

The Effect of Election Proximity on Government Responsiveness and Citizens' Participation: Evidence From English Local Elections

Comparative Political Studies

1–30

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DOI: 10.1177/0010414020912290

journals.sagepub.com/home/cps



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Abstract

Does political engagement depend on government responsiveness? Identifying the drivers of political action is challenging because it requires disentangling instrumental from expressive motives for engagement and because government responsiveness is likely endogenous. We overcome the first challenge by studying citizens' reporting of street-problems—a form of participation arguably driven by instrumental considerations. We overcome the second challenge by taking advantage of variation in local elections timing in England's district authorities. We report three key results. First, local governments address requests faster in the months leading to elections. Second, street-problem reporting increases in (pre)electoral periods. Third, the increase in requests sent in preelection periods is driven by districts in which government responsiveness is higher. These findings show that, individuals consider expected benefits when choosing to undertake at least some instrumental forms of participation. Our results also underscore the

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importance of temporal factors that increase the perceived benefits of one's political engagement.

Keywords

legislative studies, representation and electoral systems, political behavior, political engagement, politics and technology

Introduction

Nonelectoral forms of political participation contribute to better governance, but are subjected to free-rider problems. Theory suggests that such forms of participation increase with government responsiveness, because a responsive government increases the expected benefit of action (A. Campbell et al., 1954). However, demonstrating this proposition rigorously is challenging (Finkel, 1985). First, even when the decision to undertake political action is rooted in a cost–benefit calculation, it is generally hard to separate instrumental from expressive forms of participation (Fiorina, 1976). Second, government responsiveness tends to be endogenous: responsiveness might be an outcome and not a cause of an engaged citizenry, or it may be correlated with (unobserved) factors that plausibly affect citizens' choice to take action.

We address the first inferential problem by focusing on a form of political action that is overwhelmingly driven by instrumental considerations. Specifically, we examine the decisions of residents in England to log onto FixMyStreet, an innovative online platform to report street-related problems: from road potholes to street noise and graffiti. The platform collects geo-referenced user messages and forwards them to the local district authority responsible for fixing them. As many forms of political engagement, individuals decide whether to incur a relatively low-cost action (take a photo, tag the location, and write a complaint) that benefits the larger community, or to free-ride and count on other agents' initiative.

We address the endogeneity problem using plausibly exogenous variation in government responsiveness: *the timing of local elections*. Related literature works on political cycles (Nordhaus, 1975) and on recency bias in retrospective voting (Healy & Lenz, 2014; Huber et al., 2012) suggest that politicians increase effort as elections loom near. If true—and this proposition is still debated (Christensen & Ejdemyr, n.d.)—incumbents would be (on average) more responsive to constituents' requests; in our context, they would ensure complaints are addressed faster. In such case, to the extent that constituents consider the expected benefit of their (political) actions, we should

observe an increase in the rate of reporting of street-related problems prior to elections.

Local elections in England take place in May. A (naive) comparison of both response and usage of FixMyStreet in months just prior to May elections to other months, would admittedly conflate electoral and period effects. Instead, we use the fact that different districts authorities in England hold elections in different years. Assuming that the timing of elections is orthogonal to the occurrence and severity of street-related problems, we use a difference-in-difference (DiD) design to compare both (a) the time to address complaints and (b) the total number of complaints sent before (January–April) and after elections (June–December), in districts with and without local elections in a given year.

We report three main results. First, government responsiveness increases significantly before elections, as reflected in the speed at which local governments address requests. We provide evidence that this finding is not driven by a variation in the type of requests sent before elections or by councilors reporting requests as fixed at higher rates. Second, the number of requests sent to the FixMyStreet portal is significantly higher in January to April in districts that hold May elections, as compared with equivalent months in districts that do not hold elections in that year. Third, the increase in requests sent in the preelectoral period is driven by those districts in which the local government was more responsive in the previous months.

We further rule out alternative explanations for citizens' behavior. First, we test whether constituent requests are not simply driven by higher salience of politics during campaign periods, when citizens are inundated with political messaging. We show that when only general elections take place, citizens do not increase the rate of reporting street-level problems. Second, we do not find evidence that (possible) higher inefficiency of local governments around election time is causing more problems that, in turn, drive up citizen requests. Instead, the evidence at hand is more consistent with greater citizen engagement induced by heightened government responsiveness around elections, rather than by other changes occurring around electoral periods.

Our article contributes to work exploring the relationship between elections and politicians' behavior. The political cycle literature, in particular, predicts that politicians increase effort prior to elections. This is because prospective and retrospective voters focus disproportionately on that period when assessing candidates' competency (Besley, 2006) or incumbent performance (Shepsle et al., 2009). Empirical evidence of the above prediction is, however, mixed, in part because confounding factors likely bias the relationship between electoral incentives and incumbent actions (Keele et al., 2013), and in part because different studies focus on different outcomes.¹ We corroborate this prediction in

line with a small number of studies with a credible strategy for identifying the effects of electoral incentives. While those studies generally exploit exogenous variation in term limits exposure (Klašnja & Titiunik, 2017), we use instead exogenous variation in election timing.

We also contribute to a literature exploring the relationship between political engagement and the perceived returns to action. Past work has demonstrated, for example, that participation is more likely when citizens receive social recognition from undertaking a political action (Gerber et al., 2008), when elections are more salient (Arceneaux & Nickerson, 2009), when voters are more likely to be pivotal (Blais, 2000), and when politicians signal an interest in hearing from constituents (Grossman et al., 2017). We add to this literature by demonstrating the positive effect of heightened government responsiveness induced by electoral temporal proximity.² While our contribution here is empirical, our research design can be used to further explore the role of government responsiveness in other contexts or for other outcomes (e.g., Anzia, 2011).

Finally, we contribute to a growing literature examining the relationship between information and communication technologies (ICTs) and political behavior. The bulk of this work has sought to document how citizens' political behavior is affected by the expansion of mass communication platforms that disseminate *elite*-sourced information (Boulianne, 2009). A smaller, but growing, body of work instead examines how ICT innovations allow *citizens* to be sources of information by reporting trash collection problems (Buntaine et al., 2019), violence (van der Wind & Humphreys, 2016) and corruption (Blair et al., 2019) and conveying political preferences, more broadly (Grossman et al., 2014, 2019).

Government Responsiveness and Political Participation

Understanding the determinants of political participation is a primary objective of political science. Theories of participation contend that the decision to undertake political action is a function not only of citizens' individual characteristics—such as socioeconomic and civic resources (Brady et al., 1995), education (Sondheimer & Green, 2010) and cultural norms (D. E. Campbell, 2006)—but also of context conditions. Specifically, factors that increase the expected return from participation should encourage political action. For example, in competitive districts where citizens' votes are more likely to be pivotal, participation is expected to be higher (Downs, 1957). More in general, when a government is, for any reason, more responsive to citizens'

demands, the benefit of undertaking a political action is expected to be higher (A. Campbell et al., 1954).³ To the extent that voters are (at least somewhat) strategic in deciding whether to undertake political actions, and are sufficiently sophisticated to map governments actions and outcomes, then greater government responsiveness increases expected returns from action and thus the probability of political participation.

Hypothesis 1: Political participation is increasing in government responsiveness.

While the above hypothesis is a basic prediction of rational voter models, testing it empirically is hard because government responsiveness and citizen participation are likely endogenous: responsiveness could trigger participation or be the product of an engaged civil society. Alternatively, both government responsiveness and political engagement might stem from joint underlying factors, such as civic culture.

To overcome this inferential challenge and test the above proposition empirically, in this study we decompose Hypothesis 1 into three related testable hypotheses. Our starting point is the idea that the election period is both exogenous⁴ and, given its importance in determining election outcomes, can cause a temporary increase to government responsiveness. As citizens are more attuned to politics as elections looms near, politicians have a stronger incentive to deliver benefits in that period (Baskaran et al., 2015). If true, we should observe a strategic reaction in participation from citizens. However, the electoral period triggers participation also for purely expressive motives, as politics is more salient and citizens might be directly mobilized by the campaign. To identify whether citizens *strategically* increase engagement in reaction to higher government responsiveness, we thus focus on a specific form of political participation that is likely to be purely instrumental: sending requests for fixing street-related problems to the local administration. If (a) local governments respond faster to requests before elections (to increase reelection probability), if (b) citizens send requests at higher rate in this period (due to higher expected benefit), and if (c) the increase in participation is concentrated in areas in which government responsiveness is higher, then this would suggest that citizens are strategically reacting to (exogenously induced) enhanced government responsiveness. Summarizing our hypotheses:

Hypothesis 2a: Election proximity increases government responsiveness.

Hypothesis 2b: Election proximity increases instrumental forms of political participation.

Hypothesis 2c: Responsiveness induced by election proximity increases instrumental political participation.

Now assume that councilors believe that responding to citizen requests faster in the months leading to elections can help them get reelected. In most places, however, unelected bureaucrats are tasked with addressing citizen street-level complaints. A natural question arising is whether local politicians, facing upcoming elections, can influence the rate at which such requests are addressed. As local politicians nominate bureaucrats, the incentives of both civil servants and councilors are aligned, helping to ensure that politicians can effectively nudge bureaucrats to move somewhat faster before elections. We present anecdotal evidence in line with the scenario in the next section entitled “Political Context”.

A related question is about the scope conditions of our theoretical framework. Three crucial factors characterize our theoretical framework: that officials have an incentive to be responsive to citizens’ requests (for example by facing electoral incentives); that citizens have the capacity (or communication channels) to make their voice being heard; and that citizens can attribute responsibilities for the responses they observe from the government, such that officials can expect to be rewarded or punished as a function of their response to citizens’ requests. We thus do not expect to observe an effect of elections on responsiveness and participation (a) with institutions shielded from electoral cycles (e.g., regulatory authorities, central banks) or politicians who do not face reelection incentives (e.g., term-limited)⁵; (b) when it is difficult for constituents to communicate to the institution and, conversely, when the institutional capacity is weak, such that citizens have low expectations to receive an answer; and (c) when issues at hand are such that citizens cannot easily adjudicate the responsibility of the institution in delivering the service (e.g., a reform could fail because of macroeconomic conditions or for the lack of a majority so the link between responsiveness and participation could be broken for other reasons). Accountability and the existence of channels for citizens to express their views are thus fundamental prerequisites for the mechanisms we describe above to be at work.

In this study, we document how responsiveness impacts participation in a short-term window. The relation between these factors, however, is likely to take the shape of a feedback when considered dynamically. When the government is responsive and citizens are active, a virtuous democratic loop is likely to emerge for which a strong citizens–government link is created. The reverse can happen if expectations go the other way around. Distrust in politicians’ capacity to respond to citizens’ demands is considered as one of the primary causes for the decreased rates of participation in Western democracies (Mair,

2013) and developing countries alike (Grossman et al., 2018). The pattern documented in this study suggests how, dynamically, the feedback between government responsiveness and participation can lead a democracy to enter one equilibrium or the other.

Political Context

We test our hypotheses using data from English districts. England is divided into different types of subnational authorities, with varying functions and responsibilities. At the highest level, there are nine regions, which—with the exception of the Greater London Area—do not perform local administrative duties. We thus focus on districts, England’s second-tier of decentralized governments, which are imbued with executive powers. Local authority districts (henceforth districts) are responsible for the provision of the services for which most requests are sent to FixMyStreet, such as local road maintenance, waste collection, street lights and signals, and parking enforcement.

England has 201 nonmetropolitan districts, 55 unitary authorities, 36 metropolitan boroughs, 32 London boroughs, and two special status districts. Nonmetropolitan districts and the London boroughs have a two-tier structure in which districts share powers with counties and regions. Metropolitan boroughs and unitary authorities have a single level of government, responsible for administering all local services in their area (Atkinson & Wilks-Heeg, 2000). We exclude four districts from our study: the City of London Corporation (joint with Westminster) and the Isles of Scilly (joint with Cornwall), which are *sui generis* authorities with different administrative role and electoral rules.⁶

Elections in English Districts

Districts are divided into wards for electoral purposes. Wards are comprised of about 5,500 residents, on average, and elect one to three councilors depending on their size, with each voter having a number of votes corresponding to the number of councilors to be elected. Local councilors are elected for a 4-year renewable term, with local elections commonly held on the first Thursday in May.⁷ Due to a variety of electoral cycles (discussed below), not all district councilors are elected at the same time.

There are three different modes of holding elections in local councils. The entire council can be elected at once, in which case elections are held once every 4 years. Alternatively, half or one third of the council can be elected in every election cycle; here, elections are held every 2 and 3 years, respectively. Finally, in nonmetropolitan districts, elections take place in both tiers

Table 1. Pattern of Local Elections by Year.

N districts	%	2007	2008	2009	2010	2011	2012	2013	2014	2015
31	9.66				x				x	
1	0.31			x				x		
2	0.62	x		x				x		
24	7.48	x				x				x
2	0.62	x	x					x		
2	0.62	x		x		x				x
2	0.62	x	x			x				x
123	38.32	x		x		x		x		x
1	0.31	x	x		x	x				x
7	2.18		x	x	x		x	x	x	
4	1.25	x	x	x	x	x		x		x
54	16.82	x	x		x	x	x		x	x
1	0.31	x		x	x	x		x	x	x
67	20.87	x	x	x	x	x	x	x	x	x
	N elections	282	137	207	165	278	128	207	160	278

The table shows the number of districts displaying a specific pattern of elections in each year. For example, reading the first line, 31 (or 9.66% of the) districts had elections in 2010 and 2014. The line at the bottom of the table reports the total number of districts with elections in any given year.

of government, district, and counties. As both tiers have administrative functions relevant to FixMyStreet, we consider both district and council elections in our analysis. The combination of these different patterns of elections results into a considerable heterogeneity in terms of which districts hold elections in any given year. Table 1 traces the different pattern we observe in the years for which we have access to FixMyStreet data.

Although councilors are elected at the ward level, they are not solely responsible for addressing problems in the ward they represent. Given their statutory powers to address the vast majority of issues for which requests are sent to FixMyStreet and their electoral relevance for local government purposes, we consider local authority districts as the study's unit of analysis.⁸

Local Councils and Online Reporting of Street-Related Issues

Over time, online reporting of street-related problems has become both more common among citizens and more commonly regarded as an important accountability mechanism by local councilors. This pattern is evident in our data, which show a constant increase in requests over time paralleled by a

monotonic decrease in the time taken to respond to requests sent (Figure 1A and 1B). Consistent with viewing online reporting as a core constituency service request, a number of local councils have integrated FixMyStreet as their official system of problem reporting.⁹ Besides providing citizens with a more effective way to communicate with the local government, this choice reflects electoral considerations: local councilors often advertise “fixes” they helped bring about (a form of constituency services) as an indicator of performance. Consistent with these empirical patterns, Burnett and Kogan (2017) show that neighborhood-level variation in complaints for potholes in San Diego explain differences in support for incumbent local politicians.

The council’s good record at fixing potholes, for example, is prominently displayed in the home page of Steve Galloway, a councilor in the district of York, while councilor Keith Aspden advertises his record at helping to address parking issues. Like other types of political messages, promotion of fixes can also backfire: a tweet by Chester Council portraying a technician measuring a pothole but failing at taking the measurement correctly, received hundreds of comments before being deleted.¹⁰ These examples show that councilors treat FixMyStreet requests as an integral part of constituency services they ought to engage and promote, and that there is a set of constituents paying attention to these messages and using them to exercise control over local representatives.

While it is not directly the councilors who take care of fixing street-related issues, the civil servants responsible for executing these tasks are appointed by the councilors and their reappointment is more likely if councilors are reelected.¹¹ If elected officials believe that fixing neighborhood problems has a positive electoral return, as evidence seems to suggest, unelected bureaucrats at their direct dependences will likely have shared incentives and thus will also be more attentive to issues reported in the months leading to elections (Christensen & Ejdemyr, n.d.). Importantly, the full publicity of whether requests are fixed or not, substantially reduces the monitoring difficulties that may give rise to principal-agent problems.

Data

Our data include the collection of all citizen requests (and government response) made via FixMyStreet between 2007 and 2015. Using an intuitive interface, this online and mobile application platform allows users to report any type of street-related problem by following a simple four-step process: (a) selecting a location in the United Kingdom on a map, (b) selecting which category the issue belongs to, (c) summarizing the issue and explaining why it is a problem, and (d) uploading a photo capturing the

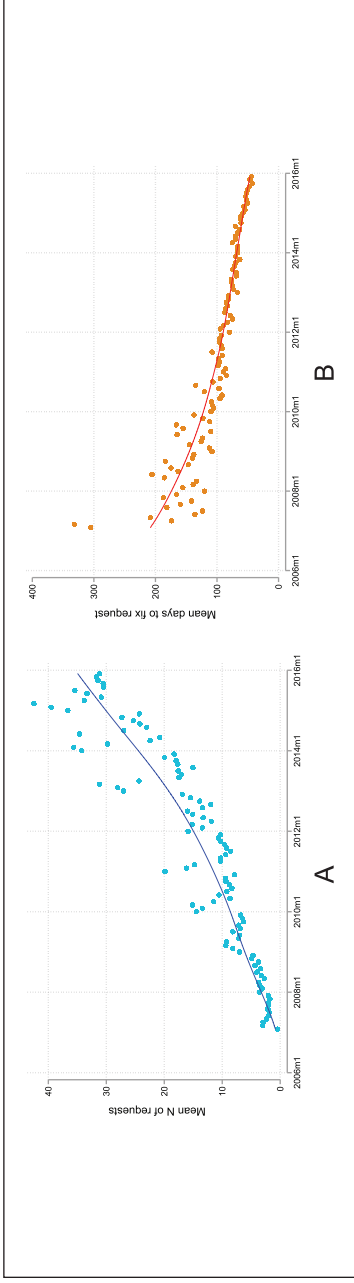


Figure 1. (A) The mean monthly number of FixMyStreet requests over time. (B) The mean number of days to fix a request.

scene. No registration is required to report a problem or make a request, only an email address, and the user is free to decide whether or not to provide her name. The platform automatically forwards the message to the appropriate local council, and the user who sent the report is notified of the council's response, under the requirements of the Freedom of Information Act. We include images of the portal home page and the reporting interface in the Supplemental Information, Section A. For each request, we are able to observe the date, time and message sending location, as well as when a response was sent from the local council.

Based on response information, we build a measure of government responsiveness as the mean number of days taken by the local council to fix requests submitted in a given month.¹² This is a more objective and high-frequency measure of government responsiveness than commonly used in the literature, such as self-reported performances (Carey et al., 2009), citizens' assessment of the quality of elected officials (Niemi et al., 1991), and congruence between the policy positions of an elected official and that of her constituency (Clinton et al., 2004). In addition, response time to constituency requests is more credibly mapped to politicians' actions as compared with, for example, voting record (subject to party discipline) or public good provision (which is slow moving and involves many actors). While similar to audit experiments that measure public officials' response rate (e.g., Butler & Broockman, 2011), our measure has the advantage of not involving deception, and capturing not only spatial but also temporal variation in responsiveness.

Figure 1A depicts the number of mean monthly requests sent via the portal since FixMyStreet's launch in February 2007, demonstrating an increase in the number of requests sent over time. The total number of requests in that period is 516,242. Correspondingly, Figure 1B shows a steady decrease in the mean number of days the local council takes to address a request: starting from a mean of over 200 days in 2007 to less than 50 days, on average, in 2016. As mentioned, these figures underscore the fact that both residents and district politicians are increasingly viewing FixMyStreet as a key mechanism for improving constituency services and outreach.

We grouped requests into eight categories based on information contained in two hand-coded variables indicating the request title and content. The most common issues are rubbish and road conditions, while the least common issues are related to street furniture. In Figure 2, we plot the share of messages sent and the response rate of local councils by category in districts with elections (treated) and without (control). Treated and control districts send similar requests, while responsiveness to requests is systematically higher in districts with elections for all categories of requests, except for the unclassified group.

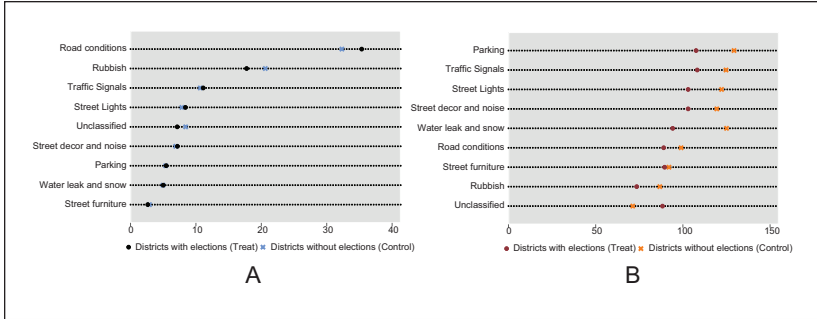


Figure 2. (A) The mean number of requests sent by type of the message and treatment status in a given month. (B) The mean number of days to fix messages by type and district.

We reverse-geocoded each request’s latitude and longitude to the corresponding electoral ward, thus obtaining the link between platform requests and the relevant district authority in any given year. The distribution of requests and responsiveness by local district is mapped in the Supplemental Information, Figure 10. Using location and time stamp, we first organize the request data such that each row represents a district-month.¹³ We then merged the request data with both local election data, from the Electoral Commission website and with sociodemographic data from the 2011 Census. Table 4, in the Supplemental Information, provides descriptive statistics for all variables used in the empirical analysis.

Estimation Strategy: Election Proximity Effects

We test whether incumbents are more responsive and citizens make more requests around elections using a DiD estimation strategy that builds on both a temporal (months before vs. months after elections) and a spatial comparison (districts with vs. without elections in a given year). We begin by plotting the raw data by district “type,” denoting districts in an election year as “treatment.”¹⁴ Districts that hold local elections in a given year (treatment) receive more requests in preelection months (January–April) as compared with districts in the same year that are not scheduled to hold local elections (Figure 3A). In districts with elections, we also observe that governments take fewer days to fix a request (Figure 3B).

Turning to a more formal test, we compare these outcomes in the months before and after elections in districts with and without local elections using the following regression model:

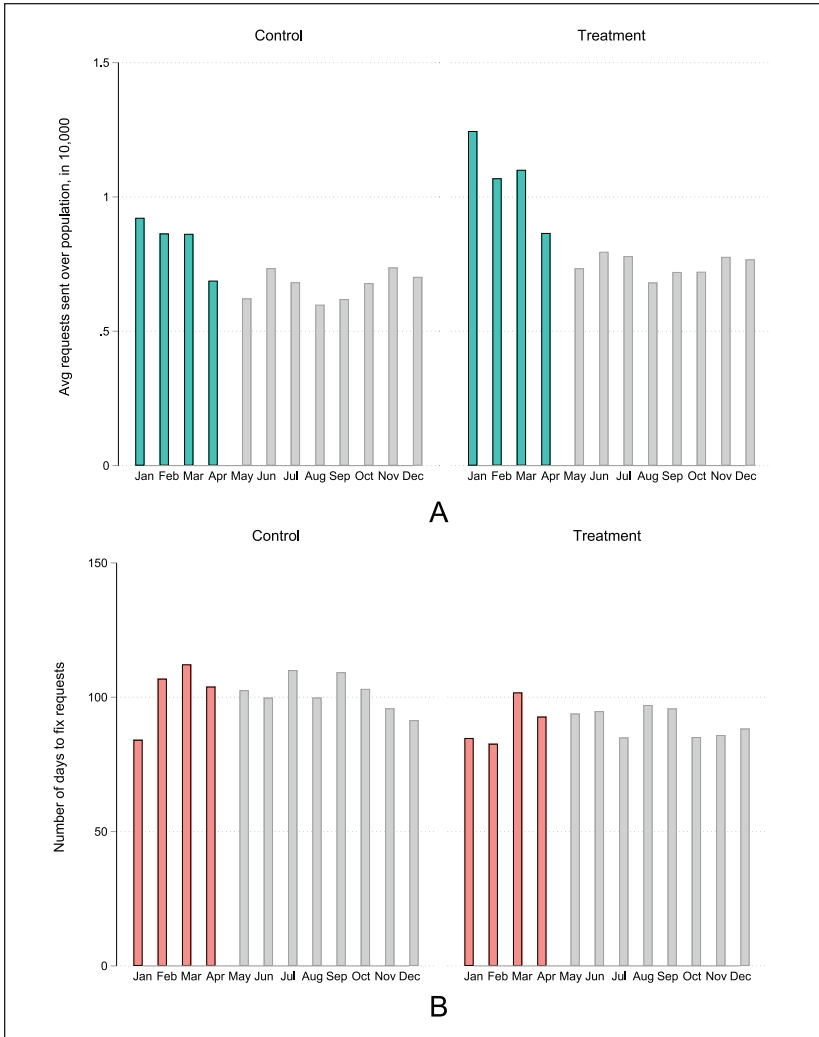


Figure 3. Mean number of requests sent (A) and mean number of days to fix requests (B) by month and treatment status.

$$Y_{it} = \alpha_i + \beta_t + \gamma_{it} \text{Treat}_{it} + \varepsilon_{it}, \tag{1}$$

where the outcome variable Y_{it} is, first, our measure of government responsiveness (log number of days to fix a request) and, second, our measure of

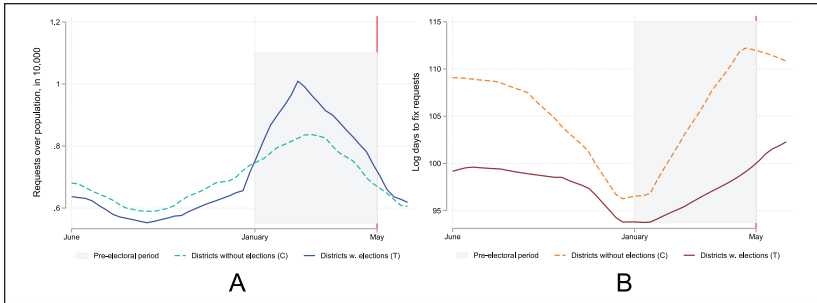


Figure 4. Parallel trend test. (A) Participation (mean number of requests). (B) Responsiveness (mean days to fix a requests).

The figure shows the local polynomial smoothed line of the mean number of requests sent in a district over population, winsorized at the top 0.01 tail (A) and of the mean number of days to fix requests (B). The solid line traces the DV in districts holding elections at time t , the dotted line in control districts; that is, without elections. The gray area shows preelection period (January–April) and election day is marked by a red vertical line.

political participation (number of requests sent per 10,000 residents) during month-year t in authority district i ; $Treat$ is a binary variable taking value 1 in districts in which local elections take place that year and in the months preceding elections (January–April), and 0 otherwise. The parameter of interest, γ , captures the effect of the preelectoral period on responsiveness and participation in districts in their election year compared with districts in the same period in nonelection years. This allows us to overcome identification issues related to a potential increase in requests in winter months. We account for time invariant confounders using district fixed effects (α_i) and for contemporaneous shocks by adding month \times year fixed effects (β_t) to the model. Standard errors are clustered at the district level.

Identification Assumptions

We test for the validity of the parallel trend assumption, required for DiD estimation, using both a nonparametric approach and a parametric approach in the spirit of Autor (2003). We start by testing the parallel trend assumption comparing our dependent variables across districts with and without elections. With local elections taking place in consecutive years in most districts, as only 60 districts have elections every 3 years (see Table 1), it is hard to examine the behavior of our outcome variables for periods longer than a few months for a sufficient number of districts. We thus consider trends in the months far from local elections for comparison. In Figure 4, we compare the

trend in requests sent (A) and days to fix requests (B) in districts that held elections in May (solid line) against districts that did not hold elections during that year (dotted line). Looking at the Figure 4A, we observe parallel trends in the period far from elections (June–January) across treatment groups. From January, instead, the number of requests in districts with elections increases rapidly, surpassing the number of requests in district without elections. This pattern is consistent with the presence of a parallel trend and with the idea that elections cause an increase in reporting. Looking at the Figure 4B, we also observe that responsiveness follows a parallel trend across treated and control districts, but in this case, our dependant variable (DV) is extremely seasonal, with control districts becoming more responsive right before January.

We thus run a more formal test to check the validity of the parallel trend assumption on the model of Autor (2003) by interacting the periods just before and just during elections with the treatment variable, which takes value 1 in election years. Consistently with the parallel trend assumption, the interaction with $t-1$ (the *lag*) is insignificant, suggesting that outcome trends between treatment and control group are not significantly different in the period before treatment starts. Results are reported in the Supplemental Information, Table 5.

In the Supplemental Information Appendix, we further provide evidence consistent with the assumption of balance between districts in electoral and nonelectoral years; that is, that treatment and control districts do not differ in observable characteristics that might explain political participation (Supplemental Information, Table 6 and Figure 14).

Results

Government Responsiveness and Election Proximity

First, we report results for the effects of election proximity on government responsiveness. Formally, we estimate Equation 1 using as dependent variable the log of the number of days the local government takes to address a request. We further control for requests' type to account for the possibility that higher responsiveness is mechanically improved by receiving types of requests that are easier to address.¹⁵

We find that local councils respond faster to requests before elections: the coefficient for *Treat* in Table 2 is both negative (-.057) and significant. Accounting for the logarithmic transformation this corresponds to an 11% decrease from an average of 52 days. In other words, local councils address

Table 2. Treatment Effect on Government Responsiveness (Log Days to Fix Requests).

Variables/ Model	Linear	Mixed	Poisson
Treatment	-0.057** (0.026)	-0.056** (0.025)	-0.014** (0.006)
Observations	19,863	19,863	19,863
District FE	Yes	No	Yes
District RE	No	Yes	No
District Controls	No	Yes	No
Month-year FE	Yes	Yes	Yes
Number of districts	322	322	322

The table shows results from the difference-in-difference estimation specified in Equation 1. The DV is the log of the average number of days to fix requests in district i and month t , our measure of government responsiveness. *Treat* is an indicator equal to 1 in the preelectoral months (January–April) in districts in which an election took place during that year. Robust standard errors are clustered at the district level. FE = fixed effect; RE = random effect. * $p < .1$. ** $p < .05$. *** $p < .01$.

issues 6 days faster, on average, in the same calendar months (January–April), in the same year, when facing elections. In Table 7 in the Supplemental Information, we show that these results are robust to using different definitions of responsiveness, such as (a) the average number of days to fix a request, without logarithmic transformation; (b) the median number of days to fix a request, rather than the average; and (c) the number of requests fixed in 30 days. We also show that results are robust to (d) controlling for the number of requests sent and (e) for the category of request sent, rather than the share of requests which can be fixed fast.

In addition, we account for the intensity of the election treatment by examining the effect of having more or less councilors up for reelection, which in turn depends on the pattern of elections a district follows (Supplemental Information, Table 8). First, our findings are robust to controlling for election pattern. This is equivalent to controlling for the “type” of district, as each has a different electoral pattern. With this test we are thus able to exclude the possibility that the prevalence of metropolitan or unitary districts up for election in a particular year drives the findings. Second, subsetting the analyses by the share of councilors running for reelection, reassuringly we find a larger increase in responsiveness when more councilors are up for reelection. In Column 5, we also weight estimates by the frequency of elections to interpret the coefficient as the effect on responsiveness for the average number of councilors running. Again, results are similar to the main analysis.

We test the validity of these results against two alternative explanations. One possible concern is that requests are reported as fixed faster before elections not because the government is more responsive, but because, before elections, local councilors are more likely to log into FixMyStreet and report problems as fixed. To address this concern, we rerun Model 1 dropping requests which were marked as fixed by the local council. Results are robust to this test (Supplemental Information, Table 9). A second concern is that the type of requests sent are different in the period before elections. In Figure 2A, we show that the type of requests sent does not change across treated and control districts, suggesting that response time is not simply a function of changes in what citizens report (e.g., issues that can be addressed fast). Tellingly, responsiveness is significantly higher in treated districts across all categories of requests (except for the residual category of unclassified requests; Supplemental Information; Figure 2B). More so, we calculate the share of requests which can be fixed faster by the local council and regress this on treatment status. We find that in preelectoral period, somewhat *fewer* requests that can have a fast-fix are sent (Supplemental Information, Table 10). In sum, the evidence at hand is consistent with the idea that greater government responsiveness to constituency services requests is due to electoral accountability considerations.

Building on a growing literature that ties government responsiveness to electoral competition (e.g., Besley & Burgess, 2002; Grossman & Michelitch, 2018), we further explore whether the effects of election proximity are stronger in more competitive districts. In the Supplemental Information, Section D.3, we describe our measurement of competitiveness and results. While we find that competitiveness is positively associated with greater responsiveness, we do not find that it moderates the effect of election proximity. In addition, we do not find that the effect of election proximity is moderated by districts' socioeconomic characteristics (Supplemental Information, Section D.4, Table 16, Columns 4–6).

Political Engagement and Election Proximity

We have shown that election proximity results in greater government responsiveness. This finding suggests, in essence, an increase in the (expected) benefits of political action during local election periods; in our case—making a request to fix a street-related problem. We therefore turn to explore whether election proximity also increases political participation. Specifically, we rerun Equation 1 using as dependent variable the number of requests sent to FixMyStreet normalized by the district population.

Table 3. Treatment Effect on Political Participation (Number Requests Sent).

Variables / Model	Linear	Mixed	Poisson
Treatment	0.079*** (0.026)	0.080*** (0.027)	0.044** (0.019)
Observations	26,690	26,690	26,690
District FE	Yes	No	Yes
District RE	No	Yes	No
District Controls	No	Yes	No
Month-year FE	Yes	Yes	Yes
Number of districts	322	322	322

The table shows results from a panel analysis as specified in Equation 1. The dependent variable is the number of requests sent in district i normalized by the district population (in 10,000 inhabitants). $Treat$ is an indicator that equals 1 in the months from January to April in districts in an election took place during year t . Robust standard errors clustered at the district level are in brackets. FE = fixed effect; RE = random effect.

* $p < .1$. ** $p < .05$. *** $p < .01$.

We find that in districts holding elections, the number of requests sent in the months leading to election day (January–April) is significantly higher than the number of requests sent in the same months in districts that did not hold elections that year. As shown in Table 3, the number of requests sent increases by 0.08, an 11% increase with respect to the mean. Estimates are consistent across model specification, using mixed effects and Poisson (Columns 2 and 3) models, instead of the DiD base specification (Column 1).

In Figure 5 and the Supplemental Information, Table 11, we show similar results disaggregated by month (rather than pooling across months). In particular, we estimate a version of Equation 1 where $Treat$ is interacted with calendar months indicators. As expected, the DiD coefficients are positive and significant in the preelectional months (January–April). Notably, in the postelection months (after May), the number of requests sent is no different across treatment conditions.

In the appendix, we report a series of additional robustness tests. Specifically, we show that results are not dependent on winsorization (Supplemental Information, Table 12, Column 1), to clustering conservatively at the level of the election pattern rather than the district (Column 2) and to removing all districts which experience elections every year (Column 3). Results are also robust to adjusting for district-level covariates instead of using districts fixed effects (Column 4) and for year instead of month-year fixed effects (Column 5). Here too, results are stronger when we consider districts with more councilors up for reelection (Supplemental Information,

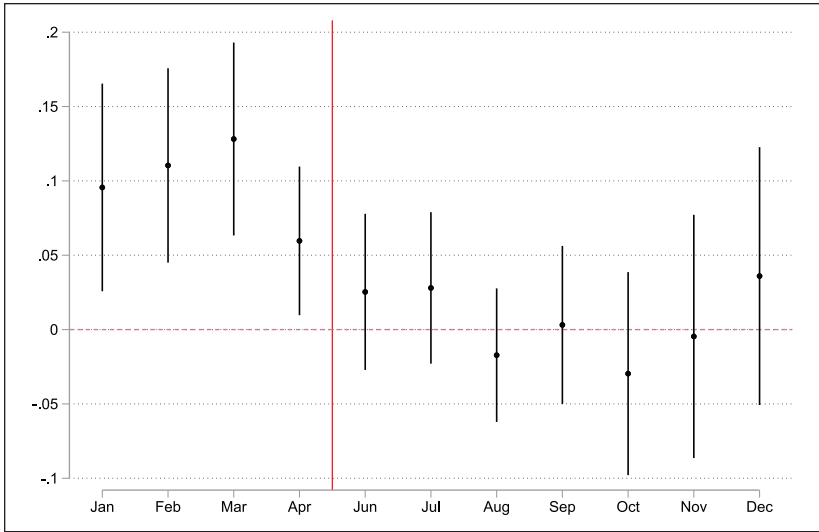


Figure 5. Effect of the month from election on the number of messages sent. The figure plots coefficients from estimating a monthly (disaggregated) specification of Equation 1 using OLS (Supplemental Information, Table 11). Coefficients represent the effect of each month in districts holding elections in May that year as compared with districts not holding elections. Robust standard errors clustered at the district level. OLS = ordinary least squares.

Table 13). Finally, in the Supplemental Information, Table 14, we show results when using annual instead of pooled data.

Are the effects of election proximity larger in more competitive councils? In the Supplementary Information, Section D.3, we show that there is an increase in the number of requests sent in competitive districts, measured as those with more contested seats as a share of total vacancies. However, the coefficient for the interaction is relatively small in magnitude and only significant at the 10% level. Moreover, we find no differential effect on requests sent when we consider the share of contested wards as an alternative measure of the competitiveness moderator (Supplemental Information, Table 15, Columns 1 and 2). These results are consistent with the finding, reported above, that election proximity does not have a differential effect on government responsiveness as a function of competitiveness. If greater competition for office does not lead governments to fix requests faster, we should also not expect citizens to perceive a higher benefit from sending requests in this period.

We also test for heterogeneous effect by the sociodemographic characteristics of the district population (Supplemental Information, Table 16, Columns 1–4). In line with the literature on the socioeconomic determinants of political participation (Alesina & La Ferrara, 2000), we find that election effects are larger in more educated and in richer districts. To the extent that higher socioeconomic status (SES) is associated with greater political sophistication—in our case, arguably a better understanding of the election-responsiveness nexus—this finding increases our confidence that the effect of election proximity on participation is not spurious, but rather a rational response to changes in expected benefits.

Finally, we test for negative weighting using the procedure elaborated by de Chaisemartin and D’Haultfoeuille (2019). In our case, 5,115 (96.3%) of the weights are strictly positive and 198 (3.7%) are negative. The negative weights sum to -0.00029 while the standard deviation of the average treatment on the treated (ATT) is equal to 51.8. β and the ATT might have opposite signs if the standard deviation of the effect of elections across cities is above 51.8 percentage point, an implausible treatment effect heterogeneity. We repeat the test for responsiveness: Here, only 2.5% of the weights are negative, summing to -0.0012 and the standard deviation of the ATTs is equal to 6.46. Again, only an extremely large treatment heterogeneity might produce coefficients of opposite signs. We conclude that β can have a causal interpretation even if election effects are not strictly constant across districts.

Higher Participation Before Elections in Responsive Districts

That both governments are more responsive and citizens more likely to participate prior to elections does not necessarily entail that citizens do so *because* of greater responsiveness in that period. To better link the increase in requests sent prior to elections to local government responsiveness, we further test whether the increase in the number of requests made prior to elections is especially concentrated in districts that exhibit greater responsiveness.

We do so using a flexible interaction model, in which we estimate the number of requests in a district-month as a function of the combined effect of the election period (*Treat*) and responsiveness (*DaysToFix*). Following Hainmueller et al. (2019), we further bin the continuous moderator *DaysToFix* (i.e., the number of days to fix a request) into four equal sized groups. This approach allows to relax the assumptions of linear interaction effect and is especially useful when there is lack of common support for all values of the moderator, as is in our case.¹⁶

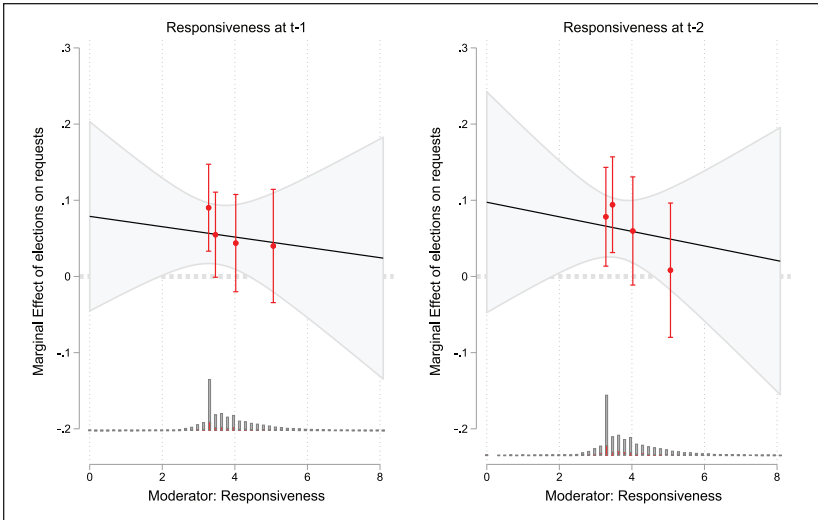


Figure 6. Effect of treatment on requests sent by levels of responsiveness. Marginal effects of treatment (election period) on requests sent normalized by 10,000 residents, conditional on council’s lagged level of responsiveness. Responsiveness is defined as the (log) mean number of days taken by the local government to fix requests. We consider responsiveness lagged by 1 month ($t-1$) in the panel on the left and by 2 months ($t-2$) in the panel on the right. All specifications include district and month fixed effects and controls for the type of requests sent. Robust standard errors clustered at the district level. Whiskers represent 95% confidence intervals.

In particular, for each district i and month t , we model the number of requests sent as a function of the interaction $\text{Treat (election year)} \times \text{Responsiveness}$, separately for each binned level of responsiveness (high, medium-high, medium-low, low). As we are interested in the effect of government responsiveness on citizens’ engagement, we use a lagged measure of responsiveness, allowing citizens sufficient time (up to 2 months) to observe the behavior of the local government. As in the other models, we include fixed effects for both district and calendar month, control for the type of requests sent and cluster robust standard errors at the district level.

Regression results from estimating this model are displayed in Figure 6. In both panels, we plot the effect of the electoral period on requests sent, including 95% confidence intervals, conditional on the council’s level of responsiveness: from high (few days to fix) to low (many days to fix). The values of responsiveness are lagged by 1 month in the left panel and 2 months in the right panel. The linear conditional effect of treatment (election period) on requests sent is weakly negative in both panels, as indicated by the downward sloping dark line.

Focusing on the conditional effect of elections by the four levels of responsiveness G_j , we find that for G_1 (very high) and G_2 (high) levels of responsiveness, a significantly higher number of requests is sent by constituents. This is evidenced by the two left coefficients in each panel that are positive and significant, for values of responsiveness that are between 0 and 3 (i.e., less than 30 days to fix a request). The rug at the bottom of the figure suggests that the mass of the distribution is concentrated in this area. By contrast, the effect of elections on messages sent is not significant when the values for responsiveness are above 3, indicating slow (G_3) and very slow (G_4) response time to address complaints.

These results are robust to a set of tests including, using a nonlogarithmic definition of responsiveness, using median rather than mean responsiveness, and when controlling for the share of requests that can be fixed fast (Supplemental Information, Figure 16).¹⁷

Results are also robust to adopting a more restrictive definition of responsiveness including only requests which were sent and fixed within the same calendar month (Supplemental Information, Figure 17, Panel [e]). Consistently with expectations, the effect of responsiveness on participation is larger in districts in which a larger share of the council is up for reelection. However, the difference in the intensity of treatment does not drive our findings. We characterize this pattern in several ways. First, we show that results are robust to controlling for how many councilors are up for reelection (Supplemental Information, Figure 17, Panel [f]). Second, we rerun the analyses subsetting for the fraction of councilors which are running for reelection. The effect of responsiveness on participation is null when very few councilors are up for reelection, while it is larger the larger the share of councilors in their campaign period (Supplemental Information, Figure 18).

In conclusion, we find both a significant increase in requests sent by residents and a significant decrease in the number of days to fix complaints in districts that hold elections, as compared with districts that happen to not hold elections that year. More so, we find that the increase in requests sent in pre-election months is concentrated in districts where the local council was highly responsive, as measured by the number of days it took to fix requests. Combined, these findings are consistent with the idea that (at least some forms of) political engagement are a function of cost–benefit considerations.

Testing Alternative Explanations

Politicization

A plausible alternative explanation for our findings is that the increase in requests is not driven by the expectation that the government will be more

responsive in the preelectoral period, but rather by an increase in the salience of politics, driven by greater exposure to campaigning efforts. We explore this possibility using data from general election years. During general elections, the salience of politics increases, while expectations for local government responsiveness should not change. A significant increase in requests sent via FixMyStreet in general elections years suggests that the mechanism causing the increase in requests is not external efficacy, but rather politicization. We test this possibility using our monthly version of the DiD specification with a modified treatment indicator taking the value 1 in general elections years for districts with no local elections, and 0 in years without general elections for districts with local elections. The indicator is set to missing when (a) there are both local and general elections in a district and (b) when there are neither local nor general elections. We are, therefore, comparing a sample experiencing only general and a sample with only local elections during a given year. In particular, for district i and year y , we estimate:

$$\text{Nrequests}_{iy} = \upsilon GL_{iy} + \phi \sum_{t=1}^{12} \text{Month}_t + \chi GL_{iy} * \sum_{t=1}^{12} \text{Month}_t + \alpha_i + \psi_y + \omega_{iy}, \quad (2)$$

where GL indicates only general or only local elections in a district-year and Month is an indicator for each month t . In Figure 7, we display the plot of coefficients from estimating this equation. In the panel on the left, we present coefficients from the interaction of months and GL when this is equal to 1 (i.e., in district-years with general elections only), while on the right, we display monthly coefficients when GL is equal to 0 (i.e., when local elections only take place). We find that while the preelectoral coefficients (January–April) for local elections are positive and significant, those for general elections are negative and never distinguishable from 0 (Figure 7 and Supplemental Information, Table 17). Thus, a higher salience of politics around election period is not likely to be driving our finding that more requests are sent prior to local elections.

It could still be the case that local and not general elections remind people about the existence of the platform and the increase in request sent we observe is the result of the priming effect elections have on platform users, rather than of expected heightened government responsiveness. If this was the case, however, we would see an even larger jump in requests sent in May, when elections take place. In this month, however, the difference in requests sent between districts with and without elections is indistinguishable from 0.

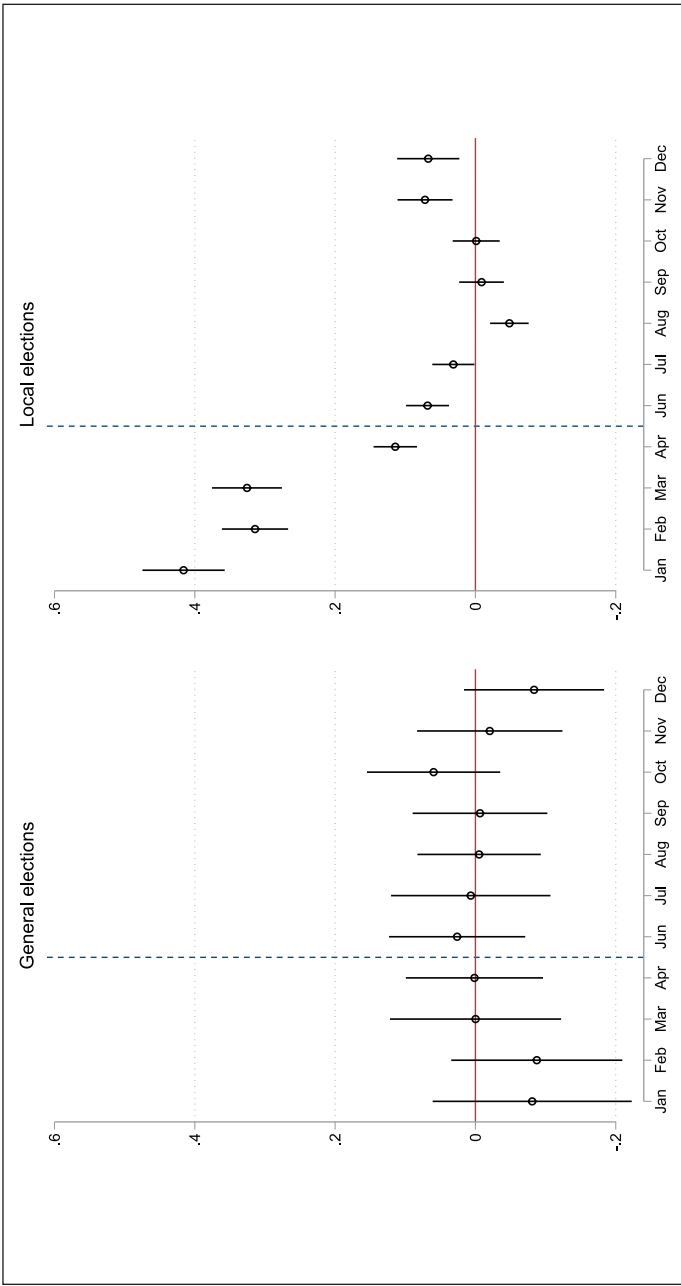


Figure 7. Alternative mechanism: Politicization.

The Figure plots coefficients from a monthly specification of the difference-in-difference estimation in Equation 1, in which $Treat$ (local election period) is replaced by an indicator variable GL that is equal to 1 in district-years with only general elections and 0 in district-years with only local elections (and missing otherwise). In the panel on the left, we plot coefficients when GL is equal to 1 and in the panel on the right when it is equal to 0 (local elections only). The dependent variable is the number of requests sent in district i for 10,000 residents. Robust standard errors are clustered at the district level.

Ineffectiveness Caused by the Electoral Period

One of the assumptions necessary for our identification strategy to hold is the orthogonality of election timing and requests sent. This requires that the decision to hold elections in year t is (a) independent of the number of requests sent, and (b) only influences the number of requests sent through increased expectations. While the first condition is clearly satisfied as election dates are preestablished, the second condition could be undermined if more street-related problems arose in preelection periods. This could be the case if, for example, district councilors were busy with the electoral campaign and thus less effective at fixing street-related issues.

There are two reasons why we do not believe this is the case. If local councilors are too busy with their campaign, we might observe a lower rate of responsiveness and a lower capacity to fix street-related problems in timely manner. Instead, we observe the opposite: in the months from January to April, responsiveness is higher than in other periods of the year (Table 2). In addition, while elected councilors are in charge of representing their constituents and following up on their requests, it is the council officers who take care of technical issues such as fixing a pothole. The effectiveness of these appointed officials is unlikely to be affected by the electoral process. We thus find no good reason to assume that an election-related ineffectiveness of the local council can account for this study's core findings.

Conclusion

This article contributes to a core political science literature on the relationship between electoral incentives on one hand and citizen engagement and politicians' behavior on the other. Specifically, we provide evidence supporting the idea that government responsiveness underscores citizens' decision to engage in nonelectoral political action. Addressing methodological shortfalls pervasive in past work, we identify a plausible source of exogenous variation in government responsiveness and hence citizens' sense of efficacy. Specifically, we take advantage of the unique structure of English local elections, which allows us to compare political engagement within the same political context (local authority districts) and at the same point in time. Our empirical strategy is further designed to isolate motives for political participation that are instrumental from expressive motives. We do so by focusing on the reporting of street-related problems, which unlike political actions such as voting, signing petitions, or attending marches and rallies, have little, if any, expressive underpinning.

We find that local councils are faster, on average, at responding to requests as elections loom large and that constituents increase the rate of reporting street-related problems to district councils in preelection periods. Importantly, we find that constituents send more requests prior to elections, *but only in districts in which preelection responsiveness is sufficiently high*. This is consistent with the idea that constituency services requests are sensitive to shifting expectations regarding the benefits of this form of political engagement. It will be instead a task for future work to establish what are the individual-level psychological dispositions (e.g., internal efficacy) leading citizens to participate more when the government is more likely to be responsive. In sum, our results show that, at least for some forms of political participation, voters are rationally calculating agents: Political action is more likely when perceptions of benefits from one's action are higher due to greater government responsiveness (induced by election proximity).

Authors' Note

Replication files are available at doi:10.7910/DVN/YDAS7N.

Acknowledgments

The authors thank Mark Cridge, Rebecca Rumbul, and Nick Jackson of mySociety for supporting this research project and for granting access to mySociety's data. The authors also thank Clément de Chaisemartin, Florian Foos, and participants at Penn's comparative politics seminar for helpful comments.

Declaration of Conflicting Interests

The authors declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The authors received no financial support for the research, authorship, and/or publication of this article.

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Supplemental Material

Supplemental material for this article is available online at the CPS website <http://journals.sagepub.com/doi/suppl/10.1177/0010414020912290>

Notes

1. Outcomes range from macroeconomic indicators (Canes-Wrone & Park, 2012), to public goods provision (Baskaran et al., 2015), corruption (Klašnja & Titiunik, 2017), and constituency services (Carey et al., 2006).
2. Similarly, Sjoberg et al. (2017) find that political engagement increases with personal experience of greater government responsiveness. Our study differs in that we identify an exogenous source of variation in government responsiveness (using variability in election timing), while in Sjoberg et al. (2017), government responsiveness (measured as response to citizens' request) may be endogenous to other factors that drive greater citizen engagement.
3. Niemi et al. (1991) refer to subjective expectations regarding the responsiveness of government authorities to citizens' demands, as sense of *external* efficacy.
4. The timing of elections is determined independently of a polity's endogenous factors, such as engaged civil society, discussed above.
5. In the context we study, local councilors do not have term limits.
6. Information about local government powers and responsibilities are available at <https://www.gov.uk/>.
7. Two exceptions in our study period are 2009, in which elections were held in June, and 2014, when elections took place on May 22 to allow overlap of local and European Parliamentary elections.
8. Using districts as unit of analysis has the additional advantage of better handling possible spillovers between wards that could confound the analysis.
9. <https://www.fixmystreet.fr/about/council>
10. <https://www.thesun.co.uk/news/5778370/cheshire-west-chester-council-pothole-tweet-backfires/>
11. Local Government Act 1972, s 112.
12. Information on whether a local council fixed a request does not exist for 25% of cases. The structure of the data does not allow adjudicating between cases where a request was not fixed and cases where requests were addressed, but the platform was not updated (by the user or by the councilor). Responsiveness is thus measured when the government fixes requests, such that requests fixed after several months reduce response rate. We consider these scenarios, however, as part of our robustness checks by assigning (a) the median and (b) the maximum number of days to fix a request, to cases with missing values. Results are similar in magnitude and significance in both cases and are available upon request.
13. We winsorize the number of requests to the highest 1% of the distribution (mostly London Boroughs) to ensure that results are not driven by few outlier districts. We divide requests by district population to obtain a normalized measure of requests sent (Supplementary Information, Figure 11). Finally, we do not consider requests for issues which are competence of the parishes, such as problems with drains and ditches. Their inclusion does not change results.
14. Raw data disaggregated by year of observation are shown in the Supplementary Information, Figure 12.

15. Logarithmic transformation accounts for the skewed distribution of the dependent variable (Supplementary Information, Section A and Figure 13). Results are robust to using instead “days to fix” without transformations.
16. For a linear estimation of the interaction $\text{Treat} \times \text{DaysToFix}$, we would need to assume that (a) the effect of the independent variable changes at a constant rate with the moderator and (b) there sufficient observations at each value of responsiveness to allow for conditional marginal estimates which are not highly model-dependent. Diagnostic tests we run reject both assumptions (see the Supplementary Information, Section E.1 and Figure 15).
17. It can be further shown that responsiveness three or more months before has no effect on the number of requests sent at time t . This is a reassuring result, in line with the idea that users—who most likely do not recall governments’ behavior as far as 3 months before—react to responsiveness when this is higher, but only within a window of time they can reasonably remember.

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