SUPPLEMENTARY INFORMATION

— For Online Publication —

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Introduction

This Supplementary Information (SI) appendix contains further details on the study's setting and data sources, discusses in greater detail some methodological choices made in estimation strategies and presents robustness tests on findings reported in the main text. Section A provides additional information on the platform used to report street-related problems and describes in detail the data used in the analysis. We show the geographic and temporal distribution of requests and days to fix them and provide additional information on how our two main dependent variables, requests sent and government responsiveness, were constructed. In Section B we show results from a balance check on the characteristics of districts with and without local elections and include the leads and lags analysis. Sections C and D include additional analyses and robustness tests on our main specification. We present results from the diagnostic tests for multiple interaction effects and robustness tests for the moderating effect of responsiveness on participation in Section E. Finally, we include results from our test on general versus local elections in tabular form in Section F.

A Additional details on data

In this section, we provide more information on the process to log a request into Fix My Street; display a heatmap of the distribution of requests by district; show the raw distribution of our dependent variable before and after winsorization; provide descriptive statistics for all the variables object of the analysis; plot the raw number of requests sent each month in districts with and without elections broken down by years; and show the distribution of our measure of responsiveness, the number of days to fix a request, with and without logarithmic transformation.

Fix My Street platform

We use data from Fix My Street, a U.K. reporting platform developed by mySociety. Residents are able to report a problem or send a request through a user-friendly online portal or a mobile app. Figure 8 shows the portal's homepage, which allows the user to input a zip code or location of the area affected by the issue she wishes to report. The following webpage, displayed in Figure 9, allows users to describe the issue and geo-locate it on a map. Users can also decide to leave a picture of the issue and a name. Based on a sample of requests examined by Solymosi et al. (2017), only 15% of users choose to provide their name. The platform automatically shares messages and requests with the relevant local authority district.

Figure 8: Fix My Street Homepage

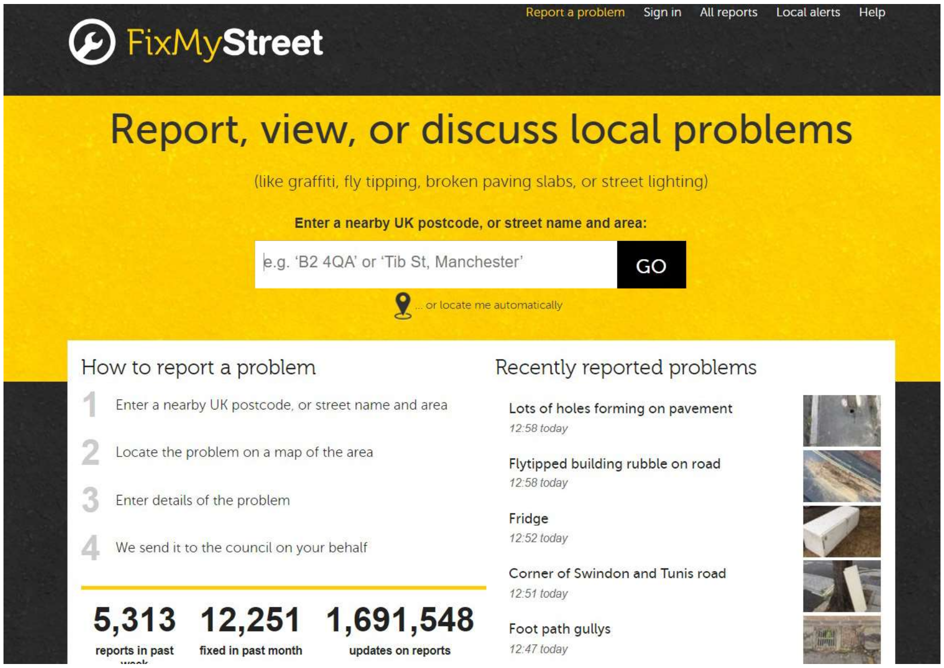
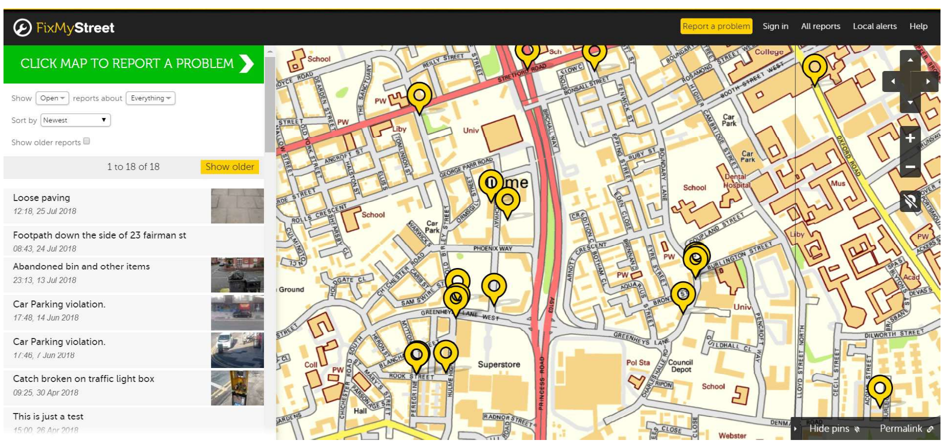


Figure 9: Fix My Street sample page for problem reporting



Distribution of requests by local district

In Figure 10, we plot the number of requests sent and the log days to fix them in the 322 authority districts in the study area, from 2007 when FixMyStreet was launched, to December 2015 — the last month in our dataset. In this Figure, we show non-winsorized data to represent the effective distribution of requests in England. The map, however, looks similar when plotting winsorized data. The number of requests sent in each district ranges from a minimum of 146 (Oadby and

Wigston districts) to a maximum of 45,468 (Bromley district, just outside London) with a median of about 1,000 requests per district. The fastest district takes, on average, 37 days to get back to citizens while the slowest 860.

(a) Participation (number of requests sent) (b) Responsiveness (number of days to fix requests)

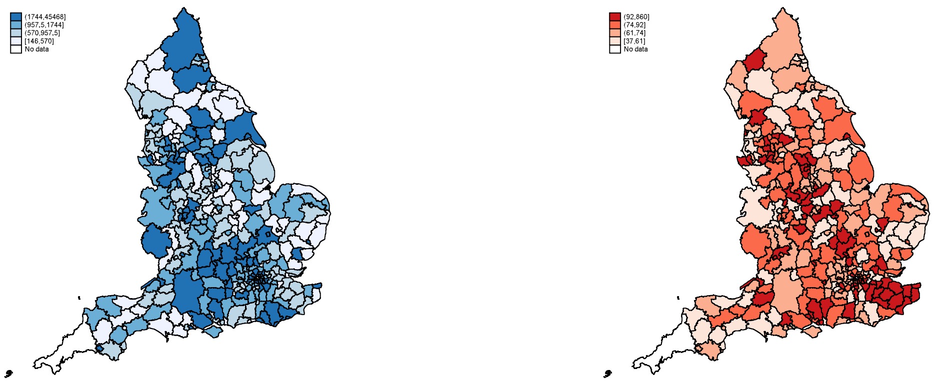
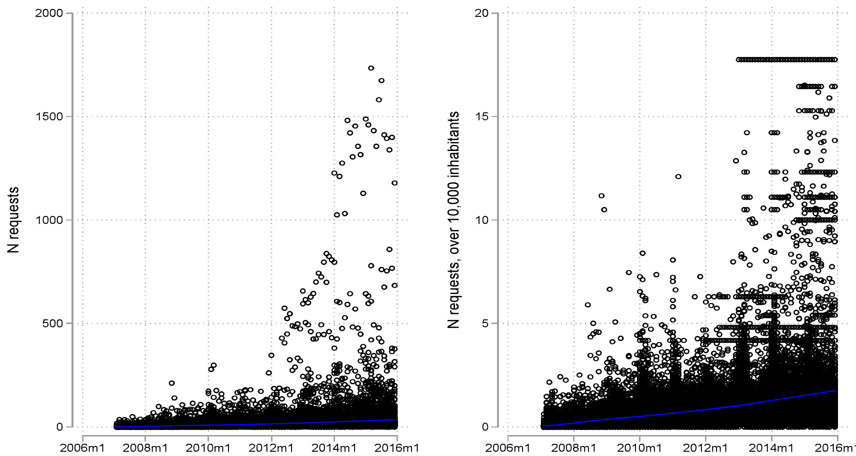


Figure 10: Heatmap of requests sent to Fix My Street and days to fix them by district

Requests sent

In Figure 11, we plot the number of requests sent before (left panel) and after winsorization (right panel) and transformation into number of requests per 10,000 residents. The number of requests sent each month ranges between 0 and 105 in 99% of the districts. However, the top 1 percentile contains outlier districts which in one month send between 100 and 466 requests, making our dependent variable subject to extreme leverage from these few observations. To ensure our findings are not being driven by outliers, we winsorize the dependent variable, assigning to the top 1 percentile the value of the last district in the 99th percentile. Results are unchanged if we drop, instead, the outlier districts. Additionally, we divide the number of requests sent by the population of the district in order to obtain the winsorized number of requests sent per 10,000 residents, as shown in the right panel of Figure 11.

Figure 11: Requests sent normalized by 10,000 residents



Note: left panel shows a scatterplot of the total number of requests sent to FixMyStreet by month. The right panel shows a similar scatterplot after winsorizing the top 1% of the distribution and dividing by the district population. Lowess smoothing lines are in blue.

Descriptive statistics

In Table 4, we provide summary statistics for all the variables used in our analysis.

Number of requests by year in treat and control group

In Figure 12 we plot our dependent variable, the winsorized number of requests sent per 10,000 residents, in each year and across treated (electoral year) and control (non-electoral year) districts. Even without controlling for district and time-level driver of requests, we observe that the number of requests sent before May (bars colored in dark green) is generally higher in districts in which elections took place during that year compared to districts without elections. This effect starts appearing in 2009 and it becomes systematic from 2011, a pattern compatible with users learning how to use the platform strategically since its launch in early 2007.

Table 4: Descriptive Statistics

	Mean	St.Dev.	Min	Max
DV				
Requests	12.660	40.935	0.000	1481.000
Requests, winsorized	11.158	18.259	0.000	149.000
Requests, pop share	0.694	1.112	0.000	17.747
IV				
Treatment	0.199	0.399	0.000	1.000
Election year	0.618	0.486	0.000	1.000
Responsiveness (Days to fix)				
Log days to fix	3.957	0.986	0.000	8.090
N days to fix	101	215.402	0.000	3261
N days to fix (larger than med)	3.823	0.639	1.099	8.066
Sent and solved same month	2.082	0.979	0.000	3.401
Share fast-fix	0.718	0.266	0.000	1.000
District-level controls				
Population (in 1000)	162.328	109.134	34.675	1073.045
Population Density	35.735	26.790	4.353	165.515
Education, median	1.999	1.152	1.000	5.000
Socialgrade, median	2.056	0.327	1.000	3.000
College, share	0.269	0.076	0.141	0.537
High income share	0.307	0.027	0.236	0.380
ELF	0.157	0.152	0.022	0.683
Whites, share	0.896	0.126	0.293	0.989
Divorced	11785	7163	2720	62168
ELF Religion	0.450	0.071	0.244	0.700
Female, share	0.509	0.006	0.480	0.526
Age	40.368	2.902	30.989	47.786
Observations	26,690			

Number of days to fix a request, logarithmic transformation

We display the distribution of our measure of responsiveness, the number of days to fix a request, before and after its logarithmic transformation (Figure 13). In the left panel, the distribution of this variable is highly skewed: the median number of days to respond to a request is 42, the standard deviation 215, and the mean 101 days, making the rest of the distribution extremely dispersed towards higher values. To meet the assumption of normality of residuals required for an OLS regression, we use a logarithmic transformation (left panel).

Figure 12: Dependent variable by treatment and year

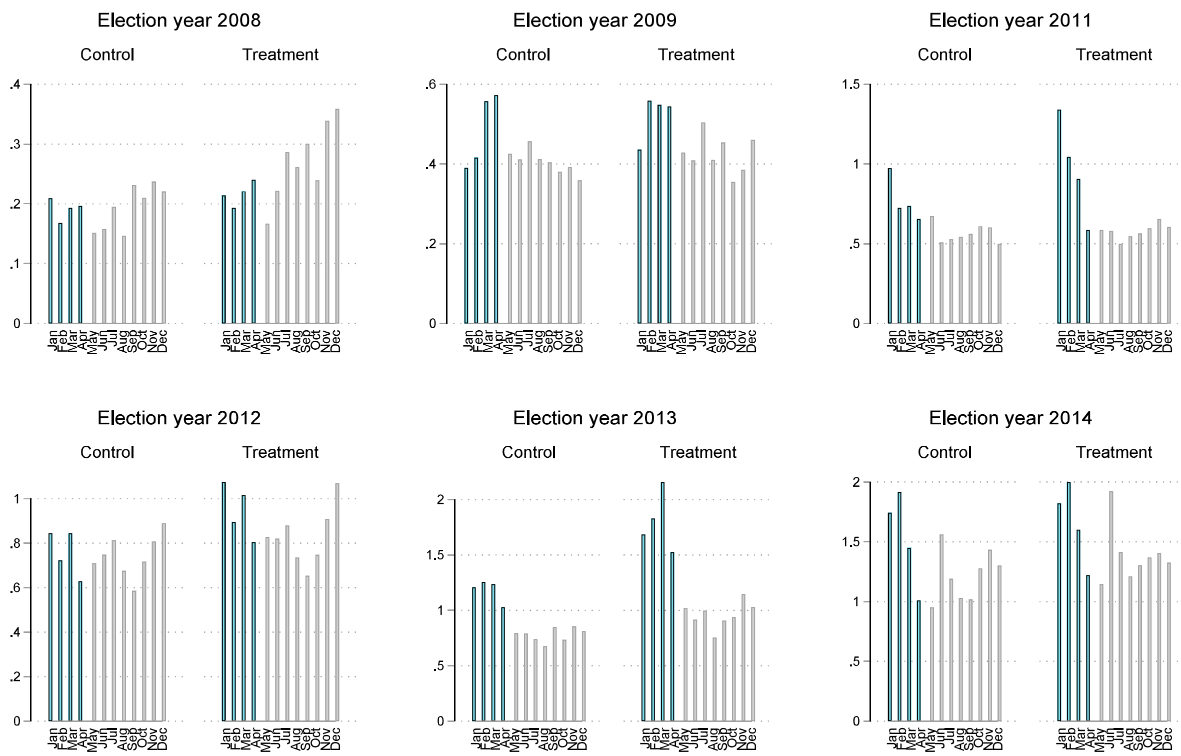
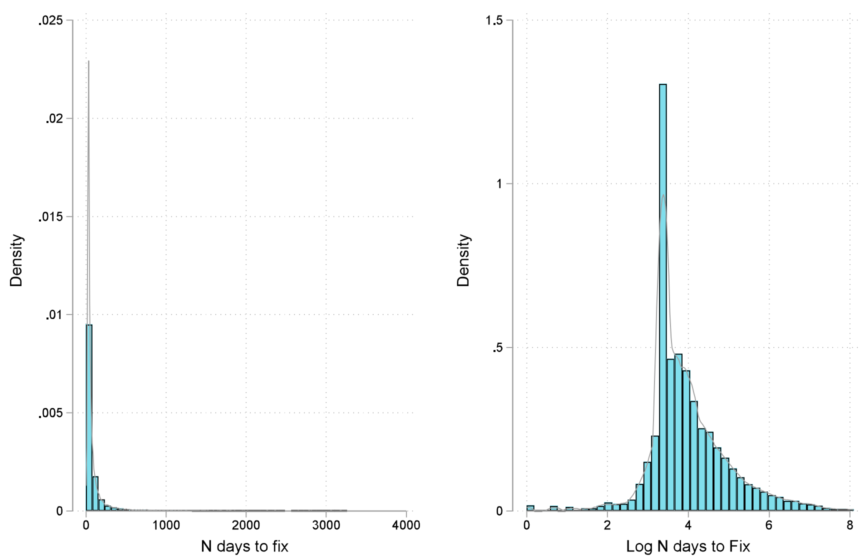


Figure 13: Number of days to fix a request, logarithmic transformation



B Identification Assumptions

B.1 Leads and lag test

We run a more formal test to check the validity of the parallel trend assumption on the model of Autor (2003). We interact the pre-treatment and treatment periods ($t-2$, $t-1$ and t, defined as in the graphic) with the treatment variable, which takes value 1 in election years. The regression has the form:

$$\begin{aligned} Y_{it} = \mu_i + \nu_t + \zeta Lead_t + \eta Lag_t + \theta Treat_{it} + \\ \kappa Treat \times Lag_{it} + \lambda Treat \times Lead_{it} + u_{it} \end{aligned} \tag{3}$$

where Y are our two usual dependent variables, *Lead* denotes the months in which some units are in the pre-electoral period (Jan-Apr), *Lag* the non-treated months (May-Aug) and we leave out as omitted period the months Sept-Dec. Our coefficient of interest is λ, capturing the effect of the months far from treatment on the outcomes of interest. We report the result of this test in SI, Table 5. Consistently with the parallel trend assumption, the interaction between treatment status and the months far from elections (the *lag*) is insignificant when considering both requests sent (column 1) and government responsiveness (column 2) as outcome, suggesting that outcome trends between treatment and control group are not significantly different.

B.2 Balance tests

We provide additional support for the validity of the study's identification strategy. In addition to the parallel trend assumption discussed in Section 5 above, we turn to test that there is no significant difference in the main characteristics of districts across treated and control groups. Recall, our empirical analysis entails a comparison between local authority districts that hold and that do not hold elections in a given year. This means that districts "switch" treatment status in different years, conditional on the occurrence of local elections, minimizing concerns of systematic differences between treated and control groups. We report here, however, a pooled balance table (Table 6) and Kernel densities (Figure 14) of district-level variables that likely are associated with

Table 5: Leads and lags tests

	(1) Participation (Requests Sent)	(2) Government Responsiveness (Log days to fix request)
Treat	-0.035	-0.052*
	(0.032)	(0.027)
Lead	0.122***	0.073***
	(0.025)	(0.026)
Treat×Lead	0.132***	-0.017
	(0.031)	(0.035)
Lag	-0.031	0.048*
	(0.022)	(0.026)
Treat×Lag	0.012	-0.047
	(0.025)	(0.036)
Observations	26,690	19,863
Number of districts	322	322
Districts FE	Yes	Yes
Year FE	Yes	Yes

Note: the dependent variable is the number or requests sent in district i normalized by the district population in column 1; in column 2, the DV is the log mean number of days to fix a request in a given district. $Treat$ is a binary indicator equal to 1 in electoral years, *Lead* a dummy equal to 1 in Jan-April and *Lag* a dummy equal to 1 in May-August. The months Sept-Dec are the omitted period. District and year fixed effects and robust standard errors clustered at the district level are included. *** p<0.01, ** p<0.05, * p<0.1.

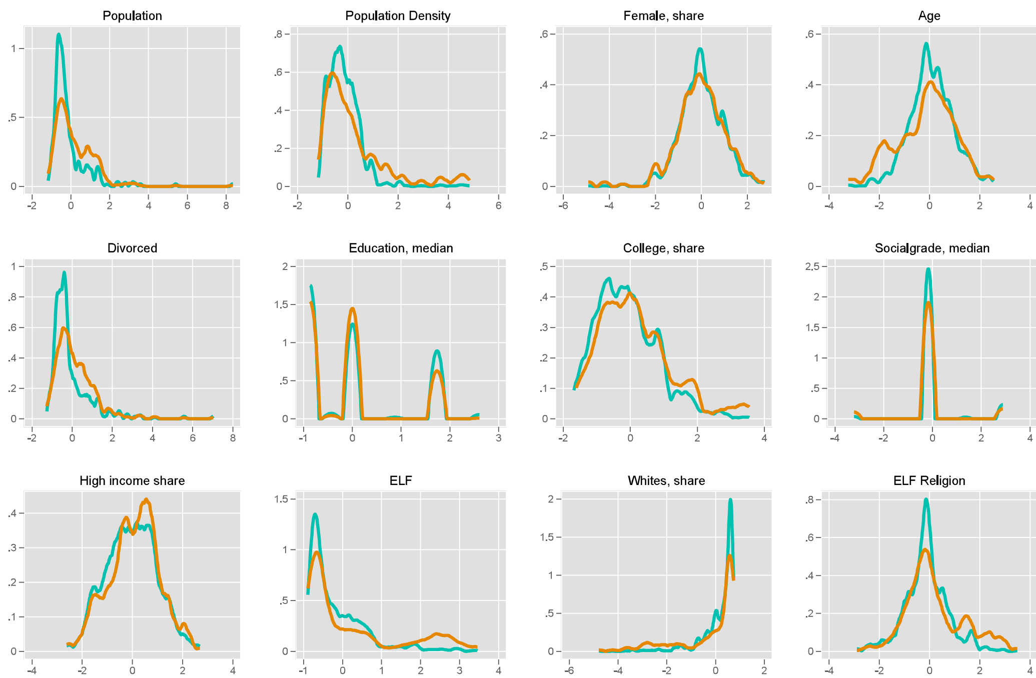
the number of requests sent.[18]

Table 6: Balance check, districts with (treat) and without elections (control)

	Mean Control	Std. D.	Mean Treat	Std. Dev	Std Diff
Population	181599.781	(108162.148)	150396.172	(108025.555)	-0.204
Population Density	42.916	(35.664)	31.290	(17.971)	-0.291
Education, median	1.925	(1.060)	2.044	(1.203)	0.074
Socialgrade, median	2.024	(0.354)	2.076	(0.308)	0.110
College, share	0.281	(0.083)	0.261	(0.069)	-0.180
High income share	0.309	(0.027)	0.306	(0.027)	-0.076
ELF	0.196	(0.192)	0.132	(0.114)	-0.287
Whites, share	0.862	(0.163)	0.917	(0.090)	0.296
Divorced	12,801	(7,011)	11,156	(7,185)	-0.164
ELF Religion	0.464	(0.083)	0.442	(0.061)	-0.218
Female, share	0.509	(0.007)	0.509	(0.006)	0.035
Age	39.989	(3.359)	40.603	(2.552)	0.145

[18] We do not include a balance table by patterns of elections as this operation requires comparing groups which are too small to return a meaningful comparison in averages across groups (See Table 1 for the numerosity of each group).

Figure 14: Kernel density of the covariates, districts with and without elections



Note: All variables are standardized.

C Government responsiveness: robustness tests

We conduct robustness tests on the regression specification estimating the effect of elections on our proxy for government responsiveness— the mean number of days taken to fix requests in that month, in logarithmic terms. In Table 7 we repeat our analysis using different definitions of government responsiveness. First, we consider the mean number of days to fix a request without logarithmic transformation (column 1). Second, we consider the median, rather than the mean, number of days to fix a request (column 2). In column 2, the coefficient has the correct sign and magnitude but falls slightly below significance level, reflecting the extreme skew of this variable towards lower values (see Section A). Third, we consider the population share of requests fixed within 30 days – the median number of days to address a request is 42 (column 3). Consistently with our expectations, this last category is the only one displaying positive and significant coefficients - in the other specifications, the sign should be and is negative. We also test our findings using two different control variables. We control for the number of requests sent in each district year (column 4), and replace our set of dummies indicating the number of requests sent per each category of requests, with a variable indicating the share of requests which can be fixed fast by the local government[19] (column 5).

In Table 8 we take into account the possibility that the intensity of the treatment (i.e. how many councilors are up for re-election) determines our findings. First, we control for election pattern, which determines whether only few, some or many of the councilors are in their electoral campaign (Column 1). Second, we subset the analyses by whether elections take place every year (few councilors up for re-election), every 2 years (some councilors) or every 3 or 4 years (many councilors). While the drop in sample size makes estimates insignificant, the coefficient shows a larger effect the larger the share of councilors up for re-election. Finally, in Column 5, we weight results by the frequency of elections to interpret the coefficient as the effect for the average number of councilors up for elections. Results are extremely similar to the main analysis.

We test against two potentially alternative explanations. First, it is possible that the government is not fixing requests more before elections but that, rather, councilors are more active in reporting requests as fixed in this period. To account for this concern, we drop all requests reported as fixed

[19] Requests are coded as "easy to fix" for issues that can be addressed in a timely manner by the local council, such as litter, broken street lights, mud on the road and other issues requiring cleaning. We define as "slow-fix" those requests which demand significant labor and capital for being addressed, such as fixing potholes.

Table 7: Treatment effect on government responsiveness, robustness tests

	(1) Days to fix (no transformation)	(2) Median days to fix	(3) Fixed in 30 pop share	(4) Control for req sent	(5) Control for fast share
Treatment	-10.942*	-11.364	0.012**	-0.057**	-0.057**
	(6.604)	(7.508)	(0.006)	(0.026)	(0.026)
Observations	19,900	19,900	26,690	19,863	19,863
Number of districts	322	322	322	322	322
District FE	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes

Note: Robustness tests: government responsiveness. In Columns 1-3, we adopt different definitions of our dependent variable, a measure of responsiveness (in the original specification , the DV is the log of the average number of days the government takes to fix a request in district i and month t). In columns 4-5 we consider different control variables. $Treat$ is binary equal to 1 in pre-electoral months (January-April) in districts in which there is an election taking place during year t. Robust standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.

by the council (22% of requests reported as fixed, N=52,761 requests) and re-run our main analysis on this restricted sample, finding extremely similar results: the coefficient for treatment is almost identical in size and significance (SI, Table 9).

We also test whether the increase in responsiveness in districts with elections can be related to a change in the type of requests sent in districts with and without elections. If districts with elections systematically receive more requests for issues which can be fixed faster, then the increase in responsiveness we observe is automatic, and not the product of higher sensibility of the local government to citizens' requests. We already show that the type of requests sent does not differ across treated and control districts in Figure 2. Here, we categorize requests based on whether they can be addressed in a timely fashion by the local administration, such as litter in the streets, fly tipping, broken glasses, mud on the road and so on. We confirm these results parametrically by considering treatment effects (pre-electoral period in districts with elections) on the share of requests which can be fixed fast (SI, Table 10). The effect is negative and insignificant, suggesting that the number of requests of the type which can be fixed faster is not different in treated districts. Additionally, Figure 2 shows that the gap in responsiveness across treated and control districts is stable across categories of requests - except for the category unclassified.

Table 8: Treatment effect on government responsiveness, intensity of treatment

	(1) Control Elec Pattern	(2) Elec every year	(3) Elec every 2 years	(4) Elec every 3 or 4 years	(5) Weight elec frequency
Treatment	-0.065**	0.642	-0.033	-0.103	-0.064**
	(0.026)	(0.422)	(0.031)	(0.067)	(0.028)
Observations	19,792	4,001	11,253	4,538	40,121
Number of districts	321	67	189	65	321
District FE	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes

Note: Robustness tests: government responsiveness. The dependent variable is log mean number of days to fix requests. $Treat$ is a dummy equal to 1 (0 otherwise) in the pre-electoral months in districts in which there is an election taking place during year t. In Columns 1, we control for the pattern of elections (corresponding to how many councilors run for re-election). In columns 2-4 we subset the analyses for how many councilors run depending on whether elections take place every year (Col 2), every 2 years (Col 3) or every 3 or 4 years (Col 4). In Col 5, analyses are weighted by the frequency of elections. Robust standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Treatment effect on government responsiveness, dropping requests marked as fixed by the council

	(1) Linear	(2) Mixed	(3) Poisson
Treatment	-0.056**	-0.056**	-0.014**
	(0.026)	(0.025)	(0.006)
Observations	19,863	19,863	19,863
District FE	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes
Number of districts	322	322	322

Note: The table shows robustness tests to our analysis on government responsiveness, dropping all requests marked as fixed by the local council. Our dependent variable is a measure of government responsiveness, the log of the average number of days a council takes to fix requests in district i and month t). $Treat$ is a dummy equal to 1 in pre-electoral months (January-April) in districts in which there is an election taking place during year t. Robust standard errors clustered at the district level are in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 10: Treatment effect on share of fast and slow solving requests

	(1) Share Fast-Fix
Treat	-0.001
	(0.006)
Observations	23,889
Number of districts	322
District FE	Yes
Month-year FE	Yes

Note: The table shows treatment effects on the share of requests which can be fixed fast. The dependent variable is the number of requests which can be addressed immediately by the local government over the total number of requests sent. *Treat* is a dummy equal to 1 in pre-electoral months (Jan-Apr) and in districts in which there is an election taking place during year t. Robust standard errors clustered at the district level are in brackets. *** p<0.01, ** p<0.05, * p<0.1.

D Political Engagement: regression tables and robustness tests

We provide additional details on the analysis on the effect of elections on requests sent, including robustness tests for our main analysis.

D.1 Results in Tabular form

Table 11: Treatment effects on political participation, by month

	(1) Linear	(2) Mixed	(3) Poisson
Treat×Jan	0.095***	0.096***	0.061
	(0.035)	(0.035)	(0.056)
Treat×Feb	0.110***	0.110***	0.078
	(0.033)	(0.033)	(0.056)
Treat×Mar	0.128***	0.128***	0.097*
	(0.033)	(0.033)	(0.056)
Treat×Apr	0.060**	0.060**	0.064
	(0.025)	(0.025)	(0.061)
Treat×Jun	0.025	0.025	0.016
	(0.027)	(0.027)	(0.060)
Treat×Jul	0.028	0.028	0.027
	(0.026)	(0.026)	(0.061)
Treat×Aug	-0.017	-0.017	-0.021
	(0.023)	(0.023)	(0.064)
Treat×Sep	0.003	0.003	0.006
	(0.027)	(0.027)	(0.065)
Treat×Oct	-0.030	-0.030	-0.050
	(0.035)	(0.035)	(0.070)
Treat×Nov	-0.005	-0.005	-0.025
	(0.041)	(0.041)	(0.070)
Treat×Dec	0.036	0.036	0.032
	(0.044)	(0.044)	(0.073)
Observations	26,690	26,690	26,690
District Effects	Fixed	Random	Fixed
District Controls	No	No	No
Month-year FE	Yes	Yes	Yes
Number of districts	322	322	322

Note: The table shows the results from a monthly version of Equation 1. The dependent variable is the number of requests sent in district i over the district population, in 10,000. $Treat$ is an indicator variable that equals 1 in districts in which there an election took place during year t. The reference category is May, the month in which elections are held. Robust standard errors are clustered at the district level.*** p<0.01, ** p<0.05, * p<0.1.

D.2 Political Engagement: robustness

Below, we report the results of a series of tests to assess the robustness of the results reported in the main text. First, we test the robustness of our results to the removal of winsorization. The coefficient becomes trice as large, but it is also more imprecisely estimated as a result of the larger variation in the dependent variable (column 1, Table 12). Importantly, results are robust to changing the level of clustering to the electoral-pattern rather than the district (column 2). Since some of the districts have elections every year and do not experience a post-electoral period that is comparable to the other districts, we drop these districts and show that results are robust to their exclusion (column 3). In column 4, we test a model in which we drop districts fixed effects and replace them with district-level controls.[20] Our core findings are substantially unchanged. Finally, our results are also robust to substituting year fixed effects to monthly-year fixed effects (column 5).

Table 13 repeats our analyses on the effect of the intensity of treatment (i.e. how many councilors run for re-election, depending on election pattern of the district). As for government responsiveness, we see an increasing effect of the intensity of treatment on participation.

In Table 14 we report the main analysis separately for every year, again, excluding years in which the UK held general elections (2010, 2015). This test has lower statistical power than the pooled analysis because we use fewer observations and because of greater sensitivity to non-systematic year-specific factors that might affect potential users' decision to report problems. These factors are absorbed by the month-year fixed effects in the pooled specification. Examining the effect of local elections on reporting intensity annually, however, allows us to observe the evolution of users' behavior as the usage of the platform spreads. We find that the significance of pre-electoral months coefficients increases with time: two years after the launch of FixMyStreet, we start observing an increase in requests in the period before elections. From three years after onwards, treatment effect is positive and significant in all years, except from year 2014, in which results are insignificant, probably due to the lower number of local elections taking place during this year. This evidence is consistent with progressive learning of the potentials of the platform by users.

[20]Control variables include median education, median social grade, mean age, proportion of females, proportion of married population and an index for ethnic fractionalization.

Table 12: Effect of the month on the number of requests sent, robustness tests

	(1) Remove winsorization	(2) Cluster election-pattern	(3) Remove districts w.election all years	(4) District controls	(5) Year fixed effects
Treat	0.188*	0.085**	0.050*	0.082***	0.241***
	(0.105)	(0.040)	(0.027)	(0.027)	(0.018)
Observations	26,690	26,607	21,139	26,690	26,690
Number of districts	322	322	255	322	322
District FE	Yes	Yes	Yes	No	Yes
District Controls	No	No	No	Yes	No
Month-year FE	Yes	Yes	Yes	Yes	No
Year FE	No	No	No	No	Yes

Note: The table shows the results from robustness tests on the main analysis. The dependent variable is the number of requests sent in district i over the district population, per 10,000 residents in all columns. $Treat$ is a dummy equal to 1 (0 otherwise) in the pre-electoral months in districts in which there is an election taking place during year t. Robust standard errors clustered at the district level are in brackets in all specifications except column 2, in which standard errors are clustered at the level of the electoral pattern. *** p<0.01, ** p<0.05, * p<0.1.

Table 13: Effect of the month on the number of requests sent, intensity of treatment

	(1) Elec every year	(2) Elec every 2 years	(3) Elec every 3 or 4 years	(4) Weight elect frequency
Treatment	-1.650***	0.039	0.083	0.076***
	(0.314)	(0.028)	(0.066)	(0.026)
Observations	5,551	15,665	5,391	53,054
Number of districts	67	189	65	321
District FE	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes

Note: Robustness tests: political engagement. The dependent variable is the number of requests sent in district i over the district population, per 10,000 residents. $Treat$ is a dummy equal to 1 (0 otherwise) in the pre-electoral months in districts in which there is an election taking place during year t. In columns 1-3 we subset the analyses for how many councilors run depending on whether elections take place every year (Col 1), every 2 years (Col 2) or every 3 or 4 years (Col 3). In Col 4, analyses are weighted by the frequency of elections. Robust standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.

Table 14: Treatment effect on requests sent, year-specific analysis

	(1) 2007	(2) 2008	(3) 2009	(4) 2011	(5) 2012	(6) 2013	(7) 2014
Treatment	-0.000	-0.053	0.017	0.183***	0.101**	0.436***	-0.036
	(0.033)	(0.044)	(0.031)	(0.049)	(0.051)	(0.074)	(0.109)
Observations	3,532	3,850	3,861	3,861	3,861	3,862	3,863
N districts	322	322	322	322	322	322	322
N districts with elections	282	137	207	278	128	207	160
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table shows the results from the same analysis as in Table 3, but repeated for every year of observation, excluding those with general elections (2010 and 2015).

D.3 Heterogeneity by competitive elections

In Table 15, we run heterogeneity analyses by the level of elections competitiveness, defined as the share of contested wards in odd columns and as the number of contested seats as a share of total vacancies in even columns.[21] Following the literature showing that competitive elections result in enhanced government responsiveness (Besley and Burgess, 2002), we test whether councils fix requests faster in districts with contested seats (SI, Table 15, columns 3 and 4). While the number of days to fix a request is significantly lower in districts with competitive elections (coefficient for $ContestedWards$ and $Seats$), there is no additional differential effect in government responsiveness caused by the proximity to the electoral period. In line with this finding, we observe also no differential effect in requests sent in districts with more contested wards (column 1) and there is a weak positive effect on requests sent in districts with more contested seats as a percentage of all vacancies (column 2).

D.4 Heterogeneity by socioeconomic characteristics

We consider whether election proximity increases requests sent and government responsiveness differently depending on the type of population inhabiting the district. We consider heterogeneous effects interacting Census characteristics of the districts such as social grade, education and ethnicity, with our treatment variable for the pre-electoral period. Results are presented in Table 16. In line with the literature on the determinants of political participation (Sondheimer and Green, 2010; Alesina and La Ferrara, 2000), we find that richer (column 1), higher educated (column 2) and whiter districts (column 4) send more requests before elections while more ethnically diverse districts (column 3) send less requests before elections. We find, instead, no effects of socioeconomic characteristics on government responsiveness. Local governments do not seem to fix requests faster before elections in districts in which a larger share of the population is rich or ethnically homogeneous. In districts with larger shares of college educated people the government is actually slower at fixing requests, but then it becomes faster in the pre-electoral period (column 6).

[21] More traditional measures of competitiveness, such as party vote share margin, do not capture competitiveness in the context of English local elections, which are regulated by a first-past-the-post electoral system determining winners at the seat level.

Table 15: Effect of elections on requests sent and government responsiveness, heterogeneity by competitive elections

	(1) Requests sent	(2) Requests sent	(3) Log days to fix requests	(4) Log days to fix requests
Treat	0.084***	0.017	-0.052	-0.071*
	(0.032)	(0.044)	(0.034)	(0.041)
Contested Wards	0.000		-0.004***	
	(0.002)		(0.001)	
Treat×Contested Wards	-0.000		0.002	
	(0.002)		(0.002)	
Contested Seats		-0.000		-0.001***
		(0.000)		(0.000)
Treat×Contested Seats		0.001*		0.001
		(0.001)		(0.000)
Observations	26,400	26,400	19,863	19,863
Number of districts	322	322	322	322
District FE	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes

Note: The table shows heterogeneities on the main analysis by contested elections. The dependent variable is the number of requests sent in district i over the district population, per 10,000 residents in columns 1 and 2; in column 3 and 4 the DV is the log of the mean number of days to fix a request in a given month. $Treat$ is a dummy equal to 1 (0 otherwise) in the pre-electoral months in districts in which there is an election taking place during year t. In column 1 and 3, competitiveness is defined as the number of contested wards, in column 2 and 4 as the number of contested seats as a share of all vacancies. Robust standard errors clustered at the district level are in brackets in all specifications. *** p<0.01, ** p<0.05, * p<0.1.

Table 16: Effect of elections on requests sent and government responsiveness, heterogeneity by socioeconomic characteristics

	(1) Requests sent	(2) Requests sent	(3) Requests sent	(4) Requests sent	(5) Log days to fix requests	(6) Log days to fix requests	(7) Log days to fix requests	(8) Log days to fix requests
Treat	0.080***	0.088***	0.076***	0.075***	-0.065**	-0.065**	-0.065**	-0.066**
	(0.027)	(0.027)	(0.027)	(0.027)	(0.026)	(0.026)	(0.026)	(0.026)
High Social Grade	0.225***				0.214***			
	(0.004)				(0.007)			
Treat×High Social Grade	0.041***				-0.009			
	(0.016)				(0.018)			
Share College Educ		0.094***				0.098***		
		(0.002)				(0.003)		
Treat×Share College Educ		0.097***				-0.035**		
		(0.021)				(0.017)		
ELF			0.200***				0.180***	
			(0.003)				(0.006)	
Treat×ELF			-0.052**				-0.026	
			(0.026)				(0.027)	
White Proportion				-0.246***				-0.222***
				(0.003)				(0.007)
Treat×White Proportion				0.066**				0.030
				(0.026)				(0.029)
Observations	26,690	26,690	26,690	26,690	19,863	19,863	19,863	19,863
Number of districts	322	322	322	322	322	322	322	322
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table shows heterogeneities on the main analysis by socioeconomic characteristics of the district. The dependent variable is the number of requests sent in district i over the district population, per 10,000 residents in columns 1 to 4; in column 5 to 8 the DV is the log of the mean number of days to fix a request in a given month. *Treat* is a dummy equal to 1 (0 otherwise) in the pre-electoral months in districts in which there is an election taking place during year t. Robust standard errors clustered at the district level are in brackets in all specifications. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

E Moderating effect of government responsiveness: diagnostics and robustness

In this Section, we include diagnostic and robustness tests for our specification considering the moderating effect of government responsiveness on requests sent during election period.

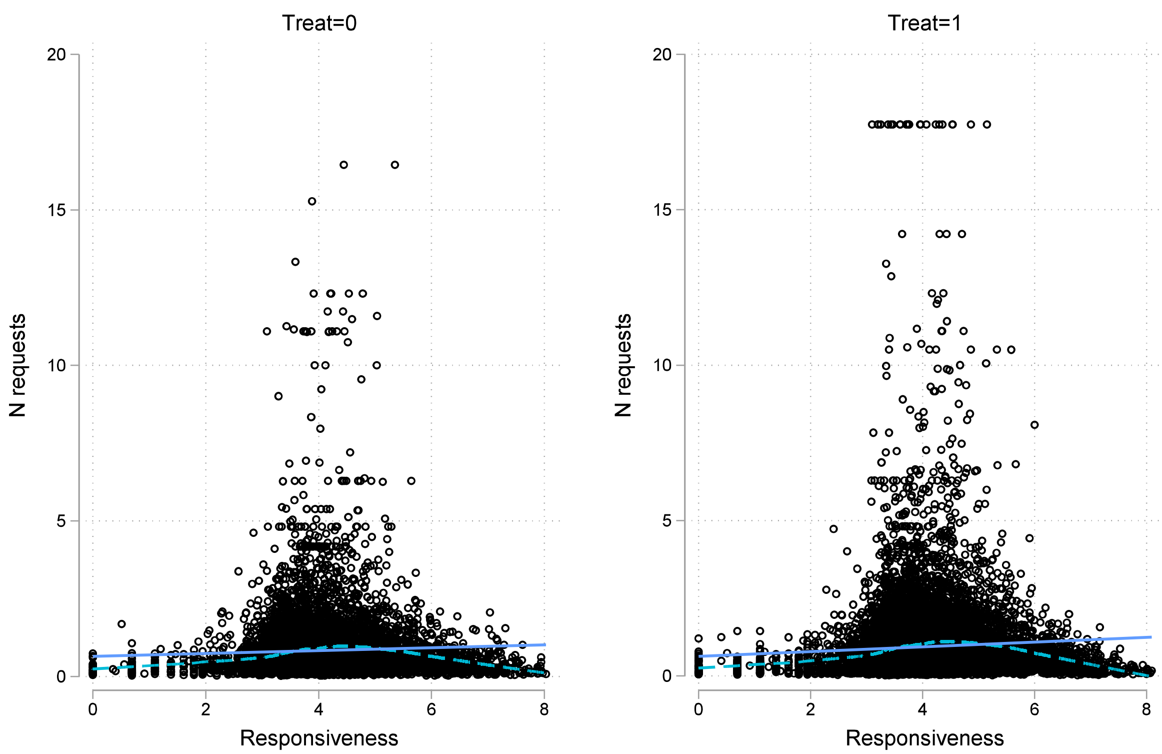
E.1 Multiple interaction: Diagnostic tests

Building on Hainmueller et al. (2019), we run diagnostic tests to check whether the assumptions for linear estimation of multiple interaction effects are met in this context. First, we check whether we can assume linear interaction effects by plotting the scatterplot of the outcome variable (Requests sent, normalized by population) against the moderator (Responsiveness, logarithm) by values of treatment (election period). The relationship between the outcome variable and the moderator is not well approximated by a linear function, as shown by the misalignment between the regression solid line and the lowess dashed line (SI, Figure 15). Second, we test whether the assumption of common support is met; i.e., that we have enough observations for each value of responsiveness across treatment status and that these observations have enough variance, such that when we compute the effect of treatment at specific values of responsiveness, marginal effect estimates are not based on extrapolation or interpolation of the functional form. We find that when $Treat = 1$, there are much fewer observations for values of responsiveness 1-3 and 6-8 then for $Treat = 0$ (SI, Figure 15). In sum, the assumptions required to estimate multiple interaction effects linearly are not met. We therefore relax these assumptions and rely on the estimation strategy proposed by Hainmueller et al. (2019), estimating the conditional marginal effect of the treatment on requests sent by binned (categorical) values of responsiveness.

E.2 Moderating effect of responsiveness: robustness tests

In Figure 16, we run our usual set of robustness tests – adopting different definitions of responsiveness and different control variables – on the specification testing the effect of elections on political engagement for different levels of government responsiveness. Notice that controlling for the share of requests fixed fast (Panel c) returns a smaller coefficient for very high values of responsiveness

Figure 15: Diagnostic test: Responsiveness and requests sent by treatment status



Note: The Figure displays a scatterplot of the relationship between requests sent and responsiveness by treatment status (pre-election period, in districts with elections). Requests sent are normalized by 10,000 residents while responsiveness is the number of days to fix a request in its logarithmic transformation.

than for medium levels. Small variations in the size of coefficients are of little concern, if we consider that (i) in these specifications, we estimate the effect of binned - and therefore less numerous - levels of responsiveness and (ii) in this specific test, we only consider requests which can be classified or not as fixed fast, thus reducing our sample size by 10%.

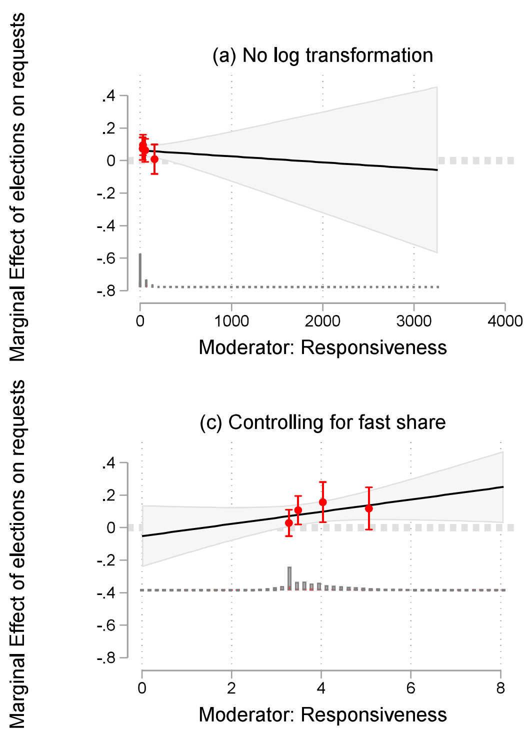
In Figure 17, Panel (e) we adopt a restrictive definition of responsiveness including only requests sent and fixed within the same calendar month. This restriction forces us to consider as not fixed about 72% of the requests sent, but we still observe the pattern of more participation in districts with more requests even if restricting to this subsample. In Panel (f), instead, we control for the pattern of election taking place in a particular district, which is a way to control for the intensity of treatment (i.e. how many councilors are up for re-election). Results are identical to those in the main analyses.

We further explore the consequences of the intensity of treatment in Figure 18. Here, we subset the analysis by cities with elections (i) every year (few councilors up for re-election), (ii) every 2 years (some councilors) or (iii) every 3 or 4 years (most councilors). When few councilors run for re-election, the effect of responsiveness on participation is null and there is no role for the moderator. Instead, consistently with expectations, the largest the share of councilors that are up for re-election, the more pronounced the effect of high levels of responsiveness (first bin) on participation.

F Alternative mechanisms

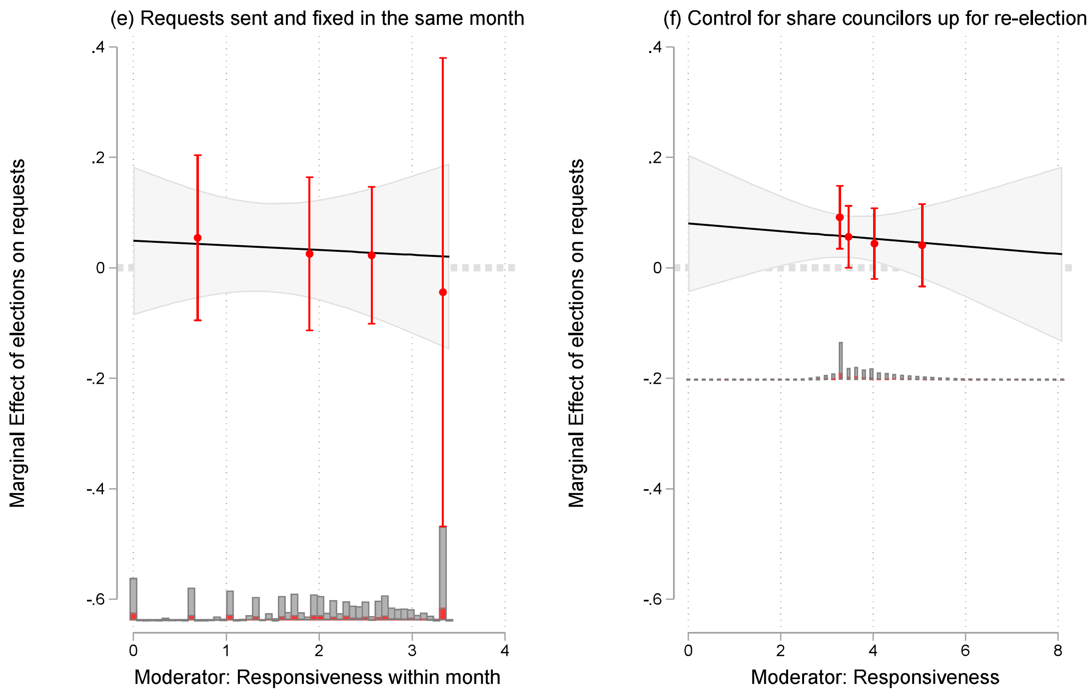
In this final section, we include the regression table from our test on general versus local elections. In Table 17, we show that only local elections cause an increase in the number of requests sent in the months leading to the election day. This supports the idea that it is local government responsiveness, and not generally higher salience of politics, which drives our findings.

Figure 16: Effect of treatment on requests sent by levels of responsiveness, robustness tests



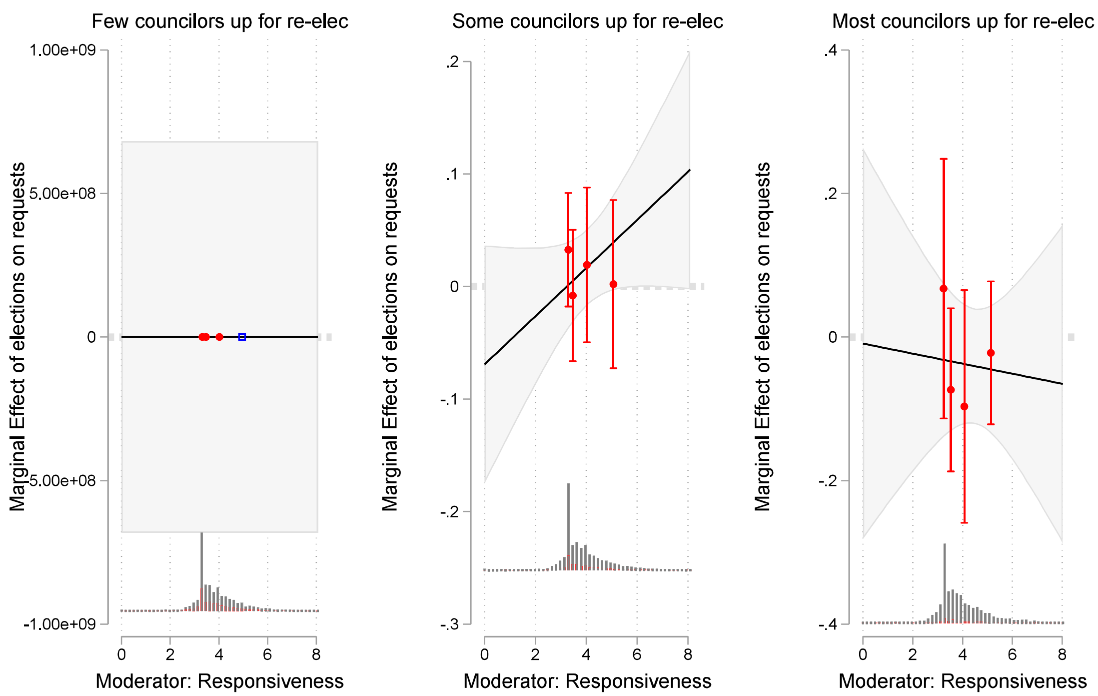
Note: The Figure shows marginal effects of treatment (election period) on requests sent normalized by 10,000 residents by four level of responsiveness, low, medium-low, medium-high and high. Responsiveness is defined as the number of days taken by the local government to fix requests, after logarithmic transformation. We consider the lagged value of responsiveness two months before requests are sent. In panel a), we consider responsiveness without logarithmic transformation. In Panel b), we consider responsiveness as the median (instead of average) number of days to fix a request. In Panel c), we control for the share of requests which can be fixed fast. In Panel d), we control for the number of requests sent. All specifications include district and month fixed effects and controls for the type of requests sent. Robust standard errors are clustered at the district level.

Figure 17: Effect of treatment on requests sent by levels of responsiveness, robustness tests 2



Note: The Figure shows marginal effects of treatment (election period) on requests sent normalized by 10,000 residents by four level of responsiveness, low, medium-low, medium-high and high. In panel (f), responsiveness is defined as the number of days taken by the local government to fix requests, after logarithmic transformation. In panel (e), we consider only requests sent and fixed within the same calendar month. In panel (f), a control for the pattern of elections in a district (corresponding to how many councilors are up for re-election) is included. We consider the lagged value of responsiveness two months before requests are sent. In panel. All specifications include district and month fixed effects and controls for the type of requests sent. Robust standard errors are clustered at the district level.

Figure 18: Effect of treatment on requests sent by levels of responsiveness, robustness tests 3



Note: The Figure shows marginal effects of treatment (election period) on requests sent normalized by 10,000 residents by four level of responsiveness, low, medium-low, medium-high and high. Responsiveness is defined as the number of days taken by the local government to fix requests, after logarithmic transformation. Each panel runs the analysis on a subgroup of observations with few, some or many councilors up for re-election depending on the pattern of elections in the district (every year, every 2 years, every 3 or 4 years). We consider the lagged value of responsiveness two months before requests are sent. In panel. All specifications include district and month fixed effects and controls for the type of requests sent. Robust standard errors are clustered at the district level.

Table 17: Alternative mechanism: politicization

	N requests sent
General Election×Jan	-0.081
	(0.072)
General Election×Feb	-0.087
	(0.062)
General Election×Mar	-0.000
	(0.062)
General Election×Apr	0.001
	(0.050)
General Election×Jun	0.026
	(0.050)
General Election×Jul	0.007
	(0.058)
General Election×Aug	-0.005
	(0.045)
General Election×Sep	-0.007
	(0.049)
General Election×Oct	0.060
	(0.048)
General Election×Nov	-0.020
	(0.053)
General Election×Dec	-0.084
	(0.051)
Observations	18,894
Number of districts	322
R-squared	0.201
District FE	YES
Year FE	YES
Trend responsiveness	YES

Note: The table shows results from a monthly specification of our difference-in-difference estimation on the model of Equation 1 in which the $Treatment$ indicator is replaced by a dummy $GeneralElections$. The dependent variable is the number of requests sent in district i over the district population, in 10,000. $GeneralElections$ is an indicator taking value 1 in general elections years and districts with no local elections, and 0 in years without general elections and districts with local elections. Robust standard errors clustered at the district level are in brackets. *** p<0.01, ** p<0.05, * p<0.1.