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Will a Five-Minute Discussion Change Your Mind? A Countrywide Experiment on Voter Choice in France[†]

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This paper provides the first estimate of the effect of door-to-door canvassing on actual electoral outcomes, via a countrywide experiment embedded in François Hollande's campaign in the 2012 French presidential election. While existing experiments randomized door-to-door visits at the individual level, the scale of this campaign (five million doors knocked) enabled randomization by precinct, the level at which vote shares are recorded administratively. Visits did not affect turnout, but increased Hollande's vote share in the first round and accounted for one-fourth of his victory margin in the second. Visits' impact persisted in later elections, suggesting a lasting persuasion effect. (*JEL* C93, D7, D83)

Consumers and voters base their economic and political decisions on preferences and beliefs shaped by their direct observations, the communication they receive, and discussions with others. Interpersonal discussions contribute to the spread of information and peer effects in technology adoption (e.g., Foster and Rosenzweig 1995; Conley and Udry 2010), educational choices (e.g., Bobonis and Finan 2009), or financial decisions (e.g., Duflo and Saez 2003; Banerjee et al. 2013), and political discussions are commonly seen as the healthy expression of a functioning democracy. To the extent that democracy revolves around the deliberation and transformation of people's preferences, rather than the simple aggregation of their votes, discussion may actually be as important a condition of democracy as the electoral participation of all citizens (Habermas 1996; Elster 1998). The importance people attach to political discussions is illustrated by DellaVigna et al.'s (2017) result that many of us vote in order to later be able "to tell others."

But discussions also affect future political behavior. In their pioneering study on the 1940 US presidential election, Lazarsfeld et al. (1944) find that most voters got their information about the candidates from family members, friends, and colleagues, rather than from the media (see also Gentzkow and Shapiro 2011). Nickerson (2008) and Bond et al. (2012) provide direct evidence of the diffusion of voter turnout off- and online in more recent elections. While diffusion can be driven

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both by discussion and direct observation of others' actions, lab and field studies which narrow the focus to interpersonal discussions (e.g., through group deliberations and deliberative polls), do tend to confirm their influence on the opinions of participants (e.g., Myers and Bishop 1970; Isenberg 1986; Luskin, Fishkin, and Jowell 2002; Druckman 2004; but see Farrar et al. 2009), including influence on issues as resistant to change as intergroup prejudices (Broockman and Kalla 2016).

In an effort to leverage the power of personal discussions, electoral campaigns around the globe increasingly rely on targeted appeals delivered to voters door-to-door (Bergan, Gerber, and Green 2005; Hillygus and Shields 2014; Issenberg 2012). But whether doorstep discussions between canvassers and voters can actually increase voter support is anything but certain: partisan activists may expose voters to more precise and newer information, but their arguments, explicitly driven by electoral motives, may inspire less trust than those of regular discussion partners, or even random strangers. Starting with the seminal work of Gerber and Green (2000), get-out-the-vote field experiments conducted in a wide variety of settings have found large effects of door-to-door canvassing on *voter turnout* (Green and Gerber 2015), leaving the question of its impact on *vote shares* unanswered.

This paper provides the first estimate of the impact of door-to-door visits on actual vote shares. Using administrative records, it reports the results of a precinct-level countrywide experiment embedded in François Hollande's campaign in the 2012 French presidential election. From February 1, 2012, which was 11 weeks before the first round of the election, up until the second round on May 6, 2012, an estimated 80,000 left-wing activists knocked on 5 million doors to encourage people to vote for the candidate of the Parti Socialiste (PS), the mainstream center-left party in France. The author's involvement as one of the three national directors of the field campaign provided a unique opportunity to evaluate its effect on the results of the election. Canvassers' visits did not significantly affect turnout, but they had large and persistent effects on vote share.

Differently from the present experiment, existing studies have typically conducted randomization of door-to-door efforts at the individual or household level, with important consequences for outcome measurement. These evaluations can adequately estimate the effect of door-to-door canvassing on voter turnout, which in many countries is recorded at the individual level and made publicly available. However, they are less suited to measure the effect of the visits on individual voter choices which, to preserve confidentiality, are neither recorded nor released. Some studies resort to polling to construct a close approximation: vote intention or, after the election took place, self-reported vote (e.g., Arceneaux 2007; Arceneaux and Kolodny 2009; Arceneaux and Nickerson 2010; Bailey et al. 2016; Barton, Castillo, and Petrie 2014; Dewan, Humphreys, and Rubenson 2014). Unfortunately, for all its merits, randomization does not eliminate well-known self-reporting biases. In phone surveys, response rates to questions on self-reported vote are typically as low as 10 or 15 percent (e.g., Barton et al. 2014; Pew Research Center 2012), and there is ample evidence that questions on political behavior are particularly prone to misreporting, including overreporting for the winner (e.g., Wright 1993; Atkeson 1999; Campbell 2010). An additional concern is that these biases might differ between treatment and control individuals (e.g., Cardy 2005; Bailey, Hopkins, and Rogers 2016; Gelman et al. 2016). The present experiment overcomes these obstacles by

conducting the randomization at the precinct level, at which administrative records of vote shares are available, while including a number of precincts large enough to secure sufficient statistical power. Prior to this study, neither the implied number of activists nor the campaign apparatus required to organize them had been available to researchers (Arceneaux 2005).

An additional benefit of the large scale of this experiment is the implied external validity. Existing get-out-the-vote experiments, even when they involve political parties and nonpartisan organizations, are conducted at a much smaller scale than most actual campaigns. This allows the researchers and the hierarchy of the campaign (the principal) to carefully select activists (the agent) who will interact with voters and to closely control the content of their discussions. In large-scale campaigns, scope for control is much more limited and the principal-agent problem is more acute, which may lower the impact (Enos and Hersh 2015). Results from framed get-out-the-vote experiments themselves show that quality matters (e.g., Nickerson 2007), and evidence from other contexts suggests that interventions generating large effects in a small, controlled setting may become unimpactful when they are scaled up (e.g., Banerjee, Duflo, and Glennerster 2008; Grossman, Humphreys, and Sacramone-Luz 2016). The present experiment, embedded into a large-scale presidential campaign, overcomes the external validity limitations of prior studies. One aspect of the limited control of the candidate's central team over local activists, however, was that only a subset of territories that participated in the door-to-door campaign also participated in the experiment. I use daily reports entered by canvassers on the campaign website and their responses to a post-electoral online survey to identify which territories did indeed use the randomization lists. In these territories alone, precincts and municipalities collectively containing 5.02 million registered citizens were randomly assigned to either a control or a treatment group.

The randomization was conducted within strata of five precincts characterized by their estimated potential to win votes. Four precincts (80 percent) of each stratum were randomly assigned to the treatment group, and one (20 percent) to the control group. A subset of the treatment precincts—those with the highest potential to win votes—were allocated to the canvassers (more details in Section IIA). Like in a standard encouragement design, I estimate the effect of a precinct being assigned to the treatment group (the intent-to-treat effect of the campaign) by comparing electoral outcomes in control and treatment precincts, and the effect of a precinct being allocated to canvassers (a local average treatment effect) by using random treatment assignment as an instrument. This strategy allowed me to maximize the effectiveness of the campaign while preserving the validity of the experimental design.

All results are based on official election outcomes at the precinct level. Surprisingly, the door-to-door visits did not significantly affect voter turnout. Had randomization been conducted at the individual level, as in existing studies, and only voter turnout been recorded, I would have concluded wrongly that the campaign had no significant impact. Instead, I find that it increased François Hollande's vote share in precincts allocated to canvassers by 3.2 and 2.8 percentage points in the first and second rounds of the presidential elections, respectively. These estimates correct for the imperfect compliance of the canvassers with their allocated lists of precincts, and are significant at the 5 percent level. Multiplying these estimates by the fraction of French doors knocked, I obtain that the canvassing campaign accounted for

approximately one-half of Hollande's lead in the first round and one-fourth of his victory margin at the second round.

The scale of the study also facilitated the assessment of downstream effects. While transitory shocks to voter turnout have been found to generate persistent effects due to long-lasting impact of the shocks themselves or to habit formation (e.g., Gerber, Green, and Shachar 2003; Meredith 2009; Cutts, Fieldhouse, and John 2009; Davenport et al. 2010; Fujiwara, Meng, and Vogel 2016; Coppock and Green 2016), the present study is the first to show that effects on vote choice can persist as well. In fact, contrasting with Gerber et al. (2011), the impact of the visits almost entirely persisted in the subsequent parliamentary elections held one month after the presidential vote. Overall, door-to-door canvassing increased the vote share obtained by Parti Socialiste candidates in these elections by 0.7 percentage points. This effect was larger than the victory margin of members of parliament from the PS elected in 2012 in 5.9 percent of the constituencies. Persistence to the 2014 European elections was smaller (about 47 percent of the original effect) and at the limit of statistical significance.

Finally, I discuss possible interpretations of the results. Although I cannot directly test them, examining the effects of the visits on the vote shares of other candidates provides suggestive evidence. The first and, to me, most likely interpretation, is that the results were driven by a persuasion effect. An alternative interpretation is that the door-to-door visits increased the participation of left-wing supporters, and that they demobilized an equal number of supporters of other parties. Of all types of voters, those who could be deemed most likely to feel cross-pressured and thus demobilized are probably the supporters of the far-right candidate Marine Le Pen, many of whom used to vote left and still maintain leftist preferences on economic issues. However, her vote share was unaffected, making the persuasion interpretation more likely than demobilization. Two different mechanisms may have driven the persuasion effect of the visits: canvassers may have persuaded voters by changing their preferences on some political issues or by changing their beliefs about the quality of the PS and of its candidate. The short average length of the visits makes the first mechanism unlikely. Instead, the fact that most voters that were canvassed had never been visited by a political activist before makes the second mechanism, a shift in the perception of the quality of candidate and party, more plausible. In addition, the increase of Hollande's vote share was a result of his taking votes away from right-wing candidates more than from other left-wing candidates.[1] But right-wing voters could be deemed less susceptible to align their preferences with the political agenda of Hollande than voters supporting other left-wing candidates, who naturally offered a closer ideological platform. This again makes it less likely that voters' political preferences changed, and more likely that their beliefs about the PS and its candidate did.

Overall, the results suggest that in elections of very high salience, voter outreach methods will have little effect on turnout, but that interpersonal discussions can have a large and long-lasting persuasion effect.

[1] Nicolas Sarkozy, the incumbent and candidate of the right-wing Union pour la Majorité Présidentielle, was the only opponent mentioned in the toolkit distributed to canvassers and, naturally, his presidency was discussed in many conversations. However, the main objective of the campaign conveyed to the canvassers was not persuading Sarkozy's voters but mobilizing left-wing non-voters (see Section IC for more details).

This paper contributes to a growing literature providing causal evidence on the drivers and effects of persuasive communication (see DellaVigna and Gentzkow 2010 for an overview). While the access to and information provided by the TV (Simon and Stern 1955; Gentzkow 2006; DellaVigna and Kaplan 2007; Enikolopov et al. 2011), the radio (Adena et al. 2015), newspapers (Gerber, Karlan, and Bergan 2009; Gentzkow, Shapiro, and Sinkinson 2011; Chiang and Knight 2011), or the internet (Falck, Gold, and Heblich 2014; Campante, Durante, and Sobbrios forthcoming) have the potential to profoundly shape voters' political preferences and, depending on the context and the media, substantially increase (e.g., Gentzkow, Shapiro, and Sinkinson 2011) or decrease (e.g., Falck et al. 2014) voter turnout, the effects of political ads disseminated by electoral campaigns through the very same channels are more modest, overall. Neither Ashworth and Clinton (2007), nor Krasno and Green (2008) find substantial effects of TV campaign ads on aggregate turnout, Broockman and Green (2014) do not find that online ads have any effect on voters' evaluation of candidates, or even name recognition, and Gerber et al. (2011) only find very short-lived effects of TV and radio ads on recipients' voting preferences. Yet, Spenkuch and Toniatti (forthcoming) find that TV ads affect the electoral results by altering the composition of the electorate, even though they leave aggregate turnout and preferences unaffected. Both Panagopoulos and Green (2008) and Larreguy, Marshall, and Snyder (forthcoming) also report effects of radio ads on vote shares, which disproportionately benefit challengers.

Well-powered precinct-level randomized evaluations of field campaigns, including those fully embedded in a candidate's campaign, have studied the effects on vote shares of campaign activities that require fewer human resources and are less direct and personal than door-to-door canvassing, such as direct mail (e.g., Rogers and Middleton 2015), phone and robo calls (e.g., Shaw et al. 2012), and town hall meetings (e.g., Wantchekon 2003). These types of contact generate relatively larger effects for weaker candidates (Gerber 2004; Fujiwara and Wantchekon 2013). The messages also generate larger effects when they emphasize valence rather than ideology (Kendall, Nannicini, and Trebbi 2015), and, in developing countries, clientelist rather than public policy platforms (Wantchekon 2003).[2]

Although logistically more demanding, door-to-door visits are more direct and personal than other types of field campaign contacts and mass media advertisements. The interactive discussions to which they lead naturally adapt to respondents' profile and questions, thus potentially affecting voter choice in a different and perhaps more dramatic way than other forms of persuasive communication. In fact, their effect on the decision to vote is itself very different (e.g., Gerber and Green 2000).

The remainder of the paper is organized as follows. Section I provides more background information on François Hollande's door-to-door campaign and on the 2012 and 2014 elections in France. Section II describes the experimental design and its implementation. Section III evaluates the overall impact of the

[2]The paper also speaks to a growing literature, in developing countries, which estimates the impact of election-related field campaigns targeting issues beyond voter turnout and vote choice, such as corruption (Banerjee et al. 2011; Chong et al. 2015), electoral misbehavior and violence (Aker, Collier, and Vicente 2011; Collier and Vicente 2014), or trust in the institutions (Marx, Pons, and Suri 2016).

door-to-door canvassing visits on voter turnout and vote shares in the presidential elections and in the following elections. Section IV interprets the results, and Section V concludes.

I. Setting

A. *The 2012 and 2014 French Elections*

In 2012, France elected both a new president and a new National Assembly. Presidential elections in France have two rounds, with the two candidates achieving the highest vote shares in the first round going on to the second. Turnout in the first round of presidential elections on April 22, 2012 was 79.5 percent of registered citizens.[3] Nicolas Sarkozy, the incumbent and candidate of the right-wing Union pour la Majorité Présidentielle (UMP), and François Hollande, the candidate of the left-wing Parti Socialiste (PS), obtained respectively 27.2 percent and 28.6 percent of the votes and qualified for the second round (see Figure 1). Compared to the 2007 presidential election, François Bayrou, the centrist candidate, lost over half of his vote share (9.1 percent compared to 18.6 percent), and the far-left candidates' portion became marginal (1.7 percent compared to 5.8 percent). The vote share of Marine Le Pen, 17.9 percent, was the highest ever obtained by her party, the far-right Front National (FN). Voter turnout in the second round, on May 6, was high again at 80.4 percent, and François Hollande was elected President with 51.6 percent of the votes.

French parliamentary elections use single-member constituencies. Similarly to the presidential elections, they consist of two rounds, unless one candidate obtains more than 50 percent of the votes in the first. Unlike in the presidential elections, all candidates who obtain a number of votes higher than 12.5 percent of registered citizens in the first round can compete in the second, but in most cases that is only two candidates. The 2012 parliamentary elections took place on June 10 and 17. Turnout was 57.2, then 55.4 percent—far lower than in the presidential elections, and lower than the previous parliamentary elections. This confirms the lesser salience of parliamentary elections in the minds of voters, as well as a general declining trend of turnout (Figure 2). The PS candidates won in 49 percent of the constituencies.

In order to examine the long-run effect of the door-to-door visits, I include the 2014 European elections in the analysis.[4] These elections took place on May 25. Unlike the presidential and parliamentary elections, the European elections use the proportionality rule, and France is divided into seven large European constituencies. Only 42 percent of the voters participated in these elections and the PS suffered a major defeat. Its candidates ranked third in all the constituencies, behind the lists of the UMP and of the FN.

[3] In France, voter turnout is computed as the fraction of number of votes cast over the number of registered citizens. Turnout figures reported throughout the paper follow this convention. Since the door-to-door canvassing campaign started after the registration deadline of December 31, 2011, it could not affect the number of registered citizens.

[4] In 2014, France also held municipal elections. However, the political orientation (left, right, etc.) of the candidates is only known in 27 percent of the municipalities, those with more than 1,000 inhabitants. Moreover, in these municipalities, the vast majority of candidates run under affiliations which are not endorsed by a national party, such as PS or UMP. Given the low resulting statistical power, I do not include the municipal elections in the analysis.

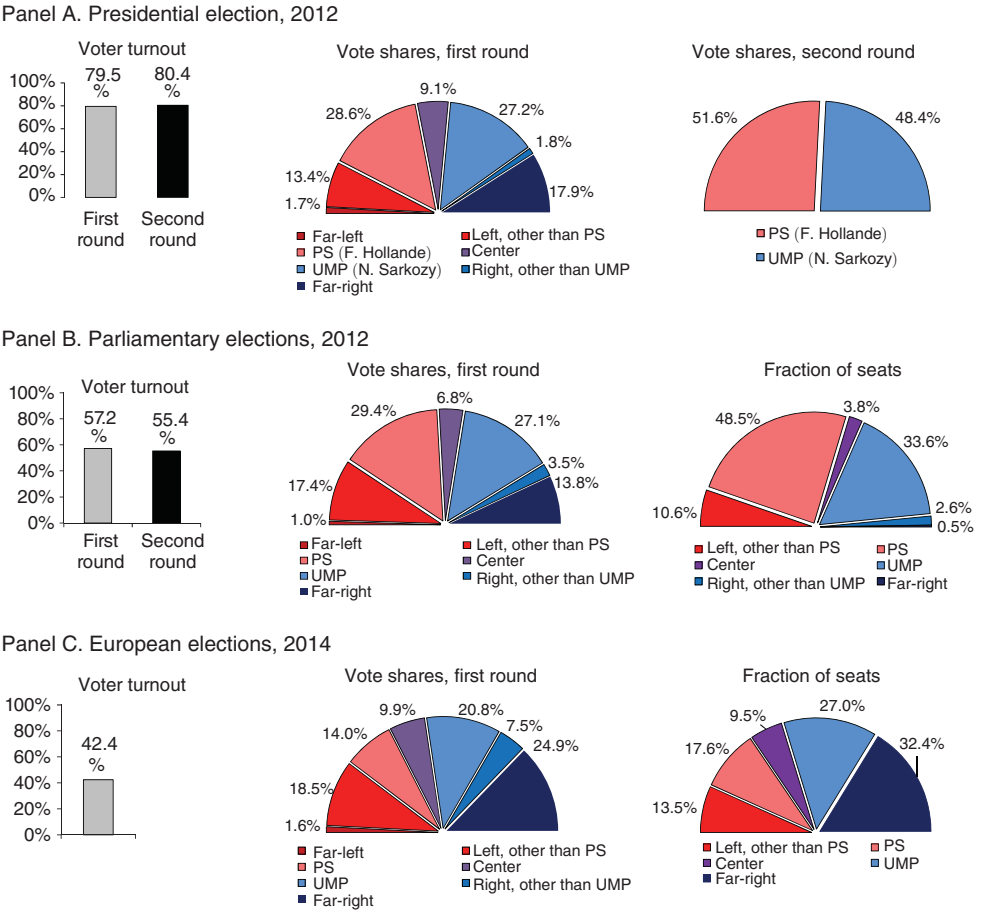


FIGURE 1. RESULTS OF THE 2012 AND 2014 ELECTIONS

Notes: In the first round of the presidential election, the far-left candidates were Philippe Poutou (Nouveau Parti Anticapitaliste) and Nathalie Arthaud (Lutte Ouvrière). The left candidates other than François Hollande were Jean-Luc Mélenchon (Front de Gauche) and Eva Joly (Europe Ecologie les Verts). The center candidate was François Bayrou (Mouvement démocrate). The right candidate other than Nicolas Sarkozy was Nicolas Dupont-Aignan (Debout la République).

Source: French Ministry of the Interior

B. *Electoral Campaigns in France versus the United States*

Among the many differences between French and US electoral campaigns, at least three should be emphasized here: funding, distribution of media access, and field activities. François Hollande's 2012 campaign spent 29 million dollars, 38 times less than Barack Obama's 1.107 billion dollars. The bulk of Obama's money was spent on radio and television advertising. Instead, all French radio and TV channels were mandated to give equal coverage to the campaign of each of the 12 candidates before the first round. Similarly, between rounds, they had to give equal coverage to Sarkozy and Hollande; in France, candidates do not compete using TV ads. As a result, one might hypothesize that French campaigns put relatively more emphasis

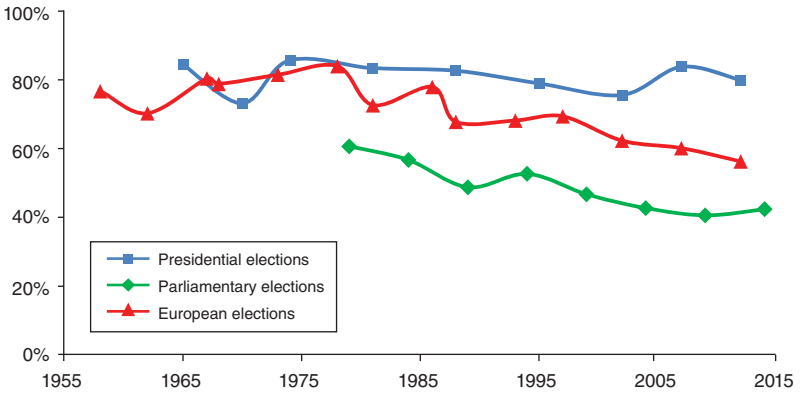


FIGURE 2. TURNOUT AT FRENCH PRESIDENTIAL, PARLIAMENTARY, AND EUROPEAN ELECTIONS, 1958–2014

Notes: French turnout rates are computed using the number of registered citizens (rather than the number of eligible citizens) as the denominator. Turnout shown for the presidential and parliamentary elections is the average between the turnout at the first and second rounds.

Source: French Ministry of the Interior

on the recruitment of volunteers and that they select their field campaign methods with great care. On the contrary, until recently, French political parties allocated few resources to the recruitment, training, and coordination of activists. In addition, local units of the PS were largely autonomous and free to choose their own campaign methods. Although it had once been common, door-to-door canvassing had progressively been replaced by other more impersonal techniques, such as handing out flyers in public places or dropping them in mailboxes (Liegey, Muller, and Pons 2013). By 2012, only few areas saw frequent door-to-door canvassing (Lefebvre 2016).

Two factors explain the emphasis the PS placed on canvassing during the 2012 presidential election. First, the 2008 campaign of Barack Obama generated unusual levels of public attention and enthusiasm across France. Prominent French politicians and think tanks called for an adoption of US electoral and campaign practices, including the organization of large field campaigns (Terra Nova 2009). The second factor, as in the United States, was academic research: the first French randomized evaluation of a door-to-door canvassing get-out-the-vote effort (Pons and Liegey forthcoming) aided in convincing the PS to scale up the method for the 2012 presidential election.[5]

As a result of these different factors, the objective set for Hollande's 2012 door-to-door canvassing campaign was ambitious: to knock on five million doors, or roughly 15 percent of all French dwellings.

C. *François Hollande's 2012 Door-to-Door Canvassing Campaign*

Four days after the second round of the presidential election, all 9,227 activists with an active profile on Hollande's campaign website received an email invitation

[5] Liegey, Muller, and Pons (2013) examine at greater length the different steps through which the PS progressively adopted door-to-door canvassing as the preferred field campaigning strategy from 2010 to 2012.

Table 1—Canvassers' Profile and Feedback on the Campaign (*Post-Electoral Survey*)

Panel A. Canvassers' profile	
Age	
29 or less	11.1%
30–45	23.0%
46–59	36.1%
60 and beyond	29.4%
Non-response	0.5%
Responsibilities within the campaign	
Volunteer	58.8%
Field organizer or head of local unit	37.1%
Département coordinator	4.2%
Non-response	0.0%
Relationship to Parti Socialiste	
Member for five years or more	52.0%
Member for less than five years	27.3%
Sympathizer and had previously been involved in a campaign	8.3%
Sympathizer and is involved in a campaign for the first time	12.4%
Non-response	0.0%
Previous field campaigning experience	
Had never done door-to-door canvassing	43.2%
Had done door-to-door canvassing a few times	34.5%
Had often done door-to-door canvassing	22.3%
Non-response	0.1%
Panel B. Involvement in the campaign	
Attended a training session on door-to-door canvassing?	
Yes	59.0%
No	41.0%
Non-response	0.0%
Number of door-to-door sessions taken part in	
1 to 2	13.3%
3 to 10	48.4%
More than 10	38.2%
Non-response	0.1%

(*continued*)

to take an online anonymous survey. 2,126 (23.0 percent) responded, of whom 1,972 (92.8 percent) had participated in the door-to-door canvassing campaign (Table 1). This survey, although likely not representative due to the low response rate, provides useful insights about the profile of the local activists. French political parties have a relatively large number of active members. On the one hand, this provided Hollande's campaign with a large number of highly motivated volunteers; 87 percent of respondents reported participating in 3 or more rounds of door-to-door canvassing, and 38 percent in more than ten. On the other hand, many of these volunteers were unaccustomed with how to welcome newer activists who were not official party members. As a result, by the end of the campaign, only 12 percent of the respondents were sympathizers involved in a campaign for the first time, while 79 percent were official members of the PS. Relatedly, two-thirds of the canvassers were over 46 years old, reflecting the skewed age pyramid of PS members.

As another consequence of the overwhelming presence of PS members among activists, the campaign could and had to rely extensively on the preexisting structure of the party. The vast majority of the field organizers coordinating the volunteers

TABLE 1—CANVASSERS' PROFILE AND FEEDBACK ON THE CAMPAIGN (*Post-Electoral Survey*) (*continued*)

Panel B. Involvement in the campaign (*continued*)	
Type of areas canvassed	
Big cities (more than 100,000 inhabitants)	25.4%
Middle-size cities (10,000–100,000)	47.2%
Rural areas (<10,000)	27.4%
Non-response	0.1%
Did you (or your local unit) use the list of priority polling stations or municipalities that was provided by the campaign?	
I never heard of this list	29.1%
We did not use this list at all, or only very little	16.3%
We used this list partially	11.2%
We went to almost all the priority polling stations or cities	43.5%
Non-response	0.0%
Did you use the toolkits provided by the campaign?	
No	33.8%
Sometimes	43.7%
Yes, most of the time	22.5%
Non-response	0.0%
How much door-to-door canvassing did you do, compared with other campaign activities?	
I did some door-to-door canvassing, but mostly other activities	24.0%
I did as much door-to-door canvassing as other campaign activities	48.4%
I mostly did door-to-door canvassing	27.6%
Non-response	0.0%
Were there sympathizers in your local canvassers' team?	
Yes	70.7%
No	29.3%
Non-response	0.0%
Did your team try to recruit sympathizers for door-to-door canvassing?	
Yes	65.1%
No	34.9%
Non-response	0.0%
Panel C. Canvassers' feedback on the campaign	
Overall, what do you think of door-to-door canvassing?	
I will not do it again	0.8%
One should do some, but not more than other campaign activities	38.5%
It is a really good technique and should be of the main campaign activities	60.8%
Non-response	0.0%
If you like door-to-door canvassing, why so?	
It is good to take part in a large and countrywide campaign activity	6.9%
It is effective	31.0%
It is fun	2.5%
It is a good way to spread the ideas and values of the left	31.3%
It is an enriching experience	27.2%
Non-response	1.0%
Overall, how helpful was the support provided by the national team and the département's team?	
It was very helpful	49.3%
It was sometime helpful	48.0%
The less we see them, the better we are	2.7%
Non-response	0.0%
Overall, on a scale from 1 to 5 (where 1 means useless and 5 excellent) how did you like the web platform "Toushollande Terrain"?	
1	1.6%
2	7.6%
3	28.3%
4	41.4%
5	21.1%
Non-response	0.0%

Notes: I report the responses of canvassers to an online voluntary postelectoral survey administered during the week following the second round of the 2012 presidential election. Observations = 1,972.

were themselves members and, often, heads of local units of the PS, and most of the *départements*'[6] coordinators had preexisting responsibilities within the party. As a result, the campaign had no direct authority on the field organizers, nor on the *département*'s coordinators. Different was the status of 15 field-based regional coordinators, who were paid by the central campaign team and worked full time under its authority. They assisted in organizing door-to-door sessions and monitoring activists, whom they encouraged and helped with reporting their activity on the campaign's website. Finally, 150 highly motivated and educated national trainers were recruited. Every Saturday, they were sent to the local headquarters of the campaign across France to train field organizers. The training sessions revolved around role playing and taught field organizers how to train and coordinate volunteers themselves. Of respondents to the post-electoral survey, 59 percent had attended a training session. This effort addressed a real need; only 22 percent of the respondents had frequently done door-to-door canvassing before the campaign. The training emphasized a simple message: the field campaign was about door-to-door canvassing, and nothing else. The emphasis placed on door-to-door canvassing was also evident in the campaign material; in addition to leaflets, canvassers received door-hangers dedicated to the door-to-door campaign (see Figure J4 in online Appendix J).

To ensure that the intervention would be administered uniformly, the training course was identical everywhere, and all canvassers received a tool kit with detailed instructions and advice on how to start and lead the conversations. The full tool kit is available in online Appendix J (Figure J1). As in most Get Out the Vote (GOTV) interventions, the instructions provided by the central campaign team were intended as a general canvass, which would be adapted according to each voter's type, interests, and questions.[7] The mobilization of left-wing voters was highlighted as the main objective, as it seemed easier and more likely to win votes than persuading undecided voters, who are the second traditional target of partisan campaigns. Reflecting this strategic choice, canvassers were instructed to systematically provide basic information about the date of the election, the location and opening times of the poll office, and the name of the PS candidate. They urged people to vote, and to vote for Hollande, using general arguments about the importance of voting and of the forthcoming elections as well as personal examples and stories. The discussions usually lasted from one to five minutes. At the end of the discussion, the canvassers typically gave their interlocutor some campaign literature: a thematic leaflet or a 23-page booklet summarizing Hollande's platform. When no one opened, a leaflet or doorhanger was left on the door.

After each canvassing session, activists registered on the campaign's website could report the number of doors knocked and opened, the precinct covered, and provide additional comments. In total, 14,728 reports were entered over the entire course of the campaign, many of which encompassed multiple canvassing sessions, conducted by different teams or on different dates. 1,955 users (21.2 percent of all users with an active profile on the website) entered

[6] *Départements* are one of the three levels of government below the national level, between the region and the municipality. There are a total of 101 départements.

[7] As Green and Gerber (2015) note in their seminal book on GOTV campaigns, scripts are helpful to guide canvassers, but they are not a substitute for informal and personalized discussions, which are central to the effectiveness of door-to-door canvassing.

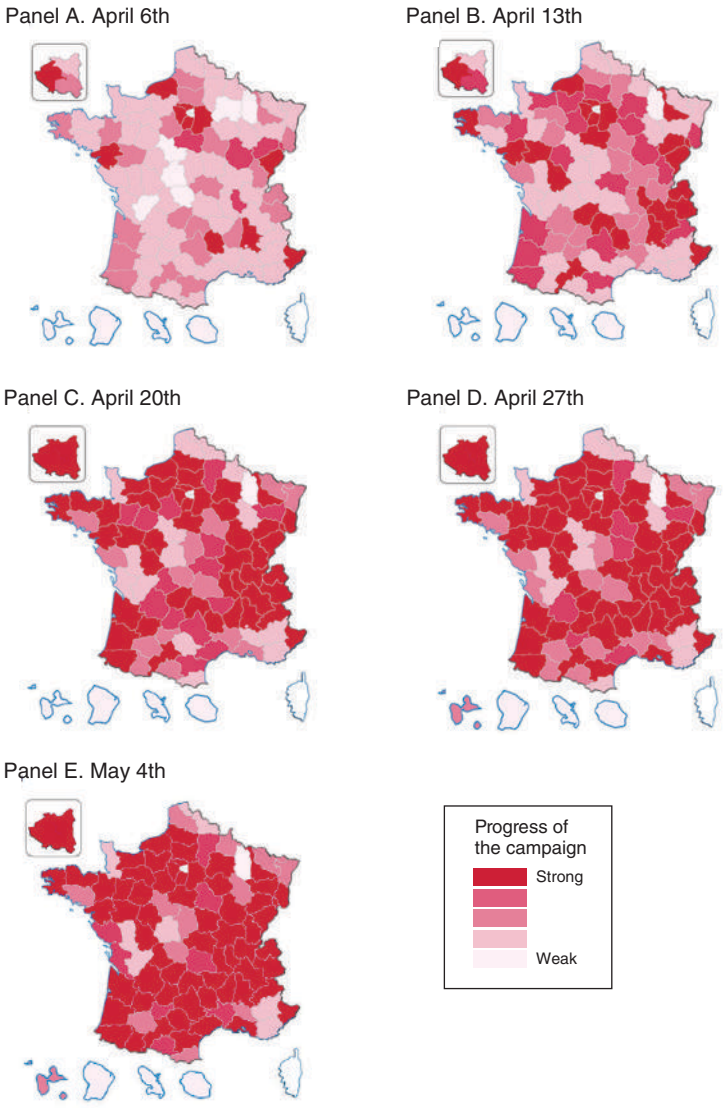


FIGURE 3. WEEKLY PROGRESS OF THE CAMPAIGN, BY DÉPARTEMENT

Note: These maps show the advancement of the campaign against the initial objectives set in terms of number of doors to knock.

at least one report, and an additional 1,420 activists (15.4 percent of those registered on the website) were mentioned in at least one report. As a counterpart to the reporting, the website allowed activists to follow the progress of the campaign in their area. In addition, field organizers and *départements*' coordinators had access to a country map which color-coded the *départements* based on the numbers of doors knocked. Figure 3 shows snapshots of the maps for the five last weeks of the campaign, and Figure J3 in online Appendix J presents the guide distributed to field organizers on how to use the website, with annotated screenshots of its different parts. In some areas, however, field organizers and activists never registered on the campaign platform, and even when they did, they only reported a fraction of all doors knocked.

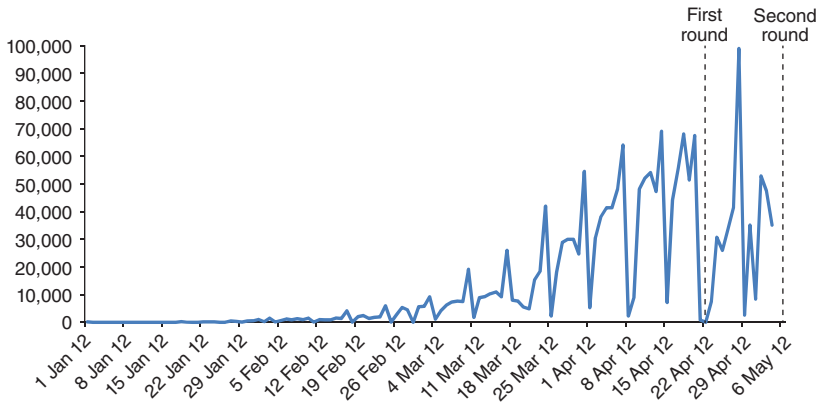


FIGURE 4. DAILY NUMBER OF DOORS KNOCKED IN THE ENTIRE COUNTRY

Note: I plot the number of doors knocked by canvassers as reported by them on the campaign's website.

With the help of the regional coordinators of the campaign, this fraction was estimated *département* by *département* to infer the total number of doors knocked. The scope of the campaign was without comparison in any previous door-to-door efforts of a French political party or organization: overall, approximately five million doors were knocked, of which slightly more than one third were reported on the website.

Figure 4 plots the number of doors knocked over time as reported on the website. As is clear from this graph, the pace of the campaign was very slow until six weeks prior to the first round. It then increased gradually and reached its peak between the two rounds. Underlying this long-term trend, short-term weekly cycles are easily identifiable. Each week, the canvassing sessions took place mostly on Fridays and Saturdays. On average, the door-opening rate was high, around 48 percent, and activists usually worked in pairs.

II. Experimental Design and Implementation

A. *Randomization*

Definition of Territories as a Set of Contiguous Municipalities.—Before the start of the door-to-door campaign, I split the entire country into territories defined as a set of contiguous municipalities sharing a common zip code.[8] Any new activist registering on the campaign's website was allocated to the territory corresponding to his zip code and put in touch with the corresponding PS local unit.

Definition of the Target Number of Registered Citizens in Each Territory.—The overall objective of knocking on five million doors was translated into a target number of registered citizens for each territory, $\widetilde{TA}$.[9] This variable was set proportionally

[8] There was one exception to this rule: in each *département*, zip codes corresponding to municipalities with a total of fewer than 5,000 registered citizens were subsumed under the same territory.

[9] The objective communicated to the canvassers in each territory was expressed as a number of doors. To go from a number of doors to a number of registered citizens, I assumed that each door represented 1.4 registered

to the total number of registered citizens in the territory and to a proxy for the potential to win votes, $\widetilde{PO}$. The variable $\widetilde{PO}$ was defined as the fraction of nonvoters multiplied by the left vote share among active voters, each taken from the results of the second round of the 2007 presidential elections.[10]

Level of Randomization.—Randomization was done within each of 3,260 territories separately. In territories where the geographic boundaries of the electoral precincts were known for all, or most municipalities (based on the 2011 voter rolls), randomization was done at the precinct level.[11] In the remaining territories, randomization was done at the municipality level. Henceforth, for conciseness, I designate the unit of randomization as "precincts," even when the randomization was done at the municipality level.[12]

Randomization.—The randomization rule was identical across all territories. It was designed in a way that ensured that precincts allocated to canvassers had the highest possible estimated potential to win votes $\widetilde{PO}$ compatible with running an experiment. I proceeded in three steps, which Figure 5 illustrates using the hypothetical example of a territory with an arbitrary number (17) of precincts.

The first step was the stratification. I computed $\widetilde{PO}$ in each precinct of the territory, and ranked precincts from the highest to the lowest $\widetilde{PO}$. I grouped precincts in strata of five: the territory's first stratum comprised the five precincts with the highest $\widetilde{PO}$, the second stratum the five precincts ranked immediately below, and so on until the last stratum, composed of the five or fewer remaining precincts. The first stratum of any territory was always included in the randomization. In some territories, additional strata were also included in the randomization, as will become clear from the second and third steps.

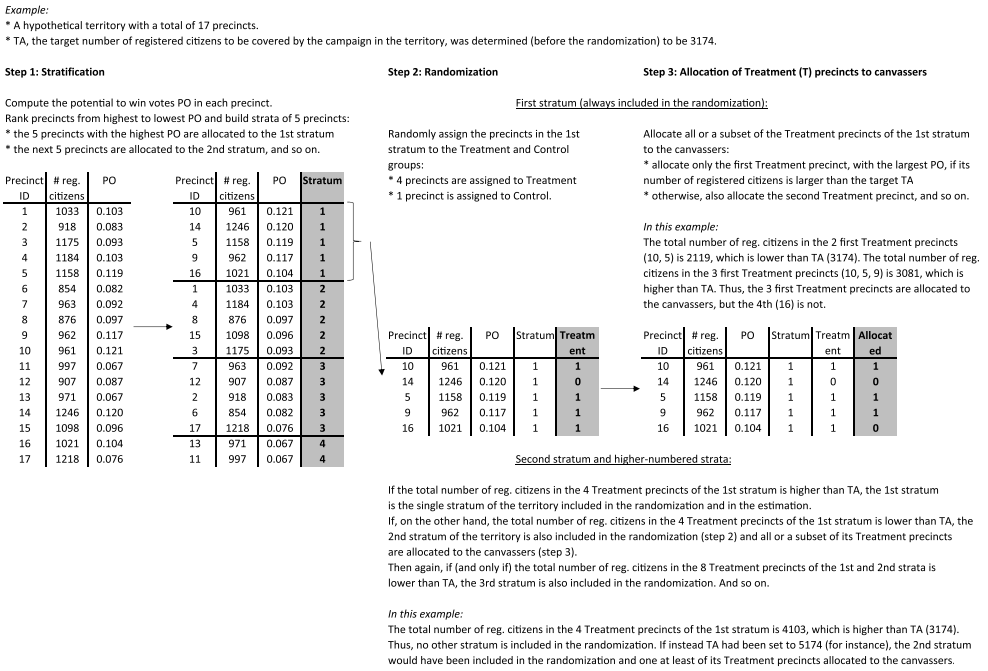
The second step was the randomization itself. Focusing first on the territory's first stratum, I randomly assigned its precincts to the treatment and control groups, using random numbers generated in Stata. When the first stratum included five precincts, exactly four (80 percent) of these precincts were randomly assigned to the treatment group, and one (20 percent) to the control group. In the small set of territories in which the first stratum included fewer than five precincts (due to the territory itself including fewer than five), each precinct was assigned with an 80 percent probability to the treatment group and with a 20 percent probability to the control group.

citizens on average, a ratio obtained by dividing the total number of registered citizens in France, 46.0 million, by the total number of dwellings, 33.2 million.

[10] This definition of $\widetilde{PO}$ could only be applied directly to precincts whose boundaries had not changed since 2007. In these precincts, I regressed $\widetilde{PO}$ (computed using this definition) on characteristics constructed based on the 2011 voter rolls (average building size, this variable squared, the proportion of buildings with fewer than 5, between 5 and 15, or more than 15 registered citizens, average age, this variable squared, the proportion of citizens younger than 25, and the proportion of citizens older than 65). I then used the estimated coefficients to predict (or, technically, construct) $\widetilde{PO}$ in precincts whose boundaries had changed since 2007.

[11] There does not exist any comprehensive database of the boundaries of French voter precincts, which are drawn by the municipalities. However, to organize its 2011 primary elections, the PS had collected voter registers in all sufficiently large municipalities. These voter registers indicate the address and precinct of each registered citizen and could thus be used to infer the geographical boundaries of the corresponding precincts.

[12] The list of 3,260 territories excludes 279 territories each counting a unique municipality of unknown precinct boundaries: in these territories, the single municipality had to be allocated to canvassers in any case (so that they could participate in the door-to-door campaign), preventing randomization.



Precinct ID	# reg. citizens	PO
1	1033	0.103
2	918	0.083
3	1175	0.093
4	1184	0.103
5	1158	0.119
6	854	0.082
7	963	0.092
8	876	0.097
9	962	0.117
10	961	0.121
11	997	0.067
12	907	0.087
13	971	0.067
14	1246	0.120
15	1098	0.096
16	1021	0.104
17	1218	0.076

Precinct ID	# reg. citizens	PO	Stratum
10	961	0.121	1
14	1246	0.120	1
5	1158	0.119	1
9	962	0.117	1
16	1021	0.104	1
4	1184	0.103	2
8	876	0.097	2
15	1098	0.096	2
3	1175	0.093	2
7	963	0.092	3
12	907	0.087	3
2	918	0.083	3
6	854	0.082	3
17	1218	0.076	3
13	971	0.067	4
11	997	0.067	4

Precinct ID	# reg. citizens	PO	Stratum	Treatment
10	961	0.121	1	1
14	1246	0.120	1	0
5	1158	0.119	1	1
9	962	0.117	1	1
16	1021	0.104	1	1

Precinct ID	# reg. citizens	PO	Stratum	Treatment	Allocated
10	961	0.121	1	1	1
14	1246	0.120	1	0	0
5	1158	0.119	1	1	1
9	962	0.117	1	1	1
16	1021	0.104	1	1	0

FIGURE 5. RANDOMIZATION RULE, AND ALLOCATION OF TREATMENT PRECINCTS TO CANVASSERS

In the third step, I defined the list of precincts of the first stratum which canvassers would be asked to cover. This list was prepared before the start of the campaign. Precincts allocated to canvassers included only treatment precincts (and no control precincts), but not necessarily *all* treatment precincts. The treatment precinct with the largest potential $\widetilde{PO}$ was always allocated to the canvassers. If the number of citizens registered in this precinct was larger than the target number of registered citizens for the territory $\widetilde{TA}$, no other treatment precinct was allocated to the canvassers. If its number of registered citizens was lower than $\widetilde{TA}$, the treatment precinct with the second largest $\widetilde{PO}$ was also allocated to the canvassers. Then again, if the combined number of registered citizens in the first and second treatment precincts was larger than $\widetilde{TA}$, no other treatment precinct was allocated to the canvassers. Otherwise, the treatment precinct with the third largest $\widetilde{PO}$ was allocated to the canvassers and the same rule was used one last time to decide whether or not to also allocate the fourth (and last) treatment precinct to the canvassers.

In the vast majority of territories, the total number of registered citizens in the treatment precincts of the first stratum was higher than $\widetilde{TA}$, and no other stratum was included in the randomization and estimation. If and only if the total number of registered citizens in the treatment precincts of the first stratum remained lower than $\widetilde{TA}$, the second stratum was also included in the randomization. The second and third steps were then repeated on this stratum. If needed, additional strata were included until the total number of registered citizens in treatment precincts allocated to canvassers was equal or higher than $\widetilde{TA}$.

Discussion of the Randomization.—Two aspects of this randomization are unusual, without posing any threat to the validity of the design. First, it is unusual

not to allocate all treatment units to receive the intervention. However, there are other randomization designs in which only a fraction of the treatment units end up receiving the intervention. For instance, in encouragement designs, a random group of subjects is offered an intervention, and only a (nonrandom) subset takes it (e.g., Hirano et al. 2000; Duflo and Saez 2003). In these designs, we typically think of take-up (conditional on treatment) as being driven by idiosyncratic (often unobservable) characteristics of individuals. For example, in a medical experiment, individuals who comply with the treatment may be unobservably different from non-compliers. In my experiment, such noncompliance is present by design. The objective in allocating only a fraction of the treatment precincts to the canvassers was to ensure that they would focus their efforts on the treatment precincts in which the potential to win votes was deemed highest. Importantly, similarly as in an encouragement design, the fact that the assignment of units to the treatment and control groups was entirely random makes it possible to estimate the impact of the door-to-door campaign causally, despite the fact that not all treatment precincts were allocated to canvassers. As shown in the empirical strategy in Section IIB, all results rely on the randomization as the unique source of identification.

Second, randomized experiments typically select the sample in a first step, and randomly assign sample units to the treatment and control groups in a second step. These two steps were not entirely separate in the present experiment. As mentioned, the first stratum of each territory was always included in the randomization, and in a few territories additional strata were included as well. The unusual aspect is that the decision to include an additional stratum in the randomization, in a particular territory, depended in part on which precincts had been assigned to treatment and control in strata already included. The probability that a second stratum would need to be included was slightly lower when the smallest precinct of the first stratum was assigned, by chance, to the control group, than when it was assigned to the treatment group (as being assigned to the control group increased the likelihood that the combined number of registered citizens in all treatment precincts of the first stratum would be higher than $\widetilde{TA}$). The same holds for the likelihood that a third stratum would need to be included in the randomization, conditionally on having included two strata already, and so on for the subsequent strata. Importantly, this does not alter the symmetry between treatment and control precincts in the final sample.[13] In addition, I check the robustness of the results by restricting the analysis to subsamples defined by the first stratum of each territory (which, again, always had to be included in the randomization) or the smallest set of strata of each territory which, based on the rule above, would be included in the randomization under any possible treatment assignment in lower-numbered strata.[14] In these two subsamples, the separation between sample selection and randomization is satisfied.

[13] To convince oneself of this, first consider the set of first strata of all territories (whether a second stratum was also included or not). By construction, the assignment of the precincts to the treatment and control groups in these first strata was random. Then consider the second stratum of all territories in which a second stratum was included (whether a third stratum was also included or not). Again, by construction, the assignment to treatment and control in these second strata was random. The same holds for the group of third strata, and so on. Therefore, adding all groups of strata together, the assignment of precincts to the treatment and control groups was random.

[14] The first stratum of each territory always falls in this set. The second stratum also falls in this set if, in the event that the smallest precinct of the first stratum was assigned by chance to the control group, the total number of registered citizens in the treatment precincts would remain lower than $\widetilde{TA}$. And so on for the subsequent strata.

The corresponding tables are included in online Appendix C (Tables C1 through C4 and C5 through C8, respectively). All main results are robust to both restrictions.

B. *Empirical Strategy*

I estimate the effect of door-to-door canvassing on voter turnout and vote shares at the 2012 presidential election as well as the 2012 parliamentary elections and the 2014 European elections. To preserve the integrity of the randomization, treatment precincts not allocated to canvassers are maintained in the treatment group in all regressions. To account for the fact that not all treatment precincts were allocated to the canvassers, I estimate two parameters of interest for each electoral outcome. First, I show the effect of a precinct being *assigned to the treatment group* (the intent-to-treat effect of the campaign), using the following OLS specification:

$$Y_i = \alpha_1 + \beta_1 T_i + X_i' \lambda_1 + \sum_s \delta_{i1}^s + \epsilon_{i1}, \tag{1}$$

where Y_i is the outcome in precinct i, T_i is a dummy equal to one if the precinct was assigned to the treatment group and zero if it was assigned to the control group, δ_{i1}^s are strata fixed effects, and X_i is a vector of controls.

Secondly, I evaluate the effect of a precinct being actually *allocated to canvassers* (a local average treatment effect) with the following specification:

$$Y_i = \alpha_2 + \beta_2 A_i + X_i' \lambda_2 + \sum_s \delta_{i2}^s + \epsilon_{i2}, \tag{2}$$

where A_i is a dummy equal to one if the precinct was allocated to the canvassers and zero otherwise, and is instrumented with T_i as shown in the following first-stage equation:

$$A_i = a + b T_i + X_i' \lambda + \sum_s \delta_i^s + \nu_i.^{15} \tag{3}$$

In all tables, I present estimates of equation (1) in panel A, and estimates of equation (2) in panel B. The key coefficients of interest are β_1 and β_2, which indicate respectively the effect of the door-to-door visits in precincts that were assigned to the treatment group and the effect in treatment precincts that were allocated to canvassers. These effects combine the direct impact of the visits on voters who received them with potential spillovers on other voters from the same precincts who did not

[15] The 2SLS estimate obtained from equations (2) and (3) can be interpreted as a local average treatment effect since all assumptions of the local average treatment effect (LATE) theorem are satisfied (Imbens and Angrist 1994). Independence of the instrument comes from the random assignment of precincts to the treatment group; the first stage is strong (see Section IIIB and Table 3); the exclusion restriction is satisfied as the assignment of a precinct to the treatment group only mattered to the extent that the precinct would be allocated to canvassers; and monotonicity is fulfilled as the rule used to allocate treatment precincts to canvassers did not generate any defier: of the treatment precincts that were not allocated to the canvassers, none would have been allocated to them if they had been in the control group (since no control group precinct was allocated to the canvassers in the first place).

receive the visit but talked to voters who did. The research design cannot distinguish between direct and indirect impacts.

All regressions use within estimators and robust standard errors.[16] I use three distinct specifications. The first does not control for any variable except for the strata fixed effects. The second controls for $\widetilde{PO}$ (the proxy for the potential to win votes), which was used to construct the strata, as well as a baseline measure of the outcome at the 2007 presidential election. The third and main specification also controls for the number of registered citizens as well as the level and the five-year change of the following census variables: the size of the municipality; the share of men; the share of the population below 14, between 15 and 29, between 30 and 44, between 45 and 59, between 60 and 74, and above 75; the share of the working population; and the rate of unemployment.[17] Finally, regressions estimating the effect of the campaign at the parliamentary elections control for constituency fixed effects to account for differences in the number and identity of competing candidates across constituencies.

C. *Identification of Territories Which Followed the Randomization Plan*

In each territory, the list of allocated precincts and, when available (and when the randomization had been done at the precinct, not municipality, level), a list of voter addresses corresponding to these precincts, could be downloaded as Excel files by the field organizers from their personal account on the campaign's website. However, a large fraction of territories which participated in the door-to-door campaign did not use the list of allocated precincts, for two main reasons: (i) never getting access to this list, as no field organizer in the territory registered on the campaign website and downloaded the list; and (ii) local units of the PS deciding autonomously which areas to cover. In sum, the difficulties that even the most professional campaigns face to control the selection of political activists' demographic characteristics and ideology (Enos and Hersh 2015) extended in this election to controlling *where* activists campaigned.

This resulted from the few resources available to the central team to coordinate the campaign locally, which limited efforts to encourage activists to register on the website and use the lists prepared by the central team. As mentioned in Section IC, the central team only directly hired and managed 15 regional coordinators. Second, the campaign website was not as advanced as technological tools used by recent US campaigns. In particular, it did not provide maps of allocated precincts and did not allow activists to prepare walk lists for door-to-door sessions organized in these

[16] The main tables do not cluster the standard errors since the unit of observation is the same as the unit of randomization (the precinct). The results are robust to using regular cluster robust standard errors at the level of the territory or *département* or allowing for correlation of the error terms at the level of the *départements* or the regions with the wild cluster bootstrap procedure (Cameron, Gelbach, and Miller 2008) and pairs cluster bootstrap procedure (Esarey and Menger forthcoming). All results with clustered standard errors are shown in online Appendix D (Tables D1 through D8).

[17] Until 1999, a general census was conducted in the entire country every five to ten years. Since 2006, the French national statistics agency (Insee) publishes yearly census results at the municipality level based on data collected continuously over five years. For instance, the 2006 census results are based on data collected from 2004 to 2008. The Insee emphasizes that any evolution should be observed over a span of five years or more to ensure that the comparison relies on entirely different datasets (Insee 2014). Accordingly, I use census results for 2006 and 2011.

precincts. In US campaigns, such features foster use of the website and compliance with addresses or precincts deemed priority by the campaign's analytics team. Instead, in this campaign, many groups of activists found it easier to campaign in areas that they already knew, including their own neighborhood. Third, the fact that many local units of the PS came up with their own prioritization of areas to cover reflects the fact that these units preexisted the campaign, and it echoes their culture of relative autonomy with respect to the hierarchy of the party and, a fortiori, with respect to the presidential candidate and his central team. Local units which did not follow the list of allocated precincts instead targeted areas based on their own understanding of electoral dynamics on their turf and a set of priorities, which included of course the presidential election, but also gave weight to strategic considerations pertaining to future local races in which members of the unit would compete.

Estimates of the effects of the campaign in territories that did not use the list of allocated precincts should be null in expectation, as areas covered in these territories are orthogonal to randomization. Including these territories in the analysis will decrease precision and may add noise, due to, for instance, tiny underlying differences between treatment and control areas, or non-zero correlation between random assignment and actual coverage in these areas. In fact, estimates presented in Tables A2 and A3 in online Appendix A that include all territories, whether or not they used the list of allocated precincts, are close to zero but consistent with substantial positive or negative effects on turnout and vote shares in territories that did use the lists.

Instead, the analysis that follows uses data from territories that used the list of allocated precincts and thus actually participated in the experiment. I identify these territories by combining two independent sources of information: responses to a question included in the postelectoral online survey on the use of allocated precincts, and daily reports entered by activists on the campaign website. Did at least one survey respondent based in the territory mention that local activists in this territory used the list of allocated precincts? Or does the territory show at least one report indicating the precinct covered, signaling actual usage of the campaign website and accountability with respect to precincts allocated by the campaign's central team? The main results shown below are based on all territories which verify either the first or the second criterion.[18] For robustness, I also show results based on sets of territories characterized using only one of the two criteria.

Seven hundred ninety-one territories verify either the first or the second criterion. This corresponds to 24.3 percent of all 3,260 territories, and 42.3 percent of the corresponding population. In these 791 territories, 966 strata containing 4,674 precincts and 5.02 million registered voters were included in the randomization.[19]

[18] The survey question used to construct the first criterion was "Did you (or your local unit) use the list of priority polling stations or municipalities that was provided by the campaign?" and the possible answers were "I never heard of this list" (1), "We did not use this list at all, or only very little" (2), "We used this list partially" (3), and "We went to almost all the priority polling stations or cities" (4). I consider that the criterion is satisfied when at least one survey respondent based in the territory provided the fourth answer. The results are robust (and nearly identical) to including territories in which at least one survey respondent provided the third or fourth answer.

[19] In most (87.6 percent) of the territories, only one stratum was included in the randomization. In 7.3 percent of the territories, two strata were included, and in the remaining 5.1 percent three or more strata were included. Table 2 verifies the symmetric distribution of observed characteristics between treatment and control precincts in territories verifying either the first or the second criterion (see Section IIIA). Table A1 does the same using all

Little more that eighty (80.2) percent (3,748) of the precincts were randomly assigned to the treatment group and 19.8 percent (926) were assigned to the control group; 57.1 percent (2,139) of the treatment precincts were allocated to canvassers.

Since the randomization was conducted on precincts defined according to the 2011 voter rolls, all results need to exclude precincts whose boundaries changed between 2011 and 2012. In addition, specifications controlling for past outcomes need to exclude precincts whose boundaries had changed between 2007 and 2011. As a result, depending on the specification, the total number of precincts used in the tables is either 3,397 (in specifications that do not control for past outcomes) or 2,665 (in specifications that do).[20]

D. *Imperfect Compliance*

Even in territories that used the lists of allocated precincts, compliance with these lists remained imperfect. In some cases, the number of canvassers was too small to cover all allocated precincts, and in others, canvassers covered precincts other than those allocated. Failure to account for the imperfect compliance with the lists of allocated precincts would lead to underestimate the impact of the visits.

Therefore, in addition to the effects reported in the tables, of a precinct being assigned to the treatment group and of a precinct being allocated to the canvassers, which are estimated using equations (1) and (2), respectively, I compute a third effect. I scale up raw regression estimates from equation (1) by a factor inversely proportional to the differential intensity of the campaign in treatment and control precincts: $m = \frac{1}{f_T - f_C}$, where f_T (respectively, f_C) denotes the fraction of registered citizens that were reached by the campaign in treatment (respectively, control) precincts. This accounts both for the fact that not all treatment precincts were allocated to the canvassers and for imperfect compliance on the part of canvassers. It also provides an estimate of the effect of the visits in precincts that were covered by canvassers and would not have been covered if they had not been assigned to the treatment group.[21]

Terms f_T and f_C can be rewritten as $f_T = \frac{x_T N}{N_T}$ and $f_C = \frac{x_C N}{N_C}$, where N is the total number of registered citizens reached by the campaign, N_T (respectively, N_C) is the number of registered citizens in treatment (respectively, control) precincts, and x_T (respectively, x_C) is the fraction of doors knocked that were located in treatment

territories, whether or not they used the list of allocated precincts. Finally, I check that the observed characteristics of precincts in territories which do not satisfy the verification criteria do not systematically differ across treatment and control: in a regression of a dummy equal to one for precincts located in territories which do not satisfy the verification criteria on the treatment dummy, the characteristics shown in Table 2, and their interaction with the treatment dummy, I test for the joint significance of the characteristics interacted with treatment and fail to reject the null (p-value of 0.98).

[20] Each year, municipalities can add new precincts, merge existing precincts, or move precinct boundaries, to take into account changes in the number of registered citizens in each neighborhood. The 2011 voter rolls collected by the PS provide a precise description of precinct boundaries in that year. I further identify boundaries' changes before and after 2011 based on changes in the number of precincts in a given municipality as well as changes in the number of registered citizens contained in each precinct.

[21] I compute this effect (by scaling raw ITT estimates by the multiplier) instead of estimating it with an IV regression (where precinct coverage would be instrumented by treatment) since available information on the extent to which a particular precinct was covered is imperfect and missing for a large fraction of precincts.

(respectively, control) precincts. Since treatment precincts include both precincts allocated to canvassers and precincts not allocated to them, f_T can further be rewritten as $f_T = \frac{x_T N}{N_T} = \frac{\left(x_{T,A} + x_{T,\bar{A}}\right) N}{\left(N_{T,A} + N_{T,\bar{A}}\right)}$, where the subscript T, A (respectively, $T, \bar{A}$) designates allocated (respectively, non-allocated) treatment precincts.

Therefore,

$$m = \frac{1}{N} \times \frac{1}{\frac{\left(x_{T,A} + x_{T,\bar{A}}\right)}{\left(N_{T,A} + N_{T,\bar{A}}\right)} - \frac{x_C}{N_C}}. \tag{4}$$

I call m the "differential intensity multiplier." Its size is driven by two factors. The first was the decision to allocate only a fraction of the treatment precincts to the canvassers: if canvassers had fully complied with the corresponding list, then we would have $x_{T,A} = 1$, $x_{T,\bar{A}} = x_C = 0$, $N = N_{T,A}$, and the complier would be equal to $\frac{N_{T,A} + N_{T,\bar{A}}}{N_{T,A}} = \frac{N_T}{N_{T,A}}$, which is the ratio between the number of registered citizens in all treatment precincts and in the subset of treatment precincts allocated to canvassers. The second factor is canvassers' imperfect compliance with the list of allocated precincts, which further increases the multiplier.

I compute the multiplier for the first and second rounds separately: m^1 and m^2. From voter rolls, in territories which participated in the experiment, $N_{T,A} = 2{,}486{,}941$, $N_{T,\bar{A}} = 1{,}613{,}156$, and $N_C = 924{,}159$. Further, using door-to-door reports indicating the precinct covered, I calculate that, by the second round, 72.5 percent of doors knocked were located in treatment precincts allocated to canvassers, 14.1 percent in treatment precincts not allocated to them, and 13.3 percent in control precincts: $x^2_{T,A} = 72.5\%$, $x^2_{T,\bar{A}} = 14.1\%$, and $x^2_C = 13.3\%$. Finally, based on the assessment that the door-to-door campaign knocked on the initial target number of doors overall, $N^2 \simeq N_{T,A}$, and I get the second round multiplier $m^2 \simeq 6.0$. Based on door-to-door reports, contacts which occurred before the first round account for 76.5 percent of all doors knocked: $N^1 = 0.765 \times N^2$. In addition, $x^1_{T,A} = 73.0\%$, $x^1_{T,\bar{A}} = 14.0\%$, and $x^1_C = 13.0\%$. Thus, I get the first round multiplier $m^1 \simeq 7.3$.

Unlike the results from equations (1) and (2) shown in the tables, the exact magnitude of the multiplier depends on the accuracy of the canvassers' reports and of the overall scale of the campaign N, and it should thus be interpreted with caution. Overestimating N would mean underestimating m (which is inversely proportional to it) and, thus, underestimating the effect of the campaign in precincts that were covered by canvassers and would not have been covered if they had not been assigned to the treatment group.

III. Results

A. *Verifying Randomization*

Randomization ensures that all observable and unobservable characteristics should be symmetrically distributed between treatment and control precincts. Table 2 verifies this for a series of observed characteristics. It presents summary

TABLE 2—SUMMARY STATISTICS

	Control group		Treatment group		*p*-value treatment = control	Number of observations
	Mean	SD	Mean	SD		
Panel A. Electoral outcomes						
Randomization at precinct level	0.504	0.500	0.504	0.500	0.992	3,397
Number of registered citizens	1,014.3	1,097.6	1,133.8	1,605.3	0.022	3,397
Potential to win votes, PO	0.089	0.035	0.089	0.033	0.970	3,397
Voter turnout, 2007 pres. election, first round	0.843	0.050	0.840	0.048	0.231	2,665
Voter turnout, 2007 pres. election, second round	0.837	0.045	0.836	0.045	0.675	2,665
PS vote share, 2007 pres. election, first round	0.274	0.081	0.279	0.081	0.172	2,665
PS vote share, 2007 pres. election, second round	0.515	0.103	0.516	0.101	0.743	2,665
Panel B. Location						
Population of the municipality	68,310.9	277,136.8	66,254.4	273,841.2	0.863	3,397
Region						
Ile-de-France	0.160	0.367	0.160	0.367	0.994	3,397
Champagne-Ardenne	0.028	0.166	0.028	0.164	0.927	3,397
Picardie	0.053	0.225	0.054	0.226	0.953	3,397
Haute-Normandie	0.043	0.203	0.041	0.199	0.861	3,397
Centre-Val de Loire	0.058	0.234	0.058	0.235	0.958	3,397
Basse-Normandie	0.024	0.152	0.025	0.156	0.851	3,397
Bourgogne	0.039	0.193	0.039	0.193	0.999	3,397
Nord-Pas-de-Calais	0.016	0.127	0.019	0.136	0.663	3,397
Lorraine	0.043	0.203	0.045	0.207	0.839	3,397
Alsace	0.016	0.127	0.019	0.137	0.616	3,397
Franche-Comté	0.024	0.152	0.024	0.153	0.984	3,397
Pays-de-la-Loire	0.067	0.250	0.064	0.245	0.815	3,397
Bretagne	0.058	0.234	0.061	0.240	0.731	3,397
Poitou-Charentes	0.024	0.152	0.024	0.154	0.939	3,397
Aquitaine	0.045	0.206	0.044	0.204	0.927	3,397
Midi-Pyrénées	0.040	0.196	0.040	0.196	0.997	3,397
Limousin	0.034	0.182	0.030	0.172	0.637	3,397
Rhône-Alpes	0.113	0.317	0.109	0.312	0.786	3,397
Auvergne	0.040	0.196	0.040	0.195	0.962	3,397
Languedoc-Roussillon	0.046	0.210	0.046	0.209	0.992	3,397
Provence-Alpes-Côte-d'Azur	0.028	0.166	0.028	0.166	0.990	3,397
Corse	0.001	0.039	0.001	0.038	0.993	3,397
Panel C. Sociodemographic characteristics of the population of the municipality						
Share of men	0.488	0.023	0.487	0.024	0.273	3,397
Share of the population with age						
0–14	0.182	0.038	0.181	0.037	0.541	3,397
15–29	0.175	0.052	0.175	0.053	0.923	3,397
30–44	0.196	0.034	0.195	0.032	0.810	3,397
45–59	0.207	0.033	0.205	0.033	0.222	3,397
60–74	0.148	0.042	0.149	0.043	0.325	3,397
75 and older	0.093	0.038	0.094	0.040	0.521	3,397
Within population of 15–64						
Share of working population	0.726	0.052	0.723	0.054	0.185	3,397
Share of unemployed (among working population)	0.123	0.050	0.124	0.051	0.575	3,397
Median income	19,234.3	3,850.1	19,262.1	3,911.2	0.870	3,246

Notes: For each variable, I report the means and standard deviations in both the control group and the treatment group and indicate the *p*-value of the difference. The unit of observation is the unit of randomization (precinct or municipality).

statistics separately for the control and treatment groups. I also show the difference between the means of the two groups and report the *p*-value of a test of the null hypothesis that they cannot be distinguished from each other. Overall, precincts in the two groups are very similar. I regress the treatment dummy on all characteristics included in Table 2 and test for their joint significance. I fail to reject the null (p-value of 0.97). One of the differences shown in Table 2 is significantly different from zero

at the 5 percent level, however: the number of registered citizens, a variable which can be particularly important for turnout. For all results shown below, I include this variable as a control in one of the specifications. This has only a minimal impact, including in regressions measuring the impact on turnout. The results are also robust to trimming the 5 or 10 percent of precincts with the largest number of registered citizens (tables included in online Appendix E).

The average precinct contained 1,110 registered citizens. All 22 metropolitan French regions were represented in the sample. The municipality of the average precinct contained 67,000 citizens. In the municipality of the average precinct, 49 percent of the inhabitants were men, 36 percent were under 30 years old, 40 percent were between 30 and 60 years old, and 24 percent were older than 60. The working population accounted for 72 percent of all people aged 15 to 64, of which 12 percent were currently unemployed, and median income was about 19,000 euros.

Finally, baseline participation, measured at the 2007 presidential election, was 84 percent, and the vote share of the PS candidate, Ségolène Royal, was 28 percent at the first round and 52 percent at the second round of this election. Treatment precincts are slightly more to the left, and characterized by a slightly lower participation than control precincts. Given the high correlation between electoral outcomes in the past and present, most specifications in the analysis below control for baseline electoral outcomes.

B. *First Stage*

As discussed in Section IIA, not all treatment precincts were allocated to the canvassers. In all tables that follow, I present estimates of equation (1), which evaluates the effect of a precinct being assigned to the treatment group, in panel A, and estimates of equation (2), which evaluates the effect of a precinct being allocated to canvassers, in panel B. Equation (2) instruments the dummy "allocated to canvassers" with the treatment assignment dummy. The estimation of the corresponding first stage equation (equation (3)) is presented in Table 3.

I control for strata fixed effects in column 1, and find a first stage of 0.565. In addition to strata fixed effects, columns 2 through 7 also control for variables included in some of the 2SLS specifications: past outcome (turnout or PS vote share at the first round, second round, or averaged over both rounds of the 2007 presidential elections), and additional controls (the number of registered citizens in the precinct or municipality, as well as the level and the five-year change of the census variables). All estimates are significant at the 1 percent level, and similar in size.

C. *Effects on the 2012 Presidential Election*

Voter Turnout.—The impact of the door-to-door visits on voter turnout in the 2012 presidential election is analyzed in Table 4. I use as the outcome voter turnout in the first round (columns 1 through 3), in the second round (columns 4 through 6), and averaged over the two rounds (columns 7 through 9). In the control group, 79.5 and 80.1 percent of the voters participated in the first and second rounds. Door-to-door canvassing had no significant effect on voter turnout in either the first or the second round. The point estimates are relatively small in all specifications, whether

Table 3—First Stage

	No control	With controls					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	0.5652	0.5233	0.5247	0.5238	0.5241	0.5245	0.5240
	(0.0137)	(0.0173)	(0.0172)	(0.0172)	(0.0173)	(0.0172)	(0.0172)
Strata fixed effects	X	X	X	X	X	X	X
Control for past outcome and PO		X	X	X	X	X	X
Additional controls		X	X	X	X	X	X
2007 outcome controlled for		Voter turnout, round 1	Voter turnout, round 2	Voter turnout, average	Vote share Royal, round 1	Vote share Royal, round 2	Vote share Royal, average
Observations	3,390	2,660	2,660	2,660	2,660	2,660	2,660
R^2	0.258	0.424	0.423	0.424	0.423	0.424	0.424
Mean in control group	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

Notes: The table shows first stage results from equation (3). The unit of observation is the unit of randomization (precinct, or municipality). Robust standard errors are in parentheses. All regressions include strata fixed effects. Regressions in columns 2 through 7 control for PO (proxy for the potential to win votes) and for past outcomes, measured at the level of randomization: voter turnout or vote share obtained by Ségolène Royal in the first round, in the second round, or averaged over both rounds of the 2007 presidential election. Additional controls include the number of registered citizens in the precinct or municipality as well as the level and the five-year change of the following census variables: the municipality's population, the share of men, the share of different age groups (from 0 to 14; from 15 to 29; from 30 to 44; from 45 to 59; from 60 to 74; above 75), the share of working population, and the share of unemployed population among the working population. Regressions controlling for past outcomes need to exclude precincts whose boundaries had changed after 2007, which explains the lower number of observations.

or not control variables are included. Considering the upper bound of the 95 percent confidence interval, I can reject any effect higher than 0.40 percentage points in the first round at the 5 percent level and any effect higher than 0.20 percentage points in the second round, in the specification including all controls (columns 3 and 6). I do not find any significant impact of the door-to-door visits either on subsamples of territories identified as following the list of allocated precincts based only on canvassers' reports (Table B2 in online Appendix B) or their answers to the post-electoral survey (Table B5).

Vote Shares Obtained by François Hollande.—I now examine the impact of door-to-door canvassing on the vote shares obtained by François Hollande. As shown in Table 5, François Hollande obtained 31.6 percent of the votes in the control group in the first round and 57.6 percent in the second round. In treatment precincts, the door-to-door visits increased his vote share by 0.63 percentage points in the first round (panel A, column 1) and by 0.48 percentage point in the second round of the presidential election (column 4). These estimates are significant at the 1 and 10 percent level, respectively. When I control for past outcomes, $\widetilde{PO}$, the number of registered citizens, and census variables, I obtain estimates of 0.44 and 0.46 percentage points at the first and second rounds, both significant at the 5 percent level (columns 3 and 6). In precincts that were actually allocated to canvassers, the effects were 0.84 and 0.87 percentage points (panel B, columns 3 and 6). Applying the first and second rounds differential intensity multipliers computed in Section IID to panel A's ITT estimates, I obtain effects of 3.24 percentage points and 2.75 percentage points in the first and second rounds. This measures the impact of the visits in precincts that were covered by canvassers and would not have been covered if they had not been assigned to the treatment group. Again, I check the robustness of the

Table 4—Impact on Voter Turnout

	First round			Second round			Average of first and second rounds		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. ITT estimation									
Treatment	0.0001	0.0008	0.0011	−0.0005	−0.0011	−0.0008	−0.0002	−0.0001	0.0002
	(0.0016)	(0.0015)	(0.0015)	(0.0015)	(0.0015)	(0.0014)	(0.0015)	(0.0014)	(0.0014)
Strata fixed effects	X	X	X	X	X	X	X	X	X
Control for past outcome and PO		X	X		X	X		X	X
Additional controls			X			X			X
Observations	3,390	2,660	2,660	3,390	2,660	2,660	3,390	2,660	2,660
R^2	0.000	0.328	0.410	0.000	0.255	0.326	0.000	0.328	0.405
Mean in control group	0.7951	0.8081	0.8081	0.8014	0.8122	0.8122	0.7983	0.8101	0.8101
Panel B. Instrumental variable estimation: "allocated to canvassers" instrumented with "treatment"									
Allocated to canvassers	0.0001	0.0015	0.0021	−0.0009	−0.0021	−0.0015	−0.0004	−0.0001	0.0004
	(0.0029)	(0.0029)	(0.0028)	(0.0027)	(0.0028)	(0.0028)	(0.0027)	(0.0027)	(0.0026)
Strata fixed effects	X	X	X	X	X	X	X	X	X
Control for past outcome and PO		X	X		X	X		X	X
Additional controls			X			X			X
Observations	3,390	2,660	2,660	3,390	2,660	2,660	3,390	2,660	2,660

Notes: Panel A shows the effect of a precinct being assigned to the treatment group (ITT results from equation (1)). Panel B shows the effect of a precinct being allocated to canvassers (2SLS results from equation (2)). The unit of observation is the unit of randomization (precinct, or municipality). Robust standard errors are in parentheses. All regressions include strata fixed effects. Regressions in columns 2, 5, and 8 also control for PO (proxy for the potential to win votes) and for past outcomes, measured at the level of randomization. Additional controls in columns 3, 6, and 9 include the number of registered citizens in the precinct or municipality as well as the level and the five-year change of the following census variables: the municipality's population, the share of men, the share of different age groups (from 0 to 14; from 15 to 29; from 30 to 44; from 45 to 59; from 60 to 74; above 75), the share of working population, and the share of unemployed population among the working population. Regressions controlling for past outcomes need to exclude precincts whose boundaries had changed after 2007, which explains the lower number of observations.

results to restricting the sample to territories identified as following the list of allocated precincts based only on canvassers' reports (Table B3 in online Appendix B) or their answers to the postelectoral survey (Table B6). In the first subsample, the effect of the door-to-door visits was 0.29 and 0.35 percentage points in the first and second rounds, but only the latter estimate is significant (at the 10 percent level). In the second subsample, the effects were 0.76 and 0.50 percentage points, but only the former estimate is significant (at the 5 percent level).

Vote Shares of Other Candidates.—The correlate of the positive effect of door-to-door canvassing on the vote share obtained by François Hollande in the first round is a negative effect on the vote shares of other candidates. In Table 6, I assess the extent to which the different candidates were affected.

The combined effect of the door-to-door visits on the vote shares of the right-wing candidates Nicolas Sarkozy and Nicolas Dupont-Aignan was negative, slightly smaller than the effect on Hollande's vote share (−0.43 percentage points, compared to 0.44 percentage points), and significant at the 1 percent level (panel A, column 8). Scaled by the differential multiplier, this corresponds to an effect of −3.14 percentage points. Instead, the effect on the vote shares of the candidates of the far-left (Philippe Poutou and Nathalie Arthaud) was close to zero (column 2).

Table 5—Impact on Hollande's Vote Share

	First round			Second round			Average of first and second rounds		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. ITT estimation									
Treatment	0.0063	0.0050	0.0044	0.0048	0.0053	0.0046	0.0056	0.0049	0.0043
	(0.0023)	(0.0019)	(0.0018)	(0.0028)	(0.0019)	(0.0018)	(0.0024)	(0.0017)	(0.0016)
Strata fixed effects	X	X	X	X	X	X	X	X	X
Control for past outcome and PO		X	X		X	X		X	X
Additional controls			X			X			X
Observations	3,390	2,660	2,660	3,390	2,660	2,660	3,390	2,660	2,660
R^2	0.003	0.516	0.528	0.001	0.632	0.645	0.002	0.645	0.655
Mean in control group	0.3157	0.2994	0.2994	0.5757	0.5597	0.5597	0.4457	0.4295	0.4295
Panel B. Instrumental variable estimation: "allocated to canvassers" instrumented with "treatment"									
Allocated to canvassers	0.0112	0.0094	0.0084	0.0084	0.0099	0.0087	0.0098	0.0092	0.0081
	(0.0041)	(00036)	(0.0035)	(0.0050)	(0.0036)	(0.0035)	(0.0042)	(0.0031)	(0.0030)
Strata fixed effects	X	X	X	X	X	X	X	X	X
Control for past outcome and PO		X	X		X	X		X	X
Additional controls			X			X			X
Observations	3,390	2,660	2,660	3,390	2,660	2,660	3,390	2,660	2,660

Notes: Panel A shows the effect of a precinct being assigned to the treatment group (ITT results from equation (1)). Panel B shows the effect of a precinct being allocated to canvassers (2SLS results from equation (2)). The unit of observation is the unit of randomization (precinct, or municipality). Robust standard errors are in parentheses. All regressions include strata fixed effects. Regressions in columns 2, 5, and 8 also control for PO (proxy for the potential to win votes) and for past outcomes, measured at the level of randomization. Additional controls in columns 3, 6, and 9 include the number of registered citizens in the precinct or municipality as well as the level and the five-year change of the following census variables: the municipality's population, the share of men, the share of different age groups (from 0 to 14; from 15 to 29; from 30 to 44; from 45 to 59; from 60 to 74; above 75), the share of working population, and the share of unemployed population among the working population. Regressions controlling for past outcomes need to exclude precincts whose boundaries had changed after 2007, which explains the lower number of observations.

The effect on vote shares of the centrist candidate, François Bayrou, and of other left-wing candidates (Eva Joly and Jean-Luc Mélenchon) was negative but not statistically significant (columns 4 and 6). The effect on the vote share of the far-right candidate, Marine Le Pen, was also small and non-significant, although positive (column 10).

D. *Effects on the 2012 Parliamentary Elections and the 2014 European Elections*

Voter Turnout.—I now investigate whether the effects of the visits were short-lived or whether they persisted in the first and second rounds of the 2012 parliamentary elections, which took place one month after the presidential, and in the 2014 European elections, which took place two years later. Tables 7 and 8 examine the effects on voter turnout and on the vote shares of PS candidates, respectively.

As in the presidential election, I find a significant effect on voter turnout neither in the parliamentary (Table 7, columns 3 and 4) nor the European elections (column 5).

Vote Shares of Candidates of the Parti Socialiste.—I now examine the impact of the visits on vote shares of PS candidates. Columns 1 and 2 of Table 8 are identical to columns 3 and 6 of Table 5. They show the impact of the door-to-door visits on François

Table 6—Impact on All Parties' Vote Shares

	Far-left		Left other than Hollande		Center		Right		Far-right	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. ITT estimation										
Treatment	0.0000	0.0003	−0.0022	−0.0011	−0.0008	−0.0007	−0.0037	−0.0043	0.0006	0.0016
	(0.0004)	(0.0005)	(0.0017)	(0.0017)	(0.0010)	(0.0010)	(0.0021)	(0.0016)	(0.0018)	(0.0016)
Strata fixed effects	X	X	X	X	X	X	X	X	X	X
Control for past outcome and PO		X		X		X		X		X
Additional controls		X		X		X		X		X
Observations	3,390	2,660	3,390	2,660	3,390	2,660	3,390	2,660	3,390	2,660
R^2	0.000	0.056	0.001	0.271	0.000	0.231	0.001	0.526	0.000	0.434
Mean in control group	0.0192	0.0200	0.1539	0.1514	0.0837	0.0865	0.2455	0.2528	0.1792	0.1873
Panel B. Instrumental variable estimation: "allocated to canvassers" instrumented with "treatment"										
Allocated to canvassers	0.0000	0.0007	−0.0040	−0.0022	−0.0015	−0.0014	−0.0066	−0.0082	0.0011	0.0031
	(0.0007)	(0.0009)	(0.0030)	(0.0033)	(0.0018)	(0.0020)	(0.0037)	(0.0030)	(0.0031)	(0.0030)
Strata fixed effects	X	X	X	X	X	X	X	X	X	X
Control for past outcome and PO		X		X		X		X		X
Additional controls		X		X		X		X		X
Observations	3,390	2,660	3,390	2,660	3,390	2,660	3,390	2,660	3,390	2,660

Notes: Panel A shows the effect of a precinct being assigned to the treatment group (ITT results from equation (1)). Panel B shows the effect of a precinct being allocated to canvassers (2SLS results from equation (2)). The unit of observation is the unit of randomization (precinct, or municipality). Robust standard errors are in parentheses. The far-left candidates (columns 1 and 2) were Nathalie Arthaud (endorsed by Lutte Ouvrière) and Philippe Poutou (Ligue Communiste Révolutionnaire). The candidates on the left other than François Hollande (columns 3 and 4) were Eva Joly (Europe Ecologie Les Verts) and Jean-Luc Mélenchon (Front de Gauche). The candidate on the center (columns 5 and 6) was François Bayrou (Modem). The candidates on the right (columns 7 and 8) were Nicolas Sarkozy (Union pour un Mouvement Populaire) and Nicolas Dupont-Aignan (Debout la République). The candidate on the far right (columns 9 and 10) was Marine Le Pen (Front National). All regressions include strata fixed effects. Regressions in even-numbered columns control for PO (proxy for the potential to win votes), past outcomes, measured at the level of randomization, and additional controls. Additional controls include the number of registered citizens in the precinct or municipality as well as the level and the five-year change of the following census variables: the municipality's population, the share of men, the share of different age groups (from 0 to 14; from 15 to 29; from 30 to 44; from 45 to 59; from 60 to 74; above 75), the share of working population, and the share of unemployed population among the working population. Regressions controlling for past outcomes need to exclude precincts whose boundaries had changed after 2007, which explains the lower number of observations.

Hollande's vote shares at the 2012 presidential election and are included for reference only. This effect translated into effects of 0.94 and 0.73 percentage points, significant at the 1 percent level, on the vote share of PS candidates in the first and second rounds of the 2012 parliamentary elections (panel A, columns 3 and 4). Remarkably, part of the effect persisted in the 2014 European elections, although the point estimate of 0.37 percentage points is only significant at the 10 percent level (column 5).

While columns 1 through 5 use expressed votes as the denominator to compute vote shares, columns 6 through 10 use registered voters as the denominator. Differently from participation and expressed votes, the number of registered voters

Table 7—Impact on Voter Turnout at the Following Elections

	2012 presidential election		2012 parliamentary elections		2014 European elections
	First round (1)	Second round (2)	First round (3)	Second round (4)	(5)
Treatment	0.0011	−0.0008	−0.0024	−0.0025	0.0014
	(0.0015)	(0.0014)	(0.0022)	(0.0024)	(0.0027)
Strata fixed effects	X	X	X	X	X
Control for past outcome and PO	X	X	X	X	X
Additional controls	X	X	X	X	X
Constituency fixed effects			X	X	
Observations	2,660	2,660	2,660	2,443	2,544
R^2	0.410	0.326	0.347	0.307	0.226
Mean in control group	0.8081	0.8122	0.5884	0.5680	0.4457
Panel B. Instrumental variable estimation: "allocated to canvassers" instrumented with "treatment"					
Allocated to canvassers	0.0021	−0.0015	−0.0046	−0.0049	0.0026
	(0.0028)	(0.0028)	(0.0042)	(0.0046)	(0.0051)
Strata fixed effects	X	X	X	X	X
Control for past outcome and PO	X	X	X	X	X
Additional controls	X	X	X	X	X
Constituency fixed effects			X	X	
Observations	2,660	2,660	2,660	2,443	2,544

Notes: Panel A shows the effect of a precinct being assigned to the treatment group (ITT results from equation (1)). Panel B shows the effect of a precinct being allocated to canvassers (2SLS results from equation (2)). The unit of observation is the unit of randomization (precinct, or municipality). Robust standard errors are in parentheses. All regressions include strata fixed effects, control for PO (proxy for the potential to win votes), past outcomes, measured at the level of randomization, and additional controls. Additional controls include the number of registered citizens in the precinct or municipality as well as the level and the five-year change of the following census variables: the municipality's population, the share of men, the share of different age groups (from 0 to 14; from 15 to 29; from 30 to 44; from 45 to 59; from 60 to 74; above 75), the share of working population, and the share of unemployed population among the working population. Regressions in columns 3 and 4 also control for constituency fixed effects to account for differences in the number and identity of competing candidates across constituencies, at the 2012 parliamentary elections.

is stable across elections. Thus, although a less common and intuitive outcome, vote shares defined as a fraction of registered voters—instead of expressed votes—facilitates the comparison of the effect size across elections. Most of the effect of the visits on the vote share of Hollande in the presidential election persisted in the parliamentary elections one month later. The effect at the first round of these elections was even slightly larger (0.42 percentage points against 0.35 percentage points for the first round of the presidential election), but it was smaller and non-significant at the second round. Persistence two years later at the European elections was smaller (0.17 percentage points, or about 47 percent of the original effect), and using this definition of vote shares, the effect is no longer statistically significant.

E. *Placebo Checks on the 2007 Presidential Elections*

I conduct a placebo exercise using results from the 2007 presidential elections. I run the three exact same specifications as for the main results. For sociodemographic controls, I use 2006 data (instead of the 2011 controls used in the main regressions). For past outcomes, I control for the results of the 2002 presidential elections. All

Table 8—Impact on Vote Shares of PS Candidates at the Following Elections

	PS vote share as fraction of expressed votes					PS vote share as fraction of registered voters				
	2012 presidential		2012 parliamentary			2012 presidential		2012 parliamentary		
	First round (1)	Second round (2)	First round (3)	Second round (4)	2014 European (5)	First round (6)	Second round (7)	First round (8)	Second round (9)	2014 European (10)
Panel A. ITT estimation										
Treatment	0.0044	0.0046	0.0094	0.0073	0.0037	0.0035	0.0032	0.0042	0.0031	0.0017
	(0.0018)	(0.0018)	(0.0026)	(0.0024)	(0.0021)	(0.0015)	(0.0016)	(0.0018)	(0.0019)	(0.0012)
Strata fixed effects	X	X	X	X	X	X	X	X	X	X
Control for past outcome and PO	X	X	X	X	X	X	X	X	X	X
Additional controls	X	X	X	X	X	X	X	X	X	X
Constituency fixed effects			X	X				X	X	
Observations	2,660	2,660	2,660	2,443	2,544	2,660	2,660	2,660	2,443	2,544
R^2	0.528	0.645	0.692	0.827	0.302	0.480	0.602	0.624	0.726	0.239
Mean in control group	0.2994	0.5597	0.3246	0.4545	0.1425	0.2355	0.4241	0.1876	0.2481	0.0605
Panel B. Instrumental variable estimation: "allocated to canvassers" instrumented with "treatment"										
Allocated to canvassers	0.0084	0.0087	0.0181	0.0142	0.0070	0.0067	0.0061	0.0080	0.0060	0.0032
	(0.0035)	(0.0035)	(0.0050)	(0.0048)	(0.0039)	(0.0029)	(0.0030)	(0.0034)	(0.0037)	(0.0022)
Strata fixed effects	X	X	X	X	X	X	X	X	X	X
Control for past outcome and PO	X	X	X	X	X	X	X	X	X	X
Additional controls	X	X	X	X	X	X	X	X	X	X
Constituency fixed effects			X	X				X	X	
Observations	2,660	2,660	2,660	2,443	2,544	2,660	2,660	2,660	2,443	2,544

Notes: Panel A shows the effect of a precinct being assigned to the treatment group (ITT results from equation (1)). Panel B shows the effect of a precinct being allocated to canvassers (2SLS results from equation (2)). The unit of observation is the unit of randomization (precinct, or municipality). Robust standard errors are in parentheses. All regressions include strata fixed effects, control for PO (proxy for the potential to win votes), past outcomes, measured at the level of randomization, and additional controls. Additional controls include the number of registered citizens in the precinct or municipality as well as the level and the five-year change of the following census variables: the municipality's population, the share of men, the share of different age groups (from 0 to 14; from 15 to 29; from 30 to 44; from 45 to 59; from 60 to 74; above 75), the share of working population, and the share of unemployed population among the working population. Regressions in columns 3, 4, 8, and 9 also control for constituency fixed effects to account for differences in the number and identity of competing candidates across constituencies, at the 2012 parliamentary elections.

regressions exclude precincts whose boundaries were changed between 2007 and 2011. Regressions controlling for the 2002 outcomes also exclude precincts whose boundaries were changed between 2002 and 2007. Table 9 shows the impact on turnout and Table 10 the impact on Ségolène Royal's vote share (the candidate of the Parti Socialiste in the 2007 presidential elections).

In the second round of the 2007 elections, turnout and Royal's vote share were very close in treatment and control precincts. The difference is close to 0 and not statistically significant across all three specifications shown in columns 4 through 6 of Tables 9 and 10. The stratification of the randomization on the potential to win votes, itself estimated based on the 2007 second round results, ensured symmetry of the treatment and control groups on these outcomes.

In the first round of the 2007 elections, instead, turnout was lower and Royal's vote share higher in treatment precincts. These differences are significant at the 10 percent level in the specification controlling only for strata fixed effects (column 1).

TABLE 9—PLACEBO-IMPACT ON VOTER TURNOUT IN 2007

	First round			Second round			Average of first and second rounds		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. ITT estimation									
Treatment	−0.0026	−0.0026	−0.0023	−0.0005	0.0001	0.0003	−0.0015	−0.0011	−0.0009
	(0.0014)	(0.0014)	(0.0014)	(0.0012)	(0.0013)	(0.0012)	(0.0012)	(0.0012)	(0.0012)
Strata fixed effects	X	X	X	X	X	X	X	X	X
Control for past outcome		X	X		X	X		X	X
Additional controls			X			X			X
Observations	2,660	2,133	2,133	2,660	2,133	2,133	2,660	2,133	2,133
R^2	0.002	0.267	0.301	0.000	0.196	0.243	0.001	0.303	0.341
Mean in control group	0.8428	0.8494	0.8494	0.8373	0.8432	0.8432	0.8400	0.8463	0.8463
Panel B. Instrumental variable estimation: "allocated to canvassers" instrumented with "treatment"									
Allocated to canvassers	−0.0048	−0.0049	−0.0044	−0.0009	0.0001	0.0005	−0.0029	−0.0021	−0.0017
	(0.0026)	(0.0028)	(0.0027)	(0.0023)	(0.0024)	(0.0024)	(0.0023)	(0.0023)	(0.0023)
Strata fixed effects	X	X	X	X	X	X	X	X	X
Control for past outcome		X	X		X	X		X	X
Additional controls			X			X			X
Observations	2,660	2,133	2,133	2,660	2,133	2,133	2,660	2,133	2,133

Notes: Panel A shows the effect of a precinct being assigned to the treatment group (ITT results from equation (1)). Panel B shows the effect of a precinct being allocated to canvassers (2SLS results from equation (2)). The unit of observation is the unit of randomization (precinct, or municipality). Robust standard errors are in parentheses. All regressions include strata fixed effects. Regressions in columns 2, 5, and 8 also control for past outcomes, measured at the level of randomization. Additional controls in columns 3, 6, and 9 include the number of registered citizens in the precinct or municipality, the municipality's population, the share of men, the share of different age groups (from 0 to 14; from 15 to 29; from 30 to 44; from 45 to 59; from 60 to 74; above 75), the share of working population, and the share of unemployed population among the working population. Regressions controlling for past outcomes need to exclude precincts whose boundaries had changed after 2002, which explains the lower number of observations.

They are no longer statistically significant when controlling for past outcome and additional controls (column 3). In particular, controlling for past outcome, the difference in Royal's vote share in the first round is close to zero (Table 10, columns 2 and 3), showing that first round voting behavior was not on differential trends in the treatment and control precincts. Thus, the 2012 and 2014 first-round results shown in Tables 4 through 8, which control for the 2007 outcomes, should not be driven by underlying differences.

Averaging over both rounds, neither turnout nor Royal's vote share are significant in any of the specifications (columns 7 through 9). Controlling for past outcomes and additional controls, the difference between treatment and control precincts is very close to zero.

IV. Interpretation of the Results

A. *Effect on the Overall Election Outcome*

Point estimates of the effects of the door-to-door visits on the vote shares of François Hollande and Nicolas Sarkozy at the first round of the presidential elections are 3.24 and −3.14 percentage points respectively in precincts that were covered by canvassers and would not have been covered had they not been assigned

Table 10—Placebo-Impact on Royal's vote share in 2007

	First round			Second round			Average of first and second rounds		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. ITT estimation									
Treatment	0.0050	0.0015	0.0007	0.0009	−0.0013	−0.0014	0.0029	−0.0002	−0.0006
	(0.0027)	(0.0024)	(0.0023)	(0.0029)	(0.0024)	(0.0023)	(0.0026)	(0.0020)	(0.0020)
Strata fixed effects	X	X	X	X	X	X	X	X	X
Control for past outcome		X	X		X	X		X	X
Additional controls			X			X			X
Observations	2,660	2,133	2,133	2,660	2,133	2,133	2,660	2,133	2,133
R^2	0.002	0.371	0.395	0.000	0.495	0.509	0.001	0.525	0.541
Mean in control group	0.2740	0.2620	0.2620	0.5146	0.5056	0.5056	0.3943	0.3838	0.3838
Panel B. Instrumental variable estimation: "allocated to canvassers" instrumented with "treatment"									
Allocated to canvassers	0.0093	0.0028	0.0015	0.0016	−0.0025	−0.0028	0.0055	−0.0004	−0.0011
	(0.0049)	(0.0045)	(0.0045)	(0.0054)	(0.0046)	(0.0046)	(0.0048)	(0.0039)	(0.0039)
Strata fixed effects	X	X	X	X	X	X	X	X	X
Control for past outcome		X	X		X	X		X	X
Additional controls			X			X			X
Observations	2,660	2,133	2,133	2,660	2,133	2,133	2,660	2,133	2,133

Notes: Panel A shows the effect of a precinct being assigned to the treatment group (ITT results from equation (1)). Panel B shows the effect of a precinct being allocated to canvassers (2SLS results from equation (2)). The unit of observation is the unit of randomization (precinct, or municipality). Robust standard errors are in parentheses. All regressions include strata fixed effects. Regressions in columns 2, 5, and 8 also control for past outcomes, measured at the level of randomization. Past PS vote share at the second round of the 2002 presidential election is proxied by the sum of first round vote shares of all left-wing candidates since the PS candidate (Lionel Jospin) failed to qualify for the second round (he unexpectedly arrived third, behind the Right and Far-right candidates). Additional controls in columns 3, 6, and 9 include the number of registered citizens in the precinct or municipality, the municipality's population, the share of men, the share of different age groups (from 0 to 14; from 15 to 29; from 30 to 44; from 45 to 59; from 60 to 74; above 75), the share of working population, and the share of unemployed population among the working population. Regressions controlling for past outcomes need to exclude precincts whose boundaries had changed after 2002, which explains the lower number of observations.

to the treatment group.[22] Assuming that the impact was of the same magnitude in all precincts covered,[23] and since the canvassers covered approximately 11 percent of all French households before the first round and 15 percent before the second round,

[22] As discussed in Section IIIC, these effects are computed by applying the first and second rounds differential intensity multipliers to ITT estimates.

[23] This assumption may at first seem implausible since the 2SLS estimates are identified out of precincts in which the potential to win votes, proxied by $\widetilde{PO}$, was deemed highest. However, $\widetilde{PO}$ was defined as the historical fraction of nonvoters multiplied by the left vote share among active voters, reflecting the initial belief that votes could most easily be won by mobilizing left-wing nonvoters. Instead, the fact that the visits did not increase turnout suggests that $\widetilde{PO}$ was a noisy proxy for the true potential to win votes. To bring more direct empirical support for the assumption above, Tables G1 through G4 in online Appendix G examine the extent to which treatment effects vary with $\widetilde{PO}$ in precincts allocated to canvassers. In Table G2, the effect on Hollande's vote share of a precinct being assigned to the treatment group is substantially larger (even though not significantly so) in high $\widetilde{PO}$ than in low $\widetilde{PO}$ precincts (panel A). This difference mechanically results from the fact that a larger fraction of high $\widetilde{PO}$ precincts were allocated to canvassers: instead, the difference between the effect of a high versus low $\widetilde{PO}$ precinct being allocated to canvassers is relatively small and its sign changes depending on the outcome and the specification (panel B). Similarly, in Table G1, panel B, the difference between the effect of a high versus low $\widetilde{PO}$ precinct being allocated to canvassers is not statistically significant, and its sign varies. A similar picture emerges when allowing for heterogeneous treatment effects along $\widetilde{PO}$ introduced as a continuous variable. The effect on voter turnout (Table G3, panel B) and on Hollande's vote share (Table G4, panel B) of a precinct being allocated to canvassers interacted with $\widetilde{PO}$ is not statistically significant in any specification and its sign varies across electoral rounds and specifications. The lack of systematic relationship between a precinct's $\widetilde{PO}$ and the effect on voter turnout or Hollande's vote share of this precinct being allocated to canvassers suggests that the choice to allocate treatment precincts with a relatively higher $\widetilde{PO}$ to canvassers had limited consequences for the external validity of the results.

I obtain that the door-to-door canvassing campaign increased François Hollande's national vote share by 0.37 percentage points in the first round of the presidential elections and that it decreased Nicolas Sarkozy's vote share by 0.36 percentage points. Overall, it thus accounted for about one-half of Hollande's 1.45 percentage point lead in the first round.

The effect on Hollande's vote share in the second round was 2.75 percentage points, implying an increase of his national vote share by 0.41 percentage points. Since there were only two candidates in the second round, it was automatically mirrored by a negative effect of the same size on the vote share of Sarkozy. In total, the visits increased Hollande's victory margin by 0.83 percentage points. Since Hollande won with 51.6 percent of the votes, against 48.4 for Sarkozy, the effect of door-to-door canvassing accounted for about one-fourth of the victory margin.

Finally, taking into account the imperfect compliance and the fraction of addresses covered, I estimate that door-to-door canvassing increased PS candidates' vote shares by 0.66 percentage points, on average, in the second round of the parliamentary elections. This is by no means negligible; PS candidates won by an even lower margin in 5.9 percent of the constituencies (15 out of 254) in which they won in the second round.

B. *Persuasion versus Mobilization*

Two mechanisms could explain the impact on the vote share of François Hollande: the persuasion of undecided active voters (who would have voted for another candidate absent the visits) and the mobilization of left-wing nonvoters (who would have stayed home). To assess the importance of the second mechanism, I first use a seemingly unrelated regressions (SUR) framework, compare the impact on turnout and on vote shares, and test the hypothesis that they are equal. I use the number of registered citizens as the denominator for both outcomes, to ensure their comparability. The results are shown in Table H1 in online Appendix H. The effect on voter turnout was negative in the second round but positive in the first round, where it corresponds to 30.5 percent of the effect on vote share (column 3). Imprecision in the point estimates implies that the real contribution of the mobilization channel may of course have been larger. However, it is unlikely to explain all the vote share increase: I reject (at the 5 or 10 percent level) the null hypothesis that the effects on turnout and vote shares were equal, in all but one specification. In addition, to test whether the effect on Hollande's vote share remains after eliminating the possible contribution of the mobilization channel, I estimate equations (1) and (2) using the difference between Hollande's vote share and voter turnout as the outcome. The results are shown in Table I1 in online Appendix I. The effect on Hollande's vote share net of the effect on turnout remains statistically significant in all but two specifications. Even though I cannot reject that the mobilization of left-wing nonvoters played a role, these results suggest that the increase in Hollande's vote share was, rather, driven by persuasion.[24]

[24] This interpretation may seem at odds with the fact that the campaign gave priority to the mobilization of left-wing supporters. However, any precinct allocated to canvassers represents several hundreds of registered citizens. As a consequence, in each precinct, these citizens display a wide array of profiles. In particular, even in precincts

An alternative interpretation is possible. The door-to-door visits may have demobilized right-wing voters at the same time as they increased participation on the left, translating into small net effects on turnout but large effects on vote shares. By improving the short-term opinions of François Hollande, visits from canvassers may have contradicted the partisan predispositions of supporters of other candidates and generated psychological tension (Fiorina 1976). One response to cognitive dissonance is to avoid situations likely to increase it (Festinger 1957, 1962)—which, in this context, would be to forego voting in the election. It is difficult to disentangle these two interpretations using aggregate data, but for a few reasons demobilization of other candidates' supporters, while not entirely implausible, seems less likely than persuasion. First, existing experiments find that partisan field campaigns increase turnout among supporters of other parties or leave it unaffected, not that they decrease it (Nickerson 2005; Arceneaux and Kolodny 2009; Foos and de Rooij 2017). Second, while negative political ads can decrease voter turnout in certain contexts (Ansolabehere et al. 1994; Krupnikov 2011; but see Wattenberg and Brians 1999; Goldstein and Freedman 2002), the campaign relied on positive rather than negative arguments, as can be seen in the toolkit and field organizers' guide included in online Appendix J (Figures J1 and J2), consistent with the emphasis put on the mobilization of left-wing supporters. Third, of all types of voters, those that could have been deemed most likely to feel cross-pressured after the visit of François Hollande's canvassers are probably the supporters of Marine Le Pen. Indeed, many voters of the Front National are former voters of the left, and many maintain leftist preferences on economic issues (Perrineau 2005; Mayer 2011).[25] The visits could have awakened this past loyalty and created a tension with the voters' new allegiance to the far-right. But as shown in Section IIIC, the visits did not decrease the vote share of the far-right candidate.

If indeed the effects were obtained by persuading swing voters to vote left, what fraction were persuaded? Since 48 percent of the doors knocked by canvassers opened, I scale the point estimates by $1/0.48$ and find that 6.7 percent and 5.7 percent of the voters living in households that opened their door were persuaded to vote for François Hollande in the first and second rounds of the presidential elections.[26]

with a large number of left-wing nonvoters, a majority of voters participate in the presidential elections, and many of them vote for right-wing candidates. In sum, although the main target of the campaign were left-wing nonvoters, only a minority of the people with whom the canvassers interacted corresponded to this type.

[25] In the one-dimensional representation of the political spectrum, the localization of the FN on the far right makes it the party most distant from the PS. But in France as in most Western European countries and the United States, the left-right split has not one, but at least two dimensions, sociocultural and economic, which overlap only imperfectly (e.g., Lipset 1959; Fleishman 1988; Knutsen 1995). On the sociocultural dimension, the platforms of the FN and the PS are diametrically opposed: vehement anti-immigrant positions and a model of authoritarian and closed society on one side; a pro-immigration stance and a model of open and libertarian society on the other (e.g., Pettigrew 1998; Arzheimer 2009; Mayer 2013). On the economic dimension, however, the distance between the FN and the PS, which traditionally promotes state interventionism against economic liberalism, is much smaller. It has further decreased since Marine Le Pen succeeded her father as the leader of the FN in 2011. Her program for the 2012 election asked for a more protective state and more public services—two points that closely echoed the program of the PS. Together with anti-elite stances directed against the corrupt political establishment and the privileged few, this economic platform was designed to attract blue-collar workers, mid-level employees, and other groups exposed to unemployment and precariousness, which until recently largely supported the left.

[26] This scaling assumes, first, that on average households that opened their doors contained as many registered citizens as those that did not, and it considers as "treated" all citizens living in a household that opened its door, regardless of whether or not they interacted personally with the canvasser. Second, I assume that the precinct-level effects of the visits were driven by voters (and household members) who received them, and not by spillovers on voters who did not interact with canvassers but were persuaded by talking with voters who did.

Applying the definition of persuasion rate proposed by DellaVigna and Kaplan (2007), I compare these fractions to the fractions of voters who would have supported candidates other than François Hollande in the first and second rounds absent the visits (respectively 70.9 percent and 45.0 percent).[27] I compute that the fraction of voters who changed their behavior in response to the visits were 9.5 percent and 12.7 percent respectively. These persuasion rates are of the same order of magnitude as those measured by studies that examine the impact of door-to-door canvassing on the decision to vote or not (see DellaVigna and Gentzkow 2010). For instance, using turnout as their outcome, Gerber and Green (2000) and Green, Gerber, and Nickerson (2003) find persuasion rates of door-to-door canvassing of 15.6 percent and 11.5 percent respectively. The persuasion rates obtained in the present experiment also compare with those associated with new exposure to media: 4.4, 11.6, and 7.7 percent for TV (Gentzkow 2006; DellaVigna and Kaplan 2007; Enikolopov et al. 2011); 19.5 and 12.9 percent for newspaper (Gerber, Karlan, and Bergan 2009; Gentzkow, Shapiro, and Sinkinson 2011). They are substantially higher than the persuasion rates of all of a candidate's combined TV advertising (an average 0.7 percent in Spenkuch and Toniatti, forthcoming) or political endorsement by a newspaper (an average 4.3 percent in Chiang and Knight 2011).

C. *Beliefs versus Preferences*

Persuasion can affect behavior through different mechanisms (DellaVigna and Gentzkow 2010). Canvassers may have persuaded voters by changing their preferences on some political issues or by changing their beliefs about the quality of Hollande. While both mechanisms are possible, the short average length of the visits makes the first mechanism perhaps less likely. The fact that most voters that were canvassed had never been visited by a political activist before (Lefebvre 2016) makes the second mechanism more plausible: these novel and surprising visits sent a strong signal about the quality of the PS and its candidate. According to this interpretation, the voters were persuaded by the signal sent by the canvassers' presence more than by their specific arguments. Door-to-door canvassing contrasted with the idea that the political world is solely populated by politicians who do not care about what voters think.[28] It showed that Hollande and his supporters were willing to bridge the gap with voters and it put forth the image of the PS as a modern and innovative party.

This interpretation is in line with theories of costly signaling such as laid out by Coate and Conlin (2004), where voters do not know whether candidates are qualified and candidates use campaign resources to convey information about their qualifications. Although I cannot directly test this interpretation, the effects of the visits on the vote shares of other candidates of different political affiliations provide some (granted, limited) empirical support. As shown in Section IIIC and Table 6, the visits decreased the vote shares of right-wing candidates by 0.43 percentage

[27] These fractions are based on the fractions of control group voters who supported candidates other than François Hollande (70.1 percent and 44.0 percent in the first and second rounds), adjusted for the effect of the visits on Hollande's and other candidates' vote shares in control precincts which were mistakenly covered by canvassers.

[28] According to a survey conducted after the 2012 presidential elections, 71 percent of French people feel that politicians care little or not at all about what they think and 66 percent do not trust political parties (Cevipof 2012).

points, which is almost as large as the effect on Hollande's vote share (0.44 percentage points) and much larger than the effect on the vote shares of other left-wing candidates (−0.11 percentage points). The effect, compared to vote shares in the control group, is more than twice as large for right-wing candidates than other left-wing candidates. Again using the SUR framework, I cannot reject that the effects were the same (p-value of 0.21, as shown in Table H2, column 4). It remains that, taken at face value, the estimates suggest the increase of Hollande's vote share was obtained by taking votes from right-wing candidates more than from other left-wing candidates. But right-wing voters were ideologically more distant from Hollande. They could thus be deemed less susceptible to align their preferences with his political agenda than voters supporting other left-wing candidates, who offered a closer ideological platform. This again makes it less likely that voters' political preferences changed, and more likely that their beliefs about the PS and its candidate did.

D. *Mechanisms Underlying Effect Persistence*

I finally discuss the persistence of the effect of the visits on vote shares obtained by left-wing candidates. Nearly all the original effect carried over to the 2012 parliamentary elections which took place one month later. This suggests that most voters persuaded by the visits were active voters, who participated not only in the presidential election but also in these lower salience elections, and that they were consistent in who they voted for. Persistence results are aggregate and thus subject to attenuation as time goes by, due to citizens moving and dying. Nonetheless, around 40 percent of the original effect carried over to the 2014 European elections. Although at the margin of statistical significance, this finding is perhaps all the more striking as the PS suffered an important defeat in the latter elections.

The persistence of the effect in the parliamentary and European elections can come from two main channels, direct and indirect. First, the direct effect of the visits may have been long-lived; it is possible that the canvassers durably changed voters' beliefs about the quality of the PS (or changed voters' preferences). Second, voting for a PS candidate today may in itself increase the likelihood to vote for a PS candidate in the future. Multiple mechanisms may explain this habit formation, including cognitive dissonance (Festinger 1957, 1962), or increased expressive utility of voting for this particular party.[29] Existing evidence that documents persistence of electoral behavior has mostly focused on voter turnout (e.g., Gerber. Green, and

[29] Two additional mechanisms may have contributed to the large effects at the parliamentary elections, even though they are unlikely to account for the bulk of them. The first is that the impact of the campaign may have interacted with Hollande's victory: while some voters were directly persuaded by the visits, others may have only been persuaded to vote on the left after they witnessed Hollande's victory, for instance because after the canvassers' visit they remained reluctant to vote left out of disbelief that the left had any chance to win the elections. The second is that some canvassers engaged in door-to-door canvassing between the presidential and parliamentary elections and that they disproportionately covered treatment precincts. While the list of precincts and addresses allocated to canvassers remained available only until the presidential election, some activists who had canvassed these areas before the presidential election may have returned there and canvassed them again before the parliamentary elections. Note however that the opposite may have happened too (canvassers going to areas which they had not been allocated during the presidential campaign, in an effort to cover their entire territory) and that the campaign for the parliamentary elections was of a much lower intensity than the presidential campaign. Additional canvassing is even less likely to explain the (lower) persistence at the European elections, where the intensity of the field campaign was much lower still.

Shachar 2003; Meredith 2009; Cutts et al. 2009; Davenport et al. 2010; Fujiwara, Meng, and Vogl 2016; Coppock and Green 2016). While estimates of the magnitude of persistence differ, Fujiwara, Meng, and Vogl (2016) find that habit formation alone can generate near-to-full persistence of the impact of rainfall shocks on participation four years later. Beyond voter turnout, Mullainathan and Washington (2009) and Kaplan and Mukand (2014) find, respectively, long-lasting effects of participation in US presidential elections on presidential opinion ratings, and persistence of the effect of the September 11th attacks on party of registration. Our study complements this literature by showing that transitory shocks to vote choice can generate persistent effects as well.

This result contrasts with Gerber et al. (2011) who find rapid decay of the effects of TV and radio ads on voting preferences, with two possible interpretations. The first is that the personal and interactive aspects of the door-to-door visits generate direct effects of a different nature than TV ads; while the latter only prime evaluative criteria, as hypothesized by the authors, the former actually change voters' views, with consequences lasting after the contact was forgotten. The second interpretation is that direct effects of both types of campaigning on voter preferences are short lived, and that persistence mostly comes from the indirect vote choice channel. While the campaign studied in this paper continued until the day before the election and did affect vote choice in that election, the advertisement campaign evaluated by Gerber et al. (2011) stopped nine months before and likely failed to affect decisions, preventing persistence through vote choice.

V. Conclusion

This paper reports the results of a countrywide field experiment conducted during François Hollande's door-to-door campaign in the 2012 French presidential election. The campaign spanned all French regions, encompassed very different types of areas, from Paris to rural villages, and reached an estimated five million households. The study contributes to a large literature on the drivers and effects of persuasive communication, and extends it in three important directions.

First, while targeted appeals transmitted in one-on-one discussions have been repeatedly found effective in increasing voter participation, the existing evidence comes from framed field experiments which can carefully select the agents carrying out these interventions, and control the content of their conversations. Results obtained in these settings may not fully extend to large-scale campaigns like the one studied here, which typically lack such control, even when they are managed very professionally (Enos and Hersh 2015). Second, and more important, the large scale of the experiment enabled, for the first time, randomization to be conducted at the precinct level while maintaining high statistical power. Unlike in prior studies randomized at the individual or household level, I can thus measure the impact of the door-to-door visits both on voter turnout and on actual vote shares, using official precinct-level election results. This provides the first hard evidence that door-to-door campaigns actually affect electoral outcomes, and constitutes perhaps the main contribution of this paper. Third, I discuss important challenges inherent to embedding an experiment in a large campaign and ways to address them effectively. For the implementing organization, the cost of giving up on covering areas deemed strategic

but allocated to the control group may be particularly dissuasive when stakes are as high as during a presidential electoral campaign. The randomization rule was thus designed to ensure that precincts allocated to canvassers had the highest possible expected potential to win votes compatible with running an experiment. In addition, resources to ensure that all local units of the Parti Socialiste and the estimated 80,000 activists who took part in the campaign downloaded the lists of allocated precincts and followed these lists were scarce, creating a threat for the implementation of the randomization plan. I combine two independent data sources (reports entered by local activists on the campaign website and answers to a postelectoral survey) to identify which territories used the list of allocated precincts. Estimates of the impact of the campaign are comparable in the sets of territories identified based on either of these datasets.

In the combined sample of 791 territories that participated in the experiment, accounting for 5.02 million registered voters, I find that door-to-door canvassing did not significantly affect voter turnout but increased François Hollande's vote share by 3.24 percentage points in the first round of the election and 2.75 percentage points in the second in precincts that were covered by canvassers and would not have been covered if they had not been assigned to the treatment group. Assuming that the effect was of similar magnitude in all areas covered by the campaign, this accounted for approximately one-half of Hollande's lead in the first round and one-fourth of his victory margin at the second round. At the same time, the intervention decreased the vote share obtained by the right-wing candidates, with no significant effects on the vote shares of other candidates in the center, on the left, or on the far-right. Although several interpretations for this are possible, the most plausible is that the effects were obtained by persuading swing voters to vote left, rather than by mobilizing left-wing nonvoters or demobilizing opponents. The effect of the doorstep discussions persisted in the 2012 parliamentary elections, which took place one month later and even to the 2014 European elections, though the effect is far weaker.

These results are surprising, given that the campaign material and instructions, though mentioning the right-wing incumbent Nicolas Sarkozy, focused on the mobilization of left-wing nonvoters. The small and non-significant effects of the visits on voter turnout also stand in contrast to much of the existing get-out-the-vote literature, which reports large effects of door-to-door canvassing across a large variety of settings (Green and Gerber 2015). Three characteristics of this experiment may contribute to explaining this difference. First, its location: findings by Bhatti et al. (forthcoming) and Pons and Liegey (forthcoming) suggest that door-to-door canvassing has substantially smaller turnout effects in France and other European countries than in the United States. Second, the fact that Hollande's campaign was partisan. Partisan mailing, phoning, and canvassing campaigns have been found to generate weaker and more varying turnout effects, including in the United States (Green, McGrath, and Aronow 2013; Gerber and Green 2016). Third, the very high salience that characterizes French presidential elections. A review of US experimental results conducted by Arceneaux and Nickerson (2009) finds that the effectiveness of door-to-door outreach is conditioned by voters' baseline propensity to vote. In the context of high-turnout elections, campaigns can mobilize low-propensity voters. But even in presidential elections, voter turnout is much lower in the United States than in the context of this study. The level of political awareness is high in French

presidential elections, and encouragement to vote by friends and family members at its peak. As a result, there may simply have been no one left to mobilize.

On the other hand, the large persuasion impact of the campaign suggests that one-on-one discussions have a strong potential to shift people's decisions even when the principal's control on the campaign's agents is limited. This finding may have implications that reach beyond political campaigns to persuasive communication directed at consumers, donors, or investors. Further research should test systematically the generalizability of these findings by identifying the conditions under which one-on-one discussions and other modes of persuasion are most effective. In the current context, two dimensions may have contributed to the large persuasion impact of door-to-door canvassing. First, the signal of quality sent by the visits may have mattered more than the actual content of the discussions, and it may have been all the stronger, as most voters contacted by the campaign had never been canvassed before. Conversely, the effect of persuasive communication may dampen as a larger number of political parties or companies engage in one-on-one discussions with voters or consumers. Second, the diversity of political parties and platforms in France results in weaker partisan affiliations and more frequent changes in vote choice than in bipartisan contexts, such as in the United States. Further research could test whether the persuasion effect varies negatively with the intensity of preexisting voters' partisan affiliations or preexisting consumers' attachment to specific brands.

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