The electoral effect of pork barrel politics: evidence from England

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Abstract

This study examines the effect of the allocation of centralised funding on electoral support for the incumbent by utilising the introduction of the "Towns Fund" in England in 2019 as a natural experiment. For causal identification, I leverage a difference-in-difference design to examine the electoral effect of this fund. My findings suggest that providing funding to constituencies significantly increased the vote share of the Conservative Party in the General Election in 2019. However, in a subset of constituencies in which the Labour Party constitutes the incumbent, the findings can not be replicated for all specifications and robustness checks. Furthermore, I do not find consistent support that the effect is stronger in economically deprived constituencies. Similarly, the results suggest that the voting outcome depends on the total amount of funding being received, however, this finding does not reach statistical significance. These results complement the literature by providing empirical evidence for pork barrel as a functioning means for vote buying for the Conservative government in 2019 in England. Furthermore, this paper emphasises that partisanship should be considered as a mediating variable when analysing the political effect of providing place-based funding.

Keywords Political economy · Towns fund · Voting behaviour · Natural experiment · difference-in-difference · Funding

1 Introduction

Utilising centralized funding allocations as a means of vote buying is a well-known phenomenon across countries and different electoral systems (Hanretty, 2021; Evans, 2011; Hauk & Wacziarg, 2007; Spáč, 2021; Kawanaka, 2007; Milligan & Smart, 2005). Generally, this so called "Pork barrel politics" often has been described to be a functioning tool for increasing the incumbent's vote share (Spáč, 2021; Khemani, 2010; Evans, 2011). However, Fukumoto (2019) and Crescenzi et al. (2020) present contradictory findings that show that funding from the European Union in England did not necessarily increase public support towards the EU and partly even worsened it. However, since their analyses do not

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examine the effect on the national incumbent but on a supra-national institution, it is plausible to expect contradicting results. To the best of my knowledge, no further research has been conducted about the effect of pork barrel in England. Thus, this study aims to provide clarity on the effects of pork barrel politics on the incumbent vote share by utilising the introduction of the Towns Fund in early 2019 to estimate the electoral impact of providing pork in the election of 2019 in England. It contributes to the literature by providing evidence for the functionality of a political instrument that has been shown to be a regular tool in England across governments and time from 1992–2012 (Fouirnaies & Mutlu-Eren, 2015).

Based on the foundation of Hanretty (2021), this paper builds a novel dataset that enables me to conduct a difference-in-difference design. Since the allocation process has been shown to be biased towards Conservative constituencies (ibid.), in order to tackle an inherent reversed causality problem and avoid partisanship bias in my estimations, I conduct two separate analyses for two samples that are divided by the winning parties of the General Election in 2017 on the constituency level. First a sample of constituencies in which the Conservatives won the General Election and secondly a sample of constituencies in which Labour won the election. By merging data from four General elections of 2019, 2017, 2015 and 2010, I am able to test my setting for the crucial parallel trend assumption for both samples.

After multiple robustness checks, my results suggest that, in the Conservative sample, funded constituencies indicate an increase of 2.23 percentage points in the Conservative vote share in comparison to non-funded constituencies. Also, more deprived regions indicate a stronger effect than less deprived constituencies, however, this effect does not reach statistical significance. Similarly, the amount of funding received, is positively associated with an increase in Conservative vote share but the effect remains statistically insignificant. Whereas the main electoral effect is statistical significant ($p < 0.05$) for both the Conservative and Labour sample in the main regressions, the coefficients are only robust throughout a variety of specifications for the Conservative sample. Thus, my results suggest that in regions that already are generally aversed to the ruling party, it is harder to gain votes by pork barrel as an incumbent. However, in regions that already tend to favour the incumbent a substantial amount of voters can be mobilized by providing funding.

2 Current literature and theory

2.1 Pork barrel

Pork barrel politics can be described as the biased distribution of centralized resources that is motivated by the legislator's desire for an electoral advantage during the next election (Evans, 2011). Its existence was found in many countries with a variety of electoral systems. In an analysis of pork barrel politics as a means of vote-buying, Khemani (2010) examined developing countries such as Peru, Brazil and India and constructed a formal model showing that extensive pork barrel politics lead to a decentralization of regional funding. Furthermore, an analysis of the "Constituency Development Fund" in Myanmar, besides its positive effects of a steeper interaction between legislator and voter, has been shown to be a means of vote-buying by providing selective benefits to certain regions

(Egreteau, 2017). Whereas the examples above focused on emerging economies, the electoral strategy of pork barrel politics is not restricted to such. For instance, Milligan and Smart (2005) shows how the allocation of regional grants in Canada was used for pork barrel politics from 1988–2001. The authors show two main findings that also seem to be common in other countries (Hanretty, 2021; Evans, 2011). Firstly, they show that districts that are governed by the national incumbent tend to receive more funding than others. Secondly, in districts in which the winning margin of the incumbent party was low and thus could be considered as "swing-district", funding allocation was even higher (Milligan & Smart, 2005). This finding is also in line with the basic assumptions of Khemani's formal model that predicts that targeting swing voters yields higher payoffs for parties until the point that reelection is assured (Khemani, 2010). This pattern has also been observed in the UK during the allocation of the "Towns fund scheme" in 2019 (Hanretty, 2021). All the studies described above share the basic presumption that providing monetary benefits to an electorate yields an electoral advantage for the provider. And indeed the literature often agrees upon the presumption that providing "pork" does positively impact the provider's electoral success. Whereas the literature on pork-barrel politics as a means for vote-buying is extensive and well investigated (Evans, 2011; Hanretty, 2021; Milligan & Smart, 2005; Golden & Picci, 2008; Veiga & Veiga, 2013), surprisingly, there is only a small number of studies that do analyse the actual political or electoral impact of providing funds to electoral districts. Nevertheless, these findings at best can be described as mixed (Neumark & Simpson, 2015).

2.2 Effect of pork barrel and economic vote

Especially in the economics literature, doubts have been expressed about the effectiveness of place-based policies. It is argued that possible heterogeneity and potential mechanisms have not yet been investigated (Neumark & Simpson, 2015). Also, the literature argues that place-based policies can trigger responses by citizens that are sometimes difficult to predict. For instance, if pork barrel funding fails to reach the majority of the people and on the contrary, leads to the rise of cost of living and housing prices, it is plausible to expect a negative effect. Furthermore, such increases in prices can lead to movements of workers and overall to a decrease in productivity in a region (Hsieh & Moretti, 2015), which eventually also can result in an unwanted electoral punishment for the "pork" provider. An empirical example of this is the effect of the place-based policy by the EU in the UK. Relying on a Regression discontinuity design on a regional level, Fukumoto (2019) finds a negative relationship between receiving funding and support for the European Union. In another context, research suggests that the pork barrel strategy was successful in increasing the provider's vote share. In an examination of 7355 mayoral elections in Slovakia, it has been shown that local grants are especially beneficial for the incumbent's vote share if they are received shortly before the election (Spáč, 2021). The effect has been particularly strong for small towns. This is in line with further research indicating that the allocation of pork barrel spending to small districts is preferred by legislators (Hauk & Wacziarg, 2007). Nevertheless, studying the literature on pork-barrel spending shows that its main assumption, a positive relationship between spending and electoral success, empirically is not as firm as it is often depicted. In fact, empirical evidence has suggested that the effect might sometimes be reversed (Fukumoto, 2019; Crescenzi et al., 2020). This is underlined by

mixed findings in the literature on the economic effect of place-based funding (Neumark & Simpson, 2015).

In this paper, I want to contribute to this discussion by providing further empirical evidence on that matter. By relying on the economic vote literature, I describe how providing "pork" to the voters can be beneficial to ruling legislators. The economic vote theory assumes that incumbents are being held accountable, especially for their performance in regard to the economy (Key, 1964; Lewis-Beck & Whitten, 2013; Dassonneville et al., 2016; Fiorina, 1978, 1981). Extensive research has been done on this topic, showing that economic variables significantly impact vote share worldwide (Lewis-Beck & Whitten, 2013), which tend to be most substantial among particularly rich countries (Duch & Stevenson, 2008). Most importantly, in regard to pork-barrel politics, research suggests that not only national economic variables can impact vote share on a national level (Becher & Donnelly, 2013). Instead, it is also local economic characteristics that can significantly impact public opinion and vote choices in national elections (Johnston & Pattie, 2001; Veiga & Veiga, 2010). Simonovits et al. (2019), argue that local economic characteristics can be understood as a mediator for the effect of national prosperity on vote share because they give further insight into the actual distribution of economic growth among regions.

Based on these findings, I argue that by providing "pork" to local districts, an economic improvement occurs that eventually results in rewarding the incumbent in a forthcoming election. Whereas above I have described how such spending also theoretically could lead to a reversed effect, the economic vote literature let me derive the following hypothesis:

H1 Regions that receive pork-barrel funding have a higher vote share for the incumbent.

Additionally, it is plausible that not all funding allocated to communities yields the same effect size in electoral advantages for the incumbent. If a budget is allocating money to a town, it is likely to be the case that the positive electoral effect might depend on the total funding amount provided. This is due to the fact that larger subsidies enable towns to implement bigger projects with a stronger impact on the region. Therefore, by relying on the economic vote literature (Key, 1964; Di Cataldo, 2017; Marlin, 1990; Wei & Zuo, 2018; Mohl & Hagen, 2010), I suggest that the more money is allocated to a town, the more the mechanism of economic improvement can take its effect and lead to an increase in the incumbent vote share. From this, I derive my second hypothesis:

H2 The larger the amount of funding, the more substantial is the effect on the incumbent vote share.

Furthermore, additional heterogeneity in the effect could result from the variance of initial demand for further subsidies within towns. Income deprivation of local regions often times varies substantially between regions. In some very deprived regions, small subsidies can already trigger substantial economic change due to a high economic potential. However, in other regions that already score high in economic attractiveness, the economic improvements might be proportionally small. Thus, the electoral returns will significantly diminish compared to more deprived regions. Deriving from the economic vote literature, I predict that decreasing marginal returns as a function of a constituency's wealth can be

observed. In other words, the effect for regions that are particularly deprived is likely to be significantly bigger than for other regions. I derive:

H3 The bigger the initial deprivation of a region, the bigger the effect of pork barrel funding on the incumbent vote share.

To summarise, the pork-barrel literature in combination with the economic vote theory strongly suggests that providing funds to local districts should increase the vote share for the providing incumbent. This paper aims to contribute to the literature by providing further empirical evidence on this matter in an English setting. In the following section, I will discuss my empirical case.

2.3 Pork barrel in England

Besides, Fukumoto (2019) and Crescenzi et al. (2020), whose reliance on a specific case in which the European Union allocated pork might lead to low external validity, to the best of my knowledge there is no further systematic examination of the effect of pork-barrel politics in the United Kingdom to this date. However, the existence of such has been revealed not only in specific cases (Hanretty, 2021), but also throughout a longer time period from 1992–2012 (Fouirnaies & Mutlu-Eren, 2015) revealing a continuous and systematic use of pork-barrel politics independent from partisanship. Underlining the existing literature, Fouirnaies & Mutlu-Eren (2015) find that in the UK, governments allocate 17% more money to councils that are being ruled by their own party. Furthermore, they also underpin the finding that pork barrel especially is operative shortly before elections and in districts that are being considered as "swing districts" (ibid.). Therefore, in the scholarly debate, pork barrel politics has been shown to be a usual instrument of political strategy. This is the reason why a UK setting constitutes a suitable candidate for examining the effects of pork-barrel politics. Particularly the introduction of the Towns Fund offers an excellent setting because it fulfils the typical characteristics of pork barrel politics. After shortly describing the allocation process of the Towns fund, I present my research design and the data that I exploit for the subsequent analysis.

2.4 Towns deal

In July 2019, Boris Johnson announced a new funding scheme of over 3.6 billion pounds that especially aimed to improve the rural areas of England. A part of this new budget was the "Town Deal" which, with 2.3 billion pounds in total, constituted the major part of the funding. With the Towns Deal, 101 towns were eligible for subsidies of up to 25–50 million pounds. The selection process mainly relied on objective characteristics, however, also qualitative assessments were considered for the selection. According to the official definition of towns: a minimum area of 200,000 m^2, individual settlements that are separated by at least 200 metres, as well as an overall population between 5000 and 225,000, there are 1082 towns in England (Shearer & Shepley, 2021). From these towns, officials of the Department for Levelling Up, Housing, Communities and Local Government decided to only consider towns for the fund that are relatively deprived in comparison to the others. That ruled out approximately 50% of all towns in England. The remaining 541 towns

have been divided into high-, medium- and low-priority groups. The priority groups were allocated according to values in income deprivation, skills deprivation, productivity, EU exit exposure, exposure to economic shocks, investment opportunity and presence of other funding. The selection process also ensured that towns in each region are represented by creating priority groups for each region. Eventually, the priority groups have been crucial for the selection process of the funding. However, due to qualitative assessments also 12 low-ranked towns have been allocated money in this process from which all have been Conservative-held. Generally, subsequent analysis has shown that the allocation process was significantly biased toward Conservative-held cities, with a particular substantial bias towards those in which the winning margin in the prior election was only marginal (Hanretty, 2021).

In Fig. 1 I have plotted all constituencies of England and coloured those which received funding depending on the incumbency in 2019, thus the winner of the 2017 General election. Blue districts have been held by the Conservative Party and red districts by Labour. Any remaining white districts either did not receive any funding from the Towns Deal or were not eligible to receive it in the first place. Since in my subsequent analysis I will rely on constituency-level data, I decided to directly present the allocation of funding accordingly. For this, I relied on (Hanretty, 2021), who geocoded each city and assigned the main Westminster constituencies accordingly. In a few cases, in which towns have been on the border of two constituencies, the constituency with the largest overlap is coded as the responding constituency. Furthermore, I need to stress that although some constituencies contain more than one town, these cases are not differentiated in the map. Overall this is the case in only 7 constituencies. Therefore on the map, 94 constituencies are indicated as being funded by the scheme, 50 of them have been held by the Conservative
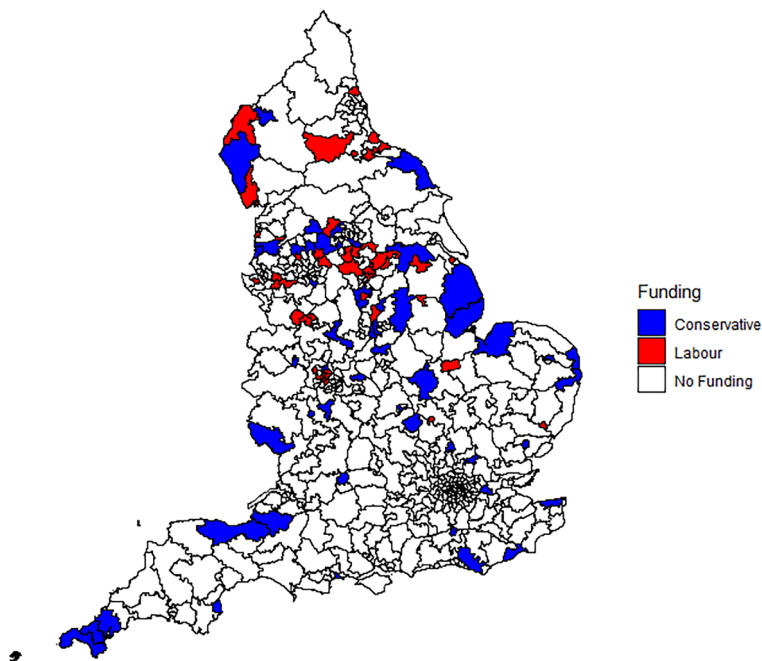

Fig. 1 Funded constituencies in England by incumbent

Party (blue) and 44 by the Labour Party (red). The map shows that indeed the funded areas are relatively spread throughout England and do not reveal any substantial spatial cluster. Nevertheless, if all funded regions are taken together there is a small accumulation in the North of England that needs to be highlighted. Moreover, if you account for incumbency it becomes apparent that in the south of England there are only Conservative-held constituencies being funded. Almost all Labour constituencies that contain a town that was being funded are located in the north around Yorkshire and the North East as well as the North West regions. Moreover, it can be seen that no funded town was located in constituencies that were governed by the Greens or the Liberal Democrats. This allocation can be explained by the strong prevalence of Labour and Conservatives in the election of 2017. Also, the distribution of Labour and Conservative funded constituencies could be explained by the clustered election results 2017.

The introduction of the Towns Deal constitutes an excellent opportunity to analyse the effect of pork barrel. This is mainly for two reasons. Firstly, scholarly Hanretty (2021) as well as publicly (Syal, 2020; Madders, 2019) it is well established that the allocation can be considered pork barrel politics. It fulfils many characteristics of a typical pork barrel case, such as a significant allocation to swing districts (Hanretty, 2021; Milligan & Smart, 2005) as well as the short amount of time between allocation and election (Spáč, 2021). Secondly, the Towns Deal can be depicted as a substantial funding source that is likely to have had an impact of some sort. With overall more than 2.3 billion pounds and individual fundings of up to 60 million pounds, I believe that salience is given in this setting.

3 Data

For the purpose of this paper, I have created a novel data set that builds on the work of (Hanretty, 2021). In his paper, he collected data on all indicators for the year 2017 on which the decision of the funding selection was being made. Furthermore, he geocoded each city and assigned them to the constituency that indicates the largest overlap. Whereas 541 towns were considered generally eligible for the funding, only 101 towns actually received money. Aggregated on the constituency level, 283 constituencies have been considered for funding whereas only 94 received a grant. This means that all 541 eligible towns were distributed among 283 constituencies. Overall, all 101 funded towns were located in 94 constituencies.

Since I am particularly interested in the effect of the funding and not in its allocation process, I merged all national election results since 2010 to my data. This enables me to conduct a test for a common trend in the dependent variable and thus to conduct a difference-in-difference design in this setting. Since election data is a rather accessible data source I could have merged more elections, however, a major constituency border change in 2010 led me to the decision to restrict my data to a time period from 2010–2019 in order to keep them as comparable as possible. Therefore, my common trend analysis will be restricted to three data points in time before the intervention in 2019. Furthermore, due to the data structure at hand, my level of analysis is at the constituency level. Each constituency that contains at least one town that was funded by the Towns Deal hereby is considered a treated unit. In addition to the election data, in order to test H2, I also merged

further information about the exact amount of money received for each town to my data and aggregated it to the constituency level. Thus, in cases in which two or more funded towns are located in one constituency, I summed the amounts to a variable measuring the total amount of money being received.

I also merged additional time-varying controls to my data. This includes the population of each constituency as well as the proportion of the population that is aged over 50 years. Both variables are accessed from the House of Commons Library (UK-Parliament, 2022).

While an analysis on the town level would have been the ideal analysis strategy to increase my number of cases, unfortunately, to the best of my knowledge there is no open data source providing such information. It needs to be considered that this could have an effect on my results. Since the districts of the constituencies often are much bigger than the funded town itself, an estimation only provides a conservative estimate because the effect of the funded town stands against the effect of the rest of the constituency. Whereas spillovers are likely to reduce this downward bias, however, it is still likely to affect the estimation.

4 Research design

4.1 Identification strategy

My empirical analysis utilises a difference-in-difference design with a one-time intervention to identify the effect of the Town Deal. My data contains $d = \{ 1,\ldots,N \}$ constituencies of England where $N = 283$. I consider elections in years $t = \{2010, 2015, 2017, 2019\}$. For my identification strategy, it is important to consider that the allocation of funding has proven to be biased towards Conservative towns (Hanretty, 2021). Thus my external intervention partially depends on the pre-existing characteristic of partisanship and thus constitutes a threat to my internal validity since potentially this could lead to a downward bias in my estimations. Therefore, to exclude this bias from my estimation I will analyse Conservative- and Labour-held constituencies separately. I do this to hold constant the empirically investigated bias among both party groups and thus simultaneously also tackle the problem of reversed causality. Separating between a Labour and a Conservative subset also provides the advantage of providing an insight into the heterogeneity of the effect. Whereas I also simply could have included a partisan dummy in my regression, I believe that separating between two samples provides a more distinct and profound understanding of the fulfilment of assumptions as well as about unique characteristics of each group.

The estimand of interest in this paper is the effect of funding on the electoral success of the incumbent. Following the standard difference-in-difference framework it can be defined as the average difference between the incumbent vote share of treated and non-treated constituencies after the funding has been allocated. It can be defined as the average treatment effect on the treated (ATT) and is described by $\alpha = E[Y_{1d,2019} - Y_{0d,2019}|D_d = 1]$. Since this definition includes an unobserved potential outcome, I follow the common difference-in-difference approach of substituting the missing quantity by relying on the strong assumption of parallel trends between treated and untreated units. Thus, in this case, I presume that the average incumbent vote share of funded regions would have had the same development as the average incumbent vote share of constituencies that have not been funded. This can be defined as $E[Y_{0d,2019} - Y_{0d,t \leqslant 2017}|D_d = 1] = E[Y_{0d,2019} - Y_{0d,t \leqslant 2017}|D_d = 0]$. Therefore, all taken together the ATT, in this case, can be identified by the subtraction of the difference of

the average incumbent vote share before and after the treatment for each treated and untreated constituency. This also can be written as:

$$\alpha = \{E[Y_{d,2019}|D_d = 1] - E[Y_{d,t \leqslant 2017}|D_d = 1]\}$$
$$- \{E[Y_{d,2019}|D_d = 0] - E[Y_{d,t \leqslant 2017}|D_d = 0]\}$$

4.2 Estimation strategy

I estimate the ATT by using a linear fixed effects regression. The coefficient of my estimand can be computed by an interaction of a treatment group dummy and a dummy indicating pre- and post-treatment. My estimation follows the following function:

$$Y_{d,t} = \eta + \alpha D_{d,t} + \mu_d + \delta_t + X'_{d,t}\beta + \epsilon_{d,t}$$

where Y is my dependent variable measured as the vote share for the incumbent Conservative Party in England on the constituency level, η is a regular constant, α is the treatment effect computed by the interaction of the treatment and the time dummy $D_{d,t}$, μ_d shows the constituency fixed-effect, δ_t is the dummy indicating the post-treatment period, $X'_{d,t}$ is a set of time-varying as well as non-time-varying controls and ϵ, which defines the error term of my function. The estimation strategy follows the same function for both Conservative and Labour-held constituencies. Labour-held constituencies are defined as constituencies in which the Labour Party won the constituency in the General Election in 2017 and thus still constitutes the incumbent MP at the time point of the allocation of the Town Fund. The definition is the same for Conservative-held constituencies. Specifically, $X'_{d,t}$ includes two time-varying variables that measure the total population of each constituency as well as the share of the population that is aged over 50 years. Furthermore, for robustness checks, it includes a variable measuring the leave vote share in the Brexit election 2016 on the constituency level which is provided by Hanretty (2017). I test the impact of this variable since Brexit led to a significant political realignment that could function as a substantial confounder as it potentially has an impact on both Conservative vote share as well as on the funding decision. The results of this can be found in the appendix in Tables 8 and 9. In my main table in the results section, I will include an economic control only in the last model to test my ascribed mechanism. Since I argue that the fund improves the economic condition and by that increases support for the incumbent, controlling for the economic conditions should significantly diminish the effect of being funded. I do this in order to test whether my theorized mechanism is in place. For this, I utilise a proxy consisting of a time-varying variable measuring the share of households which receive universal credit. Universal Credit is a social payment that low-income households can apply for. To test the robustness, in the appendix in Table 17 I include models utilizing a logarithmic version of this measure as well as an alternative measure indicating income support by a different social scheme. The results remain stable throughout all specifications. For testing H2 and H3, I rely on models that only contain treated observations in order to exclude the treatment effect from this estimation. For H2, I then utilize a standardized variable measuring per Capita funding. This variable is continuous, is measured in millions (British Pounds) and is retrieved from the official Towns Fund website. For testing H3 I rely on a measurement provided by the Department for Levelling Up, Housing, Communities and Local Government that categorised each town into three eligibility categories "Low", "Medium" and "High". In constituencies where two towns with different categories exist, I always
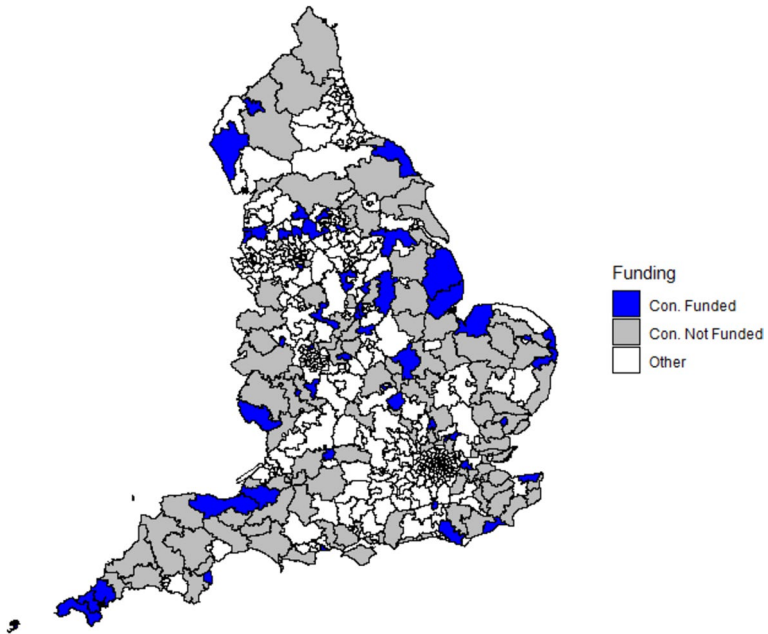
Fig. 2 Eligible funded and not funded conservative constituencies
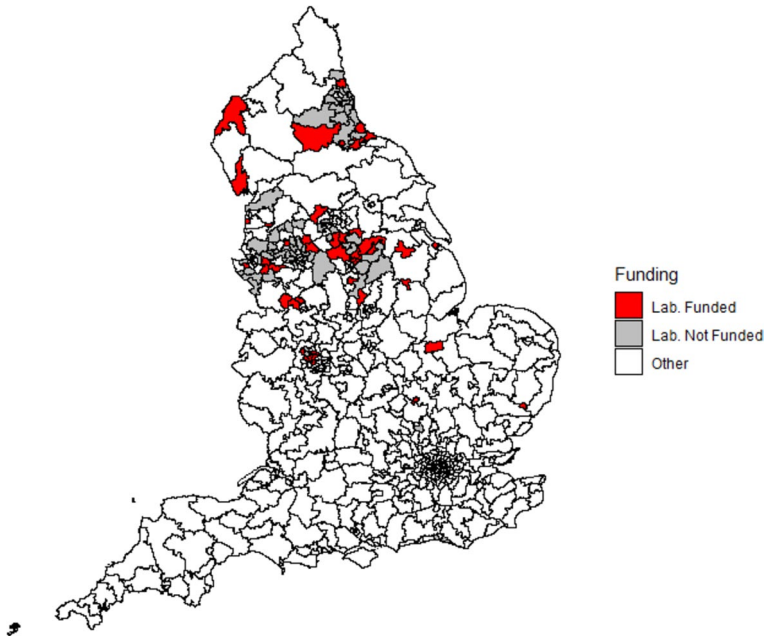

Fig. 3 Eligible funded and not funded labour constituencies

assign the higher category. I code a 1 if a constituency is in the category "High" and a 0 if it is in the category "Medium" or "Low".

For visualising which groups are compared in my difference-in-difference design, in Fig. 2 I include a map that indicates funded Conservative constituencies in blue, as well as general eligible Conservative constituencies that did not receive the funding in grey. Thus, all blue-coloured constituencies constitute my treatment group in my estimation whereas all grey-coloured constituencies are defined as my control group. The map indicates that neither of them is substantially clustered and that they are equally dispersed throughout England. White regions in the map are either constituencies that have not been won by the Conservative Party in 2017 or are Conservative constituencies that did not include a town that was included in the subset of 531 towns that fulfilled the foundational requirements for being eligible for the Towns Deal scheme. Although difference-in-difference accounts for initial differences between control and treatment group, I decided to exclude them from my control group. The reason for this is to ensure that both groups are as comparable as possible. The inclusion of major cities such as the constituencies of London would have led to major differences on all dimensions and thus could have led to biased results.

Figure 3 shows the same map but for constituencies in which the Labour Party won the election in 2017. Different to Fig. 2 of Conservative constituencies, two clear clusters become visible in the north of England. Since both funded as well as not funded constituencies are clustered around the same regions, I suggest that for the estimation such an aggregation should not lead to substantially biased results. Nevertheless, the map indicates that especially for the Labour subset an analysis with region fixed-effects might be necessary. Thus, I include a model containing such an analysis in the robustness section.

Lastly, in this paper, I have defined the incumbent to be on the national level. I assumed that treated units will reward or punish the national incumbent and not the constituency incumbent. Since 2019 was a national election I believe it is a plausible assumption to make, however, the inclusion of a set of regressions with Labour vote share as the dependent variable for the Labour sample can make sure that this is not the case. If it were the case we would assume the coefficients to be positive in such regressions. This analysis can be found in the appendix in Table 16.

5 Results

5.1 Analysis of Conservative sample

Table 1 provides a first insight into the descriptive differences between funded and non-funded Conservative-held constituencies in the year 2017. For this balance table, I aggregated the town information to the constituency level. Thus, again if two or more towns are located within one constituency I took the mean of all and by that computed the constituency value overall. Only population, the percentage of the population over 50, as well as the Brexit leave vote share are directly measured at the constituency level. Therefore, due to the computation, it needs to be emphasised that the values in Table 1 are fuzzy proxies of the actual value of each constituency because they are derived from the selection of towns that have been selected to be generally considered for the Towns Fund. Most of the variables depicted in Table 1 are non-time-varying. Only population, age and Universal Credit per Capita, vary in time and thus, later on, will be included as covariates in the regressions. An additional covariate that I will include as a robustness check in the appendix is the

Table 1 Balance table (Con. Sample)

| | N = 120 | N = 50 | P-Val. |
	Mean Con.	Mean Treat.	
Total score	3.276	4.329	0.000
Qual. score	0.392	0.434	0.384
Inc. deprivation	0.292	0.449	0.000
Productivity	0.409	0.502	0.074
Investment	0.256	0.415	0.003
Brexit	0.555	0.533	0.643
Alignment	0.256	0.415	0.003
Economic shock	0.103	0.196	0.012
Population	101344.758	100947.640	0.836
% of Pop. > 50	11.181	12.218	0.001
Brexit leave	0.591	0.593	0.849
Universal credit per capita	0.004	0.007	0.000

Total Score and Qual. Score show the calculated scores upon which the ranks for the selection of towns were calculated. Inc. Deprivation, Productivity and Investment are economic variables that measure the relative economic situation of the constituency. Brexit measures the economic impact that Brexit had on the constituency. Alignment measures the presence of other funding schemes in the constituency and economic shock measures recent shocks the constituency had. Population and % of Pop. > 50 are demographic variables. Brexit Leave shows the calculated leave vote share on the constituency level and universal credit per capital measures the percentage share of the population receiving financial support

Brexit leave vote share (Tables 8 and 9). Since the Brexit vote was conducted on the local authority level, I had to rely on the calculated Brexit leave share per constituency provided by Hanretty (2017). Separate summary statistics, describing all covariates that are included later on in the regressions can be found in the appendix in Tables 6 and 7.

Looking at the mean values of the variables it becomes apparent that especially income deprivation is substantially and significantly ($p < 0.01$) different with a value of 0.29 for control and 0.45 for treated constituencies. Also, the total score value which indicates the index value of all selection variables that has been considered for the selection process, as expected indicates a significant difference between control and treatment group. Whereas I do expect the treatment group to indicate a higher value on variables that measure economic hardship, the table shows that although six variable means are significantly different according to a t-test, neither of the groups include major outliers but are around the same range of value. Therefore, Table 1 shows that Conservative constituencies that contain funded towns and constituencies that only contain towns that had been eligible for funding can generally be described as similar to the examined demographic characteristics.

As described in my identification strategy, a foundational assumption of my design is that the trend in the dependent variable before the allocation of the funding has been parallel between treated and untreated constituencies. To test this core assumption of my design I rely on three General elections in 2010, 2015 and 2017. Major constituency border changes in the year 2010 restrict me to only consider elections to this point. For this, I compute a model with robust standard errors that is clustered by constituency. Figure 4 depicts the results of my model. The red line depicts the intervention time point which has been in early 2019 before the election. It can be seen that the linear prediction for the Conservative vote share in the election of 2010, 2015 and 2017 have been parallel and thus
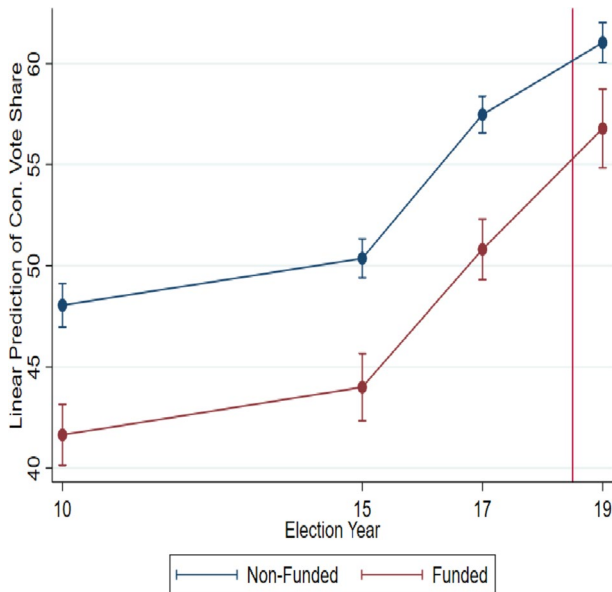
Fig. 4 Common Trend in Con. Vote Share for Con. Sample. This plot shows the prediction for the Conservative vote share of a regression including robust standard errors as well constituency fixed effects. The x-axis shows the election year. The red line indicates the intervention date in early 2019

fulfils the requirement of the parallel trend assumption. Furthermore, a substantial change in the prediction for the election in 2019 also can be observed. The difference between funded and non-funded constituencies visibly decreases and now depicts a much closer confidence interval range as in the prior elections. As predicted, Fig. 4 gives first evidence that funded constituencies indicate an increase in the incumbent vote share in comparison to non-funded constituencies. Further models that contain a lagged vote share variable as well as time-varying covariates do predict the same trend and can be found in the appendix under Fig. 7. It shows that if controls are included, the treatment effect becomes even more visible since the treated and untreated group in 2019 indicate reversed directions.

Having tested the parallel trend assumption of my setting I now run several models that constitute my main results. They include a range of different models that show how the effect alternates if different controls and variables are included. Table 2 shows the main results of my estimations. Model 1 only includes the treatment effect with no controls. It indicates a coefficient of 2.23 which is significant on a ($p < 0.05$) level. This means that Conservative constituencies which got funded in 2019, indicate a 2.23 percentage points higher vote share in the Conservative vote, than Conservative constituencies that did not receive any funding. In my second model, I include a variable measuring a one-year lagged Conservative vote share variable. The effect of funding increases to 2.5.

The inclusion of time-varying variables in model 3 does not substantially change the results of the prior model, however, leads to an even larger treatment effect of 3.55. This suggests that under the assumption of parallel trends which I have tested above in Fig. 4, there is a causal effect of the Towns Fund on Conservative vote share which increases the Conservative vote by 3.55 percentage points. The next two models are included to test my second and third hypotheses. I argue that the effect of funding depends on the amount of money being received. Particularly, I suggest that the more money a constituency receives,

Table 2 Main regression results on Conservative vote share (Con. sample)

	(1)	(2)	(3)	(4)	(5)	(6)
Funded	2.23**	2.50***	3.55***			2.56***
	(1.02)	(0.82)	(0.81)			(0.77)
Std. funding amount				6.91		
				(5.16)		
Priority					1.76	
					(1.64)	
Social payment per capita						189.56***
						(31.65)
Lag. Con. vote		0.80***	0.53***	0.47***	0.46***	0.50***
		(0.12)	(0.08)	(0.14)	(0.14)	(0.11)
Log. population			44.51**	111.44***	96.05**	42.38*
			(20.95)	(40.92)	(39.25)	(22.53)
Const. age			8.58***	7.75***	7.82***	7.49***
			(1.13)	(1.79)	(1.80)	(1.20)
Constant	59.62***	15.27*	−561.04**	−1344.09***	−1166.25**	−524.34**
	(2.83)	(8.69)	(229.91)	(465.34)	(446.72)	(246.47)
Const. fixed-effect	✓	✓	✓	✓	✓	✓
Observations	680	510	510	150	150	510

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in included in parentheses. This table provides the results of the OLS results containing constituency fixed effects as well as controls (log. population and share of population older than 50) and a lagged conservative vote share variable. Models 4 and 5 only include a subset of treated observations. The last model includes an economic control that aims to test the mechanism that is at play. The economic control variable consists of a time-varying variable that measures the share of households within a constituency receiving universal credit

the higher the effect on the Conservative vote share will be. Model 4 depicts the results of my regression that relies on a subset of only treated observations. To estimate the effect I utilize a standardized measure of the total funding being received. As expected the coefficients indicate a positive, outcome of 6.91. Nevertheless, the coefficient is not statistically significant. This indicates that this model does not support hypothesis two. My third hypothesis argues that for constituencies that have a higher priority value, thus a higher measurable demand for subsidies, the effect is stronger. For this, I combine the Medium and High-priority group and compare them with the Low-priority group. Model 5 depicts the result of this estimation. The coefficient also is not significant and indicates a positive estimate of 1.76. Similar to the third model, this suggests that the prior demand of constituencies seems to not have a statistically significant effect among the treated sub sample. In my last model in Table 2, I test the functionality of the mechanism that I suggested is in place in this setting. With the economic vote literature, I argued that pork-barrel is mediated by economic prosperity that is triggered by funding. Thus, holding economic change constant by including it in my model should mitigate the effect of the funding. As described above for this I will utilize a proxy variable that measures social payment per Capita in a constituency. Also, this model again contains the full sample. The coefficient indicates a decrease from a coefficient of 3.55 in model three, to 2.56 in model six. The effect remains highly significant ($p < 0.01$). This shows, that although this economic variable is held constant and thus can not explain variation in the dependent variable, the effect remains substantial and significant.

In the appendix, I reiterate and present all regressions with different robustness specifications, including models only containing constituency fixed effects (Tables 10 and 11) and General method of moments (GMM) estimations (Tables 12 and 13) to tackle a potential "Nickell bias" (Nickell, 1981). All coefficients largely remain stable in terms of effect size as well as statistical significance. Having described the results of the Conservative sample I now continue with the results for the Labour constituency subset. Thus, I will repeat the same set of analyses but with constituencies that were generally eligible for the Towns Deal and were won by the Labour Party in the election 2017. Again, the separation aims to assure that the investigated party bias (Hanretty, 2021), is excluded from my results. Subsequently, I will propose and run several robustness checks that aim to test the sensibility of my results.

5.2 Analysis of Labour sample

Table 3 shows the same balance table as above, but for the Labour constituencies that have won in 2017. The differences between treated and non-treated constituencies are similar to the Conservative subset. Whereas, significant distinctions between funded and non-funded constituencies occur in variables such as economic shocks, qualitative assessments, and investment opportunities, overall the difference in value are moderate and around the same value range. Thus, it is likely that both examined groups do not indicate to be outliers. Also, it needs to be stressed that since more Conservative constituencies included eligible towns, the number of observations for the Labour sample is substantially smaller for both treated and untreated units. Figures 2 and 3 again give an insight of which constituencies

Table 3 Balance table (Lab. Sample)

	N = 67	N = 44	P-Val.
	Mean Con.	Mean Treat.	
Total score	4.493	5.715	0.000
Qual. score	0.565	0.676	0.021
Inc. deprivation	0.690	0.746	0.165
Productivity	0.467	0.567	0.049
Investment	0.364	0.532	0.003
Brexit	0.344	0.436	0.090
Alignment	0.344	0.562	0.000
Economic shock	0.192	0.316	0.022
Population	97784.537	98195.455	0.843
% of Pop. > 50	12.834	12.784	0.863
Brexit leave	0.578	0.620	0.003
Universal credit per capita	0.011	0.010	0.565

Total score and qual. score show the calculated scores upon which the ranks for the selection of towns were calculated. Inc. Deprivation, Productivity and Investment are economic variables that measure the relative economic situation of the constituency. Brexit measures the economic impact that Brexit had on the constituency. Alignment measures the presence of other funding schemes in the constituency and economic shock measures recent shocks the constituency had. Population and % of Pop. > 50 are demographic variables. Brexit shows the calculated leave vote share on the constituency level and universal credit per capital measures the percentage share of the population receiving financial support
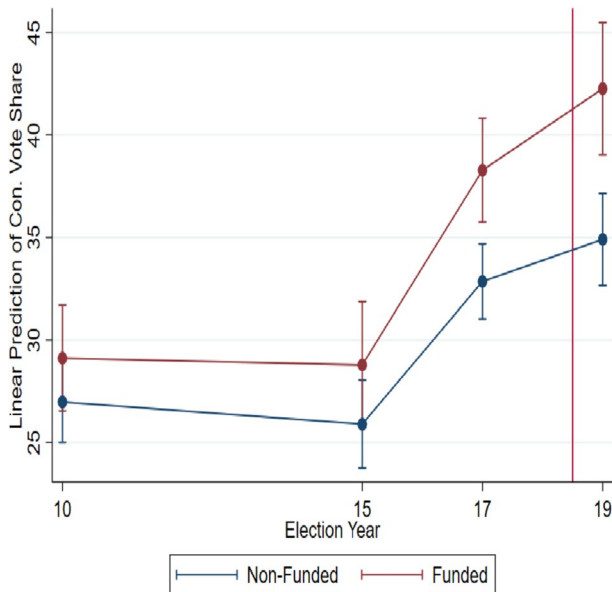
Fig. 5 Common trend in Con. vote share for Lab. Sample. This plots shows the prediction for the Labour vote share of a regression including robust standard errors as well constituency fixed effects. The x-axis shows the election year. The red line indicates the intervention date on early 2019

were eligible as well as which one eventually got funded. Therefore, they indicate which constituencies I am comparing in my regression models.

As for the Conservative sample, I have to test the crucial assumption of difference-in-difference designs. Namely, there needs to be a parallel trend in Conservative vote share between treated and non-treated constituencies before the treatment. Figure 5 shows the prediction of a model for Conservative vote share separated by treatment and control group, controlled by constituency fixed-effects as well as robust standard errors. The figure shows that also for the Labour subset the assumption of a parallel trend in the dependent variable seems to hold. Nevertheless, other than in the Conservative sample, between 2015 and 2017 there is a recognizable difference between both point estimates of the two groups. This difference becomes larger in 2019, whereas it seems to increase for the treated unit. Figure 8 in the appendix additionally shows the parallel trend if a lagged vote share variable is included. The trend does not substantially differ from the results in Fig. 5. Generally, the prediction in Fig. 5 as well as in Fig. 4 shows a positive trend for the Conservative vote share. This is likely to be a result of major losses of the Labour Party in the election in 2019. The Labour Party in this election overall lost 48 seats, whereas the Conservative Party was able to gain 49. Overall, the parallel trend in Conservative vote share in Labour-held constituencies can be seen, however, the jump in 2017 should be kept in mind. Additionally, I will test the parallel trend further in my robustness section by examining a placebo treatment.

Having tested the parallel trend, Table 4 now shows my empirical results that are obtained by the same estimation strategy as in the analysis for the Conservative sample. My first model estimates the treatment effect with no controls. The coefficient indicates a positive and significant ($p < 0.01$) estimate of 3.72. Including the one-year lagged Conservative vote share variable mitigates the coefficient to 1.92 and also turns the coefficient

Table 4 Main regression results on Conservative vote share (Lab. Sample)

	(1)	(2)	(3)	(4)	(5)	(6)
Funded	3.72***	1.92	1.85*			1.92*
	(1.30)	(1.22)	(1.09)			(1.09)
Std. funding amount				1.01		
				(8.85)		
Priority					−4.03**	
					(1.76)	
Social payment per capita						114.78**
						(44.45)
Lag. con. vote		0.39***	0.38***	0.37**	0.36**	0.40***
		(0.10)	(0.09)	(0.14)	(0.14)	(0.09)
Log. population			119.15***	128.87*	109.84*	117.46***
			(38.78)	(66.28)	(60.72)	(36.23)
Const. age			10.03***	12.98***	14.63***	9.30***
			(1.36)	(2.23)	(2.09)	(1.31)
Constant	26.12***	16.83***	−1523.49***	−1621.46**	−1421.23**	−1494.13***
	(1.28)	(2.97)	(441.52)	(752.41)	(690.04)	(412.81)
Const. fixed-effect	✓	✓	✓	✓	✓	✓
Observations	443	332	332	132	132	332

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in included in parentheses. This table provides the results of the OLS results containing constituency fixed effects as well as controls (log. population and share of population older than 50) a lagged conservative vote share variable. Models 4 and 5 only include a subset of treated observations. The last model includes an economic control that aims to test the mechanism that is at work. The economic control variable consists of a time-varying variable that measures the share of households within a constituency receiving universal credit

insignificant. Including controls in the regression only slightly alters the effect size. In Model 4 now again I test my second hypothesis. Examining the effect of the standardized funding amount variable on Conservative vote share shows that, similar to the Conservative sample, there seems to be no effect on the amount of funding being received. Interestingly, the fifth model testing H3 in Table 4, indicates a negative and significant effect of −4.03 percentage points indicating that the funding effect for constituencies with higher demand is lower than for those with low demand. Finally, the inclusion of economic controls does not seem to significantly alter the effect of the treatment in terms of significance. Looking at the results for the Labour sample it can be seen that overall funding seems to have no significant effect on Conservative vote share throughout all models.

Furthermore, reiterating the models with different tests, it can be seen that the coefficients of the labour sample are not robust to different specifications and thus should taken with a grain of salt. All robustness checks, including fixed-effects-only models as well as additional operationalization can be found in the appendix. Here I also include additional GMM estimations (Blundell & Bond, 1998) to tackle a potential "Nickel Bias" (Nickell, 1981) that can arise from the mixed inclusion of fixed effects and lagged variables. Whereas the Conservative sample analysis mostly can confirm my suggested hypotheses throughout different specifications, the Labour sample does not indicate a significant funding effect. In the following section, I will further test the robustness of my models by proposing several tests and sensibility checks.

6 Robustness

6.1 Specification

In a first step, I investigate the robustness of my models by computing three different scenarios for each sample. Firstly, from Figs. 2 and 3 I have argued that although for the Conservative sample, no clustering of eligible constituencies can be identified, especially for the Labour sample including regional fixed effects might significantly change my results. Thus, the first point estimates in Fig. 6 shows two coefficients for each sample, that estimate the treatment effect with covariates as well as region-fixed effects only. Indeed the statistical significant ($p < 0.05$) effect remains for the Conservative sample. However, for the Labour sample the treatment effect diminishes to a null result. Reiterating this analysis without a lagged vote share variable shows that also for the Conservative sample the estimate with region-fixed effects diminishes to a lower significance level ($p < 0.1$).

Secondly, although I already tested my parallel trend assumption in Figs. 4 and 5 I include a further placebo test that examines the difference of treated and non-treated constituencies before the funding. Thus, a significant result would suggest major differences and thus would mean a violation of my core assumption. The point estimate of my placebo test indicates an insignificant effect which is close to 0 in the Conservative samples. However, in the Labour sample, the placebo test indicates a significant effect and thus suggests that the assumption of parallel trends is violated for the Labour sample. This is in line with the inconsistent coefficients throughout different specifications.

For the Conservative sample, however, considering all tests from Figs. 4 and 6 it is likely that, if the fund never had been introduced, the trend for funded and non-funded constituencies would have remained on a similar trend. This is in line with my assumption and strengthens the validity of my design.

Additionally, in order to further test the heterogeneity of my effect, I estimate regressions that consider the total distance between the winning and the second-best party in the election in 2017. I do this because Hanretty 2021 emphasises in his findings that pork barrel might target especially those constituencies that can be considered as swing districts. For example, it might be that the salience of a funding intervention is higher in swing states and thus can better deploy its full effect. To test this, I created a variable that measures the total distance between the winning and the second party in 2017. The variable is coded 1 if the total distance is lower or equal 15 and 0 if it is bigger.

I recognize that a total distance of 15 might be considered as a rather large definition of a close election, however, due to my relatively small sample size I have chosen to define this threshold generously. Furthermore, sample size forced me to utilise a continuous measure for the difference between winning and second party in order to run the regression for the Labour sample. Thus, I utilize two differentiating specifications for the distance variable for the two samples. By doing this, 84 out of 283 constituencies that can be considered as a close election. The effect in constituencies with close elections does not significantly differ from constituencies without a close election in 2017 in the Conservative sample. However, in the Labour sample there is an effect of close elections, indicating that indeed a bias in this sample might blur estimations.

Whereas Fig. 6 only includes the robustness checks for my first hypothesis for clarity reasons, I also computed the same tests for my models that aim to test hypotheses two and three. Since the placebo test only tested the common trend in the dependent variable I exclude this estimation from the robustness check for my second and third hypotheses,
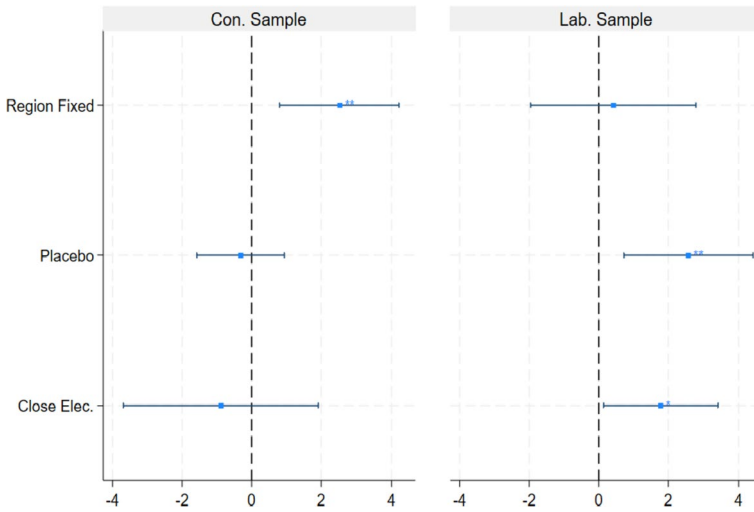
Fig. 6 Effect of funding with robustness specifications by sample. *Note:* * *p* < 0.10, ** *p* < 0.05, *** *p* < 0.01 This coefficients plot depicts the point estimates of the treatment on Conservative vote share. Each point estimate is computed with a different specification that is indicated on the y-axis. The x-axis shows the estimated effect whereas one unit is defined as one percentage point. 95% Confidence Intervals are indicated

however, all other tests are included. The full regression tables, that show the coefficients of the different specifications can be found in the appendix in Tables 14 and 15.

Overall, the results remain steady and do not substantially change in size and significance. Lastly, in this subsection, I run a set of regressions with Labour vote share as the dependent variable. I do this to test whether the constituency incumbent might be rewarded for bringing funds to her constituency. Table 16 in the appendix shows this set of regression. The statistically insignificant results suggest that Labour constituency incumbents are not being rewarded when their constituency is being funded in 2019.

6.2 Spill-over

The robustness checks above aim to control for the sensibility of my data when different specifications are included in the regressions. Whereas small changes do occur, generally my estimates seem to be quite robust throughout a variety of regressions. Nevertheless, none of the tests above has accounted a major issue that might occur in my estimation. The Tows Deal has been introduced to increase the attractiveness of rural areas of England. Thus, whereas it is a number of towns that received the funding, the money was targeting not only the treated units but also the surrounding areas nearby funded towns. Naturally, projects that have been funded have been selected also according to the overall impact on the region. Therefore, it becomes apparent that the positive impact of the allocation of funding not only triggered economic prosperity and incumbent support within the constituencies in which treated towns are located, but also are likely to have had an impact on surrounding constituencies that did not receive any funding. This spill-over effect is likely to lead to a downward bias of my point estimates because the positive effect also impacts the control group and thus mitigates the difference-in-difference estimates for the treatment.

Table 5 Regression results on Conservative vote share (both Samples)

	Con. Sample			Lab. Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Funded	2.56**	2.58***	1.77**	3.63**	2.12	2.16
	(1.03)	(0.91)	(0.87)	(1.58)	(1.51)	(1.49)
Log. population	50.27*	51.36**	48.16**	94.96**	118.21***	116.59***
	(26.23)	(24.17)	(22.97)	(39.86)	(39.44)	(36.72)
Const. age	11.01***	8.52***	7.46***	10.55***	10.04***	9.30***
	(1.33)	(1.32)	(1.22)	(1.33)	(1.37)	(1.32)
Spill-over	−1.99**	−1.79**	−1.50*	1.14	0.40	0.37
	(0.95)	(0.85)	(0.79)	(1.61)	(1.50)	(1.42)
Lag. Con. vote		0.53***	0.50***		0.37***	0.40***
		(0.12)	(0.11)		(0.09)	(0.08)
Social payment per capita			186.11***			114.72**
			(31.75)			(44.49)
Constant	−621.49**	−636.96**	−588.56**	−1190.97***	−1463.57***	−1436.50***
	(287.23)	(264.14)	(251.05)	(454.90)	(449.81)	(419.27)
Const. fixed-effect	✓	✓	✓	✓	✓	✓
Observations	510	510	510	332	332	332

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in included in parentheses. This table provides the results of the OLS results containing constituency fixed effects as well as controls (population, share of population older than 50 and a spill-over dummy) and a lagged Conservative vote share variable. The last model includes an economic control that aims to test the mechanism that is at work. The Economic control variable consists of a time-varying variable that measures the share of households within a constituency receiving universal credit

To test my results for possible spill-over I manually created a variable by coding control group constituencies as 1 if they share a common border with a treated constituency and 0 if this is not the case. By doing this I coded 117 constituencies as a 1 in the control group. To control for the effect of spill-over I now include the dummy in the regression function and repeat the analysis from above with the spill-over dummy included. Furthermore, because within funded constituencies the effect is supposed to deploy in its entirety, I did not consider spill-over effects between treated constituencies. Thus, the number of treated constituencies in the regressions in Table 5 is the same as above.

The results in Table 5 show the results of my main regressions if spill-over is being considered by coding control group constituencies sharing a border with a funded constituency as one. Since in the specifications for hypotheses two and three I only compare treated units, I exclude these from the analysis because there would be no variation in the spill-over variable. As expected the effect size of being funded on the Conservative vote share increases by around 0.33 percentage points in comparison to the same regression without spill-over. This is in line with my predicted increase of coefficients as described above. Furthermore, whereas the inclusion of a spill-over control leads to an increase in the effect size in the Conservative sample, in the Labour sample we observe a decrease of 0.14. Moreover, the effect turns statistically insignificant in models 5 and 6, when a lagged vote share variable and an economic control is included.

Overall, the main effects of the regression for the Conservative sample in this paper are robust to a variety of specifications. To challenge my coefficients, I included lagged vote share variables, constituency as well as regional fixed-effects, time-varying control

variables, a close-election specification as well as a spill-over test. Throughout all models, the coefficients for the Conservative sample draw a coherent picture indicating that the allocation of the Towns fund led to an increase in the Conservative vote. Overall, the coefficients for the Labour sample are not consistent and differ between specifications. Additionally, Fig. 6 indicates that the analysis of the Labour sample might suffer a violation of the common trend assumption which is crucial for the research design that is being exploited in this paper. This is not the case for the Conservative Sample. Here the coefficient sizes as well as their statistical significance remain similar throughout regressions and different sample specifications and even increase when spill-over is considered as a control.

7 Limitations

In the section above I challenged my results by conducting a series of tests for my assumptions and results. Nevertheless, although my results seem to be robust they come with significant limitations that need to be kept in mind when interpreting the results. Particularly, my sample size, external validity, the stable unit treatment assumption (SUTVA), as well as reversed causality, need to be discussed in this setting.

Firstly, as described above all 101 funded towns are located in 94 different treated constituencies. All other non-funded but eligible towns are spread throughout 189 control constituencies. Since I divide all constituencies into a Conservative and Labour sample, the number of treated units decreases to 50 for Conservatives and 46 for Labour constituencies. The number of control units is 120 for Conservative and 69 for Labour constituencies. The small sample size naturally can lead to wide standard errors in the estimation and thus to insignificant results. Particularly for the regressions that test Hypothesis 2 and 3, the additional decrease of the sample size might have fortified this issue.

Secondly, it needs to be stressed that naturally, the analysis and the results of this paper are restricted to the examined setting. My findings do not enable interpretations beyond my sample of analysis. Thus, my results can not be utilised to predict the effect of vote-buying by pork barrel in other countries or regions. This is important because the particular electoral system in England, with its majority system and constituencies, might have had an impact on the constitution of the effect. Thus, the findings of this paper of the positive effect of pork-barrel politics on the incumbent might vary across different countries, depending on the electoral system.

Thirdly, SUTVA assumes that no different version of treatment exists. However, as described above funding has been varying in size between 30–60 million pounds. Thus, treated units did not always receive the exact same treatment. This might lead to variance in the effect size of treated units. In my second hypothesis, by including a measure for the amount of received funds I tried to capture this bias and find a positive but not significant relationship. Thus, it is likely that treated units overall are being affected equally throughout the sample. Furthermore, on an official website by the Towns Fund (https://townsfund.org.uk/towns) it is described for each town how the money was spent. It can be seen that a large majority of projects aimed to directly or indirectly increase economic prosperity. Nevertheless, differences in treatments remain and need to be considered for the interpretation.

Lastly, but most importantly, my design might suffer a bias originating from reversed causality. Throughout this paper, I describe the effect as a one-way relationship where funding increases the incumbent vote share. However, the possibility exists that funding has been distributed to constituencies because the Conservative vote share was high. I tried to tackle this problem by dividing my sample into a Conservative and Labour sample so that the incumbency in 2017 is held constant in each sample and thus can not drive the effect. However, the

possibility remains that within my sample, funding strategically has been allocated to constituencies with high Conservative vote share. Theoretically, the allocating institution would have to have access to an anchor point of voting intention to be enabled to do this. Whereas, such opinion polls exist on a national level, only in rare cases opinion polls on the constituency level are being conducted. Thus, they would have to rely on the election in 2017. By splitting the analysis into a Conservative and Labour sample I control for this. Additionally, Hanretty (2021) found that the allocation had a partisan bias, however, that especially regions with low winning margins in the election 2017 have been chosen to be funded. Therefore, I included a close election control in Tables 14 and 15 and in the regressions of Fig. 6. Only in the Labour sample, I find that there might have been a bias of allocation towards constituencies that had close elections in 2017. Overall, I recognize reversed causality as the biggest threat to this research. This is the reason why I included many controls and tests in order to control for it. Whereas my tests indicate that indeed it is funding that led to an increase in Conservative vote share, it should not entirely be suspended that the effect might be reversed.

8 Discussion

The results of this paper mostly are in line with the hypotheses that I have derived from the pork barrel and economic vote theory. Whereas Fukumoto (2019) raises doubts about the functionality of pork barrel politics, my results strongly suggest that funding has significantly increased the incumbent vote share in the UK election 2019 by a substantial amount. Nevertheless, my analysis of two samples has revealed that this effect is heterogeneous and does not consistently occur in the Labour sample. Considering all controls, the effect for Labour is positive with an increase of 3.72 percentage points but does not hold in various specifications or when a spill-over control is included. This indicates that pork barrel might be particularly potent in voting districts that already generally support the incumbents' party, namely in constituencies that have been won by the Conservative party in 2017.

Hypothesis two is not supported by the data. Although in the Conservative sample, the high-priority group indicate a higher effect on vote share in comparison to low or medium-priority groups throughout most of the specifications, this effect does not reach statistical significance. This effect cannot be found in the Labour sample. Most interestingly, in fact, the coefficient in the Labour sample is negative and indicates a statistically significant decrease of around 4.04 percentage points. Nevertheless, the coefficients of the Labour sample are not robust to other specifications and might suffer biases. Thus, depending on the sample, the data suggest, that the effect of funding seems not to depend on the prior deprivation level of the constituency. This contrasts findings by Fukumoto (2019), who shows that pork barrel tends not to work for the affluent. But caution is needed. Especially in the Conservative sample a positive coefficient can be found consistently. The lack of statistical significance of this coefficient might be explained by an issue of statistical power that resulted due to the decrease of the number of observation in the analysis.

Lastly, I hypothesised that more funding will lead to a bigger effect of the treatment. Similarly to hypothesis two, I find a consistent statistically insignificant positive coefficient in the Conservative Sample. However, this specification also might suffer a statistical power issue arising from a decreasing sample size.

In this paper, I estimate the electoral effect of pork-barrel politics in England. Whereas I test my theoretical mechanism by holding constant the economic situation by including an economic control, I do not find that the statistical significance of being funded diminishes

to a null result. This suggests that the causal mechanism might be more complicated than just a mere improvement of economic conditions. With the data currently available I am not able to test further possible mechanisms. Nonetheless, the mechanisms constitute a subject that should further be investigated by further research.

This paper is placed in a literature that strongly depends on case studies. Whereas many scholars describe the positive effects of pork barrel for the incumbent in a variety of country settings (Spáč, 2021; Milligan & Smart, 2005; Khemani, 2010; Egreteau, 2017), Fukumoto (2019) shows that pork barrel by the EU did not work in an English setting. Crescenzi et al. (2020) shows that the effect depends on the economic impact of received funding. This paper complements the literature by providing empirical evidence for pork barrel as a functioning means for vote buying for the Conservative government in 2019. Furthermore, it emphasizes that partisanship should be considered a mediating variable when analysing the political effect of providing place-based funding. Empirically, I decided to rely on a difference-in-difference design to estimate the electoral impact of the Towns Fund. It needs to be stressed that other identification strategies such as a Regression Discontinuity Design are also often used in similar settings. For instance, Fukumoto (2019) utilized a Regression Discontinuity to estimate the electoral impact of EU funds in the UK. In the case of the Towns Fund, the lack of a clear cutoff is the reason I decided against employing an additional Regression Discontinuity Design. The fact that qualitative assessments also were considered for the selection of towns, further makes a difference-in-difference design more suitable.

9 Conclusion

This paper examines whether pork barrel politics can function as a means of vote buying. The findings of this paper are important in two ways. Firstly, they give further evidence for pork barrel as a working tool for vote buying. This paper provides an insight into how substantial the effect of pork barrel politics for the incumbent can be. Furthermore, this study includes vital considerations in regard to potential heterogeneity in the effectiveness of pork barrel politics that can be investigated in further studies. Null effects of pork barrel might be the result of downward biases that are triggered by converse effects of sub-samples within the analysis. Resonating with findings by Fukumoto (2019) and Crescenzi et al. (2020), this paper suggests that further research should test for sub-samples and heterogeneity, whereas my findings in particular stress the importance of prior support for the incumbent.

Secondly, my findings give important empirical evidence that also holds political value. Hanretty (2021) has shown that disproportionately many Conservative-held constituencies contained funded towns. Hence, he provided empirical evidence that the allocation process of the Towns fund in England in 2019 had been biased. By conducting this analysis, I now provide further empirical evidence on the impact that this funding has had on the UK election in 2019. By separating between two partisan samples in my analysis, I tackle the problem of reversed causality in this setting. I show that pork barrel substantially contributed to an increase in the Conservative vote share in 2019 in England. Both findings illustrate that policy changes in funding allocation are needed. The allocation process for the Towns Fund was not transparent at first, and measures of the selection criteria were not publicly accessible. However, the biased allocation (Hanretty, 2017) as well as the substantial impact that such funds can have electorally and which have been described in this paper, strongly suggest that transparency as well as accessibility of measures of selection criteria are needed. This would ensure that allocation processes of place-based funding are not being misused for electoral purposes but remain a means of improving living conditions as efficiently as possible.

Appendix

Summary statistics of covariates

The summary statistics below show the mean, standard deviation, the minimum and maximum of each variable that at some point is used as a control in a regression in this paper (See Tables 6, 7).

Table 6 Conservative sample

	N	Mean	SD	Min	Max
Con. Vote Share	510	54.5978	7.700102	27.95855	76.72381
Funded	510	.0980392	.2976596	0	1
Log. population	510	11.51805	.110493	11.22638	11.86197
Const. age	510	11.48445	1.81678	7.729331	16.96716
Estimated Leave proportion	510	.5916512	.0626181	.4190592	.7496078
Social Payment per Capita	510	.008556	.0107164	0	.0729846
Std. Funding Amount	510	.12396	.2067627	0	.9234379
Lag. con. vote	510	50.05452	7.368815	26.33	69.91752

Table 7 Labour sample

	N	Mean	SD	Min	Max
Con. Vote Share	332	33.27844	10.47469	6.63736	63.06292
Funded	333	.1321321	.339144	0	1
Log. population	333	11.48617	.1052773	11.1961	11.87054
Const. age	333	12.79897	1.505377	9.401559	18.79296
Estimated Leave proportion	333	.5944446	.0745589	.3392147	.7212337
Social Payment per Capita	333	.0157422	.0156949	0	.0822203
Std. Funding Amount	333	.1712836	.2263562	0	1
Lag. con. vote	333	29.95301	9.408435	6.63736	48.11368

Full regression tables with Brexit leave vote share control

See Tables 8 and 9

Table 8 Regression results on Conservative vote share (Con. Sample)

	(1)	(2)	(3)	(4)	(5)	(6)
Funded	2.23**	2.50***	3.55***			2.56***
	(1.02)	(0.82)	(0.79)			(0.77)
Std. funding amount				6.91		
				(6.76)		
Priority					1.76	
					(1.75)	
Social payment per capita						189.56***
						(31.65)
Lag. con. vote		0.80***	0.53***	0.47**	0.46**	0.50***
		(0.12)	(0.12)	(0.19)	(0.18)	(0.11)
Log. population			44.51*	111.44**	96.05**	42.38*
			(23.60)	(48.91)	(45.08)	(22.53)
Const. age			8.58***	7.75***	7.82***	7.49***
			(1.29)	(1.97)	(2.01)	(1.20)
Estimated Leave proportion			260.50**	69.34*	74.96**	264.41**
			(121.85)	(37.58)	(35.54)	(118.94)
Constant	59.62***	15.27*	−738.24**	−1396.07**	−1222.44**	−704.20**
	(2.83)	(8.69)	(332.59)	(543.29)	(502.47)	(318.00)
Const. fixed-effect	✓	✓	✓	✓	✓	✓
Observations	680	510	510	150	150	510

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in included in parentheses. This table provides the results of the OLS results containing constituencyfixed effects as well as controls (population and share of population older than 50) and a lagged conservative vote share variable. The last model includes an economic control that aims to test the mechanism that is at work. The Economic control variable consists of a time-varying variable that measures the share of households within a constituency receiving universal credit

Table 9 Main regression results on Conservative vote share (Lab. Sample)

	(1)	(2)	(3)	(4)	(5)	(6)
Funded	3.72***	1.92	1.85*			1.92*
	(1.30)	(1.22)	(1.09)			(1.09)
Std. funding amount				1.01		
				(8.85)		
Priority					−4.03**	
					(1.76)	
Social payment per capita						114.78**
						(44.45)
Lag. con. vote		0.39***	0.38***	0.37**	0.36**	0.40***
		(0.10)	(0.09)	(0.14)	(0.14)	(0.09)
Log. population			119.15***	128.87*	109.84*	117.46***
			(38.78)	(66.28)	(60.72)	(36.23)
Const. age			10.03***	12.98***	14.63***	9.30***
			(1.36)	(2.23)	(2.09)	(1.31)
Estimated Leave proportion			273.18***	−569.02***	−534.70***	261.66***
			(33.55)	(179.61)	(162.22)	(29.26)
Constant	26.12***	16.83***	−1659.73***	−1219.87*	−1043.86*	−1624.63***
	(1.28)	(2.97)	(446.05)	(633.84)	(583.88)	(417.13)
Const. fixed-effect	✓	✓	✓	✓	✓	✓
Observations	443	332	332	132	132	332

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in included in parentheses. This table provides the results of the OLS results containing constituency fixed effects as well as controls (log. population and share of population older than 50) and a lagged conservative vote share variable. The last model includes an economic control that aims to test the mechanism that is at work. The Economic control variable consists of a time-varying variable that measures the share of households within a constituency receiving universal credit

Only constituency fixed-effects and controls

Table 10 Regression results on Conservative vote share (Con. Sample)

	(1)	(2)	(3)	(4)	(5)
Funded	2.23**	3.64***			2.57***
	(1.02)	(0.87)			(0.87)
Std. funding amount			7.51		
			(5.41)		
Priority				2.19	
				(1.71)	
Social payment per capita					204.95***
					(33.59)
Log. population		42.63*	131.57***	114.63***	40.44
		(22.42)	(42.44)	(40.65)	(24.87)
Const. age		11.10***	9.66***	9.63***	9.78***
		(1.15)	(1.77)	(1.79)	(1.25)
Constant	59.62***	−536.76**	−1575.76***	−1379.24***	−498.51*
	(2.83)	(246.08)	(482.54)	(462.64)	(272.68)
Const. fixed-effect	✓	✓	✓	✓	✓
Observations	680	510	150	150	510

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in included in parentheses. This table provides the results of the OLS results containing constituency fixed effects as well as controls (log. population and share of population older than 50). The last model includes an economic control that aims to test the mechanism that is at work. The Economic control variable consists of a time-varying variable that measures the share of households within a constituency receiving universal credit

Table 11 Regression results on Conservative vote share (Lab. Sample)

	(1)	(2)	(3)	(4)	(5)
Funded	3.72***	2.89**			2.94**
	(1.30)	(1.13)			(1.18)
Std. funding amount			5.14		
			(7.17)		
Priority				−4.15**	
				(1.88)	
Social payment per capita					−128.76**
					(50.78)
Log. population		97.16**	110.20	83.02	71.95*
		(38.05)	(68.24)	(65.66)	(39.11)
Const. age		10.54***	13.37***	15.25***	9.30***
		(1.14)	(2.16)	(2.21)	(1.31)
Constant	26.12***	−1267.50***	−1401.42*	−1108.89	−937.58**
	(1.28)	(434.02)	(776.25)	(746.00)	(452.30)
Const. fixed-effect	✓	✓	✓	✓	✓
Observations	443	332	132	132	332

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in included in parentheses. This table provides the results of the OLS results containing constituency fixed effects as well as controls (log. population and share of population older than 50). The last model includes an economic control that aims to test the mechanism that is at work. The Economic control variable consists of a time-varying variable that measures the share of households within a constituency receiving universal credit

Full models with GMM estimations

See Tables

Table 12 Regression results on Conservative vote share (Con. Sample)

	(1)	(2)	(3)	(4)	(5)
Funded	2.51***	3.62***			2.55***
	(0.63)	(0.70)			(0.68)
Std. funding amount			7.54		
			(5.29)		
Priority				1.80	
				(1.63)	
Social payment per capita					198.99***
					(34.11)
Lag. con. vote	0.81***	0.52***	0.45***	0.45***	0.49***
	(0.11)	(0.11)	(0.17)	(0.16)	(0.11)
Log. population		43.37*	114.12**	97.38**	40.68*
		(24.94)	(47.61)	(44.75)	(23.80)
Const. age		9.05***	8.10***	8.21***	7.89***
		(1.42)	(2.14)	(2.14)	(1.31)
Constant	15.09**	−552.77**	−1373.26**	−1180.89**	−508.80*
	(6.39)	(272.59)	(538.42)	(506.93)	(260.11)
Const. fixed-effect	✓	✓	✓	✓	✓
Observations	510	510	150	150	510

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in included in parentheses. This table provides the results of GMM estimations containing constituency fixed effects as well as controls (log. population and share of population older than 50) and a lagged Conservative vote share variable. The last model includes an economic control that aims to test the mechanism that is at work. The Economic control variable consists of a time-varying variable that measures the share of households within a constituency receiving universal credit

Table 13 Regression results on Conservative vote share (Lab. Sample)

	(1)	(2)	(3)	(4)	(5)
Funded	2.01**	1.96*			2.03*
	(0.93)	(1.00)			(1.03)
Std. funding amount			1.74		
			(10.23)		
Priority				−4.40***	
				(1.62)	
Social payment per capita					122.36***
					(46.11)
Lag. con. vote	0.38***	0.37***	0.35***	0.36***	0.39***
	(0.09)	(0.08)	(0.13)	(0.13)	(0.08)
Log. population		119.70***	127.50*	105.04	117.86***
		(40.98)	(69.44)	(64.92)	(37.57)
Const. age		10.30***	13.35***	15.21***	9.48***
		(1.42)	(2.26)	(2.02)	(1.37)
Constant	17.03***	−1533.94***	−1607.37**	−1369.71*	−1501.57***
	(2.20)	(465.61)	(790.62)	(740.00)	(427.60)
Const. fixed-effect	✓	✓	✓	✓	✓
Observations	332	332	132	132	332

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in included in parentheses. This table provides the results of GMM estimations containing constituency fixed effects as well as controls (log. population and share of population older than 50) and a lagged Conservative vote share variable. The last model includes an economic control that aims to test the mechanism that is at work. The Economic control variable consists of a time-varying variable that measures the share of households within a constituency receiving universal credit

Common trend with lagged vote share
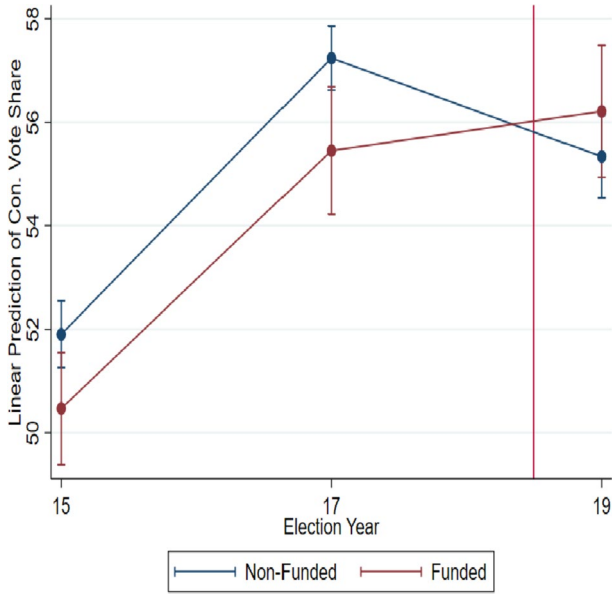
See Figs. 7 and 8

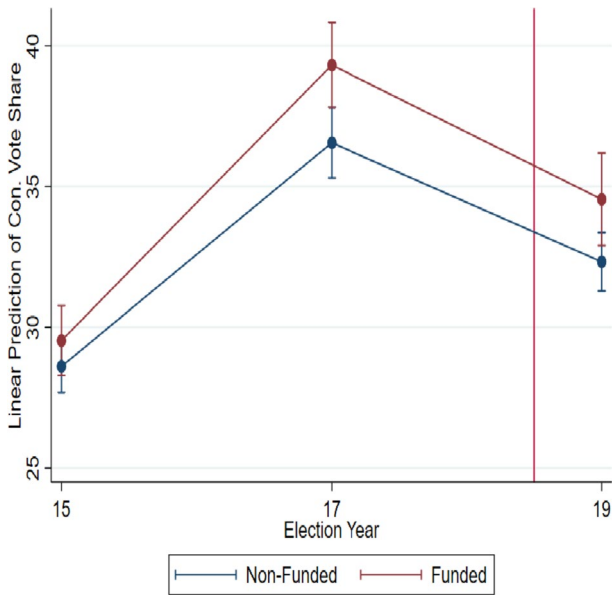**Fig. 7** Common trend of Con. sample with lagged vote share

Fig. 8 Common trend of Lab. Sample with lagged vote share

Interaction effects with robustness specifications

See Tables 14 and 15

Table 14 Regression results on Conservative vote share (Con. Sample)

	(1)	(2)	(3)	(4)
Priority	7.74***	12.61***		
	(1.90)	(1.83)		
Priority × close Elec		−2.63		
		(2.49)		
Std. funding amount			2.78	23.04***
			(7.02)	(5.43)
Std. funding amount × close Elec				−5.22
				(6.01)
Log. population	1.12	−1.69	6.57	7.45
	(6.20)	(6.69)	(7.25)	(5.70)
Const. age	−0.29	0.66**	−0.56*	0.30
	(0.30)	(0.33)	(0.33)	(0.27)
Close Elec		−6.53***		−7.20***
		(1.45)		(1.40)
Constant	39.58	67.14	−20.18	−35.28
	(70.20)	(75.73)	(82.09)	(64.74)
Region. Fixed-Effect	✓	✓	✓	✓
Observations	150	150	150	150

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in included in parentheses. This table provides the results of the OLS results containing region-fixed effects as well as controls (population and share of population older than 50). The last model includes an economic control that aims to test the mechanism that is at work. The Economic control variable consists of a time-varying variable that measures the share of households within a constituency receiving universal credit

Table 15 Regression results on Conservative vote share (Labour. Sample)

	(1)	(2)	(3)	(4)
Priority	−7.82**	7.49***		
	(3.22)	(2.31)		
Priority × close Elec		3.10		
		(3.44)		
Std. funding amount			3.48	18.03***
			(12.72)	(5.85)
Std. funding amount × close Elec				−0.95
				(7.37)
Log. population	7.79	13.25	13.23	11.08
	(11.61)	(8.28)	(11.86)	(8.15)
Const. age	−2.03*	0.32	−2.70**	0.55
	(1.09)	(0.85)	(1.08)	(0.81)
Close Elec		17.92***		16.42***
		(1.56)		(1.66)
Constant	−27.35	−133.52	−81.16	−111.41
	(126.90)	(89.82)	(130.61)	(87.40)
Region. Fixed-Effect	✓	✓	✓	✓
Observations	132	132	132	132

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in included in parentheses. This table provides the results of the OLS results containing region-fixed effects as well as controls (population and share of population older than 50). The last model includes an economic control that aims to test the mechanism that is at work. The Economic control variable consists of a time-varying variable that measures the share of households within a constituency receiving universal credit

Test for constituency labour incumbent effect

See Table 16

Table 16 Regression results on Labour vote share (Lab. Sample)

	(1)	(2)	(3)	(4)	(5)	(6)
Funded	−1.68	0.81	1.19			1.23
	(1.41)	(1.17)	(1.20)			(1.16)
Std. funding amount				−2.81		
				(7.14)		
Priority					−4.53**	
					(1.80)	
Social payment per capita						114.01***
						(41.37)
Lag. con. vote		−0.73***	−0.65***	−0.37**	−0.39***	−0.63***
		(0.11)	(0.09)	(0.15)	(0.14)	(0.10)
Log. population			324.73***	281.25***	266.08***	324.46***
			(39.74)	(66.52)	(63.74)	(42.45)
Const. age			1.27	1.30	3.00	0.53
			(1.19)	(2.09)	(2.13)	(1.42)
Constant	53.21***	75.53***	−3709.35***	−3220.66***	−3065.92***	−3696.34***
	(3.94)	(4.48)	(453.37)	(757.07)	(724.70)	(483.60)
Const. fixed-effect	✓	✓	✓	✓	✓	✓
Observations	444	333	333	132	132	333

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in included in parentheses. This table provides the results of the OLS results containing constituency fixed effects aswell as controls (population and share of population older than 50) and a lagged Conservative Vote share variable. The last model includes an economic control that aims to test the mechanism that is at work. The Economic control variable consists of a time-varying variable that measures the share of households within a constituency receiving universal credit

Robustness economic mechanism

See Table 17

Table 17 Regression results on Conservative vote share (both samples)

	(1)	(2)	(3)	(4)
Funded	3.31***	2.13**	1.93**	1.82*
	(0.76)	(1.01)	(0.76)	(1.09)
Log social payment per capita	1.90***	1.71***		
	(0.25)	(0.29)		
Legacy share			−262.77***	−148.36***
			(33.51)	(46.31)
Lag. con. vote	0.35***	0.33***	0.43***	0.41***
	(0.13)	(0.08)	(0.11)	(0.08)
Log. population	−20.01	63.21	20.23	91.88**
	(29.53)	(42.66)	(21.04)	(36.86)
Const. age	7.10***	7.47***	6.89***	8.56***
	(1.39)	(1.23)	(1.11)	(1.32)
Constant	202.53	−825.13*	−241.68	−1164.02***
	(326.02)	(488.67)	(230.99)	(425.24)
Const. fixed-effect	✓	✓	✓	✓
Observations	444	299	510	332

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in included in parentheses. This table provides the results of the OLS results containing constituency fixed effects as well as controls (population and share of population older than 50) and a lagged conservative vote share variable. Model 1 and 3 examine the Conservative sample and model 2 and 4 examine the Labour sample. To test the robustness of the findings in this paper, now in model 1 and 2 the Economic control variable consists of a logged time-varying variable that measures the share of households within a constituency receiving universal credit. In model 3 and 4 I further include an alternative economic

Robustness specifications with lag. vote share

See Fig. 9

Fig. 9 Effect of funding with robustness specifications by sample. *Note: * p < 0.10, ** p < 0.05, *** p < 0.01* This coefficients plot depicts the point estimates of the treatment on Conservative vote share. Each point estimate is computed with a different specification that is indicated on the y-axis. The x-axis shows the estimated effect whereas one unit is defined as one percentage point. 95% confidence intervals are indicated

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