)
Standard errors clustered on districts in parentheses.

SI 2 Changes in Request Types Following Term Extension (NYC 2008)

We also consider whether the composition of service requests changes differentially in districts where incumbents' election eligibility changes. For the top twenty service requests (which account for 94 percent of observations), we count the frequency of each type of request in every day and district. We then regress our treatment indicator (D_{dt}) on these frequencies, including district and day fixed effects. (We use two weeks on either side of the policy change for this analysis.) This is comparable to a joint test of orthogonality when evaluating balance and allows us to evaluate whether changes in the frequency of certain requests predicts treatment.

Looking at Table SI.2, the coefficients are uniformly small. We focus, however, on the joint hypothesis that the coefficients on these frequencies all equal zero: our F-statistic is 1.417, $p = 0.17$. We cannot, thus, reject the null that that the coefficients on all of these categories equal zero.

Table SI.2 Joint Test that Request Composition does not Predict Treatment

	Dependent variable:
	D_{dt}
Animal Care	−0.003 (0.006)
Appliance	−0.005 (0.008)
Broken Meter	−0.001 (0.004)
Building/Use	0.003 (0.003)
Complaint	0.009 (0.006)
Construction/Plumbing	0.002 (0.001)
Damaged Tree	−0.004 (0.006)
Derelict Vehicle	0.004 (0.004)
Electric	0.0002 (0.003)
Heating	0.0002 (0.0003)
Noise	−0.004 (0.003)
Nonconst	−0.005 (0.003)
Paint/Graffiti	−0.002 (0.001)
Sanitation/Cleaning	0.005 (0.002)
Sidewalk/Sewer	0.002 (0.001)
Street Light Condition	0.002 (0.002)
Street/Highway/Bridge Issues	−0.004 (0.002)
Taxi	0.003 (0.005)
Traffic Signal Condition	−0.001 (0.004)
Water	−0.002 (0.003)
Observations	1,120
F-statistic	1.417
p-value	0.17
Note:	Standard errors clustered on districts in parentheses

SI 3 Robustness to Including Incumbents Not Seeking Third Term (NYC 2008)

The treatment group for our main analysis consists of incumbents who were termed out before the October 23, 2008 decision but ran for a third term after the decision. Not all of the newly eligible councilors took advantage of the extension; eight incumbents left politics or ran for different positions (e.g., four ran for comptroller). In Table SI.3, we show that our results hold but decline in magnitude when we include all term-limited incumbents in our treatment group. The results attenuate as expected: we would not expect councilors intending to leave office to exert more effort following the term limit extension.

Table SI.3 Effect of Term Extension on Constituency Services, All Term-Limited Incumbents

	Dependent variable:			
	Response Time			
Time frame†	2	3	4	12
$\hat{\beta}$‡	−1.001	−1.202	−1.089	−0.441
	(0.703)	(0.627)	(0.496)	(0.808)
	p = 0.155	p = 0.056	p = 0.029	p = 0.586
	$\mathbb{1}$(Response Time $<$ 5 Days)			
Time frame†	2	3	4	12
$\hat{\beta}$‡	0.026	0.013	0.015	0.001
	(0.010)	(0.008)	(0.009)	(0.009)
	p = 0.013	p = 0.125	p = 0.095	p = 0.868
Observations	145,229	211,947	284,927	822,431

† Weeks on either side of the extension used to estimate Eq. 1
‡ Difference-in-differences estimator (see Eq. 1)
Standard errors clustered on districts in parentheses

SI 4 Heterogeneous Effects by Vote on the Extension (NYC 2008)

Table SI.4 indicates councilors' votes on the term extension based on their treatment status and decision to run for a third-term in office.

Table SI.4 Voting for Term Extension by Treatment Status

	Group	Freq.	Prop. Voting Yes
1	Control	12	0.330
2	Second-Term, Contesting	28	0.790
3	Second-Term, Retiring	11	0.270

In Table SI.5, we estimate whether treated incumbents that supported the term extension on October 23, 2008 saw larger reductions in response time. This analysis employs a four-week window around the policy change and does *not* indicate heterogeneous effects based on councilors' support for the reform; councilors supporting the reform do not achieve larger reductions in response times.

Table SI.5

	Dependent variable:	
	Response Time	
	Constesting Incumbents	All Incumbents
D_{dt}	-1.641	-1.188
	(0.967)	(0.632)
	p = 0.090	p = 0.061
$D_{dt} \times \mathbb{1}$(Voted Yes)	0.487	0.141
	(1.033)	(0.688)
	p = 0.637	p = 0.838
Observations	230,067	284,927

Standard errors clustered on districts in parentheses.

SI 5 Post-Extension Trends as 2009 NYC Election Approaches

In Table SI.6, we explore post-extension trends in response times leading up to the November 3, 2009 election in NYC. Since all councilors were eligible to run for reelection after the term limit extension, we do not expect different response time *trends* in districts represented by previously term-limited councilors (treated) and districts represented by councilors who were always eligible for reelection (control).

We test this expectation using Equation 2. We use two time periods: a year and six months before the election. If there were no differential changes to response times in treated and control districts, then $\hat{\beta}$ from Equation 2 should be indistinguishable from 0. Table SI.6 provides strong evidence for this: the estimates of differential time trends are very close to zero.

We emphasize that this analysis is different from our tests of the impact of the term-limit extension, for which we use Equation 1. The extension analysis shows that response times dropped in districts represented by previously term-limited councilors in the weeks after the extension. However, as we also show, this effect attenuates with time. This makes sense given the analysis presented here: as the 2009 election approaches, response time trends in treated and control districts become indistinguishable.

Table SI.6 Tests for Differential Trends before the 2009 NYC Election

	Dependent variable:	
	Response Time	
Days until election:	365	182
$\hat{\beta}$	0.0001	-0.0002
	(0.0001)	(0.0004)
	p = 0.639	p = 0.635
Observations	1,294,925	581,386

Note: Standard errors clustered on districts in parentheses

SI 6 Robustness to Trimming

The tables below demonstrate the robustness of our results to different decisions about how to trim the dependent variable, which has a long right tail.

Table SI.7 Robustness of Election Eligibility Results to Trimming Decisions

	Dependent variable:		
	Response Time		
$\max\{y\}$	1 year	2 years	3 years
$\hat{\beta}^{\ddagger}$	-0.801	-0.709	-1.241
	(0.525)	(0.521)	(0.562)
	p = 0.127	p = 0.174	p = 0.028
Observations	229,349	229,570	230,067

Uses 4 weeks on either side of the extension to estimate Eq. 1.
‡Difference-in-differences estimator (see Eq. 1)
Standard errors clustered on districts in parentheses.

Table SI.8 Robustness of Differential Time Trends to Trimming Decisions (β in Equation 2)
The linear trend in responsiveness falls significantly faster where incumbents can run for reelection.

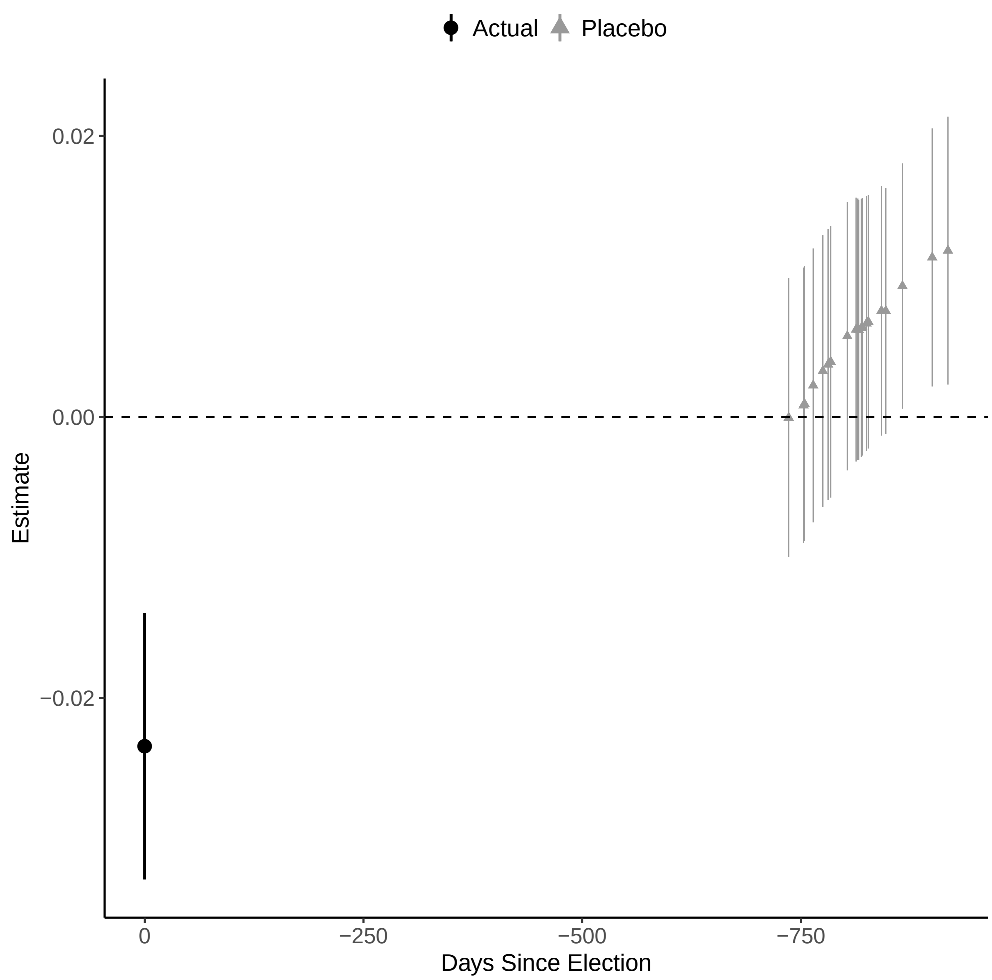
	Dependent variable:		
	Response Time		
$\max\{y\}$	1 year	2 years	3 years
	Days to Primary (NYC) or Filing Date (SF)		
$\hat{\beta}$	-0.020	-0.022	-0.016
	(0.004)	(0.005)	(0.005)
	$p < 0.001$	$p < 0.001$	$p < 0.001$
Observations	2,293,059	2,309,478	2,315,730
	Days to General Election		
$\hat{\beta}$	-0.014	-0.014	-0.007
	(0.005)	(0.006)	(0.006)
	p = 0.003	p = 0.012	p = 0.267
Observations	2,434,722	2,452,777	2,458,658

Uses 547 days before election date to estimate Eq. 2.
Standard errors clustered on districts in parentheses.

SI 7 Placebo Tests for Election Timing Results

We randomly draw 20 dates from the period following the 2005 election in NYC and the 2010 election in SF. We then re-estimate β from Equation 2. We use 730 days to estimate these placebo regressions. Thus, our "placebo" election dates have to be drawn from an interval of 200 days that is at least two years after the actual election date. This explains why the placebo dates fall well to the right of the estimate associated with the actual election dates (the left-most, black points). These tests suggest that the differential time trends that we discover in our analysis do not persist after the election.

Figure SI.2 Placebo Results for Election Timing



Displayed above are the estimates and 95% confidence intervals for $\hat{\beta}$ from Equation 2 using two years of data before each placebo date. The estimate from the actual term-limit extension is the left-most, black point.

SI 8 Separate Results for NYC 2005 and SF 2010

Table SI.9 Separate Estimates of Differential Time-Trends for NYC 2005 and SF 2010 (β in Eq. 2)
The linear trend in responsiveness falls significantly faster where incumbents can run for reelection.

	Dependent variable:			
	Response Time			
Time frame†	730	547	365	182
	NYC 2005			
$\hat{\beta}$	-0.012	-0.006	0.005	-0.022
	(0.004)	(0.004)	(0.008)	(0.019)
	p = 0.003	p = 0.097	p = 0.533	p = 0.249
Observations	2,376,717	2,238,742	1,578,977	774,375
	SF 2010 (Control: No Election)			
$\hat{\beta}$	-0.014	-0.025	-0.039	-0.002
	(0.004)	(0.006)	(0.009)	(0.022)
	p = 0.001	$p < 0.001$	$p < 0.001$	p = 0.943
Observations	160,678	120,549	81,801	42,571
	SF 2010 (Control: Term-Limited)			
$\hat{\beta}$	-0.022	-0.039	-0.071	-0.047
	(0.011)	(0.020)	(0.036)	(0.040)
	p = 0.050	p = 0.055	p = 0.049	p = 0.237
Observations	135,393	101,458	68,933	35,531

†Days before general election used to estimate Eq. 2
Standard errors clustered on districts in parentheses

Table SI.10 Separate Estimates of Changes in Legislative Activity for NYC 2005 and SF 2010
Eligible incumbents either maintain or increase their legislative effort, suggesting no reallocation.

	Dependent variable:					
	Total Legislative Actions Introduced					
	NYC 2005		SF 2010 (Control: No Election)		SF 2010 (Control: Term-Limited)	
Time frame†	365	182	365	182	365	182
$\hat{\beta}$	-0.0004	-0.0002	0.006	0.016	0.002	0.008
	(0.003)	(0.006)	(0.0004)	(0.002)	(0.002)	(0.001)
Observations	1,224	612	287	168	205	120

†Days before general election used to estimate model (excluding γ_{type}).
Standard errors clustered on districts in parentheses

SI 9 DiD Estimates by Racial Composition

Our findings suggest that public service responsiveness improves when public officials are eligible to seek reelection and as elections approach. We are also interested in whether some neighborhoods benefit more from these improvements than others. In particular, we are interested in whether the racial composition of neighborhoods is responsible for heterogeneous treatment effects in responsiveness. Public officials may, for example, favor coethnic constituents or constituents from a particular racial group.

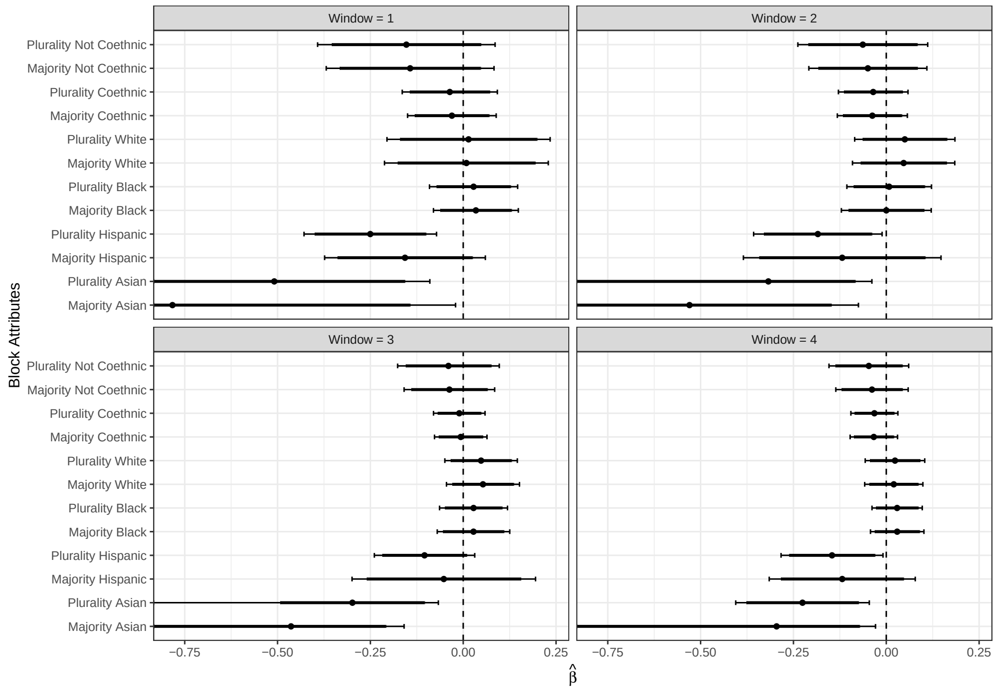
To carry out this analysis we begin by matching the 3-1-1 database with census data on the racial composition of every block in NYC. We then code two characteristics that indicate each block's relationship with the city council member representing the electoral district in which the block is located: whether or not its largest group is coethnic with the city council member; and whether or not its majority group (if any) is coethnic with the city council member. We also create variables for each block's plurality group and (if applicable) majority group without regard for its relationship with the city council member. We then re-run our analyses on different subsets of the data based on these variables.

The results are displayed in Figures SI.3 and SI.4. They suggest that the heightened responsiveness that followed the term-limit extension in 2008 is not driven by ethnic favoritism: neighborhoods in which the plurality (or majority) group is coethnic with the city council member representing the district do not see larger drops in response times than other neighborhoods. We find some evidence, however, that neighborhoods populated primarily by Hispanics or Asians see larger drops in response times, though this may not reflect the ethnicity of these neighborhoods but rather some unobserved characteristic of the neighborhoods that we are not controlling for here (e.g., location).

Moving to the 2005 council elections, response times dropped quite uniformly across neighborhoods two years to a year and a half before the elections. In general, there are few interesting heterogeneous treatment effects to report, perhaps with the exception that Asian neighborhoods saw larger drops closer to the election.

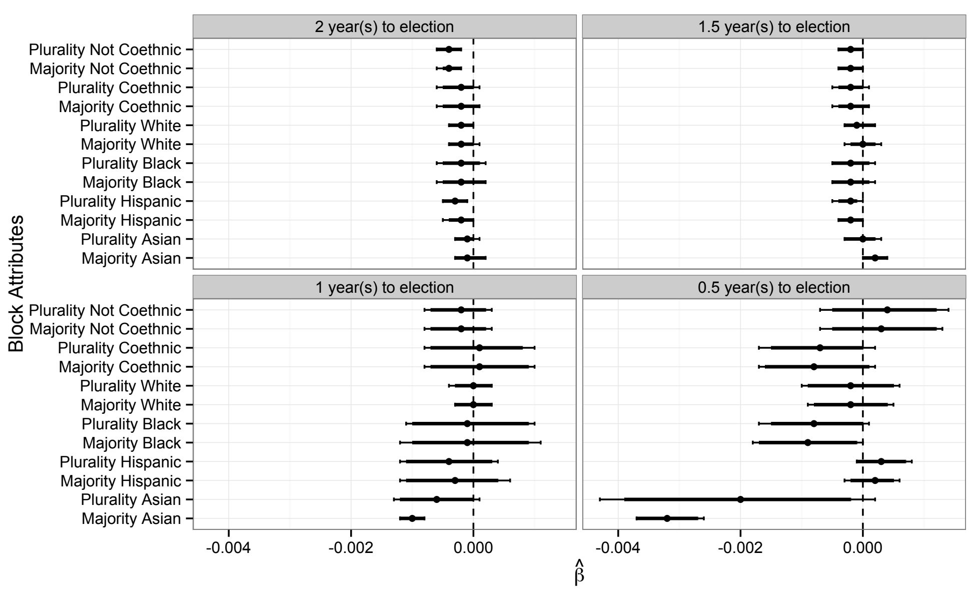
Our failure to uncover consistent heterogeneous effects related to neighborhoods' ethnic composition should not be taken to imply that there are no ethnic disparities in public service delivery. Our empirical strategy leverages changes in responsiveness and, thus, does not address level differences in service delivery across neighborhoods of varying composition.

Figure SI.3 The Term Limit Extension of 2008 and Heterogeneous Effects by Neighborhood Race



We run Equation 1 in subsets of neighborhoods defined by the attributes on the y-axis. "Window" gives the number of weeks on either side of the extension we use. The estimates of β (with 95% CIs) from these regressions are displayed above.

Figure SI.4 The 2005 NYC Election and Heterogeneous Effects by Neighborhood Race



We run Equation 2 in subsets of neighborhoods defined by the attributes on the y-axis, for different time windows before the election. The estimates of β (with 95% CIs) from these regressions are displayed above.