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**Article:**

Pattie, C.J., Whitworth, A. and Johnston, R.J. (2015) Does campaign contact influence individuals' vote choices? An alternative approach. *European Political Science*. Published Online 8 May 2015. ISSN 1682-0983

<https://doi.org/10.1057/eps.2015.24>

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# Does campaign contact influence individuals' vote choices?

## An alternative approach

### Abstract

Analyses of local campaign effects are dominated by aggregate-level analyses of constituency activity. Though individual-level data are available on whether voters are (or remember being) contacted by parties during campaigns, their analysis is fraught with difficulties, not least the extent to which memory of campaign contact is itself conditioned partly on party allegiance, creating a circularity in the analysis of the impact of party contact on vote choice. To some degree, this can be (and has been) dealt with in a regression framework. However, this does not fully deal with the potential difficulties. Ideally, experimental approaches are needed to tease out definitively the effects of campaign exposure on individual's election decisions. However, these present practical difficulties. In this paper, therefore, we utilise quasi-experimental difference-in-difference and propensity score matching methods to estimate campaign effects at the 2010 British General Election from individual-level data.

The constituency campaign effects literature in Great Britain is dominated by aggregate, constituency-level analyses (e.g. Johnston et al., 2011). As a result of this research, it is now widely accepted that spatially-varying local campaigns pay electoral dividends for parties. However, while the aggregate relationship is well-established, demonstrating the micro-foundations of the constituency campaign is more problematic: how might we uncover the impact of the local campaign on individual voters?

Most attempts to do so utilise data from election surveys which ask respondents whether they were contacted by individual parties' constituency campaigns during the election. Those reporting being contacted by a party are, other things being equal, more likely to vote for that party than are those who do not report such contacts (e.g. Denver et al., 2004; Pattie and Johnston, 2010; Clarke et al., 2004, 2009; Johnston, Cutts et al., 2012). What is more, self-reported measures of campaign contact correlate well with other measures of campaign intensity, suggesting they are good indicators of exposure to the campaign: voters are more likely to report being contacted by a party during an election campaign if they live in a constituency where that party mounted an intense campaign than where its campaign was less active (Pattie et al., 1994; Denver et al., 2004; Johnston, Pattie et al., 2012).

Using self-reported campaign exposure raises serious methodological problems, however, as respondents' recollections of being contacted during a campaign are not independent of their partisan leanings. On the contrary, individuals are more likely to remember being contacted by a party they already support than by one they do not favour. This introduces a threat of substantial selection biases: any apparent correlation between self-reported contact from a campaign and vote may simply be an artefact of the tendency for those already pre-disposed

to a party (and hence likely to vote for it anyway) to be the most likely to remember being contacted by it.

Although there is extensive evidence of substantial campaign effects in Great Britain through aggregate analyses, therefore, there is a need for more robust micro-level foundations. One strategy which would get around the selection bias difficulties inherent in conventional observational survey research would be to employ large-scale field experiments, similar to those now widely used to analyse non-partisan get-out-the-vote measures (see e.g. Green and Gerber, 2004). Unfortunately, these are more difficult to conduct where there are partisan implications. But it is possible, with careful analysis of well-constructed individual survey instruments, to move beyond aggregate studies and to attack the problem through alternative individual-level approaches. In this paper, we do so by applying both a difference-in-difference (DiD) approach and propensity score matching (PSM) methods to individual-level survey data. For reasons discussed further in the paper, our preference is for the DiD approach, but we employ PSM to provide an alternative cross-check of the DiD results.

### **Dealing with endogeneity**

A key problem in using survey data to analyse the impact of campaign activity is the endogeneity of self-reported party campaign contact and vote intention. That endogeneity can arise for at least two different reasons. First, parties use their canvassing activities to build databases recording electors' likely vote choices. This information allows them to focus on mobilising potential supporters come the general election, while avoiding contact with those unlikely to support the party. A simple correlation between contact and vote, therefore, might merely reflect how effectively parties identify those already leaning towards them. Second,

individuals may be more likely to remember being contacted by parties they already support than by parties they do not, so that self-reported campaign contact measures may exaggerate the impact of campaign activity on vote choice. The potential scale of the issue can be gauged using data from the 2010 British Election Study Campaign Internet Panel Study (BES CIPS), which interviewed a large national sample before, during and after the election campaign (13,334 individuals answered both the pre- and post-election waves of the survey: technical details of the survey are available via the BES website, at <http://bes2009-10.org/>). Respondents to the post-election wave were asked how they had voted in the election, plus whether (and how) they had been contacted by the parties during the campaign.

Our main interest here is in those who reported some form of face-to-face contact with a party's campaign, whether at home or in the street (7% of respondents reported being visited at home by Labour and 2% reported being contacted by the party in the street; the equivalent figures for contact by the Conservatives were 9% and 3% respectively; for the Liberal Democrats, they were 5% and 2%). Both forms of contacting voters are liable to be more common in competitive than in uncompetitive seats. What is more, canvassing voters at home is often targeted at particular neighbourhoods in the constituency based, increasingly, on geodemographic profiling of residents' characteristics and expenditure habits (Farrell and Webb, 2002; Cutts, 2006; Fisher and Denver, 2008).<sup>1</sup> This is not always true of other forms of contact. Contacts via electronic media in the UK has largely been co-ordinated by the party nationally with no necessary linkage to the constituency campaign. And while considerable effort was expended on telephone campaigns to complement local canvassing, for instance, some telephone calls will have been to party supporters, eliciting financial support, rather than to voters in key battleground constituencies. In any case, contact via telephones and new

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<sup>1</sup> The analyses reported below utilise a measure of face-to-face campaigning which combines both contact at home and contact in the street. We have also repeated all analyses utilising only reported contact at home. The results, not reproduced here, replicate the findings reported in this paper.

social media were comparatively rare in 2010. Leafleting, meanwhile, remains a largely locally-organised and funded activity. But the relative ubiquity of campaign leaflets and the undoubted tendency for most to move direct from the doormat to the rubbish bin with only the most limited of scrutiny by the voter renders exposure to them a rather crude indicator. In any case, as discussed below, analyses later in the paper control for party constituency spending. As printing costs accounted for 85% of local party expenditure during the 2010 official ‘short’ campaign (and the bulk of that expenditure was on leaflets and similar material), this largely captures the effect of leafleting.<sup>2</sup>

Our first step is to estimate how much difference face-to-face contact with a party’s campaign made to an individual’s chance of voting for the party. Clearly, we cannot simply compare support for a party among those who did, and those who did not, report being contacted by it during the campaign. Two factors will lead to potentially very substantial over-estimates of the campaign effect. First, voters may be more likely to recall being contacted by parties they actually voted for than by parties they did not support. Second, parties do not contact all voters willy-nilly. Rather, they focus on two groups in particular in their target seats: known supporters, to ensure they turn out to vote (mobilisation); and identified waverers, who might be persuaded to vote for the party (conversion). As a result, we would expect substantially greater voting for a party among those who did recall a contact with it than among those who did not, even if there was no independent campaign effect whatever, purely because the former group will be pre-disposed to support the party. (In reality, the mobilisation effect – ensuring known supporters do turn out – is likely to be very important in the last weeks of an election campaign. Conversion of waverers is likely to be much harder during such brief

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<sup>2</sup> We have, however, repeated our analyses with the measures of self-reported campaign contact by each party extended to include not only contacts at home or in the street, but also via leaflets. The results, not reported here, are entirely consistent with our overall argument. Effect sizes for the more extensive measure of self-reported contact are generally smaller than for the face-to-face contact measures. But all are significant and in the expected directions.

contacts – though not impossible, not least among those already inclined towards the party or considering a tactical vote – especially for local constituency campaigners.)

So how might we deal with this? The gold standard is randomized field experiments, employed with great success in both the US and the UK to look at the impact of get-out-the-vote campaigns (Green and Gerber, 2004; John and Brannan, 2008; Gerber et al., 2008, 2010; Davenport et al., 2010; Panagopoulos, 2010; Fieldhouse et al., 2013). In these studies, participants are assigned at random to different groups: a control group which receives no encouragement to vote; and at least one experimental group, which is given a get-out-the-vote incentive. Whether participants go on to vote or not (but not how they vote) is ascertained from official records. And as individuals are randomly assigned to groups, differences in turnout between groups can be attributed to the treatments themselves (i.e. to different modes of campaigning) rather than to compositional differences in the sorts of individuals in each group.

Comparisons of field experiments and conventional surveys employing respondents' self-reported exposure to campaigns and votes suggest that the latter very substantially overestimate the effectiveness of campaigns (e.g. Vavreck, 2007). In principle, therefore, one might consider a similar controlled field experiment to investigate the impact of different partisan messages on vote choice in which individuals are randomly assigned to different groups, one not being exposed to any party campaign, one canvassed by Labour alone, another by the Conservatives only, and so on. (This approach was adopted in pioneering work by Bochel and Denver, 1971.) However, while actual turnout can be confirmed, it is not possible to ascertain from official election returns which parties particular individuals voted for: experimental designs therefore have to rely on self-reported voting in a post-election

survey. It is notable that those studies which have used randomized field experiments to study the effect of campaign contact on vote choice have tended to be conducted in the USA and concentrate on relatively low-level elections (e.g. primary elections for the Texas Supreme Court; a Midwestern county legislature) and rely either on post-election survey self-reported vote or on precinct vote to measure the partisan impact (see e.g. Shaw et al., 2012; Barton et al., 2013). A threat of selection bias therefore remains. Furthermore, the prospects of obtaining agreement from the political parties to conduct such a study during a UK general election (which would require their agreement to give up control of their own campaigns in at least some key battleground seats) are, it must be said, limited in the extreme.

An alternative approach is to conduct a laboratory experiment (e.g. Ansolabehere and Iyengar, 1995; Norris et al., 1999). This does allow for genuinely random assignment of individuals to treatment groups. However, because they are conducted under laboratory conditions rather than in the midst of real elections, such studies suffer from inevitable doubts regarding their wider applicability: no matter how carefully constructed the experiment, it cannot replicate an actual vote decision as nothing depends on the participants' opinions. There is an obvious concern, therefore, over the results' face validity.

### **A quasi-experimental approach**

How can we move forward? Interestingly, Vavreck (2007), while showing that self-reports can lead to substantial over-estimates of campaign effects, also suggests a way out: when regression models predicting self-reported turnout from self-reported campaign contact control for factors such as interest in politics (which might be related to the tendency to over-report both voting and exposure to the campaign), the effect sizes become comparable to those from randomised experiments. There remain, however, potentially serious problems of



bias with this standard regression approach and it is not possible within such analyses to know how large or small such remaining biases within results are.

In this paper, we use two strategies for the analysis of individual-level campaign effects using conventional survey data.<sup>3</sup> Both control for potential biases caused by variations between treatment and control groups, including possible selection biases. The general approach can be illustrated by thinking of how we might estimate the extent to which support for party X changes over the course of an election campaign, and in particular whether being contacted by that party stimulates support. Ideally, we would follow two groups of voters throughout the campaign, one (the treatment group) which is contacted by the party and the other (the control) which is not, and measure support for the party before and after the election for both groups. Say support for party X among the treatment group rose from 30% to 40% over the course of the campaign, while among the control group support rose from 25% to 32% (figure 1). A simple estimate of the effect of contact would compare the start- and end-points for the treatment group, suggesting its campaign raised X's vote by 10 percentage points (the vertical distance between points A and B in figure 1). But there is no control group against which to compare. Perhaps it would be better to compare the post-election levels of support for party X in the treatment group with the same for the control group, suggesting a more modest campaign effect of 8 percentage points (the vertical distance between D and B in figure 1)? But without any campaign contact, support for party X still rose by 7 percentage points over the course of the campaign in the control group (from C to D). This suggests that some of the increase in support for X among the treatment group might have happened anyway. Even if the treatment group had not been contacted, therefore, it is quite possible that it, too, would have seen support for X go up by the same amount as in the control group,

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<sup>3</sup> We have also run similar models using the conventional approach of simple multivariate regression models with control variables (available in on-line appendix 1): the results produce campaign contact effects which are similar in magnitude to those discussed below.

rising from 30% to 37% (represented in figure 1 by the dotted line from A to E). The gap between the latter figure and the actual post-election level of support for X (40%) gives us our best estimate of the independent effect of the campaign (the gap between E and B in figure 1, a 3 percentage point rise), as it removes the general drift in support for the party.<sup>4</sup>

One way to achieve this is to employ difference-in-difference (DiD) methods (see e.g. Burden et al., 2013). The most common DiD application in survey-based studies of campaign effects uses a conventional regression model to hold constant other relevant influences on vote choice (see e.g. Clarke et al., 2004, 2009; Pattie and Johnston, 2010; Fisher et al., 2011). In this paper, we use an alternative DiD method which exploits the fact that each respondent to the 2010 BES CIPS was interviewed both before and after the election by including both observations as separate cases in the data set, and employs an adapted regression modelling approach in which the key variable of interest is an interaction between the time period (before or after) and exposure to the intervention, in our case campaign contact (Machin et al., 2004; Meghir and Palme, 2005; Wilkinson and McLennan, 2012; Ikenwilo, 2013). We refer to this as the ‘DiD with interaction’ approach.

We also employ a second method – propensity score matching (PSM) – to cross-check the estimates produced by our DiD with interaction approach. PSM uses a somewhat different strategy. It builds and uses a propensity score to match individuals exposed to the intervention to individuals not exposed to the intervention but otherwise similar across their

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<sup>4</sup> Formally, we can write the difference-in-difference estimate as

$$\text{Impact} = (Y_{t1} - Y_{t0}) - (Y_{c1} - Y_{c0})$$

where  $Y_{t0}$  is pre-election probability of voting for a party,  $Y_{t1}$  is the probability of actually voting for it,  $t$  is the treatment group (those reporting being contacted by the party during the campaign) and  $c$  is the control group (those who report no contact).

set of modelled characteristics and then assesses the difference in outcomes between the two matched groups (Dehejia and Wahba, 2002; Bryson et al., 2002; Jalan and Ravallion, 2003).

In the absence of data from genuine experimental work both approaches have the potential to provide better estimates of campaign effects than the simple ‘conventional regression’ approach to statistical control. That said, it should be borne in mind that, throughout these analyses, we rely on individuals’ self-reported accounts of their vote intentions before and during the ‘official campaign period’ of 4-6 weeks prior to polling day, how they actually voted, plus whether (and by who) they were contacted during the campaign, and we have no means of externally verifying these self-reports against actual votes or details of party canvassing activities. Inevitably, this means we cannot be entirely sure we have completely identified the effects of campaign contact

Both of the methods utilised here use regression models at some stage, whether to produce the main analysis (DiD with interaction) or as a step towards producing matched samples for comparison (PSM). To ensure comparability, therefore, the same explanatory variables are used throughout. So what might affect whether an individual reports being contacted face-to-face by a party during the election campaign?

In part, this will be driven by pre-existing party preferences. Parties concentrate their attention on those who already support them, or who are undecided (for instance, floating voters, previous abstainers, or new voters), rather than on supporters of their rivals (why mobilise those who will vote against you?). Through canvassing before an election they will have some idea of who many of these individuals are, or at least the neighbourhoods in which most of them live (Johnston, Cutts et al., 2012). In addition, voters are more likely to recall

being contacted by a party they already support than by one they do not. We therefore control for self-reported vote at the previous General Election in 2005 and voters' party identification on the eve of the 2010 election (this also controls for many 'background' socio-demographic correlates of voting). In addition, parties campaign hardest in those seats in which they face the most intense competition and least in seats where they are bound to either lose or win (Pattie and Johnston, 2003; Johnston et al., 2013). To capture this we include the amount each party spent on its 2010 'short campaign' (the period from the dissolution of Parliament to the date of the election) in each respondent's constituency, expressed as a percentage of the legal maximum expenditure permitted there.<sup>5</sup> It is also likely that those who actively contact their political representatives will be more likely to pay attention to the campaign and to be included in parties' data bases of voters to contact during the campaign compared with voters who do not contact politicians. We capture this using a question in the BES pre-election survey wave asking respondents whether they had sought help from their local MP. Finally, we expect that parties will be more likely to contact (and the contact to be remembered by) those who pay close attention to politics than those who pay little or no attention. This is measured by individuals' pre-election ratings, on an 11-point scale, of how much attention they pay to politics (the responses are coded so that high scores indicate most attention).

### **Tackling selection bias through using difference-in-difference (DiD) with interaction**

A conventional approach would be to conduct a regression analysis with vote choice as the dependent variable and self-reported campaign contact as the key independent variable, while controlling for the various factors identified above. While common and easy to implement,

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<sup>5</sup> The amount candidates can spend on their constituency campaigns is tightly regulated in UK elections. The legally-permitted spending limit in each seat is largely a function of the size of the electorate there. As electorates vary from seat to seat, so does the legal maximum. To provide a standardised measure of campaign intensity which is not conflated with constituency size, therefore, we express the amount spent by each candidate as a percentage of the legal maximum expenditure allowed in their seat.

however, this comes with a number of problems, not least the risk of selection bias. If self-reported contact with a party's campaign and vote choice are both influenced by a common factor – for instance, by pre-existing support for the party – there is a risk that the estimator for impact of contact on vote might be mis-specified. We need some means of minimising this risk. To deal with this, we therefore extend the regression approach by fitting a DiD model using interactions.<sup>6</sup>

At its core, DiD with interaction compares change over time in the behaviour being examined for those who are, and those who are not, exposed to some sort of intervention. The 2010 BES CIPS is well-suited for this as we know vote intention at the start of the campaign and actual vote at the election four weeks later on for individuals who are contacted by each party and for those who are not.

To fit the DiD with interactions model, the data are structured so that each BES panel respondent provides two cases. The first records that individual's position in the pre-election wave, about a month before election day. In that wave individuals were asked whether they had decided how they would vote in the upcoming election and, if they had, whether they would definitely vote for the party, or were leaning to it, though not yet firmly committed. Here, only definite intentions are treated as pre-election support for the party and 'leaners' are grouped with those who were then thinking of either voting for another party or abstaining (11% of the sample said before the campaign that they were either unsure of how they would vote or were intending to abstain; 22% said they were 'leaning' towards a party but were as yet uncommitted; and 67% said they had definitely decided which party they would vote for). Controlling for the 'definite' intentions provides a strong test for campaign effects,

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<sup>6</sup> See Ashenfelter and Card (1985) and Card and Kreuger (1994)

as around 80% of those who said before the start of the campaign that they definitely intended voting Labour, Conservative or Liberal Democrat actually did go on to report doing so: by comparison, only 55% of pre-election Labour ‘leaners’ said afterwards that they had voted for the party, as did 63% of Conservative and 71% of Liberal Democrat leaners. What is more, only a relative handful reported leaning towards each of the major parties (5% for Labour, 6% for the Conservatives and 6% for the Liberal Democrats.)<sup>7</sup> We use this expressed intention as the pre-election score for the dependent variable, vote. The second case for each respondent, taken from the post-election wave of the survey, records his or her reported vote, with the dependent variable coded to indicate this reported actual vote choice. In addition, each case has: a time variable (coded 0 for the pre-election cases and 1 for the post-election cases); dummy variables for exposure to each of the three main parties’ face-to-face campaigns (coded 1 if the respondent reported being contacted by the party during the campaign and 0 otherwise: hence in all cases these variables are coded 0 for cases at the start of the election campaign); interactions between the time and campaign contact variables; and the explanatory variables discussed above. In each model, we include contact and interaction terms for reported exposure to the campaign of the party whose vote is being analysed only, since this eases comparison with the PSM results discussed in the penultimate section of the paper. (The PSM approach is based on estimating the effect of contact by one party at a time: it does not allow us to estimate simultaneously the effects of contact by more than one party.) However, it should be noted that as all the models reported here already contain variables for all three parties’ constituency campaign expenditure, much of the effect of campaigning by rival parties has already been factored into the results.

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<sup>7</sup> The model can clearly be run with different specifications of the pre-election vote measure – definite voters only, definite voters plus ‘leaners’, and so on. The core results are robust under both specifications.

Although the dependent variable is binary in form, we employ OLS regression here. Given that interaction terms in logit or probit models are not interpretable in the same way as in linear models (Ai and Norton, 2003), and hence would not readily capture the true difference-in-difference with interaction estimate, the application of linear DiD models to binary outcomes is more appropriate and is commonplace in the literature (e.g. Weinick et al., 2000; Fu et al., 2007; Liu et al, 2010). We are particularly interested in the coefficients for the interaction terms since these are the estimates of the impact of contact on voting, other things being equal and after seeking to mitigate the problem of selection biases.

The DiD with interaction model results are shown in table 1. The variables for face-to-face contact show that the direct effects of campaign contact on vote are significant and positive in all three equations: those who recalled being contacted by a party were more likely to report either intending to vote for it before the election or actually doing so on polling day than were those who did not recall being contacted. Because the models include the interaction terms, the direct effect of campaign contact applies only to the pre-campaign period (where the variable for time and hence also the interaction term take values of 0). It shows, therefore, that reporting being contacted by a party during the campaign is associated with an intention to vote for it at the start of the campaign, either because parties disproportionately target those voters already leaning towards them in order to mobilise their supporters, or because respondents are more likely to remember being contacted by parties they already support than by parties they do not. This may, of course, also reflect longer-term local campaigning by parties in the months preceding the calling of the election, and there is evidence suggesting that such campaigning does build up support for parties (Cutts, 2006; Johnston et al., 2011; Cutts et al., 2012).

However, this is not in itself evidence that face-to-face contact during the campaign makes a difference which is the real interest here. This is measured not by the direct effect of campaign contact but, rather, by the interactions between face-to-face contact and time. Two of the interaction coefficients, for face-to-face contact by the Labour campaign in the Labour vote model and for contact by the Liberal Democrats in the Liberal Democrat vote model, are significant at the 0.01 level and in the expected direction. Labour and Liberal Democrat face-to-face efforts did win them extra support during the campaign, over and above what they might have expected before the election. But the equivalent interaction term in the Conservative model does not quite reach conventional levels of statistical significance ( $p = 0.069$ ), though it is correctly signed. In other words, once we partial out the general drift of each party's support over the course of the campaign only the Labour and Liberal Democrat face-to-face campaigns emerge clearly as electoral assets for their parties.<sup>8</sup> We have also run analyses containing contact and interaction terms for all three parties' campaigns (the results are available in on-line appendix 2). The results are as expected: reporting being contacted by a party makes voters more likely to report voting for it, and less likely to report voting for its rivals.

The effect sizes are noteworthy too, being generally modest, though still respectable. Being contacted by Labour or the Liberal Democrats raised an individual's probability of voting for each party by around 0.08 (8 percentage points) compared to a similar individual who was not contacted – which is more than enough to make the difference between winning and losing in a substantial number of marginal constituencies.

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<sup>8</sup> In part, this may reflect the tactical situation at the time of the election. The Conservatives were ahead in the opinion polls and had by some margin the best-resourced national campaign of the three main parties. Labour, meanwhile, was behind and had more limited resources to expend. It expected to lose seats to the Conservatives and was concentrating its campaign efforts heavily on holding its most marginal seats. The Liberal Democrats, too, had only limited resources to expend on their national campaign, and were focussing on the battle in the marginals (both those they held and wanted to retain, and those where they were narrowly behind and hoped to gain).



## **Running a quasi-experiment: propensity score matching (PSM)**

To check that these results are not simply an artefact of the DiD with interactions method we also use an alternative estimation strategy, propensity score matching (PSM: Rosenbaum and Rubin, 1983; Dehejia and Wahba, 2002; Bryson et al., 2002; Jalan and Ravallion, 2003).

Exposure to each party's face-to-face campaign will never be universal and parties concentrate their efforts on contacting individuals who either are liable to vote for them or have a reasonable likelihood of being persuaded to do so, especially if they live in marginal seats which could easily change hands. But even the best-organised and resourced campaign will be unable to contact every individual it may wish to reach. Hence there will be individuals who have many of the characteristics of potential supporters the party might wish to contact but who will not be contacted. PSM identifies such individuals and matches them with ostensibly similar individuals who were contacted (or, in our case, who reported being contacted). Given that matched treatment and control individuals are, by definition, designed to be similar according to the observable characteristics (though how similar depends on the effectiveness of the matching as well as the importance of any unobserved characteristics) the effect of the intervention is assumed to be the difference in voting behaviour between the matched treatment and control groups. (For a critique of propensity score methods, see e.g. Arceneaux et al., 2006, 2010.)

To achieve this matching, our PSM approach uses logit models to predict each respondent's probability of exposure to the campaign (their propensity score) given their set of explanatory characteristics.<sup>9</sup> Individuals who did report contact are matched with individuals who did not

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<sup>9</sup> To conduct the PSM we make use of the `psmatch2` command in Stata (Leuven and Sianesi, 2003).

but whose propensity scores are similar; in other words, matched individuals are similarly likely to report being contacted given their explanatory characteristics, but only one of them actually reported being contacted. Various matching algorithms are available: we used single nearest neighbour matching (i.e. matching each contacted individual to the one uncontacted individual with the nearest propensity score). After the matching process has been completed only the treatment (i.e. contacted) and matched control (i.e. uncontacted) cases are retained for analyses. This matched sample is similar to a randomised experimental design, in that the underlying characteristics of the treatment and control groups are designed to be similar across the observable characteristics, with the main exception being whether or not they were exposed to a party's face-to-face campaign. That said, the matching process is probabilistic, not exact,<sup>10</sup> and based inevitably only on observable characteristics available in the data (and hence vulnerable to omitted variable biases, which would not be expected with random allocation). The harder it is to model the likelihood of receiving the intervention (in our case is whether the individual was contacted or not), therefore, the larger the likely margin of error in the matching process is likely to be. Because of the potential for imprecision in the matching process, even in large probability samples, our inclination is to prefer a DiD to a PSM approach here. However, PSM gives us an alternative, and independent, estimate of the effects of campaign contact, so at least provides insight into whether our DiD estimates are of the right order of magnitude and direction.

Table 2 reports the logit models predicting who reported being contacted by each party's campaign. As expected, those already inclined towards each party (whether as party identifiers or past voters) were more likely to recall being contacted by it than were those who were not so inclined. Wider engagement with the political process also mattered.

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<sup>10</sup> Nor, indeed, random as in a true randomised control trial.

Respondents who had sought help from their MP were more likely to report being contacted by each party's campaign during the 2010 election than were respondents who had not and in addition the more attention individuals paid to politics the more likely they were to report being contacted by each party during the campaign. Finally, the more each party spent on its constituency campaign in each seat, the more likely individuals living there were to report being contacted by that party; countering this, the harder a party's rivals worked in a seat, the more likely voters living there were to report being contacted by those parties. The more the Liberal Democrats spent on their local campaign, for instance, the more likely respondents were to report being contacted not just by them but also by Labour and the Conservatives. Similarly, the harder Labour campaigned, the more likely respondents were to recall being contacted by the Conservatives.

These models are used to estimate a propensity score for each respondent which is then used to match contacted individuals with others who had a similar chance of being contacted given their characteristics but who did not report being contacted. Comparing voting behaviour across the matched treatment and control groups provides an alternative robust estimate of the impact of face-to-face canvassing on party support (table 3). Among these matched pairs, being contacted by a party raises the chances of voting for that party by between 5 and 10 percentage points over the chances of voting for it having not been contacted. As with the DiD models, the effects of campaign contact are larger for the Labour and Liberal Democrat than for the Conservative campaigns. Clearly, this is a small effect. But it is far from negligible and in a close contest could make all the difference between winning and losing a seat.

## **Conclusions**

The analyses reported here confirm the importance of local campaign effort in modern elections. Local campaigning pays electoral dividends. Individuals who are contacted by parties are more likely to vote for them than are individuals who are not. The methods employed here suggest that this is unlikely to be an artefact of survey response biases: no matter how we look at the data the same results recur, giving us confidence in their validity. However, the parties did not all receive the same rewards for their efforts. Both the DiD with interactions and the PSM results suggest Liberal Democrats and especially Labour obtained greater returns from their campaign contacts with voters than did the Conservatives. In part, this may reflect the long-standing observation that in recent elections the Conservatives tend to get fewer returns to their local campaigning than the other parties (e.g. Pattie et al., 1995; Denver et al., 2002). But it also reflects the parties' very different positions in the run-up to the 2010 elections (see also Fisher et al., 2011). The Conservatives were by far the best-resourced party of the three, but had a substantial job to do to win sufficient seats to form a government (a feat which would have required one of the largest swings from government to opposition of modern times). Their resources were spread widely, therefore. Labour and the Liberal Democrats, working in more straightened circumstances, had to focus their campaign resources in a relatively few places: that concentration seems to have helped them do relatively well where they were able to put up a strong local fight. A relatively effective constituency campaign could not compensate Labour for its failings in office. But it does seem to have helped Labour contain its losses somewhat. The party would almost certainly have gone down to an even more serious loss of its parliamentary base had its local campaigns been less effective.



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Table 1: Predicting the impact of self-reported face to face contact on voting at the 2010 election: difference-in-difference with time-intervention interaction OLS models with robust standard errors (source: 2010 BES Campaign Internet Panel)

	Vote 2010		
	Labour	Conservative	Liberal Democrat
Vote, 2005 (comparison = did not vote)			
Labour	0.1457**	0.0395**	0.0138
Conservative	-0.0001	0.2336**	-0.0104
Lib Dem	-0.0358**	0.0157+	0.2388**
Other	-0.0035	0.0179+	-0.0116
Too young	0.0605**	0.0302*	0.0508**
Don't know	0.0047	0.0323**	0.0325**
Party identification, pre-election (comparison = no party ID)			
Very strong Labour identification	0.6716**	-0.1485**	-0.0882**
Fairly strong Labour identification	0.4833**	-0.1138**	-0.0273*
Not very strong Labour identification	0.2278**	-0.0607**	0.0091
Very strong Conservative identification	-0.0568**	0.5953**	-0.1106**
Fairly strong Conservative identification	-0.0517**	0.5537**	-0.1059**
Not very strong Conservative identification	-0.0553**	0.3744**	-0.0666**
Very strong Liberal Democrat identification	-0.0188+	-0.1307**	0.5670**
Fairly strong Liberal Democrat identification	-0.0166+	-0.1067**	0.4781**
Not very strong Liberal Democrat identification	-0.0042	-0.0715**	0.2460**
Other	-0.0199*	-0.0382**	-0.0442
Respondent sought help from local MP (comparison = yes)			
No	0.0028	0.0017	-0.0010
Pre-election attention to politics (10=high)	0.0001	0.0052**	-0.0002
Labour short campaign spend %	0.0004**	-0.0001	-0.0003**
Conservative short campaign spend %	-0.0002*	0.0003**	0.0001
Lib Dem short campaign spend %	-0.0002**	-0.0001*	0.0005**
Labour face-to-face campaign contact	0.0386**		
Conservative face-to-face campaign contact		0.0286**	
Lib Dem face-to-face campaign contact			0.0281*
Time post-election (comparison=pre-election)	0.0308**	0.0503**	0.1412**
Time*Lab face-to-face campaign contact	0.0772**		
Time*Con face-to-face campaign contact		0.0233+	
Time*LD face-to-face campaign contact			0.0794**
Constant	0.0359	0.0389	0.0483
R <sup>2</sup>	0.5108	0.5869	0.3766
N	23410	23410	23410

+ Significant at p=0.10  
\* Significant at p= 0.05  
\*\* Significant at p = 0.01

Table 2: Propensity score matching stage 1: predicting who should have been contacted face to face by parties in the 2010 campaign (logit models. Source: 2010 BES Internet Campaign Panel)

	Contacted face-to-face by:		
	Labour	Conservative	Liberal Democrat
Vote, 2005 (comparison = did not vote)			
Labour	0.390	0.088	0.229
Conservative	0.026	0.401**	0.074
Lib Dem	-0.169	0.164	0.848**
Other	0.108	0.064	0.217
Too young	0.017	0.280	0.481
Don't know	0.044	0.033	0.578*
Party identification, pre-election (comparison = no party ID)			
Very strong Labour identification	1.234**	-0.083	0.212
Fairly strong Labour identification	0.565**	0.145	0.393*
Not very strong Labour identification	0.565**	0.145	0.215
Very strong Conservative identification	-0.470	0.894**	0.206
Fairly strong Conservative identification	-0.332	0.655*	0.066
Not very strong Conservative identification	-0.152	0.436**	0.164
Very strong Liberal Democrat identification	-0.109	-0.353	1.707**
Fairly strong Liberal Democrat identification	0.080	0.054	0.746**
Not very strong Liberal Democrat identification	0.244	0.282	0.642**
Other	0.165	-0.020	0.014
Respondent sought help from local MP (comparison = yes)			
No	-0.362**	-0.127*	-0.148*
Pre-election attention to politics (10=high)	0.058**	0.098**	0.062**
Labour short campaign spend %	0.019**	0.004**	0.002
Conservative short campaign spend %	-0.000	0.013**	0.002
Lib Dem short campaign spend %	0.004**	0.003**	0.017**
Constant	-4.187	-4.416	-4.669
-2 log likelihood			
Improvement	754.39	517.94	629.44
Sig	0.000	0.000	0.000
% correctly classified			
Nagelkerke R <sup>2</sup>	0.107	0.059	0.102
N	12598	12598	12598

- + Significant at p=0.10
- \* Significant at p= 0.05
- \*\* Significant at p = 0.01

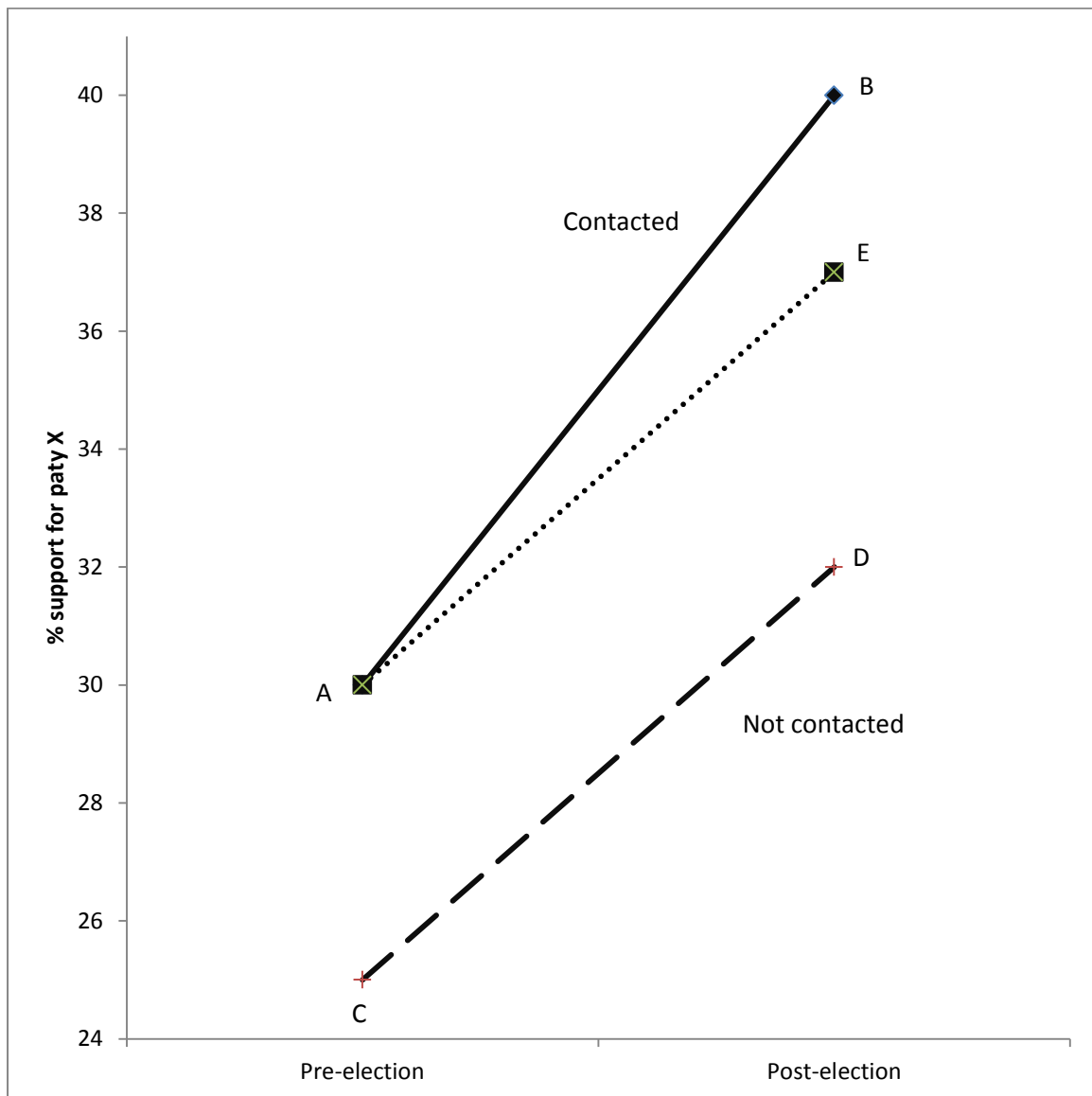
Table 3: Propensity score matching results: the effect of face-to-face contact on voting for a party in 2010

Party	Labour	Conservatives	Liberal Democrat
Contacted by party: % voting for party	53.1	52.7	49.8
Not contacted by party: % voting for party	42.7	47.7	40.0
Difference	10.4	5.0	9.8
SE	2.4	2.0	2.6
T	4.38**	2.44*	3.80**
N	2028	2820	1670

\* Significant at  $p = 0.05$

\*\* Significant at  $p = 0.01$

Figure 1: A hypothetical illustration of difference-in-difference estimation





## On-line appendices

On-line appendix 1: Predicting the impact of self-reported face to face contact on voting at the 2010 election: conventional regression: OLS models with robust standard errors (source: 2010 BES Campaign Internet Panel)

	Vote 2010		
	Labour	Conservative	Liberal Democrat
Vote, 2005 (comparison = did not vote)			
Labour	0.1793**	0.0505**	0.0422**
Conservative	-0.0052	0.2872**	-0.0143
Lib Dem	-0.0455**	0.0235+	0.3038**
Other	0.0042	0.0366*	-0.0074
Too young	0.0613*	0.0472*	0.0628*
Don't know	0.0198	0.0937**	0.0806**
Party identification, pre-election (comparison = no party ID)			
Very strong Labour identification	0.5669**	-0.2011**	-0.1935**
Fairly strong Labour identification	0.4078**	-0.1571**	-0.0903**
Not very strong Labour identification	0.2095**	-0.0952**	-0.0017
Very strong Conservative identification	-0.0944**	0.4885**	-0.2089**
Fairly strong Conservative identification	-0.0875**	0.4702**	-0.1980**
Not very strong Conservative identification	-0.0919**	0.3591**	-0.1193**
Very strong Liberal Democrat identification	-0.0500**	-0.1805**	0.3960**
Fairly strong Liberal Democrat identification	-0.0431**	-0.1509**	0.3740**
Not very strong Liberal Democrat identification	-0.0242	-0.0973**	0.2469**
Other	-0.0399**	-0.0460**	-0.0854**
Respondent sought help from local MP (comparison = yes)			
No	-0.0042	0.0021	0.0053
Pre-election attention to politics (10=high)	-0.0006	0.0070**	0.0003
Labour short campaign spend %	0.0007**	-0.0000	-0.0005**
Conservative short campaign spend %	-0.0003**	0.0005**	0.0002
Lib Dem short campaign spend %	-0.0005**	-0.0002*	0.0008**
Labour face-to-face campaign contact	0.1141**		
Conservative face-to-face campaign contact		0.0526**	
Lib Dem face-to-face campaign contact			0.0935**
Constant	0.1102	0.0757	0.2098
R <sup>2</sup>	0.4802	0.5623	0.3330
N	12155	12155	12155

- + Significant at p=0.10
- \* Significant at p= 0.05
- \*\* Significant at p = 0.01

On-line appendix 2: Predicting the impact of self-reported face to face contact on voting at the 2010 election: difference-in-difference with time-intervention interaction for all three parties: OLS models with robust standard errors (source: 2010 BES Campaign Internet Panel)

	Vote 2010		
	Labour	Conservative	Liberal Democrat
Vote, 2005 (comparison = did not vote)			
Labour	0.1455**	0.0403**	0.0146
Conservative	0.0022	0.2309**	-0.0088
Lib Dem	-0.0343**	0.0158+	0.2383**
Other	-0.0048	0.0178+	-0.0101
Too young	0.0588**	0.0305*	0.0508**
Don't know	0.0042	0.0323**	0.0340**
Party identification, pre-election (comparison = no party ID)			
Very strong Labour identification	0.6735**	-0.1433**	-0.0841**
Fairly strong Labour identification	0.4834**	-0.1110**	-0.0259*
Not very strong Labour identification	0.2294**	-0.0594**	0.0098
Very strong Conservative identification	-0.0521**	0.5910**	-0.1081**
Fairly strong Conservative identification	-0.0498**	0.5522**	-0.1049**
Not very strong Conservative identification	-0.0552**	0.3741**	-0.0655**
Very strong Liberal Democrat identification	-0.0143	-0.1266**	0.5735**
Fairly strong Liberal Democrat identification	-0.0174+	-0.1045**	0.4788**
Not very strong Liberal Democrat identification	-0.0023	-0.0695**	0.2447**
Other	-0.0203*	-0.0375**	-0.0443**
Respondent sought help from local MP (comparison = yes)			
No	0.0003	0.0010	-0.0019
Pre-election attention to politics (10=high)	0.0003	0.0054**	-0.0001
Labour short campaign spend %	0.0004**	-0.0000	-0.0002**
Conservative short campaign spend %	-0.0001	0.0002**	0.0001
Lib Dem short campaign spend %	-0.0001*	-0.0001	0.0004**
Labour face-to-face campaign contact	0.0291**	-0.0251**	-0.0064
Conservative face-to-face campaign contact	-0.0219**	0.0340**	-0.0036
Lib Dem face-to-face campaign contact	-0.0027	-0.0149*	0.0094
Time post-election (comparison=pre-election)	0.0331**	0.0453**	0.1386**
Time*Lab face-to-face campaign contact	0.0742**	-0.0020	-0.0584**
Time*Con face-to-face campaign contact	-0.0203*	0.0503**	-0.0438**
Time*LD face-to-face campaign contact	-0.0424**	-0.0360**	0.1286**
Constant	0.0317**	0.0442**	0.0447**
R <sup>2</sup>	0.5120	0.5886	0.3819
N	23410	23410	23410

- + Significant at p=0.10
- \* Significant at p= 0.05
- \*\* Significant at p = 0.01