

# Elections and Shirking: An Empirical Reappraisal<sup>\*</sup>

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October 11, 2015

## Abstract

We address a central debate in political science about whether and to what extent elections affect the quality of representation. Synthesizing prior work on incumbent shirking, we identify three components of a high-quality research design: measures that clearly capture incumbent effort, measures of multiple incumbent activities, and the identification of credible counterfactuals. Past studies have not incorporated all of these components, which could account for their mixed findings. We do, and find that incumbents shirk when they are not eligible for reelection (due to term-limits or staggered elections) and when their reelection contests are less imminent. These analyses draw on administrative records on 15 million service requests and on a new dataset on incumbents' legislative activity in U.S. cities. Our study helps make sense of inconsistent findings in the existing literature and suggests an improved approach for studying a core question in political science.

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<sup>\*</sup>We are grateful to Michael Bechtel, Gary Cox, Nick Eubank, Francisco Garfias, Justin Grimmer, Jens Hainmueller, David Hausmann, Clayton Nall, Julia Payson, and participants of Stanford's American Politics Workshop for comments on earlier drafts.

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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 1974) encourages representatives to serve their constituents for fear of being ousted on election day. Prior research on whether such a connection exists falls into two categories. First, do representatives exert less effort — “shirk” — in their final term in office (i.e., when the electoral connection is severed)? Second, do representatives shirk when elections are distant and, thus, less salient to voters (i.e., when the electoral connection is weakened)?

We bring new evidence to bear on both questions by implementing an improved empirical strategy for identifying incumbent shirking. Our survey of the existing literature — summarized in Table A.1.1 — shows that a high-quality empirical study of shirking should include three components:

- (1) *Measures of incumbent activity that clearly capture effort.* Prior work in political science often relies on the content of incumbents’ voting records and the resulting policy outcomes to measure effort. We show why it is preferable to focus on other incumbent activities, such as constituency services, if our goal is to identify shirking.
- (2) *Multiple measures of incumbent activity.* Most prior studies analyze only one incumbent activity that clearly captures effort. Unfortunately, this makes it difficult to distinguish between a shirking incumbent and an incumbent who is reallocating effort to other activities.
- (3) *A research design that establishes credible counterfactuals.* Cross-sectional comparisons of representatives from different districts can be confounded by omitted variables. Quasi-experimental designs that compare changes in effort among similar incumbents, only some of whom are up for reelection, are better able to identify shirking.

Prior empirical studies have not incorporated all of these elements, often due to data limitations. Using a new dataset on 15 million service requests placed by residents in New York City (NYC) and San Francisco (SF), our research design includes all three elements and therefore can better identify incumbent shirking. First, our data allow us to implement a detailed measure of

incumbents' efforts on constituency requests. This focus on constituency services fills an important gap in past research. Seminal work in political science stresses the central role of constituency services in the daily lives of representatives (Cain, Ferejohn and Fiorina 1987; Fiorina 1989, ch. 7; Mayhew 1987).<sup>1</sup> Yet, past empirical work has generally not studied how elections affect this important incumbent activity.<sup>2</sup> Second, to ensure that we have multiple measures of incumbent activity, we also collect original data on city councilors' legislative activity. Third, we implement a quasi-experimental research design that takes advantage of two institutions — term limits and staggered elections — that disqualify a subset of councilors from reelection contests. This allows us to compare groups of incumbents who work in the same city at the same time but face different electoral incentives. Using our new data and design, we shine new light on one of the most central concerns in all of political science: the degree to which elections affect the quality of representation.

Our results provide robust evidence that incumbents exert more effort (1) when they are eligible for reelection and (2) as their reelection contests approach. To show that election eligibility bolsters effort, we take advantage of the term-limit extension pushed through by New York City's Mayor Bloomberg in 2008. The extension suddenly allowed some city council members to seek a third term in office. In the immediate aftermath of the reform, we find that service responsiveness improved substantially in these councilors' electoral districts, relative to districts represented by first-term councilors whose eligibility was unaffected by the reform. In contrast, legislative effort — which provides delayed and more diffuse benefits to voters — remained constant before and after the extension. Second, we show that incumbents' efforts with these activities vary throughout the

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<sup>1</sup>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."

<sup>2</sup>Carey, Niemi and Powell (1998) and Carey et al. (2006) are important exceptions. Unfortunately, their measures of effort with constituency services are all self-reported by state legislators.

electoral cycle: eligible city councilors expend more effort on constituency services (but not necessarily on legislation) later in their terms when their reelection bid is more imminent. Interviews with city councilors' offices bolster these quantitative findings and suggest a plausible mechanism by which city councilors affect responsiveness to service requests in their districts, namely, by exerting significant formal and informal pressure on city agencies.

Our findings are consistent with Madison's and Mayhew's claims that elections should motivate incumbent effort. They are also consistent with the predictions of prominent theoretical models on the effects of election eligibility (e.g., Alt, Bueno de Mesquita and Rose 2011; Besley 2006; Dewan and Shepsle 2011; Przeworski, Stokes and Manin 1999) and election timing (e.g., Nordhaus 1975; Rogoff 1990; Shepsle et al. 2009; Tufte 1978). While these models generate clear theoretical predictions, empirical tests have generated mixed results. It appears that for every study that finds that incumbents shirk in their last term (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), there exists a study that finds no such effect (e.g., Besley 2006; Carson et al. 2004; Lott and Bronars 1993; Poole and Romer 1993; Keele, Malhotra and McCubbins 2013). Similarly, there is inconclusive evidence about the extent to which incumbents increase their effort or manipulate policy closer to elections. Franzese (2002, 378) reviews this literature 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)."<sup>3</sup>

The three components highlighted above are useful for understanding these contradictory findings. Empirical studies have employed a range of measures of incumbent effort (e.g., ideological congruence with voters and government expenditure), many of which do not directly capture effort. State fiscal policy, for example, ultimately reflects the counter-balancing efforts of many legislators and the governor. Does higher government spending in a state indicate shirking by fiscal hawks or doubled efforts by progressives? This measurement problem is compounded by the fact that many

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<sup>3</sup>See Canes-Wrone and Park (2012), Grier (2008), and Krause (2005) for more recent contributions.

studies analyze only one incumbent activity. Concentrating on a single activity, scholars cannot tell whether the shirking they observe is offset or magnified by changes in effort across other activities. Putting these issues aside, the mixed results could also result from the varied empirical strategies used to study the effects of electoral incentives. Estimating the causal effect of elections on incumbent behavior requires us to identify credible counterfactuals (i.e., otherwise similar incumbents with different reelection incentives), a point forcefully made by Keele, Malhotra and McCubbins (2013).<sup>4</sup> By identifying and overcoming the challenges that have limited past empirical work on this topic, we believe this paper makes an important contribution to our understanding of whether and when electoral incentives discipline representatives.

## **Elections and Incumbent Effort: Clear Theoretical Expectations**

In theory, elections improve the quality of political representation. This can happen in two ways: first, elections may weed out or discourage low quality candidates (a selection effect); and second, elections may encourage incumbents to exert effort to win over voters and secure reelection (an accountability effect) (Alt, Bueno de Mesquita and Rose 2011; Besley 2006). If we sever or weaken the electoral connection, then the latter mechanism implies that incumbents will be less accountable to their constituents and, thus, less inclined to exert costly effort on their behalf.

Past work suggests two instances when this connection between incumbents and voters is absent or attenuated. First, incumbents entering their last terms in office — due to retirement or

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<sup>4</sup>A simple example illustrates how both cross-sectional and longitudinal designs can lead to incorrect inferences about the importance of elections. Suppose that election eligibility does, in fact, reduce shirking, but that experienced politicians can also more effectively serve their constituents. If we now compare a first-term governor to a term-limited governor, we might see no difference between the two. The issue for causal inference is that shirking by the term-limited governor could be offset by her additional experience. What if we try to improve upon this design by following the same governor over time? If we want to compare her behavior during similar moments in the electoral cycle, then we would again be making comparisons confounded by different levels of experience.

term-limits — 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. Nordhaus 1975; Shepsle et al. 2009; Tufte 1978).

These claims can be summarized by a simple maximization problem, where incumbents choose how much effort  $e_t \in [0, \bar{e}]$  to exert at a cost  $c$  in each of  $T$  periods (e.g., months) in their term. If eligible, effort increases their probability of reelection, which is represented by the function  $\gamma(\cdot)$ .

$$\max_{e_1, \dots, e_T} \left\{ \sum_{t=1}^T \mathbb{1}(\text{Eligible}) \gamma(e_t, t) - c e_t \right\}$$

Incumbents maximize their payoff by selecting the effort level that equalizes the marginal benefit and cost of effort in each period (i.e.,  $e_t^* = \arg\{\mathbb{1}(\text{Eligible}) \gamma_e(e, t) = c\}$ ). While ineligible incumbents would never want to exert themselves, incumbents seeking reelection make some effort in every period, as the returns to doing so are always positive, even if sometimes minuscule.<sup>5</sup> This delivers our first prediction: *term limits reduce incumbent effort*.<sup>6</sup>

What if we now allow the returns to effort to vary across an incumbent’s term in office? In particular, suppose that the returns to effort increase as the next election approaches.<sup>7</sup> Past research has offered two reasons for this. First, if 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). Second, even if voting is prospective (rather than retrospective), reelection-seeking incumbents may ramp up their efforts as elections approach to signal their supe-

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<sup>5</sup> $\gamma_e(e, t) > 0$  for every  $t \in \{1, \dots, T\}$

<sup>6</sup>For a review of models making this and related claims, see Dewan and Shepsle (2011).

<sup>7</sup>Mathematically,  $\gamma(e, t)$  is a continuous function with increasing differences.

rior competence (Rogoff 1990).<sup>8</sup> These two strands of the literature both imply that the optimal level of effort for eligible incumbents increases as elections approach (i.e.,  $e_t^* > e_{\underline{t}}^*$  for any  $t > \underline{t}$ ). Our second prediction then is that *eligible incumbents should increase their effort levels over the course of their terms (while effort among ineligible incumbents should remain constant)*.<sup>9</sup>

## Elections and Incumbent Effort: Conflicting Empirical Results

Despite the clarity of these two predictions, one can find empirical studies that claim to support and refute both of them. Yet these studies (1) do not always rely on measures that clearly capture effort, (2) usually cannot rule out effort reallocation, and (3) use research designs of varying quality. We discuss each of these issues in turn.

### Capturing Effort

There are, as Lott (1990, 133) points out, “as many [potential measures of effort] as there are outputs that a politician produces.” However, extant studies overwhelmingly focus on voting records and the resulting policies (see Table A.1.1), analyzing whether last-period representatives (a) vote differently (e.g., Lott 1987; Lott and Bronars 1993; Snyder and Ting 2003), (b) vote in opposition to their constituents’ preferences (e.g., Besley 2006; Wright 2007; Tien 2001), or (c) favor a different set of fiscal policies (e.g., Erler 2007; Keele, Malhotra and McCubbins 2013).

Changes in voting and fiscal policy preferences do not necessarily indicate shirking, however. Suppose U.S. members of Congress change their voting patterns just before retiring (e.g. Figlio 1995; Rothenberg and Sanders 2000). Is this evidence of diminished effort? These representatives’ new voting patterns — and their decisions to retire — could be a response to changing demographics or preferences within their constituency. If so, we cannot conclude that incumbents’ vote deviations or

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<sup>8</sup>See Besley (2006) and Canes-Wrone, Herron and Shotts (2001) for other signaling models in this tradition.

<sup>9</sup>This aligns with an expansive literature on electoral cycles in incumbents’ behavior (Schumpeter 1939; Nordhaus 1975; Tufte 1978; Rogoff 1990; Schultz 1995; Franzese 2002; Canes-Wrone and Park 2012).

support for particular fiscal policies indicate ideological shirking.<sup>10</sup> Some scholars have addressed this shortcoming by measuring congruence — the extent to which incumbents' votes reflect their constituents' preferences (Besley 2006; Tien 2001; Wright 2007). While this is a promising approach, Besley (2006) points out that congruence could reflect accountability (an absence of ideological shirking) or simply that incumbents agree with their constituents and would vote congruently regardless of their election eligibility. A finding that last-term incumbents vote congruently cannot then be taken as evidence that elections do not affect shirking.

While this research has generated very mixed findings, a clearer picture emerges when we look at the smaller number of studies that measure voting abstention rates, constituency services or casework, or agency oversight — activities that clearly require costly effort by incumbents. Taken together, this work suggests that retiring or term-limited incumbents tend to cast fewer votes (Clark and Williams 2013; Wright 2007; Figlio 1995; Lott 1987; 1990; Rothenberg and Sanders 2000), interact less with constituents (Carey, Niemi and Powell 1998; Carey et al. 2006), and exercise less oversight (Cain and Kousser 2004).<sup>11</sup>

## **Reallocation versus Reduction in Effort**

Most prior studies that do use a measure of incumbent activity that can be clearly mapped to effort (e.g., voting abstention rates, casework, or oversight) focus on only *one* such activity. Unfortunately, by concentrating on only one aspect of a politician's job, studies cannot distinguish between two different outcomes: a shirking incumbent and an incumbent who is reallocating effort to other activities. An example helps illustrate this problem. Suppose an incumbent is resource constrained

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<sup>10</sup>Besley (2006) finds that term-limited governors tend to spend and tax more. Yet, he also finds that term-limited governors display *higher* levels of congruence with voters. It is hard to reconcile these findings with the common assumption that higher spending and taxation indicate *incongruence*.

<sup>11</sup>These alternative measures of effort are largely absent from the literature on election timing, which, following Nordhaus (1975), focuses on variables related to fiscal policy (e.g., Rogoff 1990; Schultz 1995).



and up against a term-limit. To bolster her legacy, this incumbent decides to devote ten additional hours per week to legislation but allocates twenty fewer hours per week to casework. If we only observe her legislative record, we would wrongly conclude that this incumbent exerted more effort in her last term when, in fact, the opposite is true. If we only gather data on one activity, we risk over- or under-stating the extent to which elections influence incumbents' behavior. This methodological concern motivates our own efforts to collect information on several incumbent activities.

## Research Design

The fundamental problem of causal inference implies that we cannot observe the same incumbent, at the same point in time, when she does and does not face (an imminent) reelection. To date, scholars have adopted one of two strategies for making causal inferences: comparisons of a cross-section or repeated cross-sections of incumbents, or, far less frequently, longitudinal analysis that tracks the same incumbents over time.

The first set of papers compares incumbents that are or are not in their last terms in office (e.g., Carey, Niemi and Powell 1998; Carey et al. 2006; Figlio 1995; Tien 2001; Wright 2007). These studies face several threats to inference in the form of omitted variables. Last-term incumbents may be systematically different from their colleagues, who tend to be less experienced and, more importantly, did not self-select into retirement.<sup>12</sup> As mentioned above, demographic or ideological changes within an incumbent's constituency could motivate both changes in voting patterns and the decision to retire.

Other studies employ panel data of governors or members of Congress from the same state or congressional district (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).<sup>13</sup> Panel data allow these scholars to exploit variation among representatives within the

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<sup>12</sup>Lott and Bronars (1993, 128) show “how misleading it can be to examine variation across individuals in order to learn about how an individual congressman will vary his behavior over time.”

<sup>13</sup>The unit of observation is the state or district, not the individual incumbent.

same state or district over time, rather than leveraging variation across officials that represent different constituencies. This approach alleviates concerns about omitted variables that do not vary at the state- or district-level over the study period (e.g., gubernatorial powers at the state-level). Nevertheless, these studies have to contend with concerns about self-selection if one of their groups is composed of retiring members.<sup>14</sup>

Finally, Rothenberg and Sanders (2000) regress changes in behavior (from one congress to the next) on whether members of Congress are retiring or pursuing another office. Rothenberg and Sanders (2000) do not focus on level differences across incumbents. Rather, they leverage changes in behavior that occur during incumbents' last period in office, relative to changes among incumbents running for reelection. We improve upon this design in two ways. First, our high frequency data allow us to avoid making comparisons across terms in office, alleviating concerns about time-varying confounders across our treatment and control units. Second, we avoid selection concerns associated with retirement decisions by restricting our comparisons to incumbents serving within the same city during the same time — and whose election eligibility is mandated by law rather than chosen.

## **Our Measures of Incumbent Effort**

We adopt two measures of incumbent effort. The first measure captures incumbents' efforts to have service requests resolved in their districts by exerting pressure on city agencies, while the second assesses their efforts to introduce new legislation.

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<sup>14</sup>This selection concern not only applies to individual retirement decisions, but also to state-level reforms regarding term limits. Keele, Malhotra and McCubbins (2013) argue that states that do not adopt term-limits provide a poor counterfactual for the states that implement the reform. After pre-processing the data to account for selection on observables, these authors do not find evidence that term-limits change per capita spending, relative to their (synthetic) control group.

## Constituency Services

Since the mid-2000s, many major U.S. cities have implemented 3-1-1 programs meant to redirect non-emergency requests from 9-1-1 and to centralize hotlines maintained by individual city departments. NYC and SF have created Open Data initiatives that make it possible for researchers — and, as we note below, politicians — to tap into this resource.<sup>15</sup> The NYC database has around 14 million observations going back to 2005; the SF database, around 1 million observations going back to 2008. To get a sense for the nature of these service requests, the most frequent request types in the NYC database (2004-2013) are displayed in Table 1.<sup>16</sup>

We match service requests with city council districts using the reported coordinates of each service request and polygon data on council district boundaries. Using information on when each request was first placed and when — if ever — it was resolved by city workers, we code our dependent variable as the number of days it took to resolve the request.<sup>17,18</sup> Thus, our measure captures how

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<sup>15</sup>The NYC data are available at <https://nycopendata.socrata.com/data?cat=city%20government>, and the SF data at <https://data.sfgov.org/Service-Requests-311-/Case-Data-from-San-Francisco-311/vw6y-z8j6> (as of summer 2015).

<sup>16</sup>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 Supporting Information 2, we demonstrate that our results are robust to different decisions about whether and how much to trim the data.

<sup>17</sup>The San Francisco data allow us to be even more precise and code the number of hours it took resolve a request. However, in the empirical section, we first convert response times to days to make the estimates from SF and NYC easier to compare.

<sup>18</sup>If a request has not yet been resolved at the time we downloaded the data (January 2014), we code it as missing. This decision does not drive our results. First, this right censoring works against us in San Francisco: the probability that an observation is censored is 1% higher in control districts, which

**Table 1** Summary Statistics for the Most frequent request types in NYC (2004-2013)

| Complaint Type         | Frequency<br>(in millions) | Percent of<br>all requests | Cumulative<br>percent | Response time statistics (in days) |        |                           |
|------------------------|----------------------------|----------------------------|-----------------------|------------------------------------|--------|---------------------------|
|                        |                            |                            |                       | Mean                               | Median | Trimmed mean <sup>†</sup> |
| 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 <sup>‡</sup>     | 3.2                        | 22.9                       | 100                   | 39.3                               | 5.0    | 21.1                      |

<sup>†</sup>Excludes response times above the 99th percentile.

<sup>‡</sup>In the analysis, we do not group these requests into this “Other” category.

quickly service requests are resolved in different city council districts. Because both NYC and SF have single-member districts, the measure gives us a sense for the responsiveness of the incumbent who represents a given district.

Interviews with city councilors’ offices in NYC suggest that this measure captures incumbents’ efforts to resolve service requests in their districts. First, councilors encourage constituents to flag their pending 3-1-1 requests. For example, on her website, Helen Rosenthal, who represents the Upper West Side, asks her constituents to “File a complaint online at [nyc.gov/311](http://nyc.gov/311), then report the problem to us along with your 311 complaint number.”<sup>19</sup> Second, the offices we interviewed reported helping constituents resolve service requests logged in the 3-1-1 system. Staff in the District 11 councilor’s office, for example, told us that after they receive notice from constituents about a pending request, the councilor’s office will immediately contact the relevant city agency. They claim that a

suggests we are understating the response times in control districts relative to treatment. Second, in New York City, there is no meaningful difference in the probability that an observation is censored across treatment and control.

<sup>19</sup>Rosenthal’s website (<http://helenrosenthal.com/>) was accessed on October 3, 2015.

call from the councilor's office expedites the resolution of the request: "we usually see a very high turnaround in how quickly requests are resolved."<sup>20</sup> Finally, some representatives admit to heightened efforts with this type of casework as election day approaches. During reelection campaigns, incumbents surface more requests from voters and, thus, devote additional staff resources to mobilizing city agencies to resolve constituents' problems.

In addition to this type of direct intervention, city councilors have established long-term strategies for monitoring and putting pressure on the city agencies responsible for different types of service requests. First, to monitor requests, Local Law 47 in NYC requires the 3-1-1 service to make periodic public reports with call data aggregated by city council district, allowing city councilors to keep track of how responsive city agencies are to requests originating in their constituencies. In SF, the 3-1-1 data include information on the supervisor district in which the service request is located. In both cities, representatives admit to consulting online resources (including interactive maps) that provide real-time monitoring of public service performance in their districts.

Second, elected officials have institutionalized communication channels that make it possible to put forth demands on city agencies. In NYC, Community Boards — volunteers who play an advisory role for city councilors and the mayor — are charged with directly communicating with city agencies. Some survey evidence suggests that city agencies respond more quickly to service requests if pressured by Community Boards. Half of the voting functionaries on Community Boards, in turn, are proposed by city councilors. Thus, Weidling (2007) concludes that while local officials were initially concerned that 3-1-1 would reduce their contact with constituents, the service has had the effect of increasing city council members' ability to act on potential voters' concerns.

Third, heads of city agencies have a strong incentive to heed the demands of elected officials. City council members approve the city budget, which includes funding for both the 3-1-1 program and many of the agencies that respond to service requests. The allocations provided to these programs 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

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<sup>20</sup> Author interview, September 2015.

million to \$13.4 million (Dept. of Public Works 2010). To increase or retain their funding, the heads of city agencies should strive to do well by elected officials. In SF, city councilors confirm the city administrator, who oversees both the 3-1-1 service and the DPW. Jointly with the mayor, the city administrator appoints the directors of both of these city agencies. These powers create incentives for the 3-1-1 service and other city agencies to remain responsive to the needs of city councilors.

This evidence, coupled with our interviews with representatives, suggests that representatives (1) care immensely about responsiveness to service requests in their districts, (2) are aware of city agencies' resource constraints, meaning that they do not *always* request added resources to their district (but do so when needed, e.g., closer to elections), and (3) are able to affect responsiveness to service requests both by tracking individual requests and by communicating with and mobilizing the resources of city agencies. Thus, systematic changes to how quickly service requests are responded to in different districts do reflect city councilors' willingness and ability to affect requests, suggesting that our measure of service responsiveness is a valid measure of incumbent effort.<sup>21</sup>

## Legislative Activity

We also collect a novel dataset on city councilors' legislative activity in NYC and SF. 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

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<sup>21</sup>Readers might be concerned that the volume of requests far outstrips the capacity of councilors. First, if councilors are unable logistically to affect response times, this works against our findings. The limited capacity of councilors offices does not imply that our measure is flawed, but rather that our effect sizes might increase if councilors had more staff resources that they could devote to monitoring and resolving 3-1-1 requests. Second, we are not claiming that councilors intervene in all 3-1-1 requests. What we are arguing is that councilors devote more resources to resolving such requests when they are electorally eligible and their reelection contests are imminent.

co-sponsored the actions.<sup>22</sup> 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.<sup>23</sup>

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.

## Effects of Reelection Eligibility on Incumbent Effort

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.<sup>24</sup> 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.

This event presents a rare opportunity to study the effects of reelection eligibility on incumbent effort. First, while city councilors had to formally approve the term-limit extension, its passage 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

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<sup>22</sup>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).

<sup>23</sup>The legislation introduced in each supervisor meeting in 2009 can be found at <http://www.sfbos.org/index.aspx?page=1589>.

<sup>24</sup>We consider only city councilors, not the mayor, in our analyses.

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, though an uncertain final vote was preceded by 20 hours of public hearings and a full day of floor debate in a sharply divided City Hall (Chan and Hicks 2008). Given the short time frame in which the bill was passed and the precipitating role of the global financial crisis, the extension was largely exogenous to individual council members — inducing an unexpected shift in some incumbents’ ability to reclaim office.

Second, the fact that the bill did not affect every city council member equally is a major advantage. If it had, it would be difficult to attribute a change in effort levels to the term-limit extension rather than some other factor that correlated with the extension and effort levels. By comparing pre- and post-extension effort levels among two groups of incumbents, only one of which was immediately affected by the extension, we are able to rule out confounding factors that affected all incumbents, such as the financial crisis or weather conditions.

More specifically, we implement a difference-in-differences (DiD) design. Our 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.<sup>25</sup> Our control group is incumbents who were allowed to seek reelection both before and after the decision.

We estimate the DiD by estimating a linear model with district and period fixed effects. When our outcome variable is response time to service requests, the model takes the following form:

$$y_{idt} = \alpha_d + \delta_t + \beta D_{dt} + \gamma_{type} + \phi_{day} + \varepsilon_{idt} \quad (1)$$

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<sup>25</sup>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 analysis. As such, all our estimates should be interpreted as the effect among compliers. We do not consider incumbents who were elected in 2001 but left the council before the term-limit extension.



where  $i$  indexes complaints,  $d$  city council district, and  $t$  period (i.e., before or after the term limit extension).  $D_{dt}$  is an indicator equal to 1 for treated city council districts after the term-limit extension. Because response times vary by the type of request — as Table 1 makes clear — we include fixed effects for complaint type ( $\gamma_{type}$ ) and the day of the week on which the complaint was lodged ( $\phi_{day}$ ). When our outcome variable is number of local laws and resolutions (co)sponsored by a council member, we estimate equation 1 without these fixed effects for complaint type or the day of the week. In all analyses, we cluster the standard errors on councilor.

The parameter of interest in equation 1 is  $\beta$ . Assuming stable unit treatment value (SUTVA), constant treatment effects, and that treated and control districts would have followed parallel trends in the absence of the treatment, this coefficient provides a causal estimate of the effect of the term-limit extension.<sup>26</sup> In our analyses of the 3-1-1 data, a negative estimate of  $\beta$  indicates that response times dropped in treated districts relative to control districts following the extension and, hence, that responsiveness improved. In the legislative activity analyses, a positive estimate would indicate increased efforts.

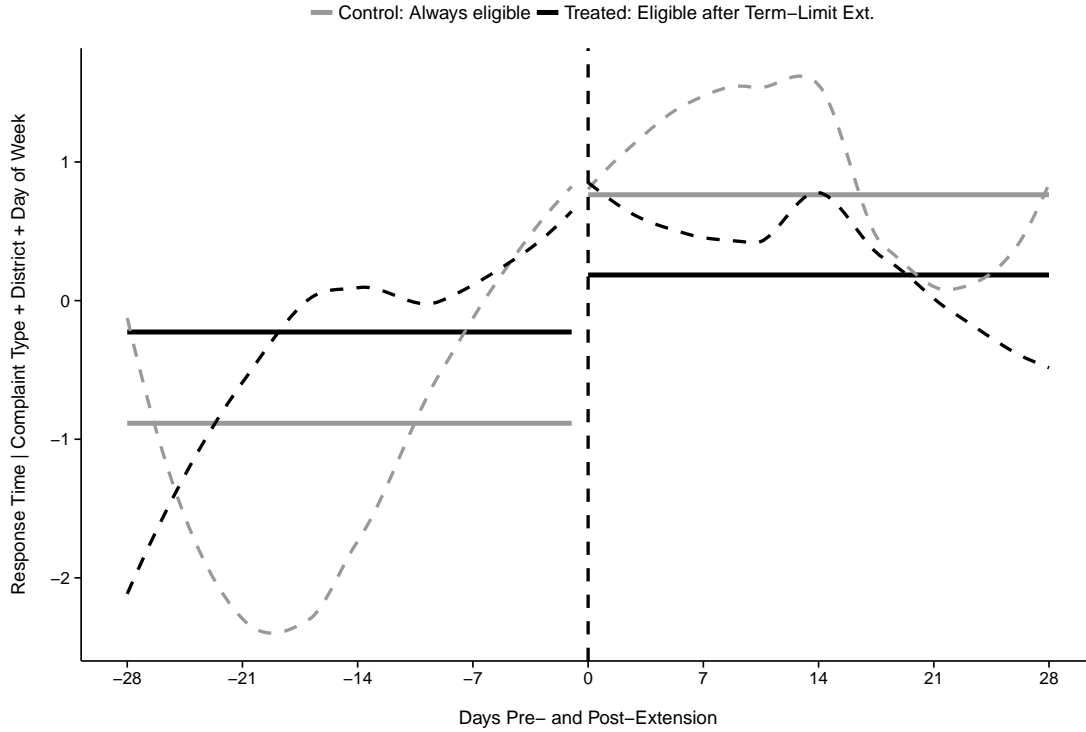
## Results

We begin by presenting visual evidence that reelection eligibility improves responsiveness to service requests. Figure 1 compares response times in the four weeks before and after the term-limit extension for districts that were (in black) and were not (in gray) affected by the term-limit extension. (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 filed.) This plot conveys three important points. First, the average response time in the control group (the solid gray lines) increases much more than the change we observe in the treated group (the solid black lines). Second, differential trends (estimated here using a non-parametric loess regression) in the period

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<sup>26</sup>If effort levels among incumbents in the control group were improving over time because of the 2009 elections, then the parallel trends assumption may not hold. However, this would bias our estimate of  $\beta$  toward 0, meaning that our estimates provide a lower bound on the effect of the extension.

**Figure 1** Non-parametric Estimates of Response Time by Treatment Status  
*Response times increase by ~2 days in control districts, but less than half a day in treated districts.*



The flat lines plot the average response times before and after the term-limit extension for councilors whose election eligibility was (in black) and was not (in gray) affected by the policy change. The dashed lines are loess smoothers with a span of 0.9. The dependent variable for this figure has been “centered” to partial out variation due to the type of response, council district in which the request was made, and day of the week on which the request was made.

prior to the policy change do not appear to account for this difference-in-differences. Response times in both control and treated districts are increasing in the three weeks prior to the term-limit extension. However, in the weeks after the reform, response times declined (almost monotonically) in treated districts, while they initially continued to increase in control districts. Finally, this figure demonstrates the importance of a well-specified counter-factual. Response times were increasing across the city when the reform passed. A simple before-after comparison focused only on those councilors affected by the reform would have wrongly concluded that the policy change had no effect.

Table 2 presents our difference-in-difference estimates ( $\hat{\beta}$ ), confirming that responsiveness in treated districts improved significantly relative to control areas after the term-limit extension. We use different time windows on either side of the extension (2-4 weeks);  $\hat{\beta}$  is negative and of similar magnitude in all cases.<sup>27</sup> Our estimates are significant at the 5%-level when we use the three or four week time windows. When we restrict attention to the two weeks immediately before and after the policy change, our estimate is slightly less precise ( $p = 0.12$ ), as we lose observations.

Our results are robust to an alternative modeling strategy. We transform our 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 ar-

**Table 2** Effect of Term Extension on Constituency Services and Legislative Activity

*The average response time fell by more than 1 day in treated districts.*

| <i>Time frame</i> <sup>†</sup> | <i>Dependent variables:</i>      |                                  |                                  |                                 |
|--------------------------------|----------------------------------|----------------------------------|----------------------------------|---------------------------------|
|                                | Response Time                    |                                  |                                  | Leg. Actions                    |
|                                | 4                                | 3                                | 2                                | 6                               |
| $\hat{\beta}^\ddagger$         | -1.237<br>(0.551)<br>$p = 0.025$ | -1.430<br>(0.673)<br>$p = 0.034$ | -1.137<br>(0.739)<br>$p = 0.124$ | -0.393<br>(1.108)<br>$p = 0.72$ |
| <hr/>                          |                                  |                                  |                                  |                                 |
| <i>Time frame</i> <sup>†</sup> | 1(Response Time < 5 Days)        |                                  |                                  |                                 |
|                                | 4                                | 3                                | 2                                |                                 |
| $\hat{\beta}^\ddagger$         | 0.020<br>(0.010)<br>$p = 0.049$  | 0.017<br>(0.009)<br>$p = 0.066$  | 0.032<br>(0.011)<br>$p = 0.004$  |                                 |
| <hr/>                          |                                  |                                  |                                  |                                 |
| Observations                   | 236,295                          | 177,647                          | 123,504                          | 160                             |

<sup>†</sup>Weeks on either side of the extension used to estimate Eq. 1

<sup>‡</sup>Difference-in-differences estimator (see Eq. 1)

Standard errors clustered on districts in parentheses

<sup>27</sup>Summary statistics for the key variables included in equation 1 are shown in Table A.2.1.

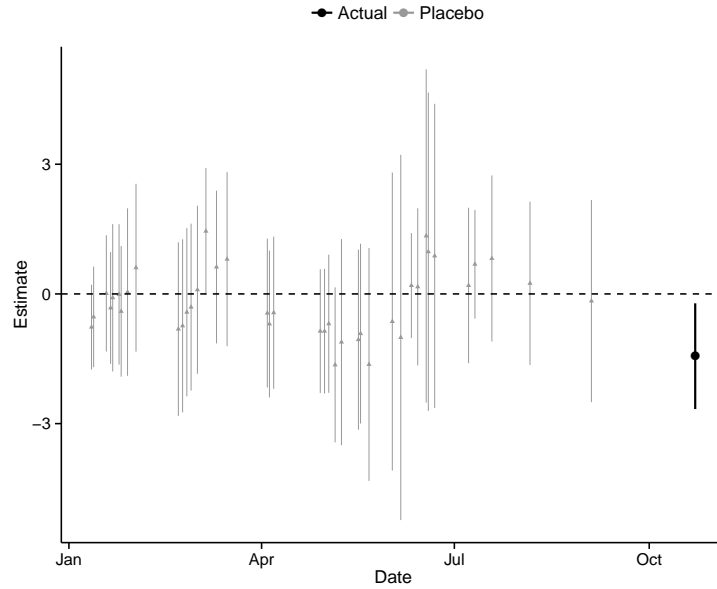
eas. This should alleviate concern that large outliers — requests resolved long after the policy change — are unduly influencing our results.

We also conduct a series of “placebo” tests to demonstrate that the differences we find cannot be attributed to differential trends prior to the policy change. We randomly draw 40 dates from January 1, 2008 to September 11, 2008 (six weeks prior to the actual term-limit extension) and then estimate equation 1 using these placebo dates to define the treatment. Figure 2 displays these estimates and their 95% confidence intervals. The estimate from the actual term-limit extension is the right-most, black point — the only coefficient that is significant at the 5%-level. Based on these placebo tests, there is no evidence that changes in responsiveness across groups prior to the term-limit extension account for our findings. This speaks to a final concern: whether treated councilors foresaw the policy change and changed their behavior prior to its passage. We find no evidence of this. Furthermore, we note that, if treated councilors anticipated the extension and increased their efforts in advance of the policy change, this biases *against* our findings by driving down response times in treated areas prior to the reform.

Do the improvements we detect matter? On average, response times decreased by 4%, or just over a day, in treated districts relative to control districts following the term limit extension. We know of no other studies that employ comparable data, so we cannot rely on the literature to benchmark our effect sizes. However, our estimates can be compared to the effects of events known to severely hamper city services. 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. Using these events as a benchmark, our results indicate that the term limit extension induced substantively meaningful improvements in responsiveness in districts held by newly eligible incumbents. Put differently, reelection incentives lead to less shirking among incumbents.

By contrast, we find no evidence that the extension induced *changes* in legislative efforts. Using equation 1 but omitting fixed effects for type ( $\gamma_{type}$ ) and the day of the week on which the request was made, the DiD estimates are noisy zeros, never beginning to approach conventional levels of

**Figure 2** Placebo Estimates of Response Time by Treatment Status  
*Placebo tests indicate that divergent pre-treatment trends do not explain the effect.*



We draw 40 dates at random from January 1, 2008 to September 11, 2008 (six weeks prior to the actual term-limit extension). We then re-estimate (1) using these “placebo” dates to define the treatment event. Displayed above are the estimates and 95% confidence intervals for  $\hat{\beta}$  from equation 1 using three weeks of data on either side of each date. The estimate from the actual term-limit extension is the right-most, black point.

statistical significance. Table 2 presents one of these DiD estimates, using the total number of local laws and resolutions (co)sponsored in the six weeks before and after the policy change as the outcome variable. These findings suggest that incumbents were not cutting back on legislative effort, as they ramped up their work on constituency services.

Overall, the results from our analyses of the 2008 term-limit extension show that election eligibility improves incumbents’ efforts with respect to constituency services, but has no discernible effects on legislative activity.<sup>28</sup> We take these results as evidence that elections can improve political

<sup>28</sup>For readers interested in what categories of service requests drive these results, in Supporting Information 5 we allow the difference-in-differences estimate to vary by complaint type.

accountability: while we see substantial improvements in service responsiveness, these improvements do not come at the expense of a reallocation of effort from the legislative arena.<sup>29</sup>

## Effects of Election Timing on Incumbent Effort

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. In each set of elections, 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), our treatment group consists of a sole incumbent seeking reelection: Carmen Chu of District 4.<sup>30</sup>

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 time-trend 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. We then test whether response times are falling faster where incumbents are eligible for reelection (i.e., whether the time-trend is more negative in treated districts). Our empirical model is

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

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<sup>29</sup>We do not find clear evidence that councilors in more competitive districts (based on their margin of victory in the 2005 primary) are driving these effects.

<sup>30</sup>All of our models include district (i.e., councilor) fixed effects, so we are not simply reporting a level difference in effort between Chu and other incumbents.

where  $i$  indexes complaints,  $d$  city council district, and  $t$  represents days before the general elections.  $D_d$  is an indicator equal to 1 for treated city council districts (those with an eligible incumbent). As before, 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-level. For the SF elections, we use term-limited incumbents and incumbents not up for election as control groups in separate regressions. To analyze legislative efforts, we use the same specification without the fixed effects for complaint type ( $\gamma_{type}$ ) or the day of the week ( $\phi_{day}$ ).

The parameter of interest again is  $\beta$ .<sup>31</sup> The key identifying assumption is that in the absence of the election, treated and control incumbents would have followed parallel trends in effort levels. In our analyses of the 3-1-1 data, a negative estimate indicates improving effort (i.e., more sharply declining response times) among treated incumbents relative to control incumbents. In the legislative activity analyses, a positive estimate indicates increased efforts.

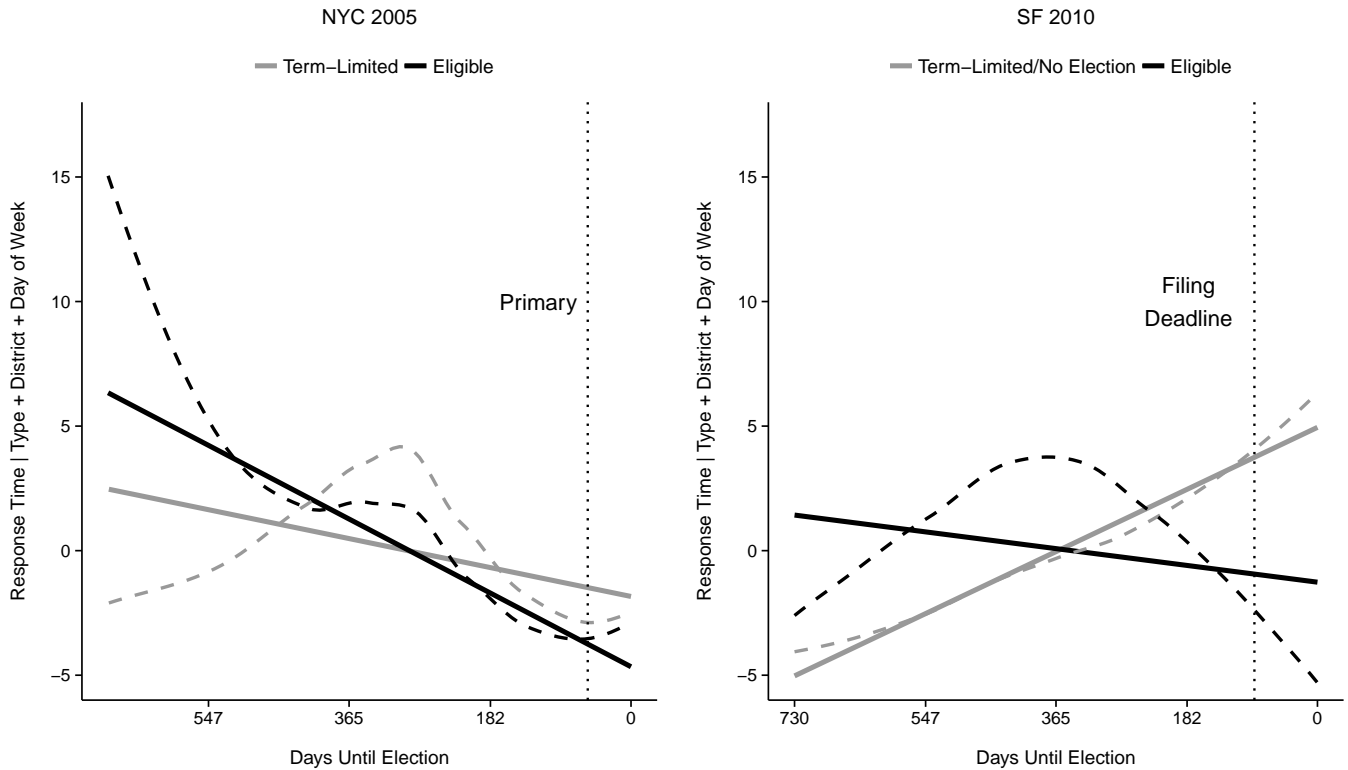
## Effects on Constituency Services

In Figure 3, we explore whether the timing of elections affects responsiveness to 3-1-1 requests using a non-parametric approach (i.e., we allow for non-linear time-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

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<sup>31</sup>There may be level differences in responsiveness across our treatment and control groups. However, this comparison is confounded by differences, for example, in experience, so we do not devote attention to the intercepts,  $\alpha_d$ .

**Figure 3** Non-parametric and Least Squares Estimates of Response Time by Treatment Status  
*In both NYC and SF, response times fall faster in districts with electorally eligible incumbents.*



The straight, solid lines are least squares estimates of response times in the two years preceding the election for councilors that are (in black) and are not (in gray) eligible to stand for reelection. The dashed lines are loess fits with a span of 0.9. The dependent variable for this figure has been “centered” to partial out the variation due to the type of response, district in which the response is made, and day of the week on which the response is lodged. The dashed, vertical black lines indicate, respectively, the date of the primary election in NYC and the filing deadline for candidates in SF. The figure on the left is based on a random sample of 200,000 observations, or just under 10% of the full sample.

months of 2005. This figure demonstrates that our findings persist, even if we allow for flexibly estimated time-trends across our groups.<sup>32</sup>

<sup>32</sup>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 general and



**Table 3** Estimates of Differential Time-Trends in Constituency Services ( $\beta$  in equation 2)  
DV: 3-1-1 Response Time

*The linear trend in responsiveness falls significantly faster where incumbents can run for reelection.*

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

<sup>†</sup>Days before election used to estimate Eq. 2

Standard errors clustered on districts in parentheses

In Table 3, we present the results from estimating equation 2. The results are presented for three separate samples, split by city and type of control group: (1) NYC using term-limited incumbents as control, (2) SF using incumbents not yet up for reelection as control, and (3) SF using term-limited incumbents as control. By splitting the control group in SF, we are able to evaluate whether our findings are comparable for term-limited lame ducks and off-cycle incumbents. The results are

primary elections (in separate regressions) and find substantively similar results (available upon request).

also split by the number of days before the election we use to estimate equation 2, corresponding to 2, 1.5, 1, and 0.5 years.<sup>33</sup>

These results confirm that response times declined more rapidly in districts with an eligible incumbent than in districts without an eligible incumbent. The estimates of  $\beta$  are negative in 11 of 12 models, and can be distinguished from 0 at conventional levels of confidence in 8. Our estimates get more uncertain as we shrink the number of days before the election we include to estimate equation 2. One likely explanation for this is that the number of observations declines with shorter time frames, decreasing our power to reject the null hypothesis of no effect.

If the differences we discover are driven by reelection incentives, then they should disappear after the election. That is, we should not find differential trends in responsiveness among our treated and control groups in the post-election period. To assess this, we recreate Figure 3 — estimating both flexible and linear time trends in response times — using data from one year *after* the elections in NYC and SF. As Figure 4 illustrates, the trends in our treated and control districts are very similar in the post-election period.<sup>34</sup> This provides further evidence that, when election contests are not imminent, response times follow similar trends in these treated and control districts.

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. Taking the estimates from column 1, the results from NYC suggest that moving six months closer to the election corresponded to a two day reduction in response times in treated districts relative to control. In SF, the results were twice as large. These effects are roughly the same magnitude as (or slightly larger than) the change in response times induced by the January 2005 blizzard in NYC or by labor day weekends. Overall,

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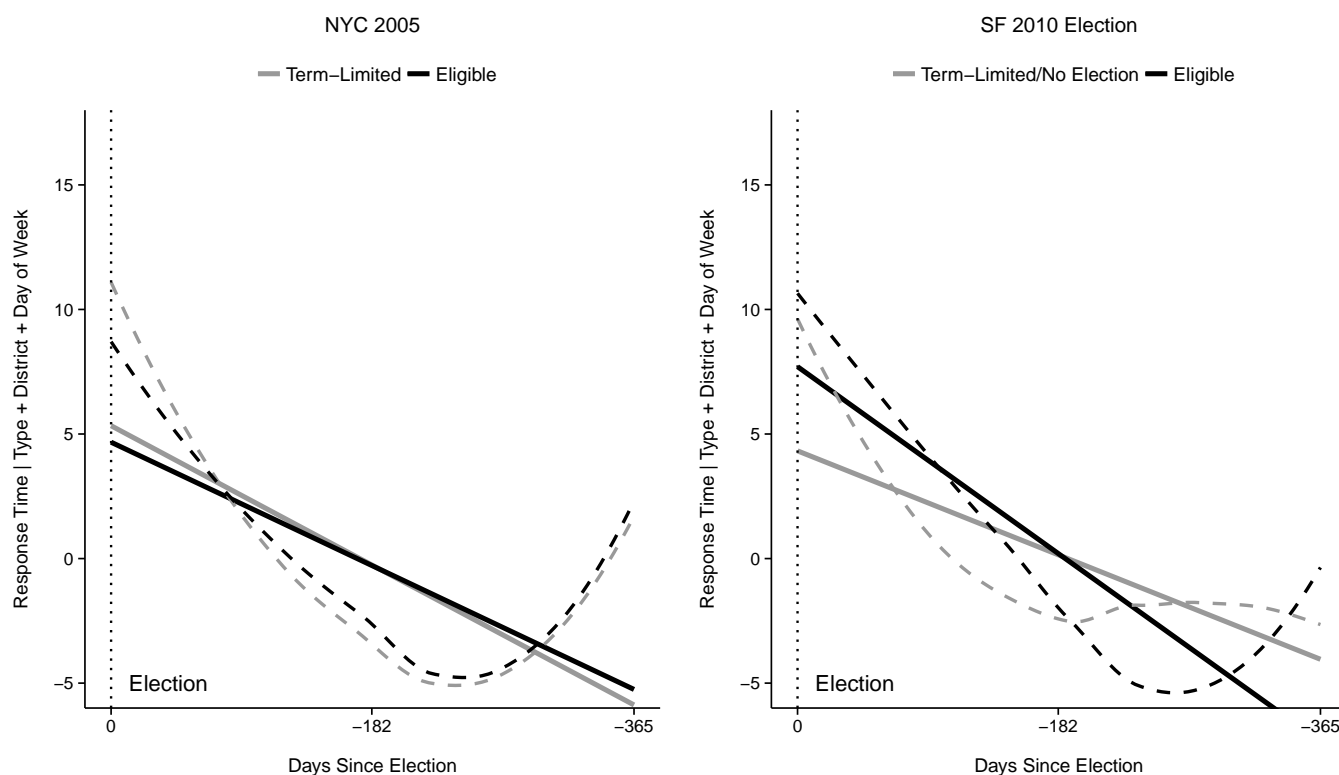
<sup>33</sup>Our data from NYC go back only to January 1, 2004, so the two-year window corresponds to 677 rather than 730 days.

<sup>34</sup>In Supporting Information 3 we perform a series of placebo tests using data from the post-election period. These tests and Figure 4 both suggest that response times do not follow different time trends following the elections.

therefore, our results suggest that districts with reelection seeking incumbents saw significant and substantively meaningful improvements relative to control districts as the elections approached.

One aspect of the SF results is worth highlighting. The estimates are more negative when the control group is term-limited incumbents rather than incumbents facing no election in 2010. Our simple decision-theoretic model from above predicts 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

**Figure 4** Response Times by Treatment Status *After* the Election  
*After the election, there is no evidence differential trends in responsiveness.*



The straight, solid lines are least squares estimates of response times in the year following the election for councilors that are (in black) and are not (in gray) eligible to stand for reelection. The dashed lines are loess fits with a span of 0.9. The dependent variable for this figure has been “centered” to partial out the variation due to the type of response, district in which the response is made, and day of the week on which the response is lodged. The dashed, vertical lines indicate the election dates. The figure on the left is based on a random sample of 200,000 observations.

reelection. Thus, from the perspective of an incumbent seeking reelection in two years, improved performance during this time period may be an opportunity to persuade future supporters at a moment when supervisors' efforts are particularly salient.<sup>35</sup>

Our election timing results are robust to an alternative coding of our dependent variable. In Supporting Information 1, we define indicator variables for whether a service request was resolved within five days. We then estimate linear probability models that are otherwise identical to the specifications employed above. We find that the probability that a service request was resolved within this short time-span increased at a faster rate in districts with eligible incumbents. This analysis and our use of different trimming rules (see Supporting Information 2) should ameliorate concerns that our results are driven by outlying response times.

## Effects on Legislative Activity

Table 4 presents our results from estimating equation 2 using the total number of legislative actions taken by a city councilor in any one meeting as our outcome variable. These legislative actions include resolutions, local laws/ordinances, and requests for hearings.<sup>36</sup> We use only two time frames (1 and 0.5 years) when estimating these models due to the availability of these data.

The table indicates that election timing does not appear to affect legislative activity in NYC. The standard errors on the interaction term are several times larger than the (substantively small) coefficient estimates. On the other hand, we find evidence that the eligible incumbent's legislative efforts improved leading up to the 2010 elections in SF. These estimates are positive and distinguish-

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<sup>35</sup> 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 our reelection seeking incumbent outperforms her off-cycle colleagues, who are not lame ducks. This finding is a feature of our unique design: in most settings, an ineligible incumbent is also a lame-duck, which typically makes it very difficult to separate these effects.

<sup>36</sup> We have also tried specifications in which we split the outcome variable by type of legislative action, with substantively similar conclusions.

**Table 4** Estimates of Differential Time-Trends in Legislative Activity ( $\beta$  in equation 2)  
DV: total legislative actions introduced

*Eligible incumbents either maintain or increase their legislative effort, suggesting no reallocation.*

|                         | NYC                |                    | SF<br>(Control: No Election) |                   | SF<br>(Control: Term-Limited) |                   |
|-------------------------|--------------------|--------------------|------------------------------|-------------------|-------------------------------|-------------------|
| <i>Time frame</i>       | 365                | 182                | 365                          | 182               | 365                           | 182               |
| $\hat{\beta}$           | -0.0004<br>(0.003) | -0.0002<br>(0.006) | 0.006*<br>(0.0004)           | 0.016*<br>(0.002) | 0.002<br>(0.002)              | 0.008*<br>(0.001) |
| Observations            | 1,224              | 612                | 287                          | 168               | 205                           | 120               |
| Adjusted R <sup>2</sup> | 0.301              | 0.305              | 0.181                        | 0.207             | 0.054                         | -0.013            |

<sup>†</sup>Days before election used to estimate (2) (excluding  $\gamma_{type}$ ); \*p<0.01

Standard errors clustered on districts in parentheses

able from 0 at the 1% level in 3 of 4 specifications. Non-parametric estimates similar to those in Figure 3 (not presented) indicate that while the eligible incumbent maintained a relatively constant level of legislative activity in the run-up to her reelection contest, activity levels dropped among both term-limited and off-cycle incumbents. This drop-off in the control group is driving the sign of the estimates. To interpret the results substantively, we would predict eligible incumbents to sponsor roughly two more actions than non-eligible colleagues in the six months prior to elections.

## Summary of Results

Our analyses of the effects of election timing on incumbent effort with respect to both service responsiveness and legislative activity lead to two conclusions. First, eligible incumbents ramp up their efforts to provide better constituency services in the run-up to elections. As our analyses of the 3-1-1 data make clear, service requests in city council districts with eligible incumbents are responded to with more urgency, relative to service requests in districts without eligible incumbents, as elections approach. Second, legislative efforts, as measured by the total local laws and resolutions (co)sponsored by city councilors, are either constant (NYC) or improve (SF) in the lead-up to elections. Our overall conclusion, therefore, is that incumbents' responsiveness to service requests improve as elections approach but that these efforts do *not* compromise their legislative activity. This conclusion mirrors the one that emerged from our analyses of the effects of election eligibility

on efforts, which showed that reelection eligibility improves service responsiveness without compromising legislative duties.

## Conclusion

Using administrative records on 15 million constituency requests, this paper finds evidence that elections shape both the extent and timing of incumbent effort — or, conversely, shirking. Two quasi-experimental research designs, carried out across three different election periods in two major U.S. cities, help us reach this conclusion. First, a policy enacted in NYC in 2008, which allowed city councilors to seek an additional term in office, allows us to analyze incumbents' reactions to a sudden change in their reelection eligibility. Theories of democratic accountability suggest that these incumbents should shirk absent a reelection incentive. We find evidence for this: after the term-limit extension, newly eligible incumbents exerted more effort with constituency services than expected in the absence of the extension. In other words, we find evidence of last-term shirking.

Second, taking advantage of term-limits and staggered elections, we estimate whether elections impact when in the electoral cycle eligible incumbents exert effort. Theories of electoral cycles suggest that incumbents should exert additional effort as elections approach, though empirical tests have failed to find robust evidence of this (Franzese 2002). Our analysis — which extends previous work by explicitly considering counterfactual trends in incumbent effort — finds evidence of electoral cycles in incumbents' behavior. More specifically, we show that incumbents engage in early-term shirking in their efforts to have constituency requests resolved.

Analyzing only one incumbent activity can lead to misleading conclusions, however. As discussed above, such analyses cannot distinguish between an overall reduction in effort and a reallocation of effort. In light of this, we also analyze an original dataset on legislative activity for each city councilor. We do not find that incumbents' efforts with legislation drop off as a result of their heightened responsiveness to public service requests. Nor do we find robust evidence that their efforts with legislation improve because of elections. This finding highlights a central issue in previous work on incumbent shirking. Had we only analyzed incumbents' legislative activity, we would have

wrongly concluded that elections have little or no impact on their effort levels. This suggests one reason why prior work on the topic has generated such mixed results.

Finally, our findings have implications for theories of democratic accountability. The idea that elections discipline politicians and, hence, that term-limits induce shirking is a central assumption in these theories (e.g., Dewan and Shepsle 2011; Przeworski, Stokes and Manin 1999). It is therefore concerning that empirical tests have failed to accumulate robust evidence about the extent and nature of elections' impact on incumbents. While we are careful not to claim that our findings generalize to other geographical contexts or other types of political office, they increase our confidence that elections serve an important role in motivating performance among representatives.

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## Appendix 1 Overview of Existing Literature

**Table A.1.1** Selected recent empirical investigations of last-period shirking in the United States

| Paper                         | Sample             | Dependent variable(s)        | Evidence of shirking? |                |          |           |
|-------------------------------|--------------------|------------------------------|-----------------------|----------------|----------|-----------|
|                               |                    |                              | Ideo.                 | Attend.        | Casework | Oversight |
| <i>Term limits</i>            |                    |                              |                       |                |          |           |
| 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   | ✓ <sup>†</sup>        | ✓              |          |           |
| 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       | ✗                     | ✓              |          |           |
| <i>Retirement</i>             |                    |                              |                       |                |          |           |
| Carson et al. (2004)          | Congress           | Vote deviation               | ✗                     |                |          |           |
| Figlio (1995)                 | Congress           | Vote deviation; attendance   | ✓                     | ✓              |          |           |
| Lott (1987)                   | Congress           | Vote deviation; attendance   | ✗                     | ✓              |          |           |
| Lott (1990)                   | Congress           | Attendance                   |                       | ✓ <sup>†</sup> |          |           |
| 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               | ✓ <sup>†</sup>        |                |          |           |
| Tien (2001)                   | Congress           | Congruence                   | ✓                     |                |          |           |
| Vanbeek (1991)                | Congress           | Vote deviation               | ✗                     |                |          |           |

Notes: ✓ = results in study can be interpreted as evidence of shirking, X = no evidence of shirking, <sup>†</sup> = 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 Summary Statistics

**Table A.2.1** Summary Statistics for NYC Service Request Data, 10/23/2007 - 10/23/2009

| Statistic                       | N         | Mean  | St. Dev. | Min | Max   |
|---------------------------------|-----------|-------|----------|-----|-------|
| <i>y</i> : Response Time (Days) | 3,420,140 | 17.48 | 56.57    | 0   | 1,043 |
| <i>t</i> : 1(Post-Ext.)         | 3,420,140 | 0.50  | 0.50     | 0   | 1     |
| <i>D</i> : 1(Compliers)         | 2,742,258 | 0.73  | 0.44     | 0   | 1     |

*Note*: Trimmed Top 0.1% of Response Times

**Table A.2.2** Summary Statistics for NYC, 1/1/2004 - 11/08/2005

| Statistic                       | N         | Mean   | St. Dev. | Min | Max |
|---------------------------------|-----------|--------|----------|-----|-----|
| <i>y</i> : Response Time (Days) | 2,378,172 | 22.65  | 60.10    | 0   | 785 |
| <i>t</i> : Days Before Election | 2,378,172 | 281.31 | 168.30   | 0   | 677 |
| <i>D</i> : 1(Treated)           | 2,376,717 | 0.87   | 0.34     | 0   | 1   |

*Note*: Trimmed Top 1% of Response Times

**Table A.2.3** Summary Statistics for SF, 11/02/2008 - 11/02/2010

| Statistic                       | N       | Mean   | St. Dev. | Min  | Max    |
|---------------------------------|---------|--------|----------|------|--------|
| <i>y</i> : Response Time (Days) | 612,338 | 24.08  | 57.86    | 0.00 | 524.97 |
| <i>t</i> : Days Before Election | 323,105 | 414.52 | 244.81   | 0    | 853    |
| <i>D</i> : 1(Treated)           | 612,338 | 0.05   | 0.21     | 0    | 1      |

*Note*: Trimmed Top 1% of Response Times

Supporting Information:  
*Elections and Shirking: An Empirical Reappraisal*

To be published online.

## Supporting Information 1 Linear Probability Models

We code an indicator variable for whether a public service request was resolved within five days of being opened. We then estimate linear probability models with these as our dependent variables. This recoding flips the interpretation of the coefficients: a positive coefficient now implies that responsiveness is improving and that a request is more likely to be resolved within several days after opening. The linear probability models for NYC 2005 and SF 2010 are included below; the linear probability models for NYC 2008 are included in Table 2 in the body of the paper.

**Table SI.1.1** Estimates of Differential Time-Trends in Constituency Services ( $\beta$  in Equation 2)

DV:  $\mathbb{1}(\text{3-1-1 Response Time} < 5 \text{ Days}) \times 100$

| <i>Time frame</i> <sup>†</sup>  | 730                               | 547                               | 365                               | 182                                |
|---------------------------------|-----------------------------------|-----------------------------------|-----------------------------------|------------------------------------|
| NYC 2005                        |                                   |                                   |                                   |                                    |
| $\hat{\beta}$                   | 0.0059<br>(0.0020)<br>$p = 0.004$ | 0.0047<br>(0.0020)<br>$p = 0.019$ | 0.0089<br>(0.0077)<br>$p = 0.247$ | 0.0066<br>(0.0090)<br>$p = 0.464$  |
| Observations                    | 2,376,717                         | 2,238,742                         | 1,578,977                         | 774,375                            |
| SF 2010 (Control: No Election)  |                                   |                                   |                                   |                                    |
| $\hat{\beta}$                   | 0.0037<br>(0.0018)<br>$p = 0.001$ | 0.0143<br>(0.0040)<br>$p < 0.001$ | 0.0042<br>(0.0043)<br>$p < 0.001$ | -0.0228<br>(0.0049)<br>$p = 0.943$ |
| Observations                    | 160,678                           | 120,549                           | 81,801                            | 42,571                             |
| SF 2010 (Control: Term-Limited) |                                   |                                   |                                   |                                    |
| $\hat{\beta}$                   | 0.0083<br>(0.0022)<br>$p < 0.001$ | 0.0245<br>(0.0116)<br>$p = 0.035$ | 0.0198<br>(0.0126)<br>$p = 0.117$ | 0.0032<br>(0.0206)<br>$p = 0.878$  |
| Observations                    | 135,393                           | 101,458                           | 68,933                            | 35,531                             |

<sup>†</sup> Days before election used to estimate Eq. 2

Standard errors clustered on districts in parentheses

## Supporting Information 2 Robustness to Trimming

**Table SI.2.1** Robustness of Election Eligibility Results to Trimming Decisions

| <i>Quantile Trimmed</i>                          | <i>Dependent variable:</i>         |                                    |                                    |
|--|------------------------------------|------------------------------------|------------------------------------|
|  | NYC 2008                           |                                    |                                    |
|  | 0.98                               | 0.99                               | 1                                  |
| $\hat{\beta}$                                    | −0.00860<br>(0.510)<br>$p = 0.086$ | −0.01219<br>(0.551)<br>$p = 0.025$ | −0.00884<br>(0.572)<br>$p = 0.038$ |
| Observations                                     | 235,495                            | 236,295                            | 236,302                            |
| <i>Note:</i> Std. errors clustered on districts. |                                    |                                    |                                    |

**Table SI.2.2** Robustness of Election Timing Results to Trimming Decisions

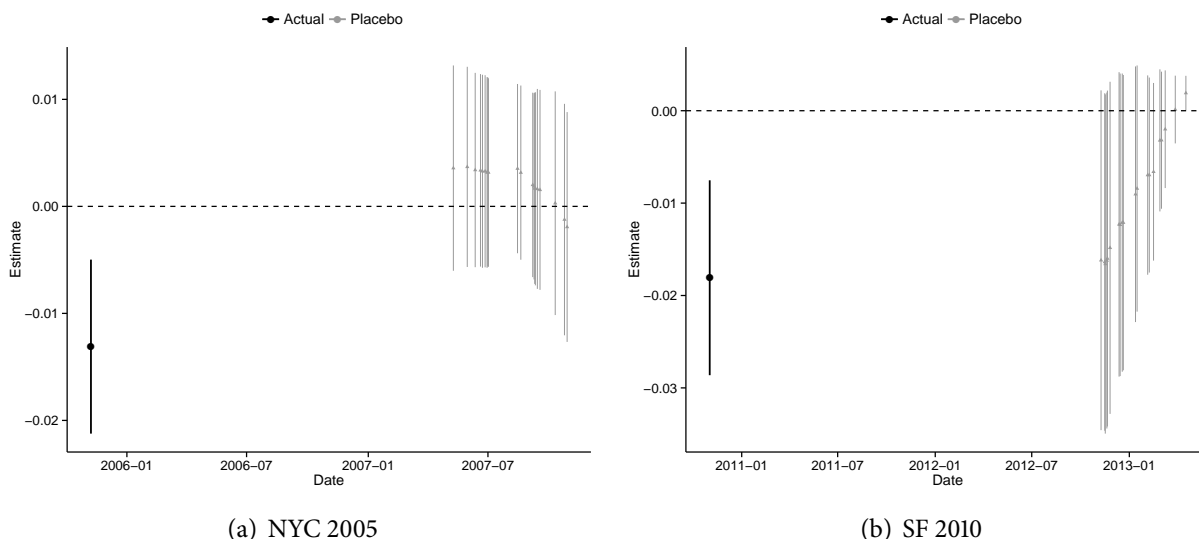
| <i>Quantile Trimmed</i>                          | <i>Dependent variable: Response Time (Days)</i> |  |  |
|--|---|--|--|
|  | NYC 2005  |  |  |
|  | 0.98  | 0.99                                   | 1                                      |
| $\hat{\beta}$                                    | −0.00860<br>(0.00311)<br>$p = 0.00575$          | −0.01219<br>(0.00410)<br>$p = 0.00293$ | −0.00884<br>(0.01162)<br>$p = 0.44678$ |
| Observations                                     | 2,352,693                                       | 2,376,705                              | 2,400,697                              |
| <i>Quantile Trimmed</i>                          | SF 2010 (Control: No Election)                  |  |  |
|  | 0.98  | 0.99                                   | 1                                      |
| $\hat{\beta}$                                    | −0.00603<br>(0.00211)<br>$p = 0.00421$          | −0.00911<br>(0.00283)<br>$p = 0.00130$ | −0.01379<br>(0.00415)<br>$p = 0.00089$ |
| Observations                                     | 157,464   | 159,071                                | 160,678                                |
| <i>Quantile Trimmed</i>                          | SF 2010 (Control: No Incumbent)                 |  |  |
|  | 0.98  | 0.99                                   | 1                                      |
| $\hat{\beta}$                                    | −0.01404<br>(0.00950)<br>$p = 0.13932$          | −0.01796<br>(0.00929)<br>$p = 0.05319$ | −0.02172<br>(0.01107)<br>$p = 0.04967$ |
| Observations                                     | 132,685   | 134,039                                | 135,393                                |
| <i>Note:</i> Std. errors clustered on districts. |   |  |  |



## Supporting Information 3 Placebo Tests: NYC 2005 and SF 2010

We randomly draw 20 dates from the period following the 2005 election in NYC and the 2010 election in SF. We then re-estimate  $\beta$  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). To save space, we pool our two types of control districts in SF. These tests suggest that the differential time trends that we discover in our analysis do not persist after the election.

**Figure SI.3.1** Placebo Results for 2005 NYC and SF 2010 Elections



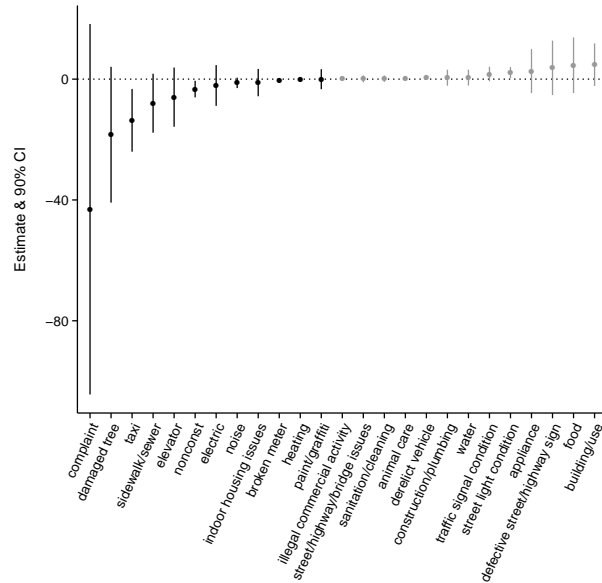
Displayed above are the estimates and 95% confidence intervals for  $\hat{\beta}$  from equation 2 using two years of data before each placebo date. The estimate from the actual term-limit extension is the left-most, black point.

## Supporting Information 4 DiD Estimates by Request Type

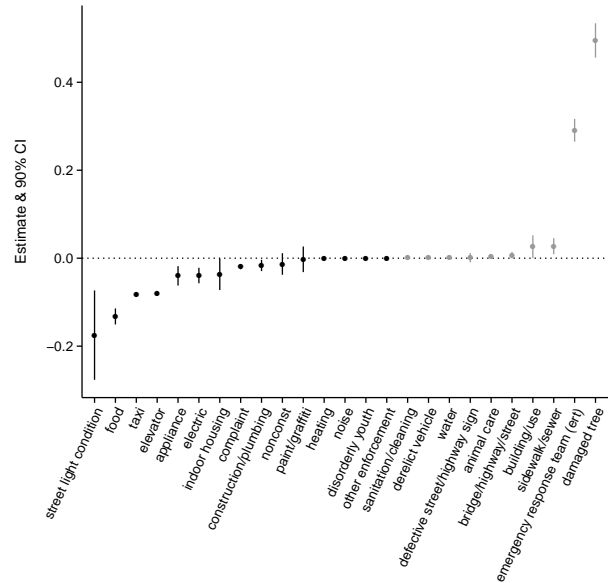
Our effects could be driven by increased responsiveness to particular types of public service requests. Eligible incumbents may, for example, focus their efforts on resolving more observable public service requests. We include exploratory analysis below, where we decompose the effects we report in the body of the paper by request type. That is, we reestimate equations 1 and 2 after first subsetting the data to a single request type. We report our estimates of  $\beta$  (and 90% confidence intervals) for the most frequent request types.

Graffiti, for example, is a service request that is more quickly resolved by eligible incumbents in NYC (as elections approach). One could argue that these types of observable complaints are where vote-seeking politicians want to focus their efforts. However, without a prior coding of complaints based on their observability (or costs) or adjustments for multiple testing, we do not venture any such claims.

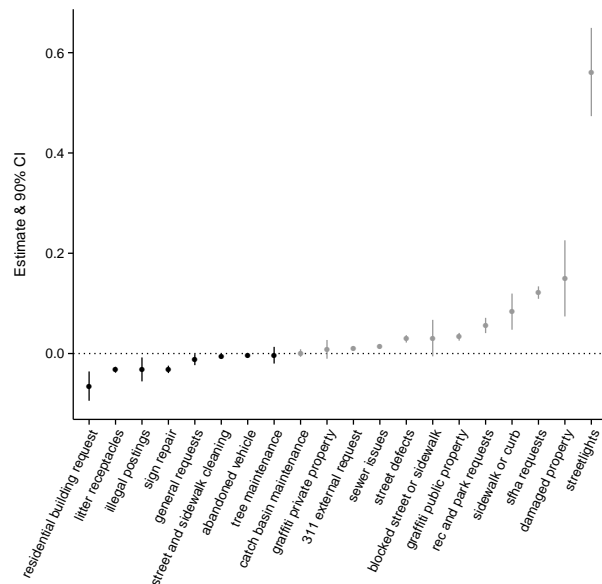
**Figure SI.4.1 NYC 2008**



**Figure SI.4.2 NYC 2005**



**Figure SI.4.3 SF 2010**



## Supporting Information 5 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 their ethnic kin as part of a vote-buying strategy.

To carry out this analysis we begin by matching the 3-1-1 database with census data on the racial composition of every block – which roughly corresponds to a neighborhood – 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 plurality (largest) group is coethnic with the city council member; and whether or not it has a majority group that is coethnic with the city council member. We also create variables that indicate 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 (available upon request) 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. It is the case, however, that neighborhoods populated primarily by Hispanics or Asians see larger drops in response times, though we should note that 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 elections.

Our failure to uncover 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.