

Political Activists as Free-Riders: Evidence from a Natural Field Experiment

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Abstract: How does a citizen’s decision to participate in political activism depend on the participation of others? We conduct a nationwide natural field experiment in collaboration with a major European party during a recent national election. In a party survey, we randomly provide canvassers with true information about the canvassing intentions of their peers. When learning that more peers participate in canvassing than previously believed, canvassers significantly reduce both their canvassing intentions and behaviour. An additional survey among party supporters underscores the importance of free-riding motives and reveals that there is strong heterogeneity in motives underlying supporters’ behavioural responses.

Keywords: Political activism, natural field experiment, strategic behaviour, beliefs, motives

1 Introduction

Democracies rely on the efforts of political activists who participate in political campaigns. In the pursuit of improving collective outcomes, however, the individual action of an activist is unlikely to accomplish change. Instead, the impact of political activists hinges on the joint effort of the group, creating a situation of strategic interdependence where the effort of an activist might depend on the effort of her peers. Such strategic interdependence of individuals’ actions is at the core of the collective action problem of political activism (Olson,

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1965; Hardin, 2015). This paper presents a natural field experiment to investigate how the effort of a political activist depends on her belief about the participation of others.

In canonical models, political activism is viewed as a public goods game with incentives to free-ride (Olson, 1965): activists are motivated instrumentally, trading off private benefits against private costs of contributing. This class of models postulates that political activists reduce their effort when fellow activists contribute more to the public good. Or, put differently, the effort choices of activists are strategic substitutes.

In contrast, a large literature argues that participation in collective action is not mainly driven by instrumental concerns but instead by social motives leading to conditional cooperation (Ostrom, 2000; Falk and Fischbacher, 2006; Uhlener, 1989). If such motives outweigh instrumental considerations, activists augment their effort in response to an increase in effort by their peers. Hence, activists' effort choices will exhibit strategic complementarity. Understanding the strategic nature of political activism is crucial to the refinement of the theoretical assumptions of models of collective action.

This paper presents the results of a natural field experiment to causally examine the strategic nature of political activism. In cooperation with a major political party in a Western European country, we implemented a pre-registered field experiment in the context of a large door-to-door canvassing campaign in the run-up to a nationwide general election. We examine whether and how party supporters' canvassing efforts depend on their beliefs about the canvassing efforts of their fellow party supporters.

Identifying the causal link between beliefs and behaviour from correlational data faces the common challenges of causal inference. First, canvassers' effort choices might directly affect their beliefs, thereby giving rise to reverse causality. Second, active canvassers might hold systematically different beliefs relative to non-canvassers, potentially inducing omitted variable bias. In correlational data, these confounds could spuriously suggest either strategic substitutability or complementarity.

Our experimental strategy circumvents these confounds by exogenously manipulating beliefs in a natural field setting. Our design proceeds as follows. We use an unobtrusive survey distributed by the party via email eight weeks before the election with the stated purpose of gathering information to organize the campaign. In this survey, we first measure party supporters' ex-ante beliefs about the door-to-door canvassing intentions of their fellow party supporters. We then exogenously shift these beliefs in a treatment group by providing true

information collected through a different survey conducted one month prior to the experiment. Supporters in a control group receive no such information. Subsequently, we elicit respondents' post-treatment beliefs about the actual canvassing turnout of their fellow party supporters. Finally, we elicit respondents' intentions to go canvassing. After the survey, we collect unique, unobtrusive real-time data on canvassing behaviour through a novel canvassing smartphone application in which door-to-door canvassers register the addresses they visit. To preserve the natural field setting we ensured that participants are at no point aware of their participation in an experiment.

We present five key results. First, on average, political activists' strongly underestimate their peers' intended engagement in the campaign.¹ Second, on average, political activists' intentions follow the predictions of a public goods game with free-riding incentives: party supporters who learn that their peers plan to exert more effort than they previously expected significantly lower their intentions to participate in the party's campaign. The response is concentrated along the intensive margin. Supporters plan to canvass 1.10 days (*s.e.* = 0.36) less relative to a control mean of 4.03 days.²

Third, we demonstrate that the reduction in canvassing intentions translates into a reduction in actual canvassing behaviour. Using real-time canvassing data collected through the party's smartphone application, we estimate a reduction of 14.39 (*s.e.* = 7.83) canvassed doors, which is equivalent to a reduction of 38% relative to the control group mean of 38.34. Furthermore, we find a statistically significant reduction of 0.093 (*s.e.* = 0.047) standard deviations in a pre-specified index combining canvassing intentions and behaviour. Our results thus imply that political activists' behaviour exhibits strategic substitutability *on average*.

Fourth, the effects are driven by party supporters with weaker social ties to the party (as proxied by prior canvassing experience, whether the respondent is a party member, and party membership duration). On the contrary, party supporters with strong ties do not exhibit a systematic pattern of strategic substitutability. Similarly, we only find evidence for strategic substitutability effects in localities with relatively weak social

¹Our subsequent results focus on underestimators that make up 82% of our sample as we have limited statistical power to draw firm conclusions for the relatively small sample of overestimators.

²We find imprecisely measured decreases in canvassing effort in response to the information treatment for overestimators, consistent with strategic complementarity. This potential discrepancy in strategic behaviour between over- and underestimators could be due to the following two explanations. First, pre-treatment beliefs could be correlated with respondents' underlying strategic types. Second, beliefs could be measured with error leading to misclassification of underestimators as overestimators (Haaland *et al.*, forthcoming).

cohesion within the local party chapter. These results highlight that social connections can counterbalance free-rider incentives and act as a force for strategic complementarity.

Fifth, we conduct an additional survey among party supporters to dissect the mechanisms underlying strategic interactions in political action. We ask party supporters how they would adjust their campaign efforts in response to learning about higher effort of their peers. We then ask them to describe the reasoning underlying their decision with both an open-ended response as well as structured response items. Our data corroborates the importance of free-riding motives as a very prevalent motive, and allows us to distinguish between different social motives: it reveals that concerns related to social identity and enjoyment value are the most prevalent motives shown by activists who exhibit complementarity in their effort choices.

Our study contributes to a growing body of literature examining whether and how social interactions and beliefs affect political behaviour such as protest participation (González, 2020; Enikolopov *et al.*, 2020; Cantoni *et al.*, 2022; Passarelli and Tabellini, 2017; Manacorda and Tesei, 2020; McClendon, 2014; Bursztyn *et al.*, 2021), voting (Gerber *et al.*, 2011, 2008; Green *et al.*, 2013; Gerber *et al.*, 2020; Kendall *et al.*, 2014), and campaign behaviour more generally (Neuenschwander and Foos, 2021). The three studies most related to this work are Cantoni *et al.* (2019) who use a similar experimental design to examine university students' protest participation in Hong Kong. Hager *et al.* (2022) study how strategic interactions of protesters in Germany vary across the political spectrum. Finally, Perez-Truglia and Cruces (2017) study the impact of information about campaign donations of neighbours on donation behaviour in the US. We make several contributions to this literature.

First, our experimental data is distinct in two main respects: we draw on a unique combination of survey and behavioural outcome data collected through a smartphone application. This feature of the data allows us to study treatment effects on both self-stated intentions and actual behaviour. Moreover, we provide evidence from a natural field experiment on behalf of the party, in which participants are not aware of their participation in an experiment.

Second, we study an electoral campaign in a liberal democracy where it is ex-ante unclear whether social motives are sufficiently strong to outweigh free-riding incentives. On the one hand, a typical public good logic applies to our setting. What matters for electoral success is the total number of convinced voters. As a result, individual effort can be easily substituted by the effort of others. On the other hand, social motives could

play an important role in our context as party supporters often form strong social ties. Indeed, campaign organizers within the party assumed strategic complementarity of effort choices. As a result, they tried to motivate their supporters by highlighting high levels of canvassing participation through e-mail notifications like “Everybody goes from door to door! Participate as well!”. Our evidence indicates that this perception of complementarity might be wrong which has important implications for parties’ mobilization strategies.

Third, in contrast to samples used in the previous literature, we leverage a heterogeneous sample of party supporters of all ages and with diverse backgrounds. The heterogeneity in our sample allows us to shed light on underlying drivers and mechanisms of our treatment effects. In particular, the heterogeneity in our data provides important evidence that strategic substitutability is most pronounced for party supporters with weak social ties to the party. These patterns are also supported by individual-level data on motives underlying the effort adjustments in response to increases in peer effort.

Our findings also inform the theoretical literature investigating political behaviour in democratic systems (Shadmehr, 2021; Shadmehr and Bernhardt, 2011; Feddersen and Sandroni, 2006; Coate and Conlin, 2004; Downs, 1957; Palfrey and Rosenthal, 1984). The systematic heterogeneity in the responsiveness to our treatment highlights that theoretical models of political behaviour should account for heterogeneity in agents’ motivation.

2 Experimental Design

2.1 Design and Sample

Setting: Our field experiment took place in the run-up to a recent general election in a Western European country. The experiment was implemented in collaboration with a major political party to study party supporters’ motivation and actual participation in the party’s door-to-door canvassing campaign. The analysis was pre-registered at the AEA RCT registry before the start of the data collection. The experimental manipulation was administered in an online pre-campaign survey sent out on behalf of the party roughly eight weeks before the election. After the intervention, we tracked party supporters’ real canvassing efforts throughout the campaign until the election.

The electoral system in our context is mostly proportional with no absolute majority realistically achievable for any party. Hence, each additional vote gained can be thought of as increasing the party's political power in a more or less continuous way.

The party with which we cooperated strongly promoted canvassing as a campaigning tool through internal communication channels. All canvassing volunteers were instructed to record every canvassed door in a novel smartphone application as a way to help the party organize its current and future campaigns. The data from the application provides unique behavioural outcomes on actual, real-time canvassing efforts.

While the overall level of canvassing activity was higher than in previous elections, there was still substantial potential to increase activity levels. Nationally, volunteers of the party reached out to 1.65% of all households. At the constituency level, the fraction of households canvassed ranged between 0 and 25% with a median of 0.5% and a 90th percentile of 4.5%. These low absolute levels of canvassing in most places imply that there was scope for volunteers to increase their level of canvassing activity even though that could imply going to less promising areas and thus lower returns to canvassing activity.

Sampling and Procedures: Our original sample comprises all party supporters who had signed up to the party's campaign email list about eight weeks before the election. At the beginning of the electoral campaign, we contacted these supporters with an email invitation on behalf of the party. The email asked supporters to participate in the survey to help organize the campaign. The invitation email was designed by the party to preserve the natural environment and ensure that participants would not be aware of being part of an experiment. A reminder email was sent ten days later. In total 1,411 party supporters responded to the online survey for this experiment.³ Random assignment and experimental manipulation took place within the online survey.

Measuring and Manipulating Beliefs: We designed the experiment to provide causal evidence on how party supporters' motivation and actual canvassing effort depend on their beliefs about the efforts of their peers.

The experimental design is illustrated in Figure A1. In a first step, we elicit participants' pre-treatment beliefs

³We simultaneously conducted a second experiment with a separate subsample of activists which is pre-registered in the same pre-analysis plan used for this paper. Supporters responding to the invite were randomly allocated between the experiment described in this paper and the experiment described in Hager *et al.* (2021) which studies how beliefs about the effort choices of members of the main competing political party affect activists' effort choices. Each individual only took part in one experiment.

about the share of party members who *plan* to go canvassing. Then, half of all respondents are randomly assigned to receive information about the canvassing plans of fellow party members (treatment group), whereas the remaining half receives no information (control group). More specifically, participants in the treatment group are truthfully informed that 37% of party members in a previous survey had stated an intention to go canvassing.⁴ After the experimental manipulation, all respondents are asked to estimate the share of members who will *actually* go canvassing. We elicit post-treatment beliefs about *actual* participation rather than *intended* participation to mitigate concerns about numerical anchoring and demand effects. The elicitation of participants' post-treatment beliefs allows us to check whether the information provision successfully shifts beliefs.

Outcome Measures: We study the canvassing effort of party supporters in the campaign by combining both survey and behavioural outcome data. We use two pre-specified self-reported measures of canvassing intentions that are collected after the treatment administration: First, we measure whether a respondent intends to do any canvassing in the campaign. This allows us to shed light on movement along the extensive margin. Second, we elicit respondents' intended number of days of participation, enabling us to analyse responsiveness to the treatment along the intensive margin.⁵

We then assess whether changes in canvassing intentions translate into changes in canvassing behaviour. We draw on unique behavioural outcome data from the smartphone application distributed by the party. The party continuously emphasized the importance of using the application to volunteers citing the need for data to plan current and future campaign activities. The application allows us to assess three pre-specified behavioural outcomes: first, an indicator for whether a supporter knocks on any doors; second, the number of doors a supporter knocks on; and third, the number of days a supporter goes canvassing.⁶

⁴We collected this data in a separate survey with another sample of party members contacted through official channels three weeks before the experiment. In this survey, we first elicited respondents' party affiliation and then asked the following question: *Do you plan to engage in canvassing on behalf of your party during this electoral campaign?* Through this survey, we did not get sufficient respondents to provide reliable estimates of constituency-level canvassing intentions. Hence, we provided only information about the national level.

⁵The intended number of days for respondents who do not plan to canvass is coded as zero days.

⁶Individuals who do not appear in the application data are coded as not having canvassed.

2.2 Descriptives and Balance

Table A1 describes the sample characteristics elicited in the survey. 24% of supporters are women, and the average age is 41 years. The gender composition of our sample is comparable to the population of party members but supporters are significantly younger than the average party member. 83% of supporters are party members with an average membership duration of 12 years. Besides basic socio-demographic information, the survey also inquires about supporters' prior canvassing experience. 38% of participants had helped in a past campaign.

Regarding our outcome variables, 49% of supporters intend to participate in door-to-door canvassing with an average of 3.85 intended days. Turning to behavioural outcomes from the app, we observe much lower actual canvassing activity relative to stated canvassing intentions: 12% of party supporters in our sample actually participate in the campaign. The respondents canvass on average 0.59 days and knock on 29 doors. The unique link between the survey and the behavioural outcome data from the natural field setting also allows us to study how intentions and actual canvassing behaviour are related. We find a sizeable positive correlation between intended days and the actual number of days of canvassing ($\rho = 0.28$, visualized in Figure A2). Similarly, we find that people's intention to do any canvassing is significantly related to whether they actually canvass ($\rho = 0.33$). This makes our sample disproportionately engaged. The sample can thus best be characterized as 'young and highly motivated supporters.' The young age in our sample also implies that supporters did not face technological barriers to using the smartphone application with which the party organized its canvassing and which we used to obtain unobtrusive behavioural outcomes. Our study includes 6.5% of all party supporters who canvassed for the party during the entire campaign. Furthermore, our survey respondents were responsible for 11% of all knocked doors during the campaign.

Finally, we do not observe significant differences between the treatment and control group for any of the covariates (Table A3). We regress the treatment indicator on all covariates to test for joint significance. The p-value of this joint F-test is 0.55, indicating that the randomization produces two highly comparable groups.

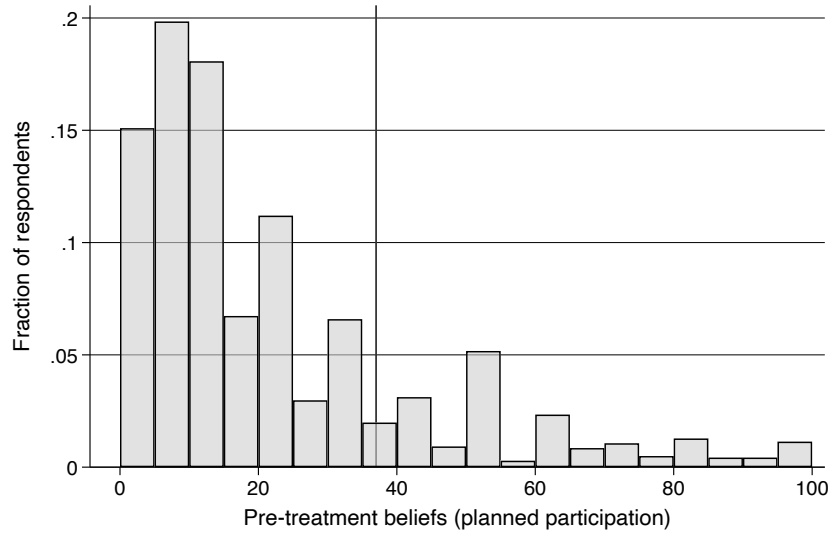
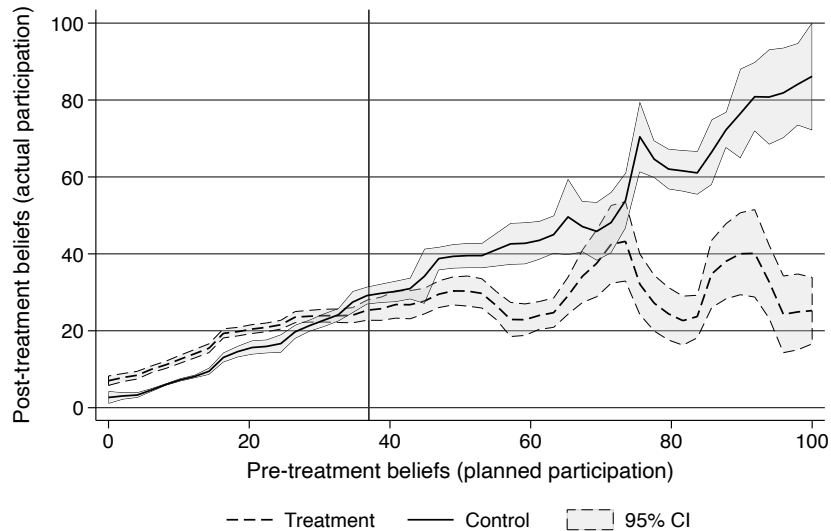
3 Belief Updating

Before turning to the analysis of treatment effects on canvassing outcomes, we test for the successful manipulation of beliefs about peer canvassing effort.

Pre-treatment Beliefs: Panel A of Figure 1 plots the distribution of pre-treatment beliefs about the percentage of party members who intend to go canvassing. The vertical line corresponds to the treatment information which indicates that 37% of party members intend to go canvassing. We observe that the distribution is highly right-skewed with a median belief of 10% of party members planning to go canvassing. Relative to the treatment information, 82% of participants underestimate their peers' canvassing intentions.

Belief Updating: The key qualification of our experimental design is that participants update their post-treatment beliefs about actual participation after receiving the treatment information. In particular, we expect underestimators to increase their post-treatment belief about the fraction of fellow party members who actually go canvassing. Vice versa, we expect overestimators to decrease their post-treatment beliefs.

Panel B of Figure 1 shows the non-parametric relationship between pre-treatment beliefs about intentions and post-treatment beliefs about actual participation by treatment status. We see that underestimators in the treatment group have higher post-treatment beliefs than control group underestimators (by, on average, 55% of the control group mean or 5 percentage points; $p < 0.01$, see column 1 of Panel A1 of Table 1). The reverse is true for overestimators. Treated overestimators update more negatively compared to overestimators in the control group (they see a decrease of 38% of the control group mean or 17 percentage points; $p < 0.01$, see column 1 of Panel B1 of Table 1). We conclude that the information provision successfully shifted participants' beliefs about the canvassing effort of their peers.

Figure 1. *Pre- and post-treatment beliefs about canvassing campaign participation***Panel A:** Distribution of pre-treatment beliefs about planned participation**Panel B:** Non-parametric relationship between pre-treatment beliefs about planned participation and post-treatment beliefs about actual participation

Notes: Panel A of Figure 1 shows the distribution of pre-treatment beliefs about the fraction of party members who plan to participate in the party's door-to-door canvassing campaign. The vertical line (37%) corresponds to the treatment information. Panel B of Figure 1 displays the non-parametric relationship between post-treatment beliefs about actual participation of party members and pre-treatment beliefs about the planned participation of party members. The estimates are obtained using local polynomial regressions of degree 1.

4 Results

4.1 Empirical Specification

Do these exogenous changes in beliefs affect party supporters' motivation and actual behaviour in the campaign? In Appendix Section A, we outline a theoretical model of activists' strategic behaviour. In particular, our model predicts that classical free-riding behaviour should take place if instrumental motives, on average, dominate the effect of social motives. Whether this prediction holds is ultimately an empirical question that we tackle in this section.

In our analysis we separately analyse treatment effects for under- and overestimators. We conduct this separate analysis for several reasons: First, the information shock for underestimators and overestimators go in opposite directions. Second, prior beliefs might be correlated with types, which could yield different behavioural responses among overestimators and underestimators. Given, however, that the large majority of our sample are underestimators most of the result section will focus on underestimators.⁷

We estimate the following specification using ordinary least squares:

$$Y_i = \beta_0 + \beta_1 T_i + \zeta^T \mathbf{X}_i + \varepsilon_i \quad (1)$$

where Y_i is the outcome variable of interest. T_i is a dummy variable taking a value of one for people who receive the treatment information and zero otherwise.⁸ \mathbf{X}_i is a set of pre-specified control variables: party membership, number of years of party membership, age, sex, whether a participant has already participated in a canvassing training, whether a participant has already downloaded the online application, whether a participant has participated in canvassing before this federal election, and whether a participant has participated in canvassing for this federal election.⁹ To account for multiple comparisons, we also examine the effects on

⁷Our results are also robust to a specification pooling underestimators and overestimators (see Appendix Table A4).

⁸We also report the results of a pooled regression with the treatment variable taking the value 1 for treated underestimators, -1 for treated overestimators and 0 for respondents in the control group in Table A7. The results of this specification are qualitatively similar to the results for the pooled specification with the binary treatment indicator.

⁹Excluding control variables leads to similar results (Online Appendix Tables A16 to A21).

a pre-specified index as a joint measure of all five self-reported survey measures and behavioural outcomes.¹⁰ The key coefficient is β_1 which captures the strategic interaction between one's own and peer effort.

4.2 Main Results

Panel A1 of Table 1 presents the main results for underestimators. Participants who are informed that fellow party members devote more effort than previously thought, on average, reduce their willingness to participate in the party's campaign. Participants intend to canvass 1.10 days ($s.e. = 0.36$) less after receiving the treatment information, which is equivalent to a reduction of 24% relative to the control group mean of 4 days. There is, however, no significant effect on the dummy for the intention to engage in any canvassing, with a point estimate close to zero ($0.002, s.e. = 0.026$).

Does lower *intended* canvassing translate into lower *actual* canvassing? Considering canvassing effort, we again find a significant reduction of 14.39 canvassed doors ($s.e. = 7.84$). This is equivalent to a 38% reduction relative to the control group mean of 38.35 doors. Similarly, the point estimate on the impact on actual days canvassed indicates a reduction of 0.16 canvassed days ($s.e. = 0.16$), corresponding to a sizeable, yet not statistically significant 22% reduction relative to the control group mean.¹¹ In line with the results on intentions, we again do not find a significant effect on whether engaged in any canvassing. Finally, we investigate the impact on the pre-specified index of all five outcomes capturing intentions and actual behaviour jointly. We observe a decrease of 0.093 ($s.e. = 0.047$) standard deviations in this summary measure of canvassing intentions and behaviour.

The treatment effects on canvassing behaviour are strongest in week seven and eight just before the election when overall activity was highest. Figure A3 displays the development of treatment effects over time and confirms this pattern. To explore why this might be the case, we study whether the information provided by the treatment could be reinforced over time by an individual's observation on the ground. Panel A of Table A13 shows that the average effects are indeed stronger for activists in localities with high overall activity levels.¹²

¹⁰The index takes into account (i) an indicator for whether a participant plans to go canvassing, (ii) the number of days that a participant plans to go canvassing, (iii) an indicator for whether a participant knocks on any door, (iv) the number of doors that a participant knocks on, and (v) the number of days a participant goes canvassing.

¹¹To show that our treatment effects are not driven by outliers, Figure A5 presents the cumulative distribution for the treatment and control group.

¹²The sample size for this specification is relatively small ($N=426$), as we only observe location for individuals who downloaded the app. Hence, we focus on interpreting pooled treatment effects. Results by pre-treatment beliefs are in Panels B and C of Table A13 and display a qualitatively similar pattern for over and underestimators.

Table 1. *Main treatment effects*

	Belief	Intentions		App Data			App Data: Week 7/8			Index	
	% canvassing	Any	Days	Any	Days	Doors	Any	Days	Doors	Overall	Week 7/8
Panel A: Underestimators											
Panel A1: Average treatment effect											
Treatment	5.027*** (0.543)	0.002 (0.026)	-1.098*** (0.361)	-0.013 (0.016)	-0.159 (0.159)	-14.388* (7.839)	-0.027* (0.014)	-0.145** (0.064)	-6.657** (2.814)	-0.093** (0.047)	-0.132*** (0.049)
Panel A2: Interaction with strength of connection to the party											
Treatment	4.959*** (0.556)	0.007 (0.026)	-1.113*** (0.366)	-0.013 (0.016)	-0.174 (0.158)	-15.822** (7.968)	-0.028** (0.014)	-0.152** (0.064)	-7.118** (2.843)	-0.096** (0.048)	-0.137*** (0.049)
Treatment × Strength of connection to party (PCA)	0.868 (0.665)	0.031 (0.027)	0.803** (0.364)	0.001 (0.013)	0.134 (0.150)	16.294** (6.933)	0.021* (0.011)	0.057 (0.066)	5.239** (2.317)	0.089** (0.044)	0.104** (0.045)
Strength of connection to party (PCA)	-2.409*** (0.586)	0.073*** (0.021)	-0.115 (0.351)	0.014 (0.011)	-0.023 (0.153)	-8.100 (7.076)	-0.000 (0.011)	0.008 (0.070)	-2.023 (2.362)	0.030 (0.042)	0.028 (0.044)
Control mean	9.083	0.473	4.028	0.123	0.701	38.348	0.084	0.297	13.318	-0.029	-0.010
Observations	1150	1148	1148	1163	1163	1163	1163	1163	1163	1148	1148
Panel B: Overestimators											
Panel B1: Average treatment effect											
Treatment	-16.686*** (1.973)	-0.081 (0.053)	-0.926 (1.227)	-0.029 (0.038)	-0.097 (0.186)	-9.009 (12.265)	-0.012 (0.034)	0.003 (0.055)	-2.561 (4.030)	-0.125 (0.086)	-0.100 (0.084)
Panel B2: Interaction with strength of connection to the party											
Treatment	-14.869*** (1.869)	-0.080 (0.057)	0.032 (1.475)	-0.017 (0.038)	0.002 (0.235)	-1.749 (12.729)	-0.002 (0.034)	0.026 (0.068)	-0.551 (4.197)	-0.058 (0.100)	-0.041 (0.096)
Treatment × Strength of connection to party (PCA)	5.901*** (2.136)	-0.020 (0.054)	2.244 (1.486)	0.024 (0.035)	0.234 (0.230)	13.941 (12.904)	0.007 (0.034)	0.035 (0.066)	3.857 (3.485)	0.136 (0.099)	0.102 (0.094)
Strength of connection to party (PCA)	-5.039*** (1.927)	0.118*** (0.041)	-0.238 (0.935)	-0.024 (0.024)	-0.056 (0.098)	-11.904 (8.440)	-0.018 (0.023)	-0.009 (0.033)	-2.878 (2.305)	0.010 (0.058)	0.023 (0.058)
Control mean	45.313	0.545	5.754	0.149	0.440	22.515	0.090	0.119	7.157	0.034	0.021
Observations	247	247	247	248	248	248	248	248	248	247	247

Notes: Table 1 presents the main treatment effects. Panel A displays effects for respondents who underestimate the share of peers who intend to canvass. Panel B displays effects for respondents who overestimate the share of peers who intend to canvass. Panels A1 and B1 display the pooled treatment effects. Panels A2 and B2 present treatment effects interacted with a summary measure of connectedness to the party. The measure for strength of connection to the party is the first principal component of three pre-specified dimensions of heterogeneity (previous canvassing experience, party membership, and party membership duration) and is standardized to have mean zero and standard deviation one. Outcome variables are as follows. “Beliefs” captures the belief about the fraction of fellow party members who actually go canvassing. “Intentions” captures whether a participant intends to engage in any canvassing (*Any*) and the intended number of days (*Days*). “App Data” captures whether a participant actually engages in any canvassing (*Any*), as well as the number of days (*Days*) and the number of doors a participant knocks on (*Doors*, winsorized at the 99th percentile). “App: Week 7/8” captures actual engagement in weeks 7 and 8 after the treatment (one or two weeks before the election). “Index” indicates two summary measures. “Overall” describes a pre-specified index of all five outcome variables capturing canvassing intentions and behaviour jointly. “Week 7/8” describes an index of the outcomes displayed under “Intentions” and “App: Week 7/8”. We construct the index by first standardizing each outcome using the control group mean and standard deviation, then calculating the total of the standardized variables, and finally re-standardizing the sum to have mean zero and standard deviation of one. Treatment effects are obtained conditional on pre-specified control variables: party membership, number of years of party membership, age, sex, whether a participant has participated in a canvassing training, whether a participant has already downloaded the online application, whether a participant has canvassed before this election, and whether a participant has already canvassed in this election. When we control for the strength of connection to the party we drop all components of the index from the control variables. We include all individuals who saw the treatment screen in our sample even when they did not complete the full survey as pre-specified in the pre-analysis plan. This leads to small variations in sample size across survey-based and behavioural outcomes. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This result suggests that learning through observation might indeed reinforce the provided information and lead to the observed dynamics.

Panel B1 of Table 1 presents the main results for overestimators, which given the low sample size are muted and statistically insignificant and, if anything, qualitatively consistent with strategic complementarity. If taken at face value, this is consistent with two potential explanations: First, as a result of measurement error in beliefs some underestimators may be erroneously classified as overestimators. This could plausibly be the case for respondents with prior beliefs of 50%, which has been shown to be an expression of uncertainty (Enke and Graeber, 2019). Second, the heterogeneity could be a reflection of type heterogeneity: underestimators may be more likely to be free-riders compared to overestimators. This is plausible if the non-exogenous prior beliefs are correlated or driven by other characteristics, for instance motivated beliefs. Overall, however, as Appendix Table A4 demonstrates substitutability holds on average across the pooled sample of under- and overestimators.

Robustness: The treatment effects are unlikely to be driven by differential selection into downloading and using the smartphone application. We do not find a significant effect on application download (Online Appendix Table A5). Furthermore, the observable characteristics of supporters who downloaded the application are similar to those who indicated any canvassing intention (Online Appendix Table A6). Hence, it is unlikely that technological barriers in using the smartphone application led to a substantial mismeasurement of canvassed doors.

It is also unlikely that differential reporting among app users drives the results. First, if learning that more people participate induces social desirability bias we would expect an increase and not a decrease in reported doors. Second, it could be that activists with a low number of canvassed doors are now reluctant to record them in the app. This would imply that treatment effects are concentrated among the lower end of the doors distribution. However, in panel (f) of Figure A5 we observe that treatment effects for underestimators are concentrated among roughly the top 60% of canvassers.

4.3 Heterogeneity in Treatment Effects

Who drives these treatment effects? The theoretical model in Appendix Section A makes predictions for heterogeneity across individuals. Specifically, the model posits that individuals who put a relatively lower weight on social as compared to instrumental motives will exhibit a stronger pattern of strategic substitutability. Vice versa, for individuals with stronger social motives, the pattern of strategic substitutability should be weaker. We test this prediction empirically.

To test for heterogeneity by social motives, we use a variety of proxies for the strength of the social connection to the party. In the pre-analysis plan we had pre-specified analysing heterogeneity by (i) any prior canvassing experience for the same party, (ii) party membership, and (iii) their years of party membership. To keep the analysis as parsimonious as possible, we first employ a simple principal component analysis (PCA) of these three variables to study heterogeneous treatment effects using the standardized first principle component of the three variables. Intuitively, we use the first principle component as an index capturing the strength of social ties to the party.

Before turning to the heterogeneity analysis, we validate this index as a well-suited measure capturing party supporters' social connectedness and social motives. Table A8 shows the correlation between the number of party members a supporter knows personally and the index using data from a post-election survey administered to a different sample of the same population of supporters six weeks after the election. We find that an increase in the number of known party members by one standard deviation (31 members) is associated with 0.18 ($p < 0.001$) higher value on the index. This supports the first principle component as a measure of party supporters' social connectedness and social motives in the following analysis.

In line with our hypothesis, underestimators with weaker connections to the party exhibit a larger negative treatment effect relative to supporters with stronger connections (Panel A2 of Table 1).¹³ The treatment effect heterogeneity is of statistical and economic significance for both intentions and behaviour: treated supporters with one-standard deviation stronger connections to the party exhibit a reduction of the treatment effect on intended days of 0.8 ($s.e. = 0.36$). The attenuation of the treatment effect on canvassing intentions translates

¹³We observe a qualitatively similar pattern for overestimators, though the results are less precisely estimated (Panel B2 of Table 1).

into an attenuated treatment effect on canvassing behaviour. Treated supporters with one standard deviation weaker connections to the party knock on 16.3 (*s.e.* = 6.9) fewer doors. The complementing treatment effect heterogeneity for behavioural outcomes over time is illustrated by Figure A8. Finally, in terms of the summary index measuring canvassing intentions and behaviour jointly, we find that treated supporters with one-standard deviation stronger connections to the party display a 0.089 (*s.e.* = 0.044) standard deviation smaller decrease in canvassing.¹⁴

To further illuminate the underlying mechanisms, we also study heterogeneous treatment effects by the perceived social cohesion of local party chapters. For this purpose, we use the question "How strong is the (social) cohesion in your local party chapter?" with answers on a Likert scale from 0 (very weak) to 6 (very strong) posed in the post-election survey mentioned above. We average responses at the local level, standardize the average, and match the resulting local levels of cohesion to individuals in our experimental sample who downloaded the application and thus have location data.

We find that individuals in locations with low levels of perceived cohesion exhibit stronger negative treatment effects. Specifically, Panel A of Table A14 shows that, on average, individuals in locations with one standard deviation higher local cohesion exhibit a 0.31 standard deviation weaker treatment effect on the pre-specified index ($p < 0.1$).¹⁵

Put together, these results suggest that social connectedness and cohesion can counterbalance strategic substitutability and act as a force for strategic complementarity in the effort choices of political activists.¹⁶

4.4 Motives Underlying Effort Adjustments

In an additional survey among 150 political activists, we elicit the motives underlying activists' hypothetical decisions to increase or decrease their participation in the campaign in response to learning about higher peer effort (see Bursztyn *et al.* (2022) for a similar approach). We recruited respondents through social media groups

¹⁴We also examine heterogeneous responses for each of the variables used in the PCA separately which further corroborates that supporters with weaker social ties to the party drive our treatment effects (Tables A9, A10, and A11). The results are also similar when we control for the interaction of pre-specified controls and the treatment dummy (Table A12).

¹⁵The sample size for this specification is relatively small ($N=408$), as we only observe location for individuals who downloaded the app and live in locations with respondents in the post-election survey. Hence, we focus on interpreting pooled treatment effects. Results by pre-treatment beliefs are in Panels B and C of Table A14 and display a qualitatively similar pattern for over and underestimators.

¹⁶An alternative mechanism might be that supporters with stronger social connections to the party learn more quickly about the true level of activism and thus are less affected by the provided information. However, this cannot explain heterogeneity in the effects on intentions, which are measured prior to any further learning about peer effort.

affiliated with various political parties, including supporters of the party we collaborated with in the field experiment presented earlier.¹⁷

We first ask supporters how they would adjust their campaigning effort in response to learning about higher effort of their peers. Next, we ask supporters to describe the reasoning underlying their decision with an unprompted open-ended question which avoids priming respondents on particular motivations. On the subsequent screen, we also elicit structured responses based on theoretical considerations and pilot data from the open-ended question (see Appendix G for the full instructions). This structured data allows us to validate our hand-coding scheme and to classify individuals whose open-ended responses we could not cleanly map into a specific motive.¹⁸ The key advantage of the open-ended elicitation relative to the structured responses is that it is less prone to ex-post rationalization of motives.

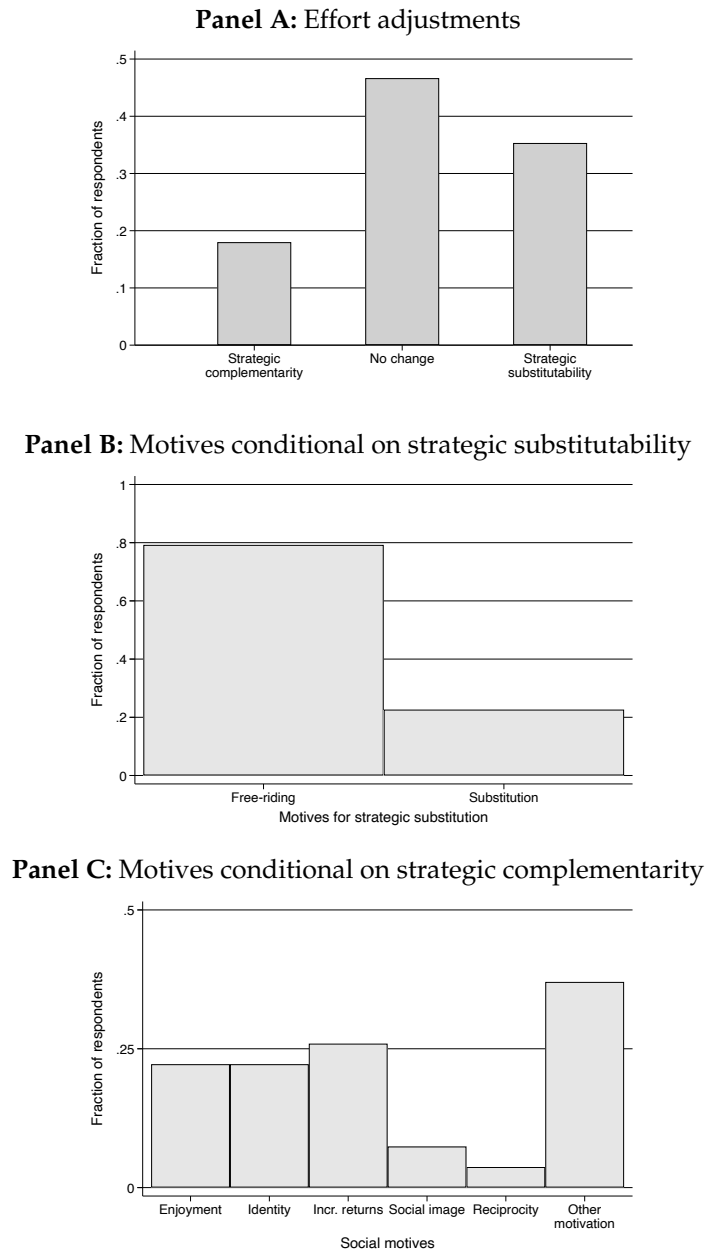
We hand-code open-ended responses using the following main categories, which are closely related to influential theoretical mechanisms in the literature: For motives connected to strategic substitutability, we code “free-rider” motives (Olson, 1965) and “substitution to other effort domains” (Dewatripont *et al.*, 2000). For motives underlying strategic complementarity, we code increased “identity” (Akerlof and Kranton, 2000), “enjoyment” (Uhlaner, 1989), “social image concerns” (Bursztyn and Jensen, 2017), “reciprocity” (Falk and Fischbacher, 2006), and perceived “increase in the returns to activism” (Barbera and Jackson, 2020). Respondents that mention a general increase in motivation that we cannot classify precisely into any of the previous motives are classified as ‘other motivation’. Appendix Table A22 contains additional details on the hand-coding procedure and example responses.

Figure 2 illustrates the results. Panel A shows that 35% of participants state that they would decrease their effort, 18% would increase their effort and 47% would not change their effort in response to learning about higher peer effort, consistent with our main field experimental evidence on strategic substitutability.¹⁹

¹⁷The sample is gender-balanced, the average age is 30 and approximately 35% of respondents are members of any political party (see Table A24 for details).

¹⁸Our hand-coded data based on the open-ended responses is very predictive for choices among the structured categories (see Table A23 for details).

¹⁹When considering heterogeneity by the same index of connectedness to respondents’ favourite party, we observe a pattern that is very similar to the experimental evidence (Panel A of Appendix Figure A11). Respondents with below-median connectedness to their favourite party are 10 percentage points more likely to exhibit strategic substitutability and 10 percentage points less likely to exhibit strategic complementarity compared to respondents with above-median connectedness. Panel B of Appendix Figure A11 shows qualitatively similar patterns of heterogeneity for social and free-riding motives.

Figure 2. *Strategic interactions and stated motives*

Notes: Panel A of Figure 2 shows the distribution of the nature of strategic interactions in a hypothetical vignette. Section G describes the vignette survey in detail. Panel B of Figure 2 displays the motives for respondents who exhibit strategic substitutability. Panel C of Figure 2 displays the motives for respondents who exhibit strategic complementarity. Motives are based on the following vignette (for the exact wording see Online Appendix section G): First, respondents are asked to imagine that, during a general election campaign, their favourite party sent a survey showing that more party members than they previously thought plan to participate in the canvassing campaign. They are then asked whether they would increase, decrease, or not change their canvassing behaviour. Finally, respondents were presented an open text box beneath the question "Why would the results of this survey influence or not influence your decision to go canvass?". The data displayed is based on hand-coding of open text responses into pre-specified categories. The sample (N=150) was recruited through social media posts in groups of potential political activists.

Next, we study activists' motives for strategic substitutability. Panel B of Figure 2 highlights that based on the open-ended data 79% of respondents who say they would decrease their effort mention free-riding incentives, while 23% mention substitution of effort to other activities.²⁰ Finally, Panel C presents the data on motives for respondents exhibiting strategic complementarity. Concerns related to social identity and enjoyment value are mentioned by 22% of respondents, respectively. Perceived increased returns are mentioned by 26% of respondents, underscoring that instrumental motives are, to some extent, also a driver of strategic complementarity. On the other hand, reciprocity and social image concerns are less prevalent with 4% and 7%, respectively. 37% fall into a category we call "other motivation", which comprises statements about a general increase in motivation in response to learning that more members plan to canvass that we could not cleanly map to any of the other motives.

Our data also allows us to explore the extent to which behavioural responses and motives underlying these responses differ across political parties in our setting. We find that respondents' motives are not predictive of being a supporter or member of our collaborating party. We regress dummies for all coded motives on a dummy indicating support for our collaborating party. An F-test of joint significance of dummies yields $p = 0.90$.

5 Conclusion

We conducted a nationwide natural field experiment in collaboration with a major European party during a recent national election. We randomly provide canvassers with true information about the canvassing intentions of their peers. When learning that more peers participate in canvassing than previously believed, canvassers significantly reduce both their canvassing intentions and behaviour. An additional survey reveals that there is strong heterogeneity in motives driving supporters' behavioural responses. It reveals that the free-rider logic underlies most of the substitutability of effort choices, while concerns related to social identity and enjoyment value are the most prevalent social motives driving complementarity of effort choices.

²⁰In Section H we provide additional evidence on a muted role of effort substitution.

How do our findings inform the broader literature on strategic interactions among political activists? First, our results underscore that strategic interactions have a quantitatively important role in shaping political activism. We document that, when being informed about an increase in peer effort, activists reduce their effort by 38% compared to the control group mean. This is sizeable but roughly in line with effect sizes of similar experiments. For example, in the same context we document a 30% reduction in canvassing activity relative to the control mean in response to increased competition by opposing activists (Hager *et al.*, 2021). In the context of student protests in Hong Kong, Cantoni *et al.* (2019) find a reduction in the likelihood of protesting of 61% relative to the control mean (for underestimators) and, in the context of right- and left-wing protests in Germany, Hager *et al.* (2022) find effect sizes between 15% and 23% of the control group mean.

Second, this paper complements existing literature that documents that the size and sign of strategic interaction vary across contexts. Interactions between activists supporting the same cause have been shown to exhibit both strategic complementarity (González, 2020) and substitutability (Cantoni *et al.*, 2019). Strategic interactions with opposing activists also vary across contexts (Hager *et al.*, 2021, 2022). This paper reconciles these seemingly conflicting results by showing that strategic interactions are not uniform, but instead can differ at the level of individuals (even within the same context and within the same political party).

We see several fruitful avenues for future work. First, while this study identifies the presence of several distinct social motives, future research may scrutinize the quantitative importance of these different social motives. Second, it would be useful to go beyond a reduced form framework and to microfound a model with different social motives explicitly. Finally, future work should try to better understand how political organizations can design their campaigns and messaging to trigger certain social motives as a way to overcome free-rider incentives.

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