The Determinants of Political Behavior : Evidence from Three Randomized Field Experiments

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Abstract

My dissertation explores the political behavior of different groups, including immigrants, the youth, and uneducated and marginalized citizens – focusing on their registration and turnout decisions, as well as vote choices, political interest, and competence. I conducted three randomized field experiments in France and combined public electoral records with comprehensive phone and in-person surveys to address the following questions: 1) What factors, including costs, information, and attitudes explain people's failure to register and vote, as well as their choice to vote for an extremist party, and 2) How do institutions and political campaigns affect these factors?

In many countries (including the US) citizens must register before voting. The first chapter, coauthored with Céline Braconnier (Université de Cergy-Pontoise) and Jean-Yves Dormagen (Université Montpellier 1), provides experimental evidence on the impact of this additional hurdle on the size and composition of the electorate. Prior to the 2012 French presidential and parliamentary elections, 20,500 households were randomly assigned to one control or six treatment groups. Treatment households received home canvassing visits providing either information about registration or help to register at home. We show that France's registration requirements have significant effects on turnout and disproportionately discourage marginalized citizens on the left of the political spectrum. While both types of visits increased registration and turnout, the home registration visits had a higher impact than the information-only visits, indicating that both information costs and administrative costs are barriers to registration. Visits paid closer to the registration deadline were also more effective, suggesting that registration requirements' effects are reinforced by procrastination. Our design allows us to distinguish selection and treatment effects of home registration. We find that home registration included additional citizens who were only slightly less likely to vote than those who would have registered anyway, and did not reduce turnout among the latter. On the contrary, citizens induced to vote due to the visits also became more interested in the elections. Overall, these results suggest that the reduction of registration requirements could substantially increase political participation and improve representation of marginalized groups without debasing the average level of competence and informedness among voters.

Immigrants in Europe have a low sense of national belonging that affects their well-being and social cohesion in the receiving societies. This low sense of belonging is often interpreted as the result of low socioeconomic status, lack of efforts to integrate, or stigmatization. The second chapter, coauthored with Guillaume Liégey, provides empirical evidence for a complementary theory centered on the paucity of outreach efforts extending a hand to immigrants. During the 2010

French regional elections, 1,350 buildings hosting 23,836 citizens were randomly assigned to receive canvassers' visits. Supporting our theory, the visits had a larger effect on immigrants' turnout than on the mainstream population, although their propensity to vote was initially similar. More broadly, exploring heterogeneous effects of an identical encouragement to vote is shown to usefully complement comparisons of turnout levels to assess the influence of factors such as immigrant origin and race on electoral participation and integration.

Since the turn of the century, political campaigns have devoted increasing resources to door-to-door canvassing, in response to compelling evidence about the impact on voter turnout. However, we lack clear evidence on the impact of door-to-door canvassing on electoral outcomes, since unlike participation vote choice cannot be measured at the individual level with administrative records. The third chapter answers this important question with a countrywide precinct-level randomized experiment in France. During the 2012 presidential elections, 22,500 precincts and 17.1 million citizens were randomly allocated to either a control group or a treatment group. Treatment precincts were targeted by canvassers supporting François Hollande, the left-wing Parti Socialiste's candidate. The effects are estimated using official electoral results at the precinct level. The visits did not affect voter turnout, but they reduced the vote share of the far-right political party's candidate and increased Hollande's vote share at the first round. Overall, they contributed to one fourth of his victory margin at the second round. The effects persisted in the subsequent parliamentary elections, suggesting that they were obtained by persuading medium and high-propensity voters to vote left, rather than by mobilizing left-wing nonvoters and demobilizing opponents. The results suggest that personal contact can be an effective way for political parties to reconnect with disgruntled citizens and to win their votes.

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This dissertation is dedicated to the thousands of political activists, who, day after day, anonymously, bring politics to the doorstep of their fellow citizens. They are the torchbearers of our democracies. This dissertation is a tribute to the inestimable importance of their work. It would not have been possible without them.

Chapter 1

Voter Registration Costs and Disenfranchisement: Experimental Evidence from France[1]

1.1 Introduction

Historically, voter registration laws were adopted primarily to combat fraud. At the same time, they created a preliminary step to voting. In many countries today, the state creates the list of eligible voters, so from the citizens' perspective, registration is automatic. However, in other countries, such as the United States, Kenya, Mexico and France, voter registration is self-initiated: citizens

[1]This chapter is coauthored with Céline Braconnier (Université de Cergy-Pontoise) and Jean-Yves Dormagen (Université Montpellier 1). We are grateful to Esther Duflo, Benjamin Olken, Daniel Posner, Alan Gerber, Steven Ansolabehere, Abhijit Banerjee, Daron Acemoglu, James Snyder, Adrien Auclert, and seminar participants at MIT, the Tobin Project Graduate Students Forum, the WPSA, EPSA, NYU-CESS, CASP and APSA conferences for suggestions that have improved the paper. We thank Caroline Le Pennec and Ghislain Gabalda for the outstanding research assistance they provided throughout the entire project and Aude Soubiron for her assistance in the administration of the interventions in the cities surrounding Bordeaux. We thank the town hall administration of each of the ten cities included in the experiment for their generous collaboration and are indebted to all canvassers who administered the interventions, including students from the Ecole Normale Supérieure, the University Cergy-Pontoise, the IEP of Bordeaux and the University of Montpellier 1, the NGO of retired workers of the MGEN, the NGO "Tous Citoyens," the NGO "RAJ-LR," local units of the Socialist Party in Cergy, Sevran and Carcassonne and the local unit of the Front de Gauche in the 20th arrondissement of Paris. We gratefully acknowledge funding from the Russell Sage Foundation, MIT France, the Tobin Project, the city of Montpellier, the University of Montpellier 1, and the University of Cergy-Pontoise.

who wish to vote must first complete and submit a registration application to the administration. In the US, 29% of citizens are not registered (US Census Bureau 2012), while in France, 7% are not registered (Insee Premiere 2012) and around 20% are "misregistered": they stay registered at a previous address and have to travel back to vote, making voting more costly (Braconnier and Dormagen 2007).

How much does self-initiated registration matter? If information and registration costs are identical for all, the registration process selects the most interested citizens and excludes citizens with low interest in voting. Alternatively, if information and registration costs vary, the process also excludes citizens who are interested in voting but face too high a registration cost. It might then substantially decrease overall turnout, marginalize subgroups of citizens for whom registration costs are high, and change electoral outcomes.

To study the effects of self-initiated registration, this article evaluates the impact of door-to-door canvassing visits in France, in the context of the 2012 presidential and general (or parliamentary) elections. In several state-initiated registration countries, including Canada, South Africa, and Indonesia, election authorities rely on door-to-door canvassing to help get voters on the rolls (Brennan Center for Justice 2009). In this experiment, the visits were carried out by non-partisan students and NGO members as well as members of political parties. Ten cities and 44 electoral precincts were included in the experimental sample. In these precincts, we identified 4,118 addresses and 20,502 households likely to host unregistered and misregistered citizens.[2] In a randomly selected one-fourth of these households, no visit was made. The remaining households were visited by canvassers before the 31 December 2011 registration deadline. Buildings were randomized such that canvassers either provided information and encouragement to register (henceforth "canvassing" visits), or they offered to register people at home (henceforth "home registration" visits). The experimental design further varied the timing of the visits (early, two to three months before the registration deadline; or late, during the last month before the deadline) and their frequency (once or twice), with a total of six different treatments.

We evaluate the effects of the interventions using administrative data on registration and turnout, data collected by the canvassers during the visits, and comprehensive survey data collected door-to-door after the elections. In the control group, 18.3% of the initially unregistered

[2]We use "address" to mean any numbered street address, which can contain one or more households. Throughout the paper, we use the words "address" and "building" interchangeably, and the words "household" and "apartment" interchangeably.

and misregistered citizens registered during 2011. Canvassing visits and home registration visits increased new registrations by 2.4 percentage points (14%) and 4.7 percentage points (26%) respectively. This suggests that both the lack of information and the administrative cost of registering hinder voter registration. In addition, late visits, which left less time to register but also less time to procrastinate, were more effective than early visits, suggesting that registration requirements' effects are reinforced by procrastination.

Increased registration resulted in increased turnout. On average, the treatments increased turnout of initially unregistered or misregistered citizens by 4.3 percentage points (27%) and 4.1 percentage points (25%) in the first and second rounds of the presidential elections, and by 1.7 percentage points (18%) and 2.2 percentage points (24%) in the general (parliamentary) elections. In addition, the visits differentially selected citizens who were otherwise underrepresented in the electorate: younger and less educated citizens, citizens less likely to speak French at home, and immigrants. These citizens also tend to vote more to the left. This suggests that the self-initiated French registration system might skew electoral outcomes away from being accurate representations of the entire citizenry.

Increased participation and representation of the citizenry in the electorate are important democratic improvements. However, one might worry that a significant fraction of citizens are not sufficiently informed, so that increasing participation would lead to noisy electoral results and bad policies (e.g., Jakee and Sun 2006). In that respect, self-initiated registration might have two virtues. First, it might select more interested and knowledgeable citizens and exclude uninformed voters. In addition, self-initiated registration might have a positive treatment or "engagement" effect: citizens who make an effort to register might get more involved politically, increasing their electoral participation and perhaps how much thought they put in their actual vote. Among possible underlying mechanisms, prospective voters who undergo the cost of registration may adjust their subsequent behavior to their reaffirmed identity as engaged citizens (Bénabou and Tirole 2006). Thus, a possible concern is that by lowering the registration costs, the visits brought in less interested and knowledgeable voters, adding noise or bias to the final results, and that home registration visits reduced the engagement effect of getting registered at the town hall.

We study these selection and disengagement effects by comparing the participation rates of newly registered citizens in the different groups. We find that their turnout is not significantly different in control households versus households which received canvassing, and only slightly lower

in households that received home registration. How much this difference reflects selection or disengagement effects of home registration is difficult to verify directly. Two treatment groups in our experimental design were introduced to accommodate this difficulty: to control for selection into home registration, we compare addresses that received an early canvassing visit and a late home registration visit with addresses that received two home registration visits. As expected, similar newly registered citizens were selected by both interventions, but a higher fraction of these citizens were registered at home in addresses that received two home registration visits, which enables us to isolate the disengagement effect of home registration. We find that home registration did not have a disengagement effect on citizens who would have registered anyway.

However, there is suggestive evidence that home registration selected for participation citizens who were slightly less interested in elections than other voters. These citizens participated at a rate of four out of five in the presidential elections, a turnout only slightly lower than newly registered citizens who would have registered regardless of whether or not they receive a visit, and as high as previously registered citizens. However, their decline in turnout between the presidential and general elections was larger, suggesting that their participation depends relatively more on the saliency of the elections.

But political interest and knowledgeability are not necessarily fixed. They can be increased by inducing citizens to become active voters. Indeed, all interventions increased political interest and informedness among citizens who were initially unregistered and misregistered: they reported holding more frequent political discussions during the electoral campaign, and they were more likely to be able to locate their political preferences and that of prominent politicians on the left-right axis. This alleviates the concern that increased registration and participation may add noise to the electoral results.

The remainder of the paper is organized as follows. Section 2 discusses the existing literature on voter registration and other strands of the literature on which this study builds. Section 3 provides more background information on the 2012 elections, the French registration system and the interventions. In Section 4, we describe the sample population and the data used in the paper. Section 5 presents a simple model of the two-step process of registering and voting which frames the interpretation of the empirical results. Section 6 evaluates the overall impact of the visits on registration, turnout, and electoral outcomes. Section 7 investigates whether registration costs serve to select more knowledgeable citizens and to engage them in the electoral process. Section 8

concludes with a discussion.

Our study complements the existing empirical literature on the institutional determinants of voter participation, from Harold Gosnell's (1930) groundbreaking work on differences between electoral systems in Europe and in the US to the examination of recent changes in voter identification requirements (e.g., Gosnell 1930; Tingsten 1937; Rusk 1970; Converse 1972; Powell 1986; Jackman 1987; Franklin 1996; Lijphart 1997; Wolfinger, Highton and Mullin 2005; Blais 2006; Mycoff, Wagner and Wilson 2009). Most studies of voter registration exploit temporal and spatial variation in voter registration laws to estimate the effect of these laws on turnout. Some find little to no effect of introducing voter registration, motor voter provisions, election-day registration, or new closing date (e.g., Martinez and Hill 1999; Knack 2001; Brown and Wedeking 2006; Burden and Neiheisel 2013), while others find strong effects (e.g., Rosenstone and Wolfinger 1978; Wolfinger, Glass and Squire 1990; Knack 1995; Mitchell and Wlezien 1995; Rhine 1996; Highton and Wolfinger 1998; Ansolabehere and Konisky 2006; Vonnahme 2012). An important concern is that it is often difficult to separate changes in registration laws from other institutional changes and concomitant trends, as illustrated by the controversy regarding the causes of the decline in voter turnout at the turn of the 19th Century in the US (Kelley, Ayres, and Bowen 1967; Burnham 1965, 1974; Rusk 1970, 1974; Converse 1972, 1974). In addition, the adoption of different registration rules by different states or counties might reflect unobserved motives correlated with participation. This omitted-variables problem is also a potential concern for a second strand of the literature, based on individual survey data, which estimates determinants of registration and turnout separately and predicts high turnout rates among non-registrants (Erikson 1981; Timpone 1998). Next to observational studies, experimental studies can provide useful insights on individual responses to institutional changes facilitating registration (e.g., Bennion and Nickerson 2009). In an unpublished study perhaps most closely related to this project, Nickerson (2010) finds that home registration visits have large effects on registration numbers, but that citizens registered as a result of the visits are less likely to vote than previously registered citizens. Building on these earlier studies, we introduce two important distinctions: how much do lack of information, the administrative cost of registering, and procrastination hinder registration when it is self-initiated; and what are the selection and treatment effects of visits facilitating registration? These distinctions enhance the generalizability of the findings to a wide array of possible changes in the registration rules, each of which combines some but not necessarily all dimensions disentangled here.

This article also builds upon existing research on the effects of electoral institutions on the composition of the electorate and on electoral outcomes. Most papers comparing voters and nonvoters conclude that universal turnout would benefit left-wing parties only marginally (e.g., Teixeira 1992; Highton and Wolfinger 2001; Citrin et al. 2003; Brunell and DiNardo 2004; Bernhagen and Marsh 2007; Rubenson et al. 2007; but see Mackerras and McAllister 1999). The underlying assumption that the preferences reported by nonvoters accurately reflect how they would vote if induced to vote is, however, debatable (Lijphart 1997). Another set of studies exploits institutional variations, with similar methodological limitations as emphasized above, and mixed findings (e.g., Filer, Kelly, and Morton 1991; Nagler 1991; Franklin and Grier 1997; Knack and White 1998, 2000; Stein 1998; Brians and Grofman 2001). More recent studies based on quasi-experimental variations find substantial effects on vote shares of the introduction of new voting technologies (Fujiwara 2013) or the adoption of compulsory voting (Fowler 2013). Our article extends their conclusions to the case of voter registration and provides a richer description of the enfranchised citizens: we not only measure their gender, education, income and occupation, but also the language they speak at home, the intensity of their religious practice (if any), whether they come back from work after the town hall's closing hours, and other variables.

Thanks to our rich survey data, our article also contributes to the literature on voters' interest and information. A difference repeatedly found between voters and nonvoters is that the latter are less interested and informed (Converse 1964; Palfrey and Poole 1987). Given this difference, institutions facilitating participation might bring in voters who are unlikely to cast a well-considered ballot and they might add noise to the final results (Jakee and Sun 2006; Selb and Lachat 2009; Saunders 2010). But interest and informedness are not necessarily static: citizens induced to become active voters might also increase their interest and knowledge (Robson 1923; Lijphart 1997). Existing empirical evidence for this mechanism is scarce. Bilodeau and Blais (2011) compare the political interest of citizens and immigrants from countries with and without compulsory voting and obtain null results, but acknowledge methodological limitations. Our study fills an important gap in this respect.

Finally, our study speaks to a large economic literature on the procedural costs incurred when applying to a service or aid program, and their effects on program take-up and applicants' selection (e.g., Nichols et al. 1971; Nichols and Zeckhauser 1982; Besley and Coate 1992). Two recent experiments examine interventions that reduce procedural costs by enrolling people in an aid program

at their homes. Devoto et al. (2012) find substantial effects of home procedural assistance on the take-up of connections to the water main, and provide suggestive evidence that a simple door-to-door information campaign on the program has intermediate effects, as in our study. Alatas et al. (2013) find that imposing some procedural costs leads to a better selection of applicants than when people are enrolled in the program door-to-door. Our study finds similar results: citizens registered due to the visits are slightly less likely to vote than those who register when they have to bear the full procedural costs.

1.2 Setting

1.2.1 The 2012 French presidential and general elections

French presidential elections have two rounds: any candidate who gets endorsed by at least 500 locally elected officials can compete at the first, and the two candidates who get the highest vote shares qualify for the second. 79% of the registered citizens participated in the first round of the French presidential elections on 22 April 2012. François Hollande of the left-wing Parti Socialiste and Nicolas Sarkozy of the right-wing UMP qualified for the second round. Turnout at the second round on 6 May was high again (80%) and François Hollande was elected president with 52% of the vote. Similarly to the presidential elections, the general elections consist of two rounds, unless a candidate obtains more than 50% of the votes during the first round. They took place on June 10 and 17. Fewer voters (57 and 55%) participated in these elections than the presidential elections and the previous general elections (Figure 1). The Parti Socialiste won in 57% of the constituencies.

1.2.2 The French registration system

French voter rolls are updated and made publicly available each January, and the registration deadline for a given election is December 31 of the previous year: only citizens who had registered before 31 December 2011 could vote at the French 2012 elections. Given the timing of the 2012 elections, the 2011 registration period took place even before the electoral campaign had begun, as is usually the case. To register, one must file an application, submitting a form, an ID, and proof of address, such as a recent electricity bill. The address is used to allocate each registered citizen to the electoral precinct closest to his place of residence. Most people register in person at the town

hall, although the registration file, once signed by the applicant, can be brought to the town hall by a third party, mailed in, or, in some cities, completed online.

Since 1997, teenagers who turn 18 are, in principle, automatically registered. Apart from this group, it is citizens' responsibility to register and re-register each time they move. Those who move without updating their registration status become misregistered. They are registered to vote, but cannot vote at the polling station nearest to their actual place of residence. Voting is relatively more costly for them: they have to travel back to the polling station corresponding to their previous address on Election Day, or to go to a courthouse or police station at least one week before to apply for a proxy vote allowing a trusted person to vote on their behalf at their former polling station. After a while, as political propaganda and voter IDs repeatedly fail to be delivered to them, the misregistered citizens get struck from the lists and join the ranks of the unregistered citizens, which further include people who turned 18 before 1997 and naturalized citizens who never registered.

In 2011, as in other pre-presidential years, a large fraction (9%) of eligible citizens registered for the first time or updated their registration status (Insee 2012). Nonetheless, 7% of all people living in metropolitan France who were eligible to register remained unregistered (Insee Premiere 2012) and around 20% were misregistered.[3]

1.2.3 Interventions

The experimental design is shown in Figure 2. In a randomly selected one-fourth of the households, no visit was made (hereafter, the control group). In a second randomly selected quarter of the households, canvassers encouraged the unregistered and misregistered citizens to register and provided general as well as city-specific information about the process (hereafter, the canvassing group); after a conversation of one to five minutes, they distributed a leaflet customized with the logo of their organization that summarized this information (an example can be found in Appendix 1). In a third quarter of the households, the canvassers offered to register people at home so that they would not have to register at the town hall (hereafter, the home registration group): the canvassers filled out the registration form of those who accepted, completed it with a picture of ID,

[3]While the number of unregistered citizens can be directly estimated as the difference between the number of eligible citizens and those actually registered, no similar method can be used to compute the number of misregistered citizens. In France, the fraction of misregistered citizens is probably between 12% and 25%: the first estimate is based on answers from a 2007 representative pool and does not take into account citizens registered in the correct city, but at an old address (Cevipof 2007). The second estimate is based on the study by Braconnier and Dormagen (2007) conducted in neighborhoods likely to host more misregistered citizens than the national average.

collected a proof of address, and brought the file to the town hall themselves. Some applications required several visits, for example, when one of the documents was missing or was rejected by the town hall as invalid. The remaining quarter of households received two separate visits (hereafter, the two-visits group).

The canvassing, home registration, and two-visits groups were each further randomly divided into two subgroups. Half of the canvassing and home registration households were visited early, two to three months before the registration deadline, whereas the other half received a late visit, during the last month before the deadline. Half of the two-visits households received an early canvassing visit and a late home registration visit, whereas the other half received two home registration visits.

The visits were carried out by 230 students, NGO members, and party activists.[4] This diversity increases the external validity of the study. Thanks to extensive training, it did not threaten its internal validity: all canvassers were engaged in role-plays, and were asked to draw a sharp line between the two types of visits.

1.3 Sample population and data

1.3.1 Addresses and apartments included in the sample

This study took place in ten cities, located in three regions, and ranging in size from 10,000 inhabitants to more than 200,000.[5] The main criteria for selection of the cities were the availability of groups of people willing to take part in the experiment as unpaid canvassers and the logistical and financial support that the municipality could provide. In each city, we selected precincts characterized by relatively lower turnout rates at previous elections, and thus likely to host many unregistered and misregistered citizens. The 44 sample precincts are therefore not representative of France, but they are quite representative of areas that would be the most affected by changes in the registration process.

In each precinct, addresses and apartments in which unregistered and misregistered citizens were likely to reside were identified as follows. We first collected the list of citizens registered at

[4]The party activists belonged to the Parti Socialiste or the Front de Gauche, another left-wing party. Contacts had been established with local units of other political parties as well, albeit unsuccessfully.

[5]Cities in the experiment are: Cergy, Saint-Denis, Sevran, and the 20th arrondissement of Paris (in the region Ile-de-France), Montpellier and Carcassonne (in Languedoc-Roussillon), and Blanquefort, Eysines, Le Taillan, and Lormont (in Aquitaine). All cities are localized on a map included in Appendix 2.

the precinct as of January 2011 and ordered it by address. Between May and September 2011, surveyors went to each address and wrote down names found on the mailboxes or on intercoms and the corresponding apartment numbers. This preliminary work was conducted at 6,030 addresses, excluding addresses that were not found or were inaccessible to the canvassers. When all names found on a mailbox also appeared on the voter roll, we excluded the corresponding apartment from the experiment given the low probability of finding unregistered or misregistered citizens there. We found 20,502 apartments likely to host unregistered or misregistered citizens, located at 4,118 addresses, which we call the experimental sample.[6] These addresses were randomly allocated to the control group and the six treatment groups after stratification by precinct and number of registered citizens at each address.

Panels A and B of Table 1 present summary statistics for addresses and apartments in the experimental sample. We also identify significant differences between the control group and all treatment groups pooled together, and test the joint significance of the differences with each treatment group taken separately. First, we find that the average address contains eight apartments, of which five were included in the experimental sample, and that the average sample apartment features 1.3 last names found on its mailbox that did not match with any name on the January 2011 voter roll. The differences between the control group and the treatment groups are not significant for any of these variables. Second, housing price data at the address level was obtained from the real estate company www.MeilleursAgents.com for cities located in Ile-de-France. The average housing price is approximately 3,000 euros per square meter: this is relatively high due to the proximity of Paris, but lower than the cites' average.

1.3.2 Initial numbers of unregistered and misregistered citizens

Studies of voter turnout can use the voter rolls as their sample. Unfortunately, these rolls are of little help when it comes to studying unregistered and misregistered citizens. Indeed, there is no systematic list of all citizens at each address who are eligible to register, to which the voter rolls could be compared. We can nonetheless estimate the initial numbers of unregistered and misregistered citizens using the reports provided by canvassers: for each apartment that opened its door, canvassers estimated the numbers of well-registered, misregistered and unregistered citizens,

[6] In 17% of addresses, it was impossible to link apartments to mailboxes, due to the lack of any number or available identification, so that all apartments were covered by canvassers, whether included in the sample or not.

as well as the number of foreigners. We address several issues when exploiting this data, as is detailed in Appendix 3, and finally estimate that at the beginning of 2011, in the experimental sample, the average apartment hosted 0.23 well-registered citizens and 0.92 citizens in our target (0.63 misregistered citizens and 0.29 unregistered citizens). Taking into account all the apartments and addresses located in the precincts of the study, there were initially approximately 56.2% well-registered citizens, 29.9% misregistered citizens, and 13.9% unregistered citizens.

1.3.3 Individual registration and turnout data

We identify the citizens who registered in 2011 by comparing the January 2011 and January 2012 administrative voter lists. We identify their apartment based on the information listed in their address and by matching their last name or marital name with the names initially found on the mailboxes. This enables us to identify the apartment number of 89% of newly registered citizens. The 2012 voter lists provide each registered citizen's name, address, gender, and date and place of birth. In addition to this publicly available data, we obtained the registration date, previous registration status, and previous city of registration, if any, for all citizens who registered in 2011.

Beyond registration, we measure the individual participation of all registered citizens at the 2012 French presidential and general elections. Attendance sheets signed by voters who cast a ballot on Election Day are available for consultation until ten days after each poll. We took pictures of these sheets and digitized them. Thanks to this administrative data, we measure the actual voting behavior of all registered citizens and do not have to rely on survey reports, which are often unreliable when it comes to voter turnout (Ansolabehere and Hersh 2011). Altogether, our analysis is based on approximately 135,000 individual turnout observations.

1.3.4 Characteristics of the unregistered and misregistered citizens

To get further information about the experimental sample population, a postelectoral survey was administered by 50 surveyors to a sample of 1,500 respondents living in the cities of Saint-Denis, Cergy, Sevran and Montpellier. Respondents were surveyed at their household within the month following the second round of the general elections. The survey was administered only to French citizens who were not registered at their address as of January 2011, independently of their registration status by the registration deadline, so that the sample selection was unaffected by the

interventions. The response rate was very close in control and treatment households.[7]

Panel C of Table 1 presents summary statistics for the respondents to the survey. The average respondent is 36 years old, which is more than 10 years younger than the average French adult, and lives with two other household members. 40% of the respondents are males, and 54% are in a relationship. 42% do not have any diploma or have less than an end-of-high-school diploma, which is less than the overall adult population, reflecting the younger age. 10% – slightly more than the overall adult population – are unemployed, and 27% are inactive. 55% live in social housing, 14% own their house or apartment, and 31% live in private housing. 42% earn less than the minimum wage (1100 euros a month). 40% speak a language other than French with family members. Half of the respondents have lived in the city for more than 10 years, and 17% arrived less than two years ago. 76% were born in France, and 24% in the same "département." 22% were naturalized French and 22% hold another citizenship. Finally, two thirds are adherent of a religion, and one third are regular churchgoers. The differences between the control group and the treatment groups pooled together are not significant for any of these variables. However, the differences with treatment groups taken separately are jointly significant at the 5 and 10% level for 4 and 2 variables, respectively, out of 31.

In addition to this socioeconomic information, the postelectoral survey included a series of questions about the respondents' political preferences, vote choices, political interest and competence.

1.4 Model

The following model serves three purposes. First, we extend the standard cost-benefit model of the voting decision (Downs 1957; Riker and Ordeshook 1968) to account for registration as a first separate stage and we model its connection with the second stage, voting. Second, we describe likely type differences between two categories of citizens – "compliers" and "always-takers" – along the two dimensions that explain individuals' decisions to register and vote: benefits of voting and the registration cost. Our terminology follows Angrist, Imbens, and Rubin (1996): the "compliers" are citizens registered as a result of the visits, and the "always-takers" are newly registered citizens who would have registered regardless of whether or not they receive a visit.[8] Third, we study what

[7] More information about the sampling frame of the postelectoral survey is available in Appendix 4.

[8] We assume that there are no defiers: all citizens who register if they do not receive any visit also register if they receive a visit.

can be inferred about the magnitude of *unobserved* benefits of voting and registration cost, from the *observed* participation of the compliers and the always-takers. This theoretical structure will guide the interpretation of our empirical findings on voter turnout: does the compliers' lower average participation reflect a higher cost to register or a lower political interest than always-takers? In other terms, does their failure to register, absent any visit, result from benefits of voting that are too low, or from a registration cost that is too high?

1.4.1 Two stages: registration, and voting

Each unregistered citizen needs to decide whether to register and second, whether to vote.

Individual i is characterized by her net registration cost c_i and her average net benefits of voting b_i. c_i includes gathering information about the registration process and actually going through the process. It is higher for those who are less comfortable with bureaucratic tasks, who live further away from the town hall or work during opening hours, who have unconventional living situations that do not easily meet residency requirements, or who move frequently and thus have to re-register more often. c_i may also depend on the person's wealth: a given time spent to go to the town hall and register imposes a higher monetary cost on the rich, but it may impose a higher utility cost on the poor, whose marginal utility of consumption is higher (e.g., Alatas et al. 2013). b_i includes expressive and instrumental benefits, minus the cost of voting. For simplicity, we assume that there is only one electoral round and that there is no intertemporal actualization rate.

In the first stage, if i registers, she has to pay c_i and expects to get second-stage utility $g(b_i)$. i decides to register if $c_i \leq g(b_i)$. If she receives the visit of canvassers, her registration cost decreases to λc_i with $\lambda \in [0,1)$, and i decides to register if $\lambda c_i \leq g(b_i)$.

In the second stage, i can cast a vote if she registered in the first stage. She decides to vote if $b_i + \varepsilon_i \geq 0$, where ε_i is a shock realized after registering, with density f_ε, distribution F_ε, and $E[\varepsilon_i] = 0$. ϵ represents all factors that affect the benefits of voting and which are unknown at the time of registering, including, for instance, corruption scandal affecting the candidate i was planning to vote for; new polls affecting her expectations about the closeness of the election; transition to or from unemployment which affects her views about the general economic situation; unexpected travel plans which force her to be absent on election day thereby increasing the cost of voting.

We infer that i's second-stage utility, conditional on being registered, is

$$g(b_i) \equiv \int_{-b_i}^{\infty} (b_i + \varepsilon) f_\varepsilon(\varepsilon) d\varepsilon.$$

Her propensity to vote, conditional on being registered, is

$$v(b_i) \equiv P(b_i + \varepsilon_i \geq 0) = 1 - F_\varepsilon(-b_i)$$

such that $v(b)$ and $g(b)$ both increase in b.

1.4.2 Two simple cases: uniform benefits of voting or registration cost

Let us now analyze the differences between compliers and always-takers along benefits of voting and the registration cost. Since the compliers only register when registration is facilitated, we expect them to be characterized by lower benefits of voting and/or a higher registration cost on average. This is indeed the conclusion that we reach when we consider two simple cases, where benefits of voting or registration cost are uniform across all individuals.

Uniform benefits of voting

We first consider the case where the benefits of voting are uniform across all i's ($b_i = b$). Always-takers and compliers are characterized respectively by $c_i \leq g(b)$ and by $g(b) < c_i \leq {}^{g(b)}/_{\lambda}$ (see Figure 3a). Compliers face a higher registration cost than always-takers, but have identical benefits of voting and the same propensity to vote, conditional on being registered.

Uniform registration cost

We next consider the case where the registration cost is uniform across all i's ($c_i = c$). The always-takers are then characterized by $g^{-1}(c) \leq b_i$ and the compliers by $g^{-1}(\lambda c) \leq b_i < g^{-1}(c)$ (Figure 3b). The visits result in the registration of citizens who face the same registration cost as always-takers but have lower benefits of voting and a lower propensity to vote, conditional on being registered.

1.4.3 General case

We now turn to the more general case, in which both benefits of voting and registration cost vary across citizens. Is it still the case that the compliers are characterized by lower benefits of voting and/or a higher registration cost than always-takers, and under which conditions?

Differences between always-takers and compliers

The distribution of types over the entire population of unregistered citizens is now described by the continuous bivariate random vector of benefits of voting and registration costs (B, C), with joint density function $f(b, c)$ and marginal density functions $f_B(b)$ and $f_C(c)$.

The always-takers are characterized by $c_i \leq g(b_i)$ and the compliers by $g(b_i) < c_i \leq {}^{g(b_i)}/_{\lambda}$ (Figure 3c). Among citizens facing a given registration cost, it is immediate that compliers have lower expected benefits of voting than always-takers. Similarly, among citizens with a given expected benefit of voting, compliers face a higher registration cost than always-takers. However, these results do not mechanically extend to the comparison of all compliers and always-takers. As an example, consider the case represented in Figure 3d. The density function $f(b, c)$ is such that $g(b_i) \leq g_1$ or $g(b_i) \geq g_2$ any i. In addition, for all i such that $g(b_i) \leq g_1$, $c_i \leq g(b_i)$; and for all i such that $g(b_i) \geq g_2$, $c_i \geq g(b_i)$. Then, all the always-takers have benefits of voting lower than g_1, and all the compliers have benefits of voting higher than g_2: on average, compliers have higher benefits of voting than always-takers. It is equally easy to construct density functions such that, on average, compliers have a lower registration cost than always-takers.

Let us identify sufficient conditions that rule out these cases, and describe the type difference between always-takers and compliers under these conditions. All the proofs are included in Appendix 5. The most important condition is the following:

Condition ID (increasing differences): $-f(b, c)$ satisfies log-increasing differences in b and c: $\frac{f(b',c')}{f(b',c)} < \frac{f(b,c')}{f(b,c)}$ for any $b' > b$ and $c' > c$.

This condition is satisfied, for instance, by any bivariate normal density with negative correlation between b and c. It means that there are relatively fewer citizens with a higher c among citizens with a higher b. It directly implies that people with a higher b have a lower c, on average. This corresponds to the expectation that factors such as education, age and high socioeconomic status both increase the benefits of voting and decrease the registration cost.

In addition, we use the following regularity condition:

Condition R1 (regularity condition): For any b, and any $b" \geq b'$ with $b' \epsilon [g(b), g(b)/\lambda]$, $\frac{b" f(b"|b)}{F(b"|b)} \leq \frac{b' f(b'|b)}{F(b'|b)}$.

Claim 1: Under *Conditions ID* and *R1*, compliers have lower benefits of voting on average than always-takers: $E[b_i \mid i \text{ is complier}] \leq E[b_i \mid i \text{ is always-taker}]$.

Claim 2: Under *Conditions ID* and *R1*, compliers face a higher registration cost on average than always-takers: $E[c_i \mid i \text{ is complier}] \geq E[c_i \mid i \text{ is always-taker}]$.

Claim 3: Under *Conditions ID* and *R1*, compliers have a lower propensity to vote on average than always-takers: $E[v(b_i) \mid i \text{ is complier}] \leq E[v(b_i) \mid i \text{ is always-taker}]$.

Claim 4: Under *Conditions ID* and *R1*, compliers who vote have lower benefits of voting on average than always-takers: $E[b_i \mid i \text{ is complier}, i \text{ votes}] \leq E[b_i \mid i \text{ is always-taker}, i \text{ votes}]$.

In sum, under *Conditions ID* and *R1*, compliers have lower benefits of voting and face a higher registration cost on average than always-takers. They have a lower propensity to vote, and those who vote have lower benefits of voting: the compliers who vote are more likely than always-takers who vote to vote based on recent shocks (captured by ϵ) and to express short-term preferences rather than long-term interest in politics.

Learning about the compliers' benefits of voting and registration cost

Does the compliers' failure to register, absent any visit, result from benefits of voting that are too low, or from a registration cost that is too high? To answer this question, we would like to test the predictions that compliers have lower benefits of voting and face a higher registration cost than always-takers (*Claims 1* and *2*) and, in addition, to examine whether the difference is larger along the first or the second dimension. Unfortunately, benefits of voting and registration cost are usually unobserved. What we can and will observe, however, is voter turnout. This will enable us to test *Claim 3*. But we can do more: under certain conditions, specified below, we can draw inferences from the *observed* participation of the compliers and the always-takers to their *unobserved* benefits of voting and registration cost.

Condition R2 (regularity condition): $z(b) \equiv E_f[c_i \mid i \text{ is complier}, b_i = b] = \frac{\int_{g(b)}^{g(b)/\lambda} c f(b,c) dc}{\int_{g(b)}^{g(b)/\lambda} f(b,c) dc}$ increases in b.

Claim 5: Under *Conditions ID*, *R1* and *R2*, for a given share of compliers and unchanged conditional densities $f(c \mid b)$, an increase in the compliers' propensity to vote, generated by an

increase in the relative number of compliers with a higher b, is concomitant to an increase in their benefits of voting and registration cost.

Claim 5 can be read as a thought experiment. Suppose we build a prior about the compliers' average propensity to vote, benefits of voting, and registration cost. Suppose further that their true, observed participation, turns out to be higher than our prior. Then, under *Conditions ID*, *R1* and *R2*, we should infer both that their benefits of voting are higher than our prior and that their registration cost is higher than our prior. In other words, we should infer that the compliers' failure to register, absent the visits, has less to do with low benefits of voting and more with high registration costs than we initially thought.

1.4.4 Three extensions of the model

Canvassing visits vs. home registration visits

Compared to the canvassing visits, home registration visits bring the registration cost further down, by a factor of $\lambda' < \lambda < 1$, which selects compliers with different characteristics.

Claim 6: Under *Conditions ID* and *R1*, for any $\lambda' < \lambda < 1$, λ' visits select compliers with a higher registration cost, lower benefits of voting, lower propensity to vote, and lower benefits of voting conditional on voting than λ visits.

In addition, registering someone at home might reduce the engagement effect of getting registered and thus decrease her benefits of voting and her propensity to vote (for a longer discussion of this effect, see Section 7): an individual's benefits of voting b_i might be endogenous to the way in which she gets registered.

The mobilization effect of the campaign

i's propensity to vote might also depend on the mobilization effect of the campaign, in particular for high-salience elections. Then, i's propensity to vote becomes $w(b_i) \geq v(b_i)$, an effect which she does not take into account in her decision to register.

We investigate the case in which citizens with lower benefits of voting experience a larger mobilization effect but continue to vote relatively less: $w(b') - v(b') \leq w(b) - v(b)$ and $w(b') \geq w(b)$ for any $b' \geq b$. The first assumption is microfounded in Appendix 5.

Claim 7: All previous results hold in this extended version of the model.

Claim 8: The difference between compliers' and always-takers' predicted turnout is lower once the mobilization effect is taken into account.

Misregistered citizens

We now discuss the extension of the model to citizens initially misregistered (instead of unregistered).

Each misregistered citizen can be characterized by c_i, b_i, and k_i, the additional cost of voting (time and financial cost) that i faces if she votes in her previous precinct rather than at the precinct closest to her new address. The distribution of types over the entire population of misregistered citizens is described by the continuous multivariate random vector (B, C, K), with density function $f(b, c, k)$. Similarly to unregistered citizens, misregistered citizens expect to get second-stage utility

$$g(b_i) = \int_{-b_i}^{\infty} (b_i + \varepsilon) f_\varepsilon(\varepsilon) d\varepsilon$$

if they update their registration status. However, if they fail to do so, their expected utility is no longer 0, but

$$g(b_i - k_i) = \int_{-b_i+k}^{\infty} (b_i - k_i + \varepsilon) f_\varepsilon(\varepsilon) d\varepsilon$$

since they can still vote at their previous precinct.

The always-takers are characterized by $c_i \leq g(b_i) - g(b_i - k_i)$ and the compliers by $g(b_i) - g(b_i - k_i) < c_i \leq \frac{g(b_i) - g(b_i - k_i)}{\lambda}$.

We call $f_k(b, c)$ the distribution of types of misregistered citizens who face the additional cost k of voting at their previous address and define $g_k(b) \equiv g(b) - g(b - k)$.

We define three new conditions, for any k:

Condition ID_k: $-f_k(b, c)$ satisifies log-increasing differences in b and c.

Condition $R1_k$: For any b, and any $b" \geq b'$ with $b' \epsilon [g_k(b), g_k(b)/\lambda]$, $\frac{b" f_k(b"|b)}{F_k(b"|b)} \leq \frac{b' f_k(b'|b)}{F_k(b'|b)}$.

Condition $R2_k$: $z_k(b) \equiv E_{f_k}[c_i \mid i \text{ is complier}, b_i = b]$ increases in b.

Claim 9: For any k, if *Conditions* ID_k, $R1_k$ and $R2_k$ hold, all results established for unregistered citizens hold for misregistered citizens facing an additional cost k of voting at their previous address.

It is important to note that, absent any further restriction on $f(b, c, k)$, the same results do not necessarily hold for all misregistered citizens pooled together.[9] This has an important consequence

[9]For instance, Claim 9 predicts that compliers with a given k have a *lower* b than always-takers facing the same

for our empirical analysis: when we compare the propensity to vote of compliers and always-takers and explore to what extent the former have lower benefits of voting and a higher registration cost, we should control for possible compositional differences by including unregistered citizens and misregistered citizens with different k separately in the regression.

1.5 Overall impact on registration, turnout, the composition of the electorate, and electoral outcomes

This section discusses the main findings. The first subsection presents results on the impact of the visits on registration and identifies the registration barriers that were alleviated by the interventions. The second presents the impact of the visits on voter turnout. Beyond participation, the third and fourth subsections describe the socioeconomic characteristics and political preferences of the citizens selected by the visits.

1.5.1 Impact on registration

To begin with, we examine the impact of the interventions on registration. Ideally, we would like to use the individual registration status of citizens who were initially unregistered or misregistered as the outcome. But remember that we do not have any systematic list of these citizens. We thus have to use a slightly different outcome: the number of new registrations in each household.[10] We compute the average number of new registrations in the control group and in each treatment group and divide it by the initial number of unregistered and misregistered citizens, 0.92 (from Section 4.2), to obtain the fraction that registered. As shown in Figure 4, there were 0.17 new registrations in the average control household: absent any visit, 18% (0.17 / 0.92) of the citizens who were initially unregistered or misregistered got registered. This fraction was higher in all treatment groups. To investigate the statistical significance of the differences shown in Figure 4 more systematically, we

k. But suppose a distribution $f(b, c, k)$ such that b is higher for misregistered citizens with a lower k, and the share of compliers is larger among misregistered citizens with a lower k. In such a case, it is possible that, averaging over all values of k, compliers have a *higher* b than always-takers.

[10]This number can take higher values than 1, in apartments hosting multiple citizens, and it is necessarily equal to 0 in apartments hosting only foreigners.

estimate the following OLS regression:

$$NR_{i,b} = \alpha + \sum_{t=1}^{6} \beta_t T_b^t + X_{i,b}^{'} \lambda + \sum_{s} \delta_b^s + \epsilon_{i,b} \qquad (1)$$

where $NR_{i,b}$ is the number of new registrations in apartment i of building b, T_b^t are dummies corresponding to the six treatment groups, δ_b^s are strata fixed effects, and $X_{i,b}$ is a vector of apartment and building characteristics. $X_{i,b}$ includes the number of mailboxes in building b (a proxy for social housing since buildings with social housing are typically bigger) and the number of last names found on the mailbox of apartment i that were absent from the 2011 voter rolls (a proxy for the initial number of unregistered and misregistered citizens in the apartment). The key coefficients of interest are the β_t's, which indicate the differential number of new registrations in apartments of the different treatment groups. The β_t's are intent-to-treat estimates: they are not adjusted to take into account the fraction of opened doors.[11] In this and all other regressions, we adjust standard errors for clustering at the building level since the randomization was conducted at this level.

The results from Equation [1] are presented in Table 2, column 1.[12] On average, the visits increased the number of new registrations by 0.048. This effect is statistically significant at the 1% level. Using the initial number of unregistered and misregistered citizens as the denominator, this effect corresponds to an increased fraction of registered people among these citizens of $\frac{0.048}{0.92} = 5.2$ percentage points. Using the number of new registrations in the average control apartment as the denominator, it corresponds to an increase of $\frac{0.048}{0.168} = 29\%$. "Early Canvassing" and "Late Canvassing" visits increased the number of new registrations by 0.014 (1.5 percentage points, or 8%) and 0.031 (3.4 percentage points, or 18%) respectively. The increases are of 0.032 (3.5 percentage points, or 19%) and 0.054 (5.9 percentage points, or 32%) for the "Early Home registration" and "Late Home registration" visits, and 0.060 (6.5 percentage points, or 36%) and 0.096 (10.4 percentage points, or 57%) for the "Early Canvassing & Late Home registration" and "Early Home registration & Late Home registration" visits. All individual effects are significant at the 1 or 5% level, except

[11] On average, 46.2% of the households visited only once opened their door, and 65.1% of the households visited twice opened their door at least once.

[12] Tables A1 and A2 in Appendix 7 show the robustness of the results to slightly different definitions of the outcome variable. In Table A1, we use the address (and not the apartment) as the unit of observation. This decreases statistical power, but it enables us to take into account newly registered citizens whose address is known but whose apartment could not be identified. In Table A2, we use the net number of new registrations instead of the gross number as the outcome: citizens removed from the voter rolls are subtracted from new registrations.

for the "Early Canvassing" visits.

The visits were targeting unregistered citizens and two types of misregistered citizens: citizens initially registered in another city, and citizens initially registered at another address in the same city. Taking into account the fractions of citizens of these three categories in our initial sample, we find that the visits increased their registration rate by 47%, 18%, and 32% respectively, so that 50% of the compliers[13] are citizens who were initially unregistered and 32% and 18% citizens who were initially registered in another city or at another address in the same city.[14]

Which mechanisms explain these effects? The variations in the timing and type of visits in the canvassing and home registration groups were introduced to disentangle two types of obstacles hindering registration – administrative cost of registering and lack of information about the process – and to examine whether these obstacles are reinforced by procrastination. We now estimate linear combinations of the β's to study more closely the respective importance of these three impediments to registration and the extent to which the visits alleviated them. We report the point estimates and standard errors at the bottom of Table 2, column 1.

First, on average, early and late canvassing visits significantly increased the number of new registrations by 0.022 (2.4 percentage points, or 13%), an effect significant at the 5 percent level. This suggests that imperfect information prevents some eligible citizens from registering to vote. Additional evidence supports the view that, to a large extent, increased information explains the impact of canvassing visits: many respondents to the postelectoral survey were unaware of the December 31 deadline and assumed that they could register up to a few days before the elections. In addition, discussions held at the door brought anecdotal evidence that many citizens are unaware of the documents required for the registration application, and that misregistered citizens often have mistaken beliefs about the administrative steps they must take to update their registration status.[15]

Second, the far more intensive home registration visits increased the number of new registrations by 0.043 (4.7 percentage points, or 26%), nearly doubling the effect of canvassing visits, a

[13]Following Angrist, Imbens, and Rubin (1996), we use the following definitions. The "compliers" are citizens registered as a result of the visits. The "always-takers" are newly registered citizens who would have registered regardless of whether or not they receive a visit.

[14]See Table A3 in the Appendix for results disaggregated by initial registration status.

[15]It must, however, be pointed out that the impact of canvassing may not exclusively be explained by the supply of information. Canvassing may also have served as a reminder of civic duty norms among respondents. The mere presence of canvassers working on a volunteer basis may have acted as a reminder of civic responsibility highlighting the ethical importance of registering to vote and participating in elections. Our experiment design does not enable us to distinguish, in the impact of canvassing, what pertains to the provision of information vs. the reminder of civic duty.

difference significant at the 10% level: Conditional on available information, the administrative cost of registering also impedes registration.

Third, to measure the possible influence of procrastination, we compare the impact of visits conducted in October and November 2011 to that of visits conducted in December 2011. Late canvassing and home registration visits had a larger effect than early visits, a difference also significant at the 10% level. The sign of this difference might be surprising at first, since early visits left more time to register. The larger effect of late visits is likely the sign that registration requirements' effects are reinforced by procrastination. First, late visits left less time to procrastinate. Second, naive individuals (in the terminology of O'Donoghue and Rabin (1999)) who received them had been procrastinating for a longer time, and were thus more likely to become sophisticated by talking to the canvassers. Previous empirical evidence of procrastination among registration applicants supports this interpretation (Bennion and Nickerson 2011), as does anecdotal evidence about long queues of citizens registering within the last days and last hours before the registration deadline.[16]

1.5.2 Impact on voter turnout

We now turn to the central question of our experiment and examine the extent to which the impact of the visits on registration translated into increased turnout. Averaging on the first and second rounds of the presidential elections and the first and second rounds of the general elections, Figure 5 shows, for each group, the number of votes cast by citizens who were initially unregistered or misregistered and who registered in 2011.[17] Using the initial average number of unregistered and misregistered citizens per household, 0.92, as the denominator, we also compute and show the fraction of citizens who were initially misregistered or unregistered and voted. Their average participation increased from 13% in the control group to up to 19% in the group "Early Home registration and Late Home registration."

To examine the effect of each intervention on turnout at each electoral round, we estimate OLS

[16] Alternative interpretations seem less likely. The visits might have been complementary to the media campaign, whose intensity increased as the deadline came closer. However, the 2011 media and public information campaign on registration was concentrated in the very last days before the deadline, at a moment when most late visits had already been conducted. Alternatively, the visits might have been complementary to the saliency of the presidential elections, which increased over time. However, the presidential campaign did not start until January 2012, after the registration deadline: François Hollande held his first campaign meeting on January 22, and Nicolas Sarkozy on February 19.

[17] This is the number of votes cast by these citizens in their precinct: it does not take into account votes cast in their old precinct by citizens who stayed misregistered, which we do not observe.

specifications of the form in Equation [1], using the number of votes cast by newly registered citizens in each apartment $NV_{i,b}$ instead of $NR_{i,b}$ as the outcome. The results are presented in Table 2, columns 2 through 6. In the average control household, 0.148 citizens (16.1%) who were initially unregistered or misregistered voted at the first round of the presidential elections and 0.151 (16.4%) at the second round. 0.090 (9.8%) and 0.082 (8.9%) voted at the first and second rounds of the general elections. Averaging over all treatments, the visits increased participation among initially unregistered or misregistered citizens by 0.040 votes (4.3 percentage points, or 27%) and 0.038 votes (4.1 percentage points, or 25%) at the first and second rounds of the presidential elections, and by 0.016 (1.7 percentage points, or 18%) and 0.020 (2.2 percentage points, or 24%) at the first and second rounds of the general elections. These effects are statistically significant at the 1% level. Taking the average on all rounds, the effect was of 0.029 votes (3.2 percentage points, or 25%). The individual effects of the six interventions follow similar patterns as for voter registration. All interventions significantly increased voter turnout, except for the "Early Canvassing" visits.

1.5.3 Impact on the composition of the electorate

The finding that facilitating registration can dramatically increase turnout is particularly important in a context where abstention steadily increases and threatens the legitimacy of elected governments, and at a time when mobility, a major factor behind misregistration, is itself on the rise. Whether facilitated registration would also change electoral outcomes depends on the difference between the characteristics and political preferences of registered citizens and citizens who would register if the costs were lower. Anecdotally, in Montpellier, activists belonging to the right-wing party UMP started covering one precinct but interrupted their participation in the experiment halfway through because they got the impression that the people they were encouraging to register were not right-leaning. Let us now examine more systematically the effects of the interventions on the social makeup of the electoral rolls.

All respondents to the postelectoral survey were initially unregistered or misregistered. We first identify the variables which best predict registration among them and the extent to which their influence was affected by the visits. Formally, we estimate the following OLS model:

$$I_{i,b} = \alpha + \beta T_b + \sum_k \gamma_k Z^k_{i,b} + \sum_k \delta_k Z^k_{i,b} \times T_b + \epsilon_{i,b} \qquad (2)$$

where $I_{i,b}$ is a dummy equal to 1 if citizen i of building b is registered in his city and 0 otherwise and T_b is a dummy equal to 1 if her building was allocated to one of the treatment groups. The key coefficients of interest are the γ_k's and the δ_k's, which measure the effect of the characteristics $Z_{i,b}$ and of their interaction with the treatment dummy. Figure 6 shows the effect of any characteristic k in the control group (γ_k) and in the treatment groups ($\gamma_k+\delta_k$) and reports the statistical significance of the γ_k's and the δ_k's.

In the control group, all other things being equal, gender and marital status significantly predict registration: males and single persons are less likely to register. The resource model of political participation predicts that citizens with less time, civic skills, and money are less likely to participate (Brady, Verba and Schlozman 1995). In line with these predictions, we find that the likelihood to be registered is lower among the following groups of citizens: those who come back from work after the town hall's opening hours; those with no diploma or with less than an end-of-high-school diploma; those who speak another language than French or a combination of French and another language at home; poorest citizens and, perhaps surprisingly, richest citizens, compared with those with a monthly income between 1100 and 1500 euros. Finally, those who arrived in the city a short time ago are less likely to be registered, probably because the requirement to re-register after each move makes registration more costly for them (Squire, Wolfinger and Glass 1987). Some of these variables were also identified as strong determinants of registration by previous empirical studies (e.g., Pan Ké Shon 2004). The novelty is that we can assess the extent to which the influence of these variables was compensated by the visits. We find that males, uneducated citizens, citizens speaking a language other than French at home, citizens with a high monthly income, and citizens coming back from work after the town hall's opening hours were significantly more likely to register in the treatment groups than in the control group. We would expect some of the coefficients to be significant by random chance. We thus test the joint significance of the γ_k's and the joint significance of the δ_k's and reject both nulls with a p-value of 0.00 (Table 3).[18]

Next we turn to comparing the compliers and always-takers with previously registered citizens, using data from the voter rolls available for all groups. Since this data does not have any information

[18]Table 3 also reports results obtained when allowing the δ_k's to vary by treatment group. They are jointly significant in the door-to-door canvassing group and the two-visits group (p-values of 0.06 and 0.00) but not in the home registration group (p-value of 0.15). However, we fail to reject the nulls that the δ_k's are jointly equal in any two of the three groups. Finally, we test the robustness of these joint significance tests to the choice of the outcome variable. The results are robust to using registration anywhere or the standardized average of participation as the outcome, not registration at the current address (which excludes registration at another address in the city).

on unregistered citizens, we cannot estimate Equation [2]. Instead, we first restrict the sample to registered citizens in the control group, and regress a dummy equal to 1 if the citizen is newly registered and 0 if he was previously registered, on a set of selection characteristics. The results are shown in Table 4, column 1. We find that newly registered citizens are younger, are more likely to be born further away from the city where they live, and are more likely to be immigrants than previously registered citizens. Next we include all newly registered citizens in the sample and use T_b as the outcome (column 2). The compliers are less likely than the always-takers to be born in another region and they live at addresses where previously registered citizens have a lower turnout on average. This suggests that the interventions helped counterbalance a social environment otherwise relatively less conducive to political participation. However, compliers do not differ from always-takers on other dimensions, including age and being an immigrant.

Overall, these findings suggest that the self-initiated registration process disenfranchises some categories of citizens that are also more likely to face economic and social exclusion – the young, the uneducated, and immigrants – and that our visits fostered better representativeness of the citizenry in the electorate by increasing the number of registrations among these people. These citizens might have different political preferences as well: does the selection operated by the registration process affect electoral outcomes?

1.5.4 Impact on the preferences of the electorate

Unlike with administrative registration and participation data, we have to rely on people's self-reported choices of candidates at each round to measure their political preferences. The vote shares obtained by left-wing candidates were 67%, 74%, 69% and 75% at the two rounds of the presidential and general elections respectively, at the precinct of the average newly registered citizen. Yet, their own reported likelihood to vote for left-wing candidates was significantly higher: 83%, 90%, 91%, and 95% respectively. Compliers were equally likely to report voting for a left-wing candidate as always-takers. These findings are robust to excluding the precincts where visits were made by partisan canvassers. Nonetheless, there are several important caveats that one must bear in mind when considering these results. First, respondents' answers might be biased by social desirability bias and overreport for the winner. Second, in France, left-wing voters are known to be more inclined to take part in surveys than right-wing voters. This selection bias might affect the results of our survey as well. Third, the visits may have affected the preferences of the compliers and their

expressed vote. Indeed, existing research suggests that exogenous increases in political information can cause a relative shift in partisan opinion (Fowler and Margolis 2013).

As a complementary approach, we predict differences between the political preferences of the newly registered and the previously registered citizens and between the compliers and always-takers based on their demographics. Formally, we proceed in three steps. First, we regress the preferences expressed by the respondents to the postelectoral survey on three demographic characteristics available on the voter rolls for all registered citizens, as specified in Equation [3]:

$$\text{Left}_{i,b} = \alpha_1 + \alpha_2 \text{Gender}_{i,b} + \alpha_3 \text{Age}_i + \alpha_4 \text{Immigrant}_{i,b} + \epsilon_i \qquad (3)$$

where $\text{Left}_{i,b}$ is a dummy equal to 1 if the respondent located himself on the left of the left-right axis or had a preference for a left candidate (and 0 if he located himself on the right), $\text{Gender}_{i,b}$ is equal to 1 if the respondent is a male and $\text{Immigrant}_{i,b}$ is equal to 1 if the respondent is an immigrant. The results are presented in Table 5, Panel A. Age and being an immigrant are strong predictors of preference on the left, and have the expected sign.

Second, we use the estimated coefficients $\widehat{\alpha_1}$, $\widehat{\alpha_2}$, $\widehat{\alpha_3}$ and $\widehat{\alpha_4}$ to predict the political preferences of all registered citizens in the sample, $\widehat{\text{Left}_{i,b}}$.

Third, we estimate differences between the predicted political preferences of the newly registered and the previously registered citizens and between the compliers and always-takers. Formally, we estimate the following model:

$$\widehat{\text{Left}_{i,b}} = \alpha + \beta N_{i,b} + \delta T_b \times N_{i,b} + \epsilon_{i,b} \qquad (4)$$

where $N_{i,b}$ is a dummy equal to 1 if i is a newly registered citizen. Table 5, Panel B performs this analysis. We predict that newly registered citizens are 1.7 to 3.4 percentage points more likely to be on the left than those previously registered, except for the first round of the general elections but that there is no significant difference between the political preferences of newly registered citizens in the control and treatment groups. This suggests that the political preferences of compliers are similar to the always-takers but more to the left than previously registered citizens. This finding supports the view that, in the sample areas, the citizens disenfranchised by the registration process are ideologically more to the left than the median registered citizen.

1.6 Selection and engagement effects of the registration process

Increased participation and representation of the citizenry in the electorate are important democratic improvements. However, one might worry that a significant fraction of citizens are not sufficiently informed, so that increasing participation would lead to noisy electoral results and bad policies. In that respect, self-initiated registration might have two virtues. First, it might select more interested and knowledgeable citizens and exclude uninformed voters. In addition, self-initiated registration might have a positive treatment or "engagement" effect: citizens who make an effort to register might get more involved politically, increasing their electoral participation and perhaps how much thought they put in their actual vote. Several factors might underlie this engagement effect. Deciding to register is a way to state one's intention to vote, which might have a self-prediction effect analogous to asking people in advance if they intend to vote (e.g., Greenwald et al. 1987; Nickerson and Rogers 2010; but see Smith, Gerber, and Orlich 2003). But deciding to register is more than a simple statement: it is actually costly. People who have registered might choose to vote to repay the sunk cost of registration and justify the corresponding effort (Erikson 1981; Arkes and Blumer 1985). The effort made to register might also be used by the registrant to manage his self-concept as an engaged citizen (Bénabou and Tirole 2006). The registrant might then adjust his subsequent participation according to this (re-)affirmed identity. Finally, the self-determination theory provides substantial evidence that one's sense of autonomy when performing a given task (here, registration) affects one's intrinsic motivation to perform follow-up tasks (here, voting) (Ryan and Deci 2000).

We should expect the selection and treatment effects of self-initiated registration to be mirrored in selection and treatment effects of the visits. The selection effect is that the visits may have selected compliers who were less interested and knowledgeable and with a lower propensity to vote than the always-takers. As shown in the model of Section 5, we expect this effect to be larger for home registration than canvassing visits. The treatment effect is that the visits themselves may have affected participation, even for people who would have registered anyway. This impact may be negative: the home registration visits might have had a disengagement effect on those who registered at home. But this impact may also be positive: the visits might have had the traditional get-out-the-vote effect first established by Gerber and Green (2000) and they may have had an

empowerment effect: citizens induced to register and to vote by the interventions may have taken more interest in the campaign and in the electoral results.

This section estimates these different effects. The first subsection compares the participation rates of newly registered citizens in the treatment groups and in the control group, as any difference should reflect the combination of the treatment and selection effects of the visits. We find that the turnout of newly registered citizens was not significantly different in the control group versus the canvassing group, and that it was only slightly lower in the home registration group. The second subsection isolates the get-out-the-vote effect of the visits and finds that it was null. The third subsection tests whether home registration visits had a disengagement effect, and finds that they did not. The fourth and fifth subsections isolate the selection effect: they show that the interventions selected citizens who are slightly less likely to participate, and whose participation depends more on the saliency of the elections. Finally, the sixth subsection finds that the interventions had an empowerment effect: they increased political interest and informedness among citizens who were initially unregistered and misregistered.

1.6.1 Differences in participation rates

Figure 7 shows the participation rates of newly registered citizens in the control and treatment groups, as well as the national average and the participation of citizens who were previously registered (prior to 2011) and who live in the sample addresses.

As is evident from Figure 7, turnout was very high at the presidential elections overall, and much lower at the general elections. An overwhelming majority of the newly registered citizens participated in the 2012 elections. Newly registered citizens in the control and treatment groups were 4 to 17 percentage points more likely to participate than previously registered citizens at each electoral round. Their participation was higher than the national average at the presidential elections, and lower at the general elections. Finally, newly registered citizens in the treatment groups were almost equally likely to participate as those in the control group.

To investigate these differences more systematically, we estimate specifications of the form in Equation [5]:

$$V_{i,b} = \alpha + \beta M_{i,b} + \gamma N_{i,b} + \sum_{t=1}^{6} \delta_t T_b^t \times N_{i,b} + \epsilon_{i,b} \qquad (5)$$

where $V_{i,b}$, $M_{i,b}$ and $N_{i,b}$ are dummies equal to 1 if, respectively, i participated in the election, if she was previously registered but her name was not found on any mailbox and if she is a newly registered citizen. Previously registered citizens whose name was found on a mailbox in 2011, and who are thus presumed to be well-registered, are the omitted category.[19]

The results are shown in Table 6. M is negative, large, and significant at all rounds: previously registered citizens who now live elsewhere have lower turnout rates than those who likely have not moved. In addition, compared to the reference group, the participation of newly registered citizens is significantly higher at all rounds except the second round of the presidential elections. The difference between the participation of newly registered citizens in the treatment groups and in the control group is significant only for the second round of the presidential elections and for the first round of the general elections. Using the average individual participation as the outcome (column 5), we find an overall difference of 2.2 percentage points, significant at the 10% level. The difference is not significant in the canvassing group, but significant at the 5% level in the home registration group. Finally, the participation of newly registered citizens in the home registration group was significantly lower than in the canvassing group. The next subsections disentangle the different mechanisms which may have contributed to these differences.

1.6.2 Get-out-the-vote effect

We isolate the get-out-the-vote effect of the visits by considering citizens whose turnout could only have been affected by it: citizens who registered in 2011 but before the visits, or who were registered before 2011. We estimate Equation [6] on this sample:

$$V_{i,b} = \alpha + \sum_{t=1}^{6} \beta_t T_b^t + X_{i,b}^{'}\lambda + \sum_{s} \delta_b^s + \epsilon_{i,b} \qquad (6)$$

where $X_{i,b}$ includes age, gender, the number of previously registered citizens in the apartment, and the number of mailboxes in the building.

[19] We include neither strata fixed effects nor any control variable in this regression: to the extent that the impact of our interventions on registration varied across different strata and along these variables, they would capture part of the difference between the participation of newly registered citizens in the control and treatment groups. An alternative to estimating Equation [5] would be to infer the voter turnout of different groups by dividing the number of votes in these groups by the number of new registrations – specifically, by computing nonlinear combinations of the estimates derived from Equation [1] and shown in Table 2. However, this strategy would artificially decrease the precision of our estimates. Equation [5] makes a more effective use of our sample, as it uses one observation per registered citizen, instead of one observation per household.

Table 7 presents the results. As shown in Panel A, the interventions did not significantly affect the participation of citizens who had registered prior to the visits at any of the four rounds or their average participation. Panel B allows β_t to vary by category of citizens registered prior to the visits. On average, the visits did not affect the participation of any of the subgroups.

We conclude that the visits did not have any get-out-the-vote effect. This is perhaps not surprising, given that the visits were at least four months before the first round of the presidential elections: get-out-the-vote interventions have been found to have no significant effect on turnout when they take place more than three weeks before the election (Nickerson 2006).

1.6.3 Engagement effect of self-initiated registration

The home registration visits might have reduced the engagement effect of self-initiated registration. It is difficult to isolate this disengagement effect as any difference between the participation of citizens registered at home and at the town hall can also reflect a selection effect: citizens registered at home differ from those registered at the town hall on several dimensions. To control for the selection effect, our strategy, inspired from Karlan and Zinman (2009), was to encourage some citizens to register at the town hall during an early visit and surprise them by offering home registration in a later visit. By that time, we expected that the most motivated citizens would already have registered at the town hall: if home registration has a disengagement effect, they would be protected from it. But the less motivated citizens, still not registered, would accept to register at home so that the two visits combined would select the same citizens as if home registration had been offered from the start.

The treatment groups "Early Canvassing & Late Home registration" and "Early Home registration & Late Home registration" were designed to implement this strategy. We focus on apartments that opened their door during the late visit and were thus all offered home registration. Figure 8 shows the average number of new registrations made at home and at the town hall in these apartments at three stages: before the early visit, after the early visit, and after the late visit. Our strategy was successful. First, by the time of the registration deadline, the average number of new registrations was very close in the two groups, suggesting that newly registered citizens selected by the two interventions are identical. As an additional support for this claim, we successfully check that newly registered citizens in the two groups are identical for all observable characteristics: gender, age, being an immigrant, the initial number of names of citizens not registered found on the

mailbox corresponding to the apartment, and the number of mailboxes of the corresponding address (See Table A4 in the Appendix). Second, the number of home registrations was much higher in the group "Early Home registration & Late Home registration," where citizens were offered to register at home from the start. We can therefore attribute to the disengagement effect of home registration any difference between the number of votes cast by initially unregistered and misregistered citizens in the two groups.

We estimate the following model:

$$NV_{i,b} = \alpha + \beta T_b^{EH\&LH} + \epsilon_{i,b} \qquad (7)$$

where $T_b^{EH\&LH}$ is a dummy equal to 1 for apartments in the treatment group "Early Home registration & Late Home registration" and 0 in the group "Early Canvassing & Late Home registration." Table 8 presents the results. We first check that the number of new registrations does not differ significantly between the two groups (column 1) and that there is a statistically significant difference (at the 1% level) between the number of home registrations in both groups (column 2). Despite this difference, we cannot reject the null that the number of votes cast by initially unregistered and misregistered citizens is identical in both groups for any of the four electoral rounds and for their average (columns 3 through 7). In sum, we do not find any evidence of a disengagement effect of home registration on participation. In other words, asking citizens to take an action to register does not have a stronger engagement effect than offering them to register at home.

1.6.4 Selection of citizens only slightly less likely to vote...

It follows from the absence of get-out-the-vote and disengagement effects of the visits that turnout differences between newly registered citizens in the control and treatment groups shown in Table 6 result entirely from the difference between the propensity to vote of always-takers and compliers.

How can the latter be inferred from the former? Denote by $\overline{V_0}$ and $\overline{V_T}$ the average turnout of newly registered citizens in the control and in the treatment groups; by $\overline{V_A}$ and $\overline{V_C}$ the average turnout of always-takers and compliers; and by p_C the proportion of compliers among all newly registered citizens in the treatment groups. Then, $\overline{V_0} = \overline{V_A}$, $\overline{V_T} = \overline{V_A}(1 - p_C) + \overline{V_C}p_C$. This gives

$$\overline{V_A} - \overline{V_C} = \frac{1}{p_C}\left(\overline{V_0} - \overline{V_T}\right)$$

We first compute the difference between the propensity to vote of always-takers and the compliers selected by all treatment groups. From Table 2, column 1, we get $p_C = \frac{0.048}{0.168+0.048}$. Therefore, $\frac{1}{p_C} = \left(\frac{0.168+0.048}{0.048}\right) = 4.5$. In addition, from Table 6, column 5, we have that, averaging over the four electoral rounds, $\overline{V_0} - \overline{V_T} = 0.022$. We infer that $\overline{V_A} - \overline{V_C} = 4.5 \times 0.022 = 9.9$ percentage points. With the same method, we find that compliers' propensity to vote was 2.7 and 11.2 percentage points lower than always-takers at the first and second rounds of the presidential elections and 18.9 and 8.5 percentage points lower at the general elections. The large implied participation of the compliers is striking, especially at the presidential elections: 84.8% in the first round and 78.4% in the second.[20]

We now consider the propensity to vote of compliers selected by each intervention separately. As we see in column 5, on average, the propensity to vote of newly registered citizens was lower in all treatment groups, compared to the control group. However, this difference is significant neither in the group "Early Canvassing" nor in the group "Late Canvassing," and we fail to reject the null that, on average, compliers selected by a canvassing visit had the same propensity to vote as always-takers. On the contrary, the difference with the control group is significant in both the "Early Home registration" and "Late Home registration" groups. We infer from the estimated δ's that the propensity to vote of compliers selected by home registration visits was 16.7 percentage points lower than the always-takers, on average.[21]

1.6.5 ... but whose participation depends more on the saliency of the elections

If the citizens selected by the visits are less politicized, we should also expect their participation to depend relatively more on the saliency of the elections. The French 2012 electoral cycle was an

[20] A potential concern is that differences between the propensity to vote of compliers and always-takers might capture compositional effects. Remember that the compliers account for relatively more citizens who were initially unregistered than the always-takers. But citizens with different initial registration statuses might have different benefits of voting. For instance, compliers who were initially unregistered might be less interested in politics than those who were initially misregistered. To compare compliers and always-takers who share the same initial registration status, we allow the γ and the δ_t's to vary by initial registration status r in Equation [5]: $V_{i,b} = \alpha + \beta M_{i,b} + \sum_{r=1}^{4} \left(\gamma^r N_{i,b}^r + \sum_{t=1}^{6} \delta_t^r T_b^t \times N_{i,b}^r\right) + \epsilon_{i,b}$ (8), where $N_{i,b}^1$, $N_{i,b}^2$, $N_{i,b}^3$ and $N_{i,b}^4$ are dummies equal to 1 if i is newly registered and if she was, respectively, previously unregistered, registered in another city, registered at another address in the same city, or automatically registered. The results are presented in Table A5. On average, controlling for the initial registration status, the propensity to vote of newly registered citizens was 2.7 points lower in the treatment groups than in the control group. The difference with the estimate we obtain without controlling for initial registration status (2.2) is not statistically significant (p-value of 0.88).

[21] 0.167 is the product of the difference between the propensity to vote of always-takers and compliers selected by home registration averaged over the four rounds, 0.034 (Table 6, column 5) and $\frac{1}{p_C} = \left(\frac{0.168+0.043}{0.043}\right) = 4.9$ (Table 2, column 1).

ideal context to test this hypothesis: the general elections were of much lower salience than the presidential elections, and they were characterized by a turnout rate lower by 20 percentage points. We compare the percent decline in turnout between the high-salience presidential elections and the medium-salience general elections for the compliers and other registered citizens. Formally, we run seemingly unrelated regressions of Equation [5] using participation at each round as a different outcome, and we compute the point estimates and standard errors of non-linear combinations of the coefficients. Consider, for instance, the previously registered citizens whose name was found on a mailbox, who are the omitted category in Equation [5]. The percent decline in their turnout between the presidential and the general elections is $\frac{1/2(\alpha_{G1}+\alpha_{G2})-1/2(\alpha_{P1}+\alpha_{P2})}{1/2(\alpha_{P1}+\alpha_{P2})}$ where α_{P1}, α_{P2}, α_{G1} and α_{G2} are the estimated constants for each round. The results are presented in Table 9. Panel A estimates the turnout decline between the presidential and general elections among previously registered citizens whose name was found on a mailbox and among newly registered citizens in the control group. We find that the decline was significantly stronger among the latter (42.8% vs. 38.4%). Panel B shows the turnout decline among newly registered citizens in the control group and treatment groups. The decline was larger among newly registered citizens in the treatment groups (45.3% on average), but the difference is significant only for the home registration group. When we control for the initial registration status, we find that the turnout decline was larger by 3 percentage points among newly registered citizens in the treatment groups, a difference significant at the 10% level.[22]

These findings suggest that facilitating registration does select slightly less interested voters, whose participation depends more on the saliency of the elections, and that the propensity to vote of the marginal registrant decreases as registration is made easier. However, the selection effect of the visits is relatively small: these findings are hard to reconcile with a model in which the registration cost is the same for all citizens and where citizens who fail to register have much lower benefits of voting than others. On the contrary, using the theoretical insights from the model in Section 5, the high propensity to vote of the compliers suggests that both their benefits of voting and their registration costs are relatively high and that their failure to register, absent any visit, is mostly driven by registration costs that are too high.

[22]The detailed results by initial registration status are available in Table A7 of the Appendix. Based on these results, we compute the weighted average of the difference in turnout decline between the control and treatment groups across newly registered citizens with different initial registration statuses.

1.6.6 Empowerment effect

The concern that interventions which make voting less costly might disenfranchise uninformed voters, thus leading to noisy electoral results, roots in a vision in which political interest and competence are fixed. An alternative view is that citizens induced to vote may also become more interested in the campaign and in the elections. Being registered to vote might alter one's relationship to politics and electoral campaigns. To test this hypothesis and evaluate the impact of the visits on politicization, we group a series of 36 questions on political interest and competence asked during the postelectoral survey into a global index and 12 sub-indices, defined to be the equally weighted average of the z-scores of their components, following Katz, Kling, and Liebman (2007).[23]

As can be seen in Figure 9, the interventions increased the overall index of political interest and competence among citizens who were initially unregistered or misregistered by 0.06 standard deviations, an effect significant at the 5% level. The effect is of similar magnitude (0.6, 0.7 and 0.5 standard deviations) in the canvassing, home registration, and two-visits groups, and it is significant in the first two of these groups, at the 10 and 5% levels respectively (Table 10). The effect is positive for all but one of the 12 sub-indices, and it is significant for 4 of them: the ability to locate one's political preferences on the left-right axis; the ability to locate prominent local and national politicians on this axis; the ability to state the candidate one voted for or one would have voted for at each round; and the frequency of the political discussions held during the campaign with family members, friends, colleagues, and neighbors. These results suggest that the visits and the subsequent registrations increased both interest in the electoral campaigns and political competence, of which the command of the left-right axis is a key component (Gaxie 1978; Palfrey and Poole 1987; Powell 1989). On the contrary, the effects on political efficacy and on politicians' appraisal are very small and not significant. This is perhaps not too surprising, but it increases our confidence that the other positive effects we measure are not just the expression of gratitude or of a stronger desire to fulfill surveyors' expectations among those who received the visits.

As a result of these effects, the overall level of political interest and competence was similar at the time of the postelectoral survey between newly registered citizens in the control and treatment

[23]The z-scores are calculated by subtracting the mean among newly registered citizens in the control group and dividing by the standard deviation among them. Some turnout data was missing for a few registered citizens. Following Katz, Kling and Liebman (2007), if an individual's participation is known for at least one of the four rounds, then any missing values for the other rounds are imputed at the mean of the relevant group so that the estimates are the same as the average of those that would be obtained for the components of the index.

groups. To the extent that these findings do not solely reflect a direct effect of the discussions with the canvassers, they lend support to the view that inducing citizens to become active voters can increase their political interest and competence.

1.7 Conclusion and discussion

This project examined the effects of a series of canvassing and home registration interventions targeting unregistered and misregistered citizens in ten French cities. The experiment found that the self-initiated registration system excludes a large fraction of the citizenry which is otherwise prepared to vote. Lack of information and the cost of going through the administrative registration process are equally important impediments to registration, and they are reinforced by procrastination. These obstacles decrease registration and voting disproportionately for some segments of the population who are ideologically more to the left, including younger and less educated citizens, as well as immigrants. Self-initiated registration could theoretically serve to select more interested and competent voters, and to increase their political involvement. It is true that, compared to citizens registered due to the visits, those who register on their own are a little more likely to participate in the elections, and their participation depends less on the saliency of the election. Still, the most striking finding of our experiment resides in the fact that a large majority of compliers took part in the Spring 2012 elections, and more than four out of five of them participated in the presidential elections. Moreover, we do not find any evidence for a disengagement effect of home registration. Quite the contrary, the postelectoral survey brings suggestive evidence that citizens registered and induced to vote due to the interventions also became more interested in the campaign and in the elections than if they had remained unregistered.

Predicting the effects of changes in the registration rules

Any change in the registration rules might create a temporary information gap which, our results suggest, should not be underestimated. However, new rules could also contribute to facilitate the acquisition of information about registration. For instance, postponing the registration deadline to a few weeks before the elections, when electoral campaigns are most intense, would facilitate the transmission of information from political activists to unregistered citizens and could decrease procrastination. Registration rules which both increase information and decrease the cost to register,

should bring still greater effects.

Further down the line, can our results serve to anticipate the effects of moving away self-initiated registration towards an automatic registration procedure administered by the state? While our experiment does not enable us to outline the general equilibrium effects of switching to automatic registration, we can try and identify the direct effects of removing the registration cost. In automatic registration systems, the state can rely on different techniques to register voters (Sénat 2006; Brennan Center for Justice 2009). Door-to-door canvassing is one of these techniques, used for instance in Canada, South Africa and Indonesia. However, substitute techniques, including civil registry and data-sharing from tax authorities and other government agencies, are more frequent. Unlike door-to-door canvassing, these techniques do not involve any personal contact with the new registrants. Thus, they might have a different, and perhaps negative, treatment effect on the participation of registered citizens. The selection effect of these techniques, however, should be similar to the effect measured in this study: a sizable fraction of the electorate that is only slightly less likely to vote than citizens already registered would be brought in by the shift to universal registration.

In our experiment, the treatment group which offered home registration to the largest group of citizens was "Early Home registration & Late Home registration". We estimate that this intervention increased overall participation from 64.7 to 68.6% in the first round and 65.6 to 69.3% in the second round of the presidential election, and from 41.2 to 42.1% and 39.4 to 41.2% in the corresponding general elections.[24] These estimates are lower bounds of the increased turnout that would result from making registration universal. Were it universal, the unregistered or misregistered citizens who refused home registration would be registered too, and a fraction would vote. The data produced in the study does not enable us to estimate this fraction precisely, but there are reasons to believe that it would be relatively high. Indeed, the debriefing meetings we held with the canvassers found that only a slim minority of respondents who refused to register invoked the rejection of elections and voting as their motivation. Another factor may account for the fractions of citizens who refused to register at home: the trust people had to show toward the canvassers. Accepting the offer of home registration implied entrusting unknown canvassers with copies of electricity bills, ID cards or passports, and trusting them to file the registration application with the town hall prior to the

[24]To derive these estimates, we proceed in several steps, described in detail in Appendix 6. First we estimate increased participation among citizens initially unregistered and misregistered who live in apartments that opened their door to canvassers at least once. Then we account for the fact that a fraction of the citizens who stay misregistered at the end of the registration period participated in the elections by travelling back to their previous address or voting by proxy. Finally we factor in the participation of well-registered citizens.

December 31 deadline. Canvassing is much less developed in France than in the US (Pons 2013) and there is no tradition of voter registration drives. The respondents in our sample were thus offered a service that they were unfamiliar with. An automatic registration procedure led by the state would naturally not be confronted with such confidence issues.

Beyond enhancing participation, our findings suggest that implementing an automatic voter registration policy would likely increase the social and ethnic representativeness of the electoral rolls and of active voters. Would this transformation of the electorate alter election outcomes? At the level of our 44 precincts, the citizens disenfranchised by the registration process are ideologically more to the left than the median registered citizen. These results may be linked to the characteristics of the areas concerned. But in any event, election outcomes would be more in line with the true distribution of political opinions and orientations within the population on the whole.

Generalizability of the findings

To what extent do our results generalize to other countries with self-initiated registration? A recent experiment conducted in the US finds comparable impact of home registration visits on registration, but lower impact on turnout (Nickerson 2010). There are two complementary interpretations of these different findings. The first is that unregistered citizens in the US have lower benefits of voting than those in France. Indeed, in our study, the comparison between citizens registered as a result of canvassing visits and those registered through the more intensive home registration visits brings suggestive evidence that the propensity to vote of the marginal registrant decreases as the registration cost decreases. But the registration cost has substantially decreased in the US, following the 1993 National Voter Registration Act.

An alternative interpretation is that low-salience congressional and off-year gubernatorial elections account for the bulk of Nickseron's sample and that American elections are less salient than French elections, on average: participation at the US 2012 presidential elections was 58%, versus 74% for the French 2012 presidential elections. In our study, we find that the participation of citizens registered as a result of the visits depends more on the saliency of the elections than that of other citizens, which completes the argument.

The generalizability of the findings should be tested more directly by future research. To the extent that the results do generalize more broadly, they lend support to the view that the costs related to electoral participation remain one of the major causes of abstention. This view is some-

what counterintuitive: the cost of voting has steadily decreased in most countries since the 19th century, with the transition from censitary to universal suffrage, elimination of literacy tests and poll taxes, increased density of polling stations, and decreased travel cost (Garrigou 1992). As a result, researchers today show a (perhaps natural) tendency to analyze voter turnout trends and differences between different groups of citizens in terms of benefits rather than costs. An important reason why the cost to register has such an important effect might be that, differently from the cost of voting itself, each person pays it separately: whereas all citizens vote on the same day, only a small fraction of citizens have to register every year and they can register at different dates. This not only generates procrastination, it makes registration much less subject to social pressures than voting. The lessons might extend beyond voter registration. For instance, similarly to the registration process, voter ID laws only require a minority of citizens to take an action (those who do not have any ID yet), and this action can be done on many possible days. Voter ID laws might thus create similar distortions as the ones measured in this study.

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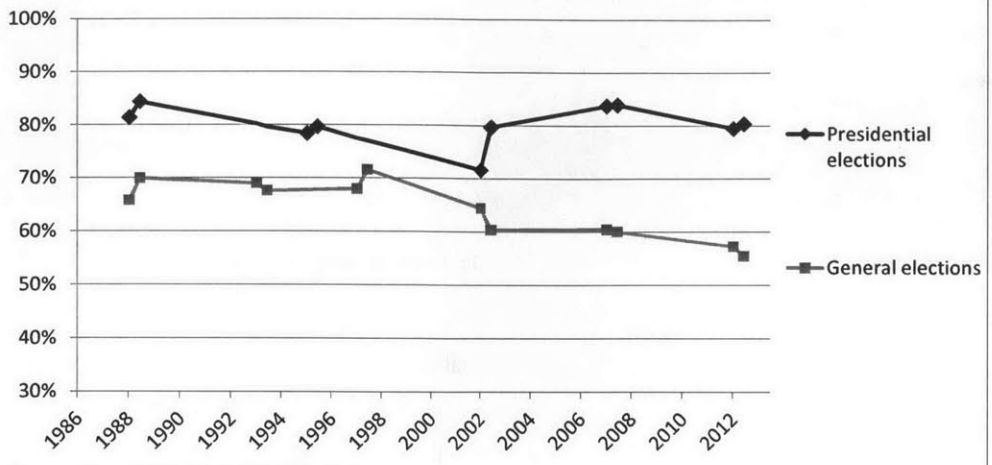
Figure 1. Turnout at French Presidential and General elections, 1988-2012



Source: French Ministry of the Interior
Notes: These official turnout figures use the number of registered citizens (not the number of eligible citizens) as the denominator.

Figure 2. Experimental design

		Early visit	Late visit
Control Group (1,026 addresses)			
Canvassing group	**Early Canvassing (515 addresses)**	Canvassing	
	Late Canvassing (515 addresses)		Canvassing
Home registration group	**Early Home registration (511 addresses)**	Home registration	
	Late Home registration (518 addresses)		Home registration
Two visits group	**Early Canvassing & Late Home registration (519 addresses)**	Canvassing	Home registration
	Early Home registration & Late Home registration (514 addresses)	Home registration	Home registration

Figure 3. Graphic representation of the different cases discussed in the model

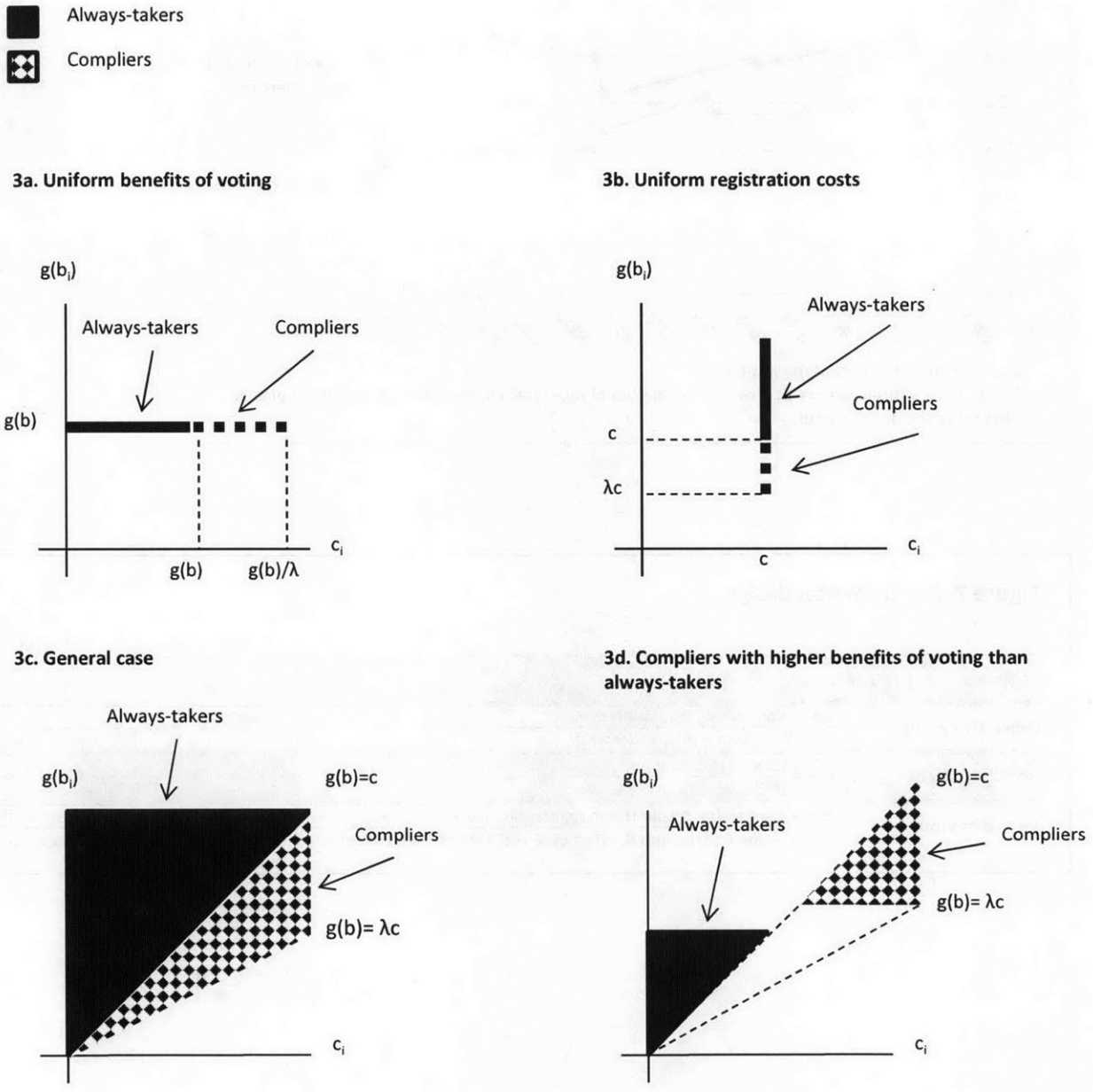
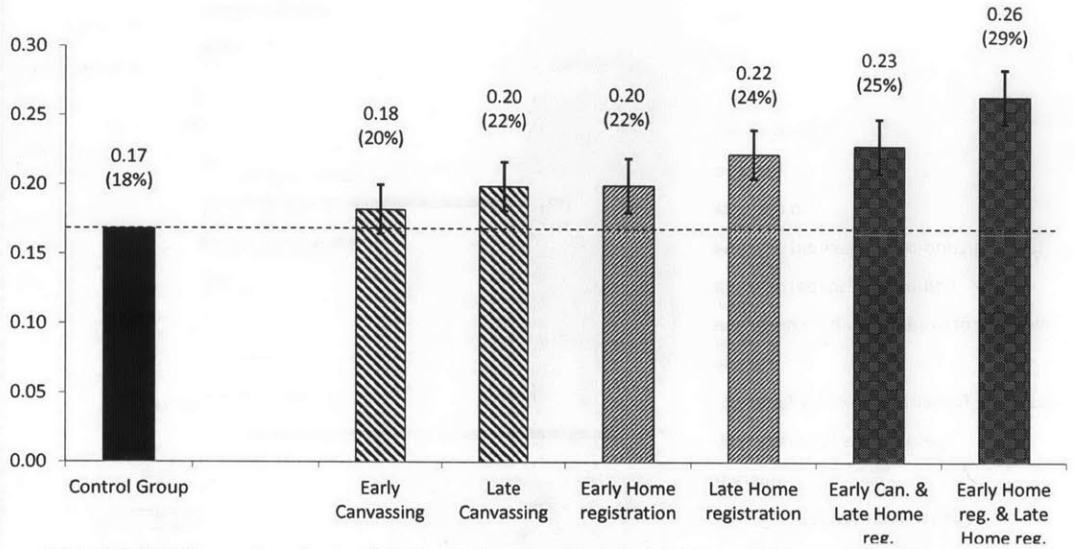
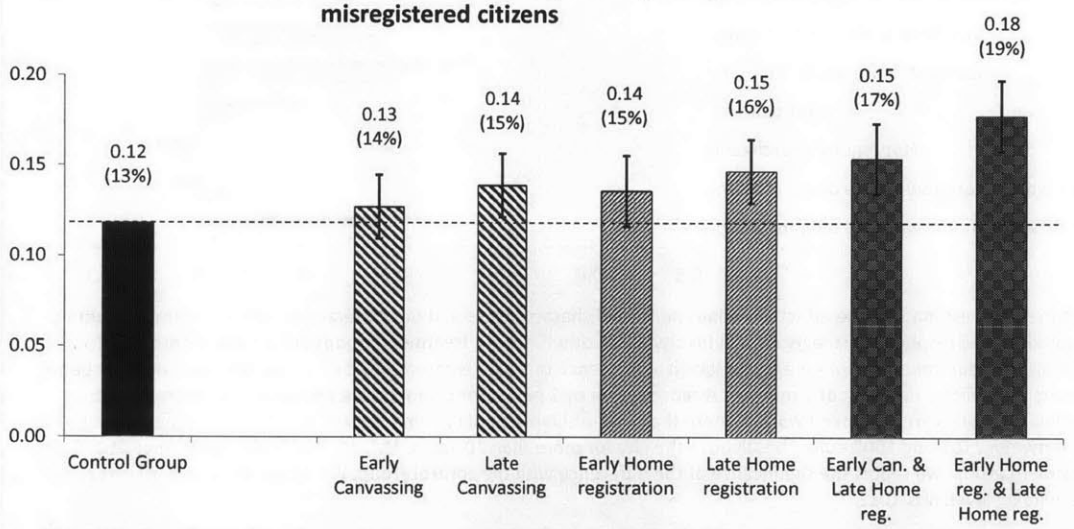


Figure 4. Impact on the number of new registrations among initially unregistered and misregistered citizens



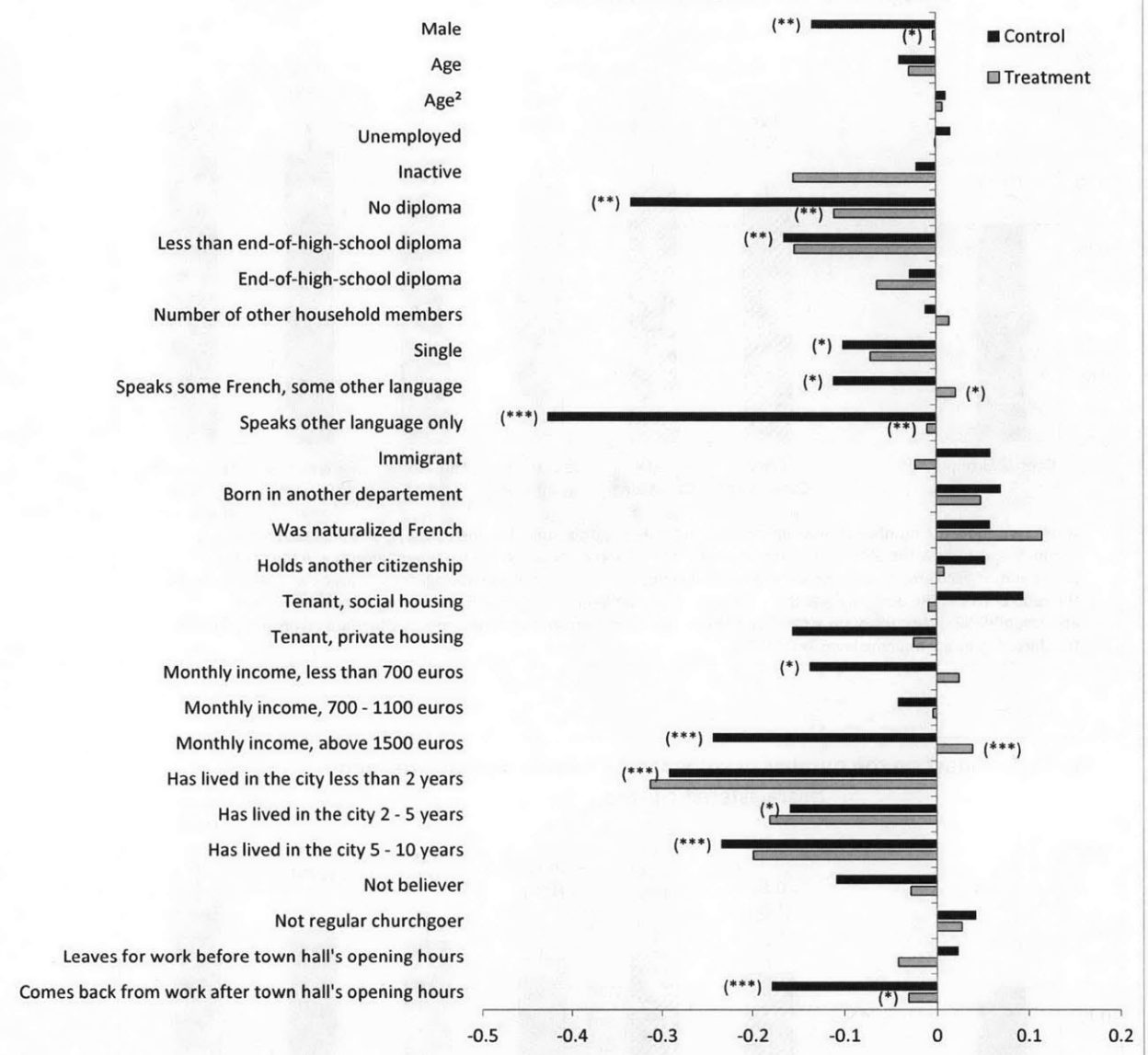
Notes: We show the number of new registrations in the average apartment of the control group and each treatment group. We also show the 95% confidence interval of the difference between the treatment groups and the control group and, in each group, we estimate the fraction of initially unregistered and misregistered citizens who registered, as the ratio between the outcome and the estimated initial number of unregistered and misregistered citizens per apartment (0.92). We control for strata fixed effects and apartment and building controls. Standard errors are adjusted for clustering at the building level. N is 20458.

Figure 5. Impact on the number of votes cast by initially unregistered and misregistered citizens



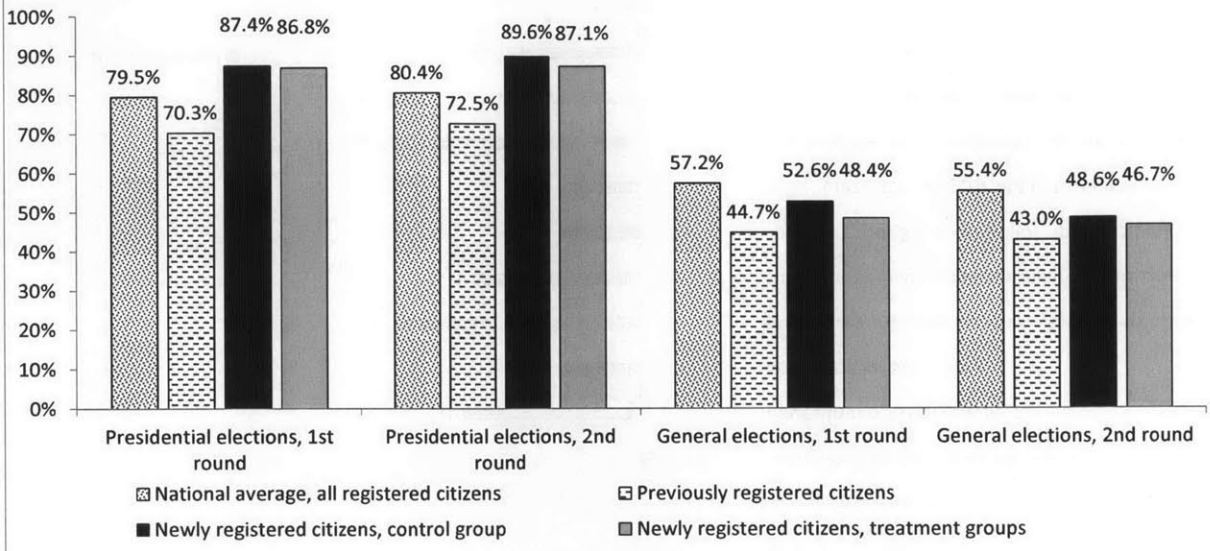
Notes: We show the number of votes cast on average at the four electoral rounds by initially unregistered and misregistered citizens in the average apartment of the control group and each treatment group. We also show the 95% confidence interval of the difference between the treatment groups and the control group and, in each group, we estimate the fraction of initially unregistered and misregistered citizens who voted, as the ratio between the outcome and the estimated initial number of unregistered and misregistered citizens per apartment (0.92). We control for strata fixed effects and apartment and building controls. Standard errors are adjusted for clustering at the building level. N is 20458.

Figure 6. Impact on the selection operated by the registration process



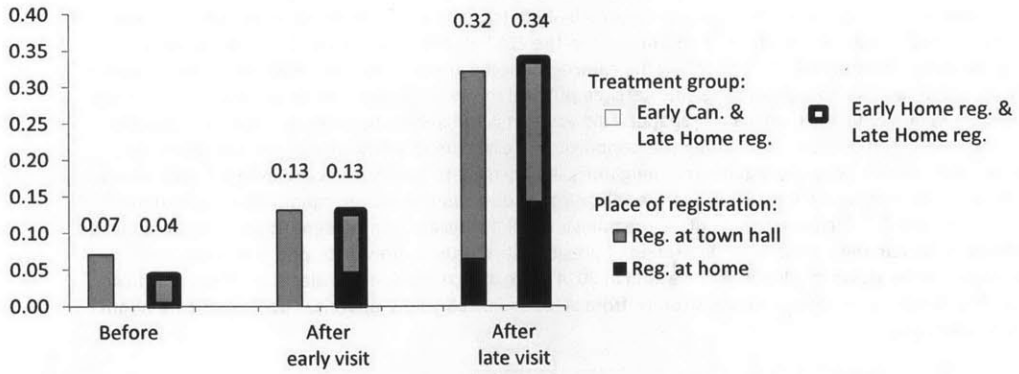
Notes: We report the point estimate of the effect of various individual characteristics and their interaction with a treatment dummy on a dummy equal to 1 if the respondant is registered in his city and 0 otherwise. All treatment groups are pooled together. All independent variables are dummies, except for age, for which a difference of 1 year is represented by 0.1 points, age² and number of household members, for which a different of 1 member is represented by 1 point. For characteristics measured by more than 1 dummy, the omitted categories are employed worker, more than end-of-high-school diploma, speaks only French, owner of his house, monthly income between 1100 and 1500 euros, has lived in the city for more than 10 years. ***, **, * indicate significance at 1, 5 and 10%. For the treatment group, we report the significance of the difference with the control group. We adjust the standard errors for clustering at the building level. *N* is 1012.

Figure 7. Electoral participation by registration status and treatment group



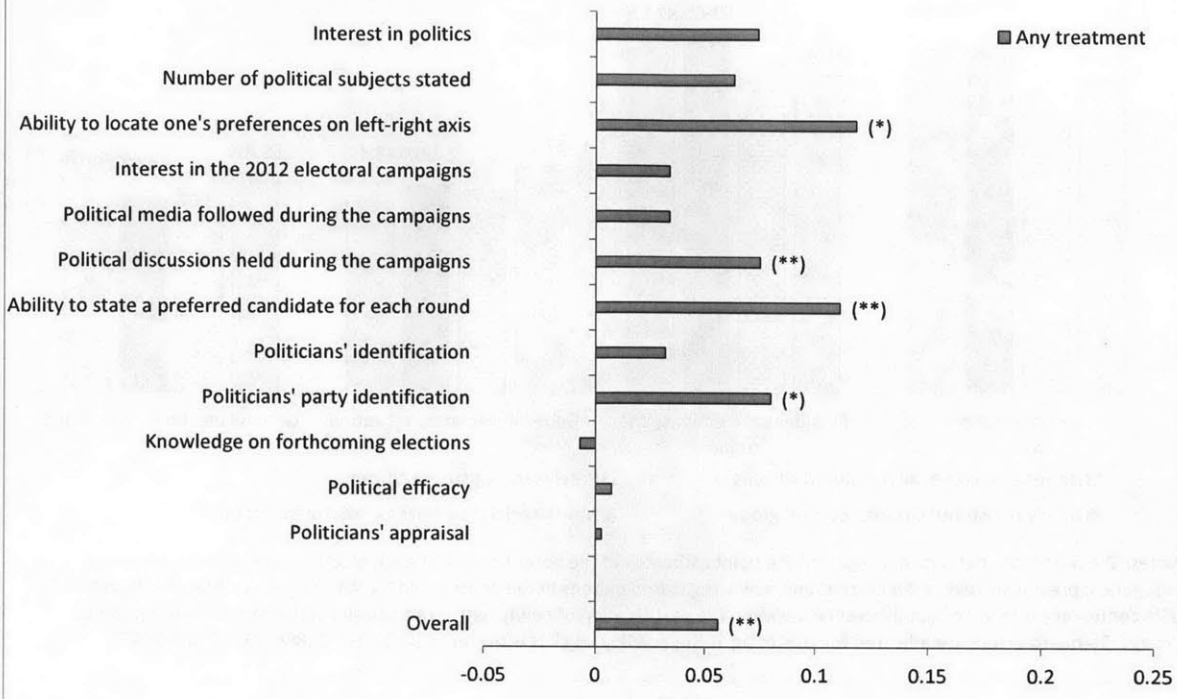
Notes: We report the national average and the point estimates of the voter turnout at each electoral round of the following categories: previously registered citizens and newly registered citizens in the control and treatment groups. We also show the 95% confidence interval of the difference between the participation of newly registered citizens in the control and treatment groups. Standard errors are adjusted for clustering at the building level. N is respectively 33897, 33896, 33912 and 33878.

Figure 8. Controlling for the selection effect of home registration



Notes: We show the average number of new registrations made at home and at the town hall in the apartments of the groups "Early Canvassing and Late Home registration" and "Early Home registration and Late Home registration" which opened their door for the late visit. The numbers of new registrations are shown before the early visit, after the early visit, and after the late visit and the registration deadline. N is 1399.

Figure 9. Impact on the level of politicization



Notes: All outcomes are summary indices defined to be the equally weighted average of z-scores of their components. For each outcome, we plot the point estimate of the difference between the control group and any treatment group. ***, **, * indicate significance at 1, 5 and 10%. We control for a series of individual characteristics and adjust the standard errors for clustering at the building level. *N* is 1219.

The indices are built based on the following variables. Interest in politics: how much are you interested in politics, how is your interest in politics evolving. Number of political subjects stated: number political subjects considered most important, number political subjects most important during the presidential campaign. Ability to locate one's preferences on the left-right axis: all positions except for doesn't know and neither left nor right. Interest in the 2012 electoral campaigns: how closely did you follow the presidential campaign, how closely did you follow the campaign for the general elections. Political media followed during the campaign: since last January how often have you watched political shows on TV, listened to political shows on the radio, read political articles in newspapers, in online newspapers, did you watch the debate between Hollande and Sarkozy between the two rounds. Political discussions held during the campaign: since last January how often have you discussed politics with your family, your friends, your colleagues, your neighbors. Ability to state a preferred candidate for each round: candidate he voted for or would have voted for. Politicians' identification: knows name of mayor, candidate arrived in third position at first round of presidential elections, president, prime minister, MP. Politicians's party identification: knows political party of mayor, candidate arrived in third position at first round of presidential elections, president, prime minister, MP. Knowledge on forthcoming elections: which elections to be held in 2014, date of next presidential elections. Political efficacy: can politics affect your life, likelihood to receive new assistance from state soon. Politicians' appraisal: politicians care about people like you, trust in politicians.

Table 1: Summary statistics

	Any treatment					Treatment groups included separately		
	Control group		Treatment groups		*P-value* Treatment	Test: joint significance of treatment dummies		Number of obs.
	Mean	SD	Mean	SD	= Control	Test statistic	*P-value*	
Panel A. At the address level								
Number of mailboxes	7.9	11.0	7.8	10.3	0.661	0.11	0.995	4118
Number of apartments included in sample	5.1	7.7	4.9	7.0	0.600	0.13	0.993	4118
Housing price	3103	871	3150	874	0.477	0.20	0.978	941
Panel B. At the apartment level								
Number of additional names on mailbox	1.3	0.7	1.3	0.7	0.213	0.58	0.747	20502
Panel C. At the individual level								
Age	36.3	13.6	36.3	13.0	0.978	2.60	0.017	1450
Gender	0.403	0.491	0.425	0.495	0.462	0.35	0.910	1464
In couple	0.543	0.499	0.523	0.500	0.508	1.57	0.152	1458
Number of other household members	1.9	1.6	2.0	1.7	0.695	1.10	0.362	1463
Education								
No diploma	0.146	0.354	0.146	0.354	0.994	0.47	0.832	1450
Less than end-of-high-school	0.278	0.449	0.278	0.448	0.994	2.68	0.014	1450
End-of-high-school	0.256	0.437	0.218	0.413	0.164	1.76	0.105	1450
More than end-of-high-school	0.320	0.467	0.357	0.479	0.205	1.37	0.226	1450
Activity								
Employed	0.623	0.485	0.615	0.487	0.806	2.47	0.022	1458
Unemployed	0.103	0.305	0.112	0.315	0.651	1.24	0.283	1458
Inactive	0.274	0.447	0.273	0.446	0.970	1.09	0.369	1458
Housing situation								
Owner	0.139	0.347	0.113	0.317	0.298	1.44	0.196	1440
Tenant, social housing	0.554	0.498	0.598	0.490	0.357	0.73	0.625	1440
Tenant, private housing	0.307	0.462	0.289	0.453	0.681	1.57	0.152	1440
Personal monthly income								
Less than 700 euros	0.225	0.418	0.197	0.398	0.313	1.21	0.297	1281
700 - 1100 euros	0.206	0.405	0.210	0.408	0.869	0.48	0.825	1281
1100 - 1500 euros	0.260	0.440	0.277	0.448	0.557	0.71	0.645	1281
Above 1500 euros	0.309	0.463	0.315	0.465	0.840	0.43	0.857	1281
Born in France	0.758	0.429	0.753	0.432	0.823	0.93	0.472	1455
Born in same department	0.246	0.431	0.232	0.422	0.609	2.13	0.048	1450
Was naturalized French	0.210	0.408	0.238	0.426	0.296	1.27	0.269	1393
Holds another citizenship	0.213	0.410	0.234	0.423	0.428	1.70	0.118	1404
Speaks French with family members								
French only	0.581	0.494	0.612	0.487	0.350	0.78	0.583	1457
Some French, some other language	0.404	0.491	0.371	0.483	0.302	0.70	0.650	1457
Other language only	0.014	0.118	0.017	0.130	0.672	0.64	0.699	1457
Has lived in the city								
For 2 years	0.168	0.374	0.185	0.389	0.488	1.33	0.243	1458
2 - 5 years	0.179	0.384	0.156	0.363	0.366	1.83	0.090	1458
5 - 10 years	0.156	0.364	0.157	0.364	0.970	1.18	0.315	1458
More than 10 years	0.497	0.501	0.501	0.500	0.919	1.19	0.312	1458
Adherent of a religion	0.667	0.472	0.687	0.464	0.538	1.88	0.082	1414
Regular churchgoer	0.355	0.479	0.323	0.468	0.331	0.55	0.772	1373

Notes : For each variable, we report the means and standard deviations in both the control group and in all treatment groups pooled together and indicate the p-value of the difference. We then take each treatment group separately, test the hypothesis of joint significance of the treatment dummies, and indicate the test statistic and its p-value.

The unit of observation is the address in Panel A, the apartment in Panel B, and the respondent to the post-electoral survey in Panel C. In Panels B and C, standard errors are adjusted for clustering at the address level.

Table 2: Impact on the number of new registrations and votes cast by the newly registered citizens

	(1)	(2)	(3)	(4)	(5)	(6)
	Number of new registrations	*Number of votes cast by initially unregistered and misregistered citizens*				
		Presidential elections		General elections		Average on all
		1st round	2nd round	1st round	2nd round	rounds
Early Canvassing (EC)	0.014	0.012	0.012	0.001	0.010	0.009
	(0.012)	(0.012)	(0.012)	(0.008)	(0.008)	(0.009)
Late Canvassing (LC)	0.031	0.028	0.024	0.016	0.015	0.021
	(0.012)**	(0.012)**	(0.012)**	(0.010)	(0.008)*	(0.009)**
Early Home registration (EH)	0.032	0.027	0.018	0.012	0.016	0.018
	(0.013)**	(0.013)**	(0.012)	(0.009)	(0.008)*	(0.010)*
Late Home registration (LH)	0.054	0.043	0.043	0.015	0.015	0.029
	(0.013)***	(0.012)***	(0.012)***	(0.008)*	(0.008)*	(0.009)***
Early Canvassing & Late Home registration (EC&LH)	0.060	0.047	0.048	0.024	0.024	0.036
	(0.013)***	(0.012)***	(0.012)***	(0.009)**	(0.008)***	(0.010)***
Early Home registration & Late Home registration (EH&LH)	0.096	0.084	0.083	0.030	0.043	0.060
	(0.014)***	(0.012)***	(0.013)***	(0.009)***	(0.009)***	(0.010)***
Strata fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Apartment & Building controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20458	20458	20458	20458	20458	20458
R-squared	0.03	0.03	0.03	0.02	0.02	0.03
Mean in Control Group	0.168	0.148	0.151	0.090	0.082	0.118
Linear combinations of estimates:						
Average effect of all interventions	0.048	0.040	0.038	0.016	0.020	0.029
1/6 (EC + LC + EH + LH + EC&LH + EH&LH)	(0.008)***	(0.008)***	(0.008)***	(0.006)***	(0.005)***	(0.006)***
Average effect of Canvassing	0.022	0.020	0.018	0.009	0.012	0.014
1/2 (EC + LC)	(0.010)**	(0.009)**	(0.009)*	(0.007)	(0.007)*	(0.008)*
Average effect of Home registration	0.043	0.035	0.030	0.013	0.016	0.023
1/2 (EH + LH)	(0.011)***	(0.010)***	(0.010)***	(0.007)*	(0.007)**	(0.008)***
Difference between average effect of Home reg. and Can.	0.021	0.015	0.013	0.005	0.003	0.009
1/2 (EH + LH) - 1/2 (EC + LC)	(0.011)*	(0.010)	(0.010)	(0.007)	(0.007)	(0.008)
Difference between average effect of Late visit and Early visit	0.020	0.016	0.019	0.009	0.002	0.011
1/2 (LH + LC) - 1/2 (EH + EC)	(0.011)*	(0.010)	(0.010)*	(0.007)	(0.007)	(0.008)

Notes : Clustered standard errors are in parentheses. ***, **, * indicate significance at 1, 5 and 10%. We take the apartment as the unit of observation and include all newly registered citizens in the sample apartments.

In Panel B, we estimate the impact of the interventions on the number of votes cast by citizens who were initially unregistered or misregistered for each electoral round separately (columns 2 through 5) and the average on all four rounds (column 6).

We finally report point estimates and standard errors of linear combinations of the coefficients.

Table 3: Impact on the selection operated by the registration process

Joint significativity of all selection variables interacted with...		(1) Registered in his city	(2) Registered (in his city or elsewhere)	(3) Registered at his address	(4) Votes
Panel A. Any treatment					
Constant	statistic	10.62	1.28	1.75	7.38
	p-value	0.000***	0.008***	0.010**	0.000***
Any treatment group	statistic	2.21	1.39	0.93	2.27
	p-value	0.000***	0.086*	0.575	0.000***
Observations		1012	1009	1012	1012
R-squared		0.18	0.11	0.09	0.12
Panel B. Treatment groups included separately					
Constant	statistic	9.98	1.67	1.65	6.93
	p-value	0.000***	0.017**	0.020**	0.000***
Door-to-door canvassing group	statistic	1.45	1.09	1.04	1.27
	p-value	0.064*	0.341	0.405	0.159
Home registration group	statistic	1.29	1.32	0.70	1.55
	p-value	0.146	0.128	0.873	0.035**
Two visits group	statistic	2.22	1.85	1.06	1.78
	p-value	0.000***	0.005***	0.384	0.008***
Home registration group -	statistic	0.53	1.18	0.95	0.72
Door-to-door canvassing group	p-value	0.979	0.246	0.539	0.855
Two visits group -	statistic	1.36	1.49	0.95	0.80
Home registration group	p-value	0.104	0.050*	0.545	0.764
Observations		1012	1009	1012	1012
R-squared		0.23	0.17	0.14	0.16

Notes: ***, **, * indicate significance at 1, 5 and 10%. The respondent to the post-electoral survey is the unit of observation.
We regress individual registration or participation on various individual characteristics and their interaction with treatment dummies. In Panel A, all treatment groups are pooled together and we report the joint significativity of all characteristics and of the characteristics interacted with a treatment dummy. In Panel B, treatment groups are included separately, and we report the joint significativity of all characteristics, of the characteristics interacted with three treatment dummies, and of the difference between characteristics interacted with two different treatment dummies.
We consider four outcomes: registration in the individual's city; registration in this or another city; registration at his address; standardized average of his participation at the four electoral rounds of 2012. The first and third outcomes are administrative data. The second and fourth are self-reported.
In columns 1, 3, 5 and 7, the characteristics are interacted with a dummy equal to 1 for any treatment group and 0 otherwise. In the other columns, the characteristics are interacted with three treatment dummies.

Table 4. Impact on the selection operated by the registration process - voter rolls

	(1) Newly registered vs. previously registered in control gr.	(2) Newly registered in treatment gr. vs in control gr.
Gender	0.003	-0.011
	(0.009)	(0.010)
Age	-0.137	0.030
	(0.016)***	(0.025)
Age^2	0.008	-0.004
	(0.001)***	(0.003)
Born in another city of the département	0.045	-0.008
	(0.018)**	(0.029)
Born in another département of the region	0.106	-0.042
	(0.018)***	(0.027)
Born in another region	0.215	-0.063
	(0.017)***	(0.022)***
Born abroad	0.202	-0.025
	(0.017)***	(0.023)
Voter turnout of previously registered in same addre:	0.053	-0.108
	(0.055)	(0.060)*
Constant	0.449	0.840
	(0.047)***	(0.057)***
Observations	5656	5138
R-squared	0.09	0.01

Notes : Clustered standard errors are in parentheses. ***, **, * indicate significance at 1, 5 and 10%. We take the registered citizen as the unit of observation. Column 1 includes all registered citizens in the control group and regresses a dummy equal to 1 if the citizen is newly registered and 0 if he was previously registered on the independent variables. Column 2 includes all newly registered citizens and regresses a dummy equal to 1 if the citizen is in the treatment group and 0 if he is in the control group on the independent variables.

Table 5: Impact on the political preferences selected by the registration process

	(1)	(2)	(3)	(4)	(5)
	Position on the left	Vote for left candidate			
		Presidential elections		General elections	
		1st round	2nd round	1st round	2nd round
Panel A. Determinants of left/right position and vote choice among respondents to the postelectoral survey					
Gender	-0.036	-0.005	0.013	-0.030	0.006
	(0.043)	(0.041)	(0.034)	(0.048)	(0.046)
Age	-0.002	-0.003	-0.003	0.001	-0.001
	(0.002)	(0.002)**	(0.002)**	(0.002)	(0.002)
Immigrant	0.151	0.109	0.084	0.155	0.158
	(0.038)***	(0.038)***	(0.032)***	(0.042)***	(0.041)***
Constant	0.845	0.893	0.951	0.747	0.864
	(0.060)***	(0.060)***	(0.054)***	(0.076)***	(0.086)***
Observations	424	421	415	249	197
R-squared	0.03	0.02	0.02	0.04	0.05
Panel B. Predicted position on the left and vote shares for the entire sample of registered citizens					
Newly registered x Any treatment	0.001	0.001	0.001	-0.002	0.001
	(0.003)	(0.003)	(0.002)	(0.005)	(0.005)
Newly registered	0.027	0.034	0.032	-0.005	0.017
	(0.003)***	(0.003)***	(0.002)***	(0.004)	(0.004)***
Constant	0.773	0.779	0.847	0.837	0.846
	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***
Observations	28083	20196	20792	12365	9782
R-squared	0.02	0.05	0.05	0.00	0.01

Notes: Clustered standard errors are in parentheses. ***, **, * indicate significance at 1, 5 and 10%.
In Panel A, the unit of analysis is the respondent to the post-electoral survey and the outcomes are reported left/right position and vote choice at each of the four rounds. Only respondents who are actually registered in their city are included in the sample and only citizens who voted are included in the sample for the regression of the corresponding electoral round. The outcomes are regressed on all variables available both for respondents to the postelectoral survey and for the entire sample: age, gender, immigrant.
Panel B uses the coefficients estimated in Panel A to predict the left/right position and vote choice of each registered citizen in the four cities included in the survey sample and compares the predicted position of different types of citizens. Only citizens who actually voted are included in the sample for the regression of the corresponding electoral round. For the second round of the general elections, we exclude the cities Saint-Denis and Sevran, in which only one (left-wing) candidate remained at the second round.

Table 6: Electoral participation of citizens by registration status and treatment group

	(1)	(2)	(3)	(4)	(5)
	Presidential elections		General elections		Average on
	1st round	2nd round	1st round	2nd round	all rounds
Newly registered x Early Canvassing (EC)	-0.009	-0.010	-0.026	0.009	-0.008
	(0.017)	(0.015)	(0.027)	(0.027)	(0.016)
Newly registered x Late Canvassing (LC)	-0.002	-0.024	-0.022	-0.008	-0.014
	(0.017)	(0.017)	(0.032)	(0.029)	(0.017)
Newly registered x Early Home registration (EH)	0.006	-0.058	-0.040	-0.024	-0.028
	(0.017)	(0.018)***	(0.027)	(0.025)	(0.016)*
Newly registered x Late Home registration (LH)	-0.011	-0.030	-0.065	-0.059	-0.040
	(0.018)	(0.017)*	(0.026)**	(0.027)**	(0.016)**
Newly registered x Early Can. & Late Home reg. (EC&LH)	-0.018	-0.013	-0.033	-0.025	-0.021
	(0.018)	(0.017)	(0.028)	(0.026)	(0.016)
Newly registered x Early Home reg. & Late Home reg. (EH&LH)	-0.002	-0.012	-0.060	-0.003	-0.019
	(0.016)	(0.015)	(0.027)**	(0.028)	(0.016)
Newly registered	0.111	0.114	0.041	0.020	0.071
	(0.011)***	(0.010)***	(0.018)**	(0.017)	(0.010)***
Previously registered, name not on mailbox	-0.184	-0.172	-0.117	-0.109	-0.145
	(0.007)***	(0.007)***	(0.008)***	(0.008)***	(0.006)***
Constant	0.764	0.782	0.485	0.466	0.624
	(0.004)***	(0.004)***	(0.005)***	(0.005)***	(0.004)***
Observations	33897	33896	33912	33878	33789
R-squared	0.05	0.05	0.01	0.01	0.04
Linear combinations of estimates:					
Av. difference between newly registered in treatment gr. and control	-0.006	-0.025	-0.041	-0.018	-0.022
1/6 (EC + LC + EH + LH + EC&LH + EH&LH)	(0.012)	(0.011)**	(0.019)**	(0.019)	(0.011)*
Av. difference between newly registered in Canvassing gr. and control	-0.006	-0.017	-0.024	0.000	-0.011
1/2 (EC + LC)	(0.014)	(0.013)	(0.024)	(0.023)	(0.014)
Av. difference between newly registered in Home registration gr. and contro	-0.003	-0.044	-0.053	-0.041	-0.034
1/2 (EH + LH)	(0.014)	(0.014)***	(0.023)**	(0.022)*	(0.013)**
Av. difference between newly registered in Home reg. gr. and Can. gr.	0.003	-0.027	-0.029	-0.042	-0.024
1/2 (EH + LH) - 1/2 (EC + LC)	(0.014)	(0.014)*	(0.022)	(0.021)**	(0.013)*

Notes: Clustered standard errors are in parentheses. ***, **, * indicate significance at 1, 5 and 10%. We take the individual participation at a given electoral round as the unit of observation and include all previously registered citizens (registered before 2011) and newly registered (registered in 2011). For the former, we control for whether the name was found on a mailbox at the corresponding address or not, as a proxy for the quality of registration (well- or mis-registered).

We estimate differences in the propensity to vote of previously and newly registered citizens, and newly registered citizens in the control and the treatment groups for each electoral round separately (columns 1 through 4) and for their average (column 5).

We report point estimates and standard errors of linear combinations of the coefficients.

Table 7: Impact of the visits on the participation of citizens registered prior to the visits

	(1)	(2)	(3)	(4)	(5)
	Presidential elections		General elections		Average on
	1st round	2nd round	1st round	2nd round	all rounds
Panel A. All citizens registered prior to the visits					
Any treatment	-0.013	-0.005	-0.006	0.000	-0.006
	(0.012)	(0.012)	(0.014)	(0.014)	(0.010)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes
Individual and Building controls	Yes	Yes	Yes	Yes	Yes
Observations	8367	8367	8401	8394	8349
R-squared	0.05	0.04	0.10	0.09	0.09
Mean in Control Group	0.733	0.752	0.472	0.452	0.602
Panel B. Groups of citizens registered prior to the visits included separately					
Any treatment x Registered before 2011, name on mailbox	-0.018	-0.002	-0.018	-0.018	-0.014
	(0.016)	(0.017)	(0.021)	(0.021)	(0.015)
Any treatment x Registered before 2011, name not on mailbox	-0.001	-0.006	0.008	0.023	0.006
	(0.021)	(0.020)	(0.021)	(0.021)	(0.017)
Any treatment x Registered in 2011 before the visits	-0.047	-0.025	-0.023	-0.019	-0.026
	(0.020)**	(0.019)	(0.043)	(0.039)	(0.022)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes
Individual and Building controls	Yes	Yes	Yes	Yes	Yes
Observations	8367	8367	8401	8394	8349
R-squared	0.75	0.77	0.53	0.51	0.76
Mean in Control Group, Registered before 2011, name on mailbox	0.765	0.770	0.484	0.473	0.623
Mean in Control Group, Registered before 2011, name not on mailbox	0.636	0.677	0.415	0.398	0.531
Mean in Control Group, Registered in 2011, before visit	0.952	0.948	0.628	0.567	0.772

Notes : Clustered standard errors are in parentheses. ***, **, * indicate significance at 1, 5 and 10%. We take the individual participation at a given electoral round as the unit of observation and include all citizens registered prior to the visits. Panel A pools all these citizens together. Panel B distinguishes between 4 categories: citizens registered before 2011 whose name was found on a mailbox; citizens registered prior to 2011 whose name was found on no mailbox (they have likely moved out); and citizens registered in 2011, but prior to our visit.
We estimate differences in the electoral participation of these citizens in the control group and all treatment groups pooled together for each round separately (columns 1 through 4) and for their standardized average (column 5).

Table 8: Treatment impact of home registration

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Number of new registrations		Number of votes cast by initially unregistered and misregistered citizens				
			Presidential elections		General elections		Average on all
	All	At home	1st round	2nd round	1st round	2nd round	rounds
Early Home registration & Late Home registration	0.017	0.074	0.021	0.028	-0.016	0.011	0.011
	(0.036)	(0.021)***	(0.033)	(0.035)	(0.024)	(0.025)	(0.026)
Constant	0.321	0.064	0.266	0.270	0.158	0.147	0.210
	(0.024)***	(0.011)***	(0.023)***	(0.023)***	(0.017)***	(0.016)***	(0.018)***
Observations	1399	1399	1399	1399	1399	1399	1399
R-squared	0.00	0.01	0.00	0.00	0.00	0.00	0.00

Notes : Clustered standard errors are in parentheses. ***, **, * indicate significance at 1, 5 and 10%. We take the apartment as the unit of observation and include all newly registered citizens living in apartments which opened their door at the second visit, in the treatment groups "Early Canvassing & Late Home registration" and "Early Home registration & Late Home registration". The omitted group is "Early Canvassing & Late Home registration".

Table 9: Percent decline in turnout between the presidential and general elections, by registration status and treatment group

Panel A. Comparison between newly registered citizens and previously registered citizens

	(1)
Previously reg. citizens, all groups	-0.384
	(0.005)***
Newly reg. citizens, control group	-0.428
	(0.016)***
Difference between newly reg. citizens and previously reg. citizens	-0.044
	(0.016)***

Panel B. Comparison between newly registered citizens in the treatment groups and in the control group

	(1)	(2)	(3)
	All treatment gr.	Canvassing gr.	Home registration gr.
Newly reg. citizens, treatment groups	-0.453	-0.434	-0.467
	(0.008)***	(0.016)***	(0.013)***
Difference between newly reg. citizens in treatment groups and control group	-0.025	-0.006	-0.039
	(0.018)	(0.022)	(0.021)*
Difference between newly reg. citizens in treatment groups and control group, controlling for initial registration status	-0.030	-0.011	-0.042
	(0.018)*	(0.023)	(0.021)**

Notes: ***, **, * indicate significance at 1, 5 and 10%. We report the point estimates and standard errors of non-linear combinations of coefficients obtained after running seemingly unrelated regressions of equation [5].

As an example of how to read the table, the coefficients in Panel A mean that the participation of previously registered citizens whose name was found on a mailbox declined by 38.4% between the presidential and general elections. Newly registered citizens in the control group experienced a decline of 42.8%, 4.4 percentage points stronger than the previously registered. In Panel B, the last line reports the weighted average of the difference between participation decline for newly registered citizens with different initial registration status in the treatment and control groups.

Table 10: Impact of the interventions on level of politicization

	(1)
Early Canvassing (EC)	0.021
	(0.041)
Late Canvassing (LC)	0.090
	(0.035)**
Early Home registration (EH)	0.095
	(0.038)**
Late Home registration (LH)	0.036
	(0.036)
Early Canvassing & Late Home registration (EC&LH)	0.046
	(0.037)
Early Home registration & Late Home registration (EH&LH)	0.044
	(0.038)
Individual controls	Yes
Observations	1219
R-squared	0.18
Linear combinations of estimates:	
Av. difference between newly registered in treatment gr. and control	0.055
1/6 (EC + LC + EH + LH + EC&LH + EH&LH)	(0.025)**
Av. difference between newly registered in Canvassing gr. and control	0.056
1/2 (EC + LC)	(0.031)*
Av. difference between newly registered in Home registration gr. and control	0.065
1/2 (EH + LH)	(0.030)**
Av. difference between newly registered in Two visits gr. and control	0.045
1/2 (EC&LH + EH&LH)	(0.030)

Notes : Clustered standard errors are in parentheses. ***, **, * indicate significance at 1, 5 and 10%. The respondent to the post-electoral survey is the unit of observation. The outcome is the standardized average of 36 indicators of level of politicization.
We report point estimates and standard errors of linear combinations of the coefficients.

1.9 Appendix

1.9.1 Example of leaflets handed out by the canvassers

ÉLECTION PRÉSIDENTIELLE 2012 / INSCRIPTIONS SUR LES LISTES ÉLECTORALES

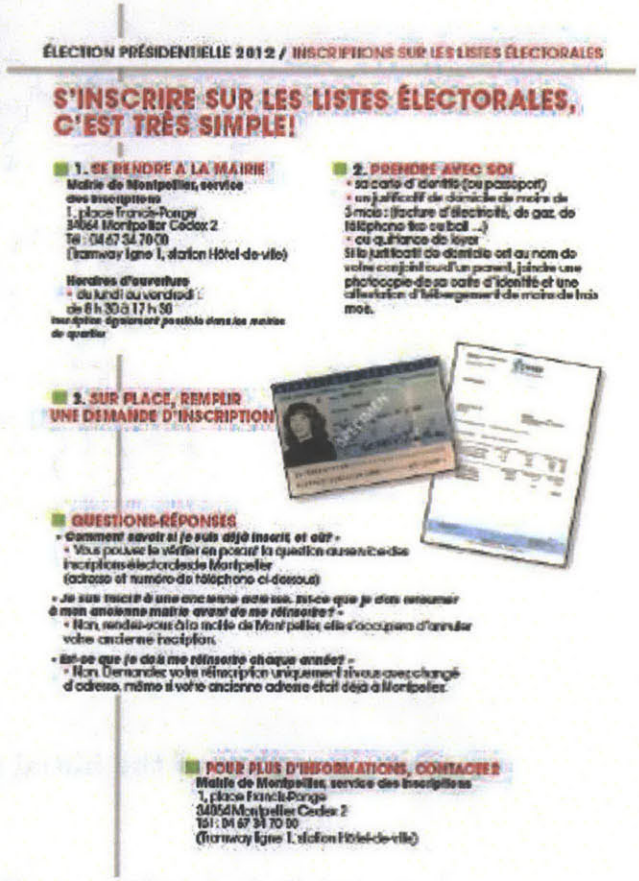
Etes-vous inscrit sur les listes électorales ?

Vous avez déménagé, êtes-vous réinscrit ?

POUR POUVOIR VOTER À LA PRÉSIDENTIELLE EN 2012, INSCRIVEZ-VOUS À LA MAIRIE DÈS AUJOURD'HUI !

- Seuls les Français inscrits sur les listes électorales pourront voter en 2012
- Si vous êtes encore inscrit à votre ancienne adresse, demandez votre réinscription pour voter près de chez vous

UM1 Les étudiants de l'université de Montpellier 1 se mobilisent pour l'inscription en nombre des habitants de Montpellier sur les listes électorales.



ÉLECTION PRÉSIDENTIELLE 2012 / INSCRIPTIONS SUR LES LISTES ÉLECTORALES

S'INSCRIRE SUR LES LISTES ÉLECTORALES, C'EST TRÈS SIMPLE!

1. SE RENDRE À LA MAIRIE
Mairie de Montpellier, service des inscriptions
1, place Francis-Ponge
34064 Montpellier Cedex 2
Tél : 04 67 34 70 00
(tramway ligne 1, station Hôtel-de-ville)

Horaires d'ouverture
• du lundi au vendredi :
de 8 h 30 à 17 h 30
Inscription également possible dans les mairies de quartier

2. PRENDRE AVEC SOI
• sa carte d'identité (ou passport)
• un justificatif de domicile de moins de 3 mois : (facture d'électricité, de gaz, de téléphone fixe ou bail ...)
• ou quittance de loyer
Si le justificatif de domicile est au nom de votre conjoint ou d'un parent, joindre une photocopie de sa carte d'identité et une attestation d'hébergement de moins de trois mois.

3. SUR PLACE, REMPLIR UNE DEMANDE D'INSCRIPTION

QUESTIONS-RÉPONSES
• *Comment savoir si je suis déjà inscrit et où ?*
• Vous pouvez le vérifier en posant la question aux services des inscriptions électorales de Montpellier (adresse et numéro de téléphone ci-dessous)
• *Je suis inscrit à une ancienne adresse. Est-ce que je dois retourner à mon ancienne mairie avant de me réinscrire ?*
• Non, rendez-vous à la mairie de Montpellier, elle s'occupera d'annuler votre ancienne inscription.
• *Est-ce que je dois me réinscrire chaque année ?*
• Non. Demandez votre réinscription uniquement si vous avez changé d'adresse, même si votre ancienne adresse était déjà à Montpellier.

POUR PLUS D'INFORMATIONS, CONTACTEZ
Mairie de Montpellier, service des inscriptions
1, place Francis-Ponge
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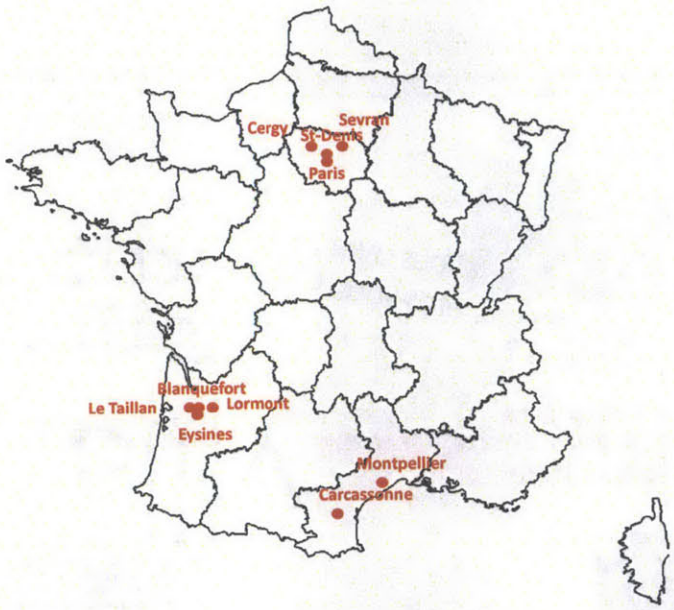
1.9.2 Localization of the 10 cities included in the experiment



1.9.3 Estimate of the initial numbers of unregistered and misregistered citizens

We estimate the initial numbers of unregistered and misregistered citizens based on the information collected by the canvassers during the door-to-door visits. For each apartment that opened its door, the canvassers reported the number of well-registered, misregistered and unregistered citizens, as well as the number of foreigners.

We first focus on addresses for which it was possible to link apartments to mailboxes. In this group of addresses, we were able to collect monitoring sheets for addresses accounting for 89 percent of all apartements. The canvassers were able to identify the number of well-registered citizens, misregistered citizens, unregistered citizens, and foreigners in 67 percent of these apartments.

We address three issues when examining this data. First, the apartments that opened their

door might systematically differ from those that remained closed. Second, some citizens who were not on the 2011 voter rolls had already registered by the time of the visits. Third, the information recorded by the canvassers might be subject to systematic bias.

1st caveat : Non-representativeness of apartments which opened their door

Apartments with relatively more individuals may be more likely to open their door. Failing to take this into account would result in overestimating the size of the average household. More generally, any systematic difference between apartments which opened their door and the others might introduce biases when estimating the number of citizens of each type. We address this difficulty by comparing the household composition of apartments which opened their door twice vs. only once in addresses that were targeted for two visits.

The strategy is as follows. Whether the apartment hosts at least one unregistered citizen or not can be described by a dummy. Similarly, whether it hosts exactly one, two, three or more unregistered citizens can be described by a series of dummies. The same of course is true for well-registered citizens, misregistered citizens, and foreigners.

For any dummy, denote by N_i and f_i the (unobserved) number and proportion of apartments for which the dummy is equal to $i = (0 \text{ or } 1)$. Denote p_i^1, p_i^2, $p_i^{1\&2}$ and ρ_i to be the (unobserved) probabilities that an apartment of type i opens its door in phase 1 (early visits), in phase 2 (late visits), or in both phases and the correlation between the two Bernoulli random variables describing door-opening in phase 1 and 2. We assume that $p_i^1 = p_i^2$ and that $\rho_1 = \rho_0 = \rho$: the correlation between the two Bernoulli variables is the same for apartments of type 0 and of type 1. Denote N, X_i^1 and $X_i^{1\&2}$ to be the (observed) total number of apartments and the numbers of apartments of type i which opened their door in phase 1 or in both phases.

This gives us a system of 4 equations in 5 unknowns:

$$\begin{cases} X_0^1 = N_0 p_0^1 & (1) \\ X_1^1 = (N - N_0)\, p_1^1 & (2) \\ X_0^{1\&2} = N_0 p_0^{1\&2} & (3) \\ X_1^{1\&2} = (N - N_0)\, p_1^{1\&2} & (4) \end{cases}$$

But $p_i^{1\&2}$ can be written as a function of p_i^1 and ρ for $i = (0 \text{ or } 1)$: $p_i^{1\&2} = \left(p_i^1\right)^2 + \rho \times p_i^1 \left(1 - p_i^1\right)$. This reduces the number of unknowns to 4 and makes the system solvable.

We find that N_0 must be a solution to the following equation:

$$\begin{aligned} & (N_0)^2 \left[X_1^1 X_0^{1\&2} - X_0^1 X_1^{1\&2}\right] && = 0 \\ + \quad & N_0 \left[N\left(X_0^1 X_1^{1\&2} - X_1^1 X_0^{1\&2}\right) - \left(X_0^1\right)^2 \left(X_1^1 - X_1^{1\&2}\right) - \left(X_1^1\right)^2 \left(X_0^1 - X_0^{1\&2}\right)\right] \\ + \quad & \left[N\left(X_0^1\right)^2 \left(X_1^1 - X_1^{1\&2}\right)\right] \end{aligned}$$

This admits two possible roots. N_0 is equal to the root between 0 and N.

From N_0 we immediately infer $N_1 \equiv N - N_0$ and $f_1 \equiv \frac{N_1}{N_1+N_0}$[25]

. We repeat this exercise for any dummy, and infer that, according to the information recorded by the canvassers, the average sample apartment included 0.58 well-registered citizens, 0.30 misregistered citizens, 0.27 unregistered citizens and 0.48 foreigners, for a total of 1.62 adult members.

2nd caveat: Taking into account the timing of the visits

Some of the citizens identified as well-registered by the canvassers had registered between January 1st 2011 and the visits. Since we know the date of the visits as well as the registration date, we can precisely estimate the importance of this phenomenon. We find that, on average, 0.03 citizens who were initially misregistered and 0.02 citizens who were initially unregistered had registered by the time of the visits.

Combined with the information recorded by the canvassers, this means that, as of January 2011, apartments in the experimental sample included 0.53 well-registered citizens, 0.33 misregistered citizens, 0.29 unregistered citizens and 0.48 foreigners.

3rd caveat: Systematic biases in the information recorded by canvassers

The information recorded by canvassers might be systematically biased. Many citizens are confused about their registration status. In particular, many citizens do not know that registration is address-specific and thus claim that they are well-registered even though they moved away without updating their registration status. The context of the visits did not enable the canvassers to ask more questions than what was natural to advise the respondents on what course of action to take. Moreover, the reemphasized civic norm of participation might have resulted in some respondents consciously lying about their registration status, to avoid being perceived as deviants.

[25] f_1 can be estimated using this method only if $X_0^{1\&2} \neq 0$, $X_1^{1\&2} \neq 0$ and $(X_1^1 \neq 0$ or $X_0^1 \neq 0)$. We use the following approximations for characteristics for which these conditions are not satisfied: $f_1 \sim 1$ if $X_0^{1\&2} = 0$; $f_1 \sim 0$ if $X_1^{1\&2} = 0$; $f_1 \sim 0$ if $X_0^{1\&2} \neq 0$ and $X_1^{1\&2} \neq 0$ but $X_1^1 = 0$; $f_1 \sim 1$ if $X_0^{1\&2} \neq 0$ and $X_1^{1\&2} \neq 0$ but $X_0^1 = 0$.

Altogether, we expect the misidentification of misregistered citizens as registered citizens to be the by far largest bias. Fortunately, there is a more reliable method to estimate the number of well-registered citizens. We count the number of citizens listed on the 2011 voter rolls and whose name was found on the mailbox of an apartment in the experimental sample and we add the number of citizens who were automatically registered as they turned 18[26]. On average, we find that experimental sample apartments host 0.23 well-registered citizens. We infer that the extra 0.30 (0.53 - 0.23) are actually misregistered citizens.

In the end, our preferred estimate is that initially, experimental sample apartments included 0.23 well-registered citizens, 0.63 misregistered citizens, 0.29 unregistered citizens and 0.48 foreigners.

The composition of the total sample population

While the estimates above were derived for apartments located in addresses in which it was possible to link apartments to mailboxes, we assume that the number of initially misregistered citizens and unregistered citizens was similar in apartments located in the other addresses. Adding the citizens who were automatically registered as they turned 18, we estimate that apartments in the experimental sample hosted 12,878 misregistered citizens and 5,977 unregistered citizens. In addition, 4,422, 15,371 and 4,434 well-registered citizens were hosted respectively by the experimental sample apartments, by non-experimental sample apartments located in the experimental sample addresses and by addresses located in the initial sample but not included in the experimental sample because no additional name was found on the mailboxes. In sum, in the areas covered by this study, we estimate that there were initially 24,227 (56.2 percent) well-registered citizens, 12,878 (29.9 percent) misregistered citizens and 5,977 (13.9 percent) unregistered citizens.

Fraction of citizens initially unregistered and misregistered who live in apartments that opened their door to canvassers at least once

Based on canvassers' reports, the average apartments targeted for two visits which opened its door at least once hosts 0.37 well-registered citizens, 0.19 misregistered citizens, 0.17 unregistered citizens and 0.33 foreigners. After applying the same transformations as above, we estimate that

[26]This sum might slightly differ from the actual number of well-registered citizens for two different reasons, with opposite sign. First, this method identifies as well-registered citizens people who have actually moved out but whose name is still on the apartment's mailbox, because a relative still lives there, for instance. Second, this method fails to identify as well-registered citizens people who live in the apartment, but whose name is not listed on the mailbox.

69.9 percent of citizens initially unregistered or misregistered live in apartments which opened their door to canvassers.

1.9.4 Sampling frame of the postelectoral survey

The postelectoral survey was administered between June 18, the day following the second round of the general elections, and July 15. All surveyors were students in political science, economics, social sciences, or law. To facilitate the coordination of the surveyors, the survey took place in only four cities, Saint-Denis, Cergy, Sevran and Montpellier, which account for 84 percent of the entire sample.

The survey was administered only to French citizens who were not registered at their address as of January 2011. For this purpose, for each address, surveyors were given a list of names of individuals that they should NOT survey: citizens who were registered on the 2011 voter rolls and citizens who were automatically registered in 2011. After introducing themselves and explaining the purpose of their visit, the surveyors asked the person who had openened the door whether he was a French citizen. If yes, they asked him whether he accepted to respond, wrote down his first and last name and rapidly checked that he was not listed on their list. If not, they went on administering the questionnaire. If their interlocutor was not French, not willing to answer, or if his name appeared on the list, they asked whether they could survey another member of the household. Surveyors were instructed to survey no more than one person in each apartment.

The surveyors did not know the treatment condition of the buildings where they conducted surveys. Still, we could not exclude ex ante that the response rate might be different in the control and treatment groups. Therefore, half of the addresses were randomly selected to be covered twice: in these addresses, surveyors knocked again at all doors that had remained closed the first time. Since we do not find any statistically significant difference between the answer rates in the control and in the treatment groups, we do not exploit this feature when analyzing the data.

Finally, administering the questionnaire required 15 to 20 minutes on average. Only 2 percent of respondents who started answering the questionnaire refused to go to the end.

1.9.5 Proofs of claims stated in Section 4

To prove the claims of Section 4, we use the following definitions and theorems.

First-order stochastic dominance

The distribution function F first-order stochastically dominates G if, for every weakly increasing $z : \mathbb{R} \to \mathbb{R}$, $\int_{-\infty}^{\infty} z(b)f(b)db \geq \int_{-\infty}^{\infty} z(b)g(b)db$, where f and g are the density functions corresponding to F and G.

Monotone Likelihood Ratio dominance

F dominates G in the Monotone Likelihood Ratio sense if $l(b) \equiv \frac{g(b)}{f(b)}$ is weakly decreasing.

Increasing differences

$f(b,c)$ satisfies log-increasing differences in b and c iff $\frac{f(b',c')}{f(b',c)} < \frac{f(b,c')}{f(b,c)}$ for any $b' > b$ and $c' > c$.

This condition is verified by any bivariate normal density with negative correlation:

Let's consider any bivariate normal density $f(b,c)$ with correlation $\rho < 0$. The bivariate density is fully characterized by ρ, μ_b, μ_c, σ_b and σ_c:

$$f(b,c) = \left(2\pi\sigma_b\sigma_c\sqrt{1-\rho^2}\right)^{-1} \times \exp\left(-\frac{1}{2(1-\rho^2)}\left(\left(\frac{b-\mu_b}{\sigma_b}\right)^2 + \left(\frac{c-\mu_c}{\sigma_c}\right)^2 - 2\rho\left(\frac{b-\mu_b}{\sigma_b}\right)\left(\frac{c-\mu_c}{\sigma_c}\right)\right)\right)$$

for any $b \in]-\infty,\infty[$ and $c \in]-\infty,\infty[$.

Now take any b and any $c' > c$.

$$\frac{f(b,c')}{f(b,c)} = \exp\left(-\frac{1}{2(1-\rho^2)}\left(\left(\frac{c'-\mu_c}{\sigma_c}\right)^2 - \left(\frac{c-\mu_c}{\sigma_c}\right)^2 - 2\rho\left(\frac{b-\mu_b}{\sigma_b}\right)\left(\frac{c'-c}{\sigma_c}\right)\right)\right)$$

Now taking any $b' > b$ and using the fact that $\exp(x)$ is strictly increasing for any x and our assumption that $\rho < 0$, we get

$$\frac{f(b',c')}{f(b',c)} < \frac{f(b,c')}{f(b,c)}$$

$$\Leftrightarrow -\frac{1}{2(1-\rho^2)}\left(\left(\frac{c'-\mu_c}{\sigma_c}\right)^2 - \left(\frac{c-\mu_c}{\sigma_c}\right)^2 - 2\rho\left(\frac{b'-\mu_b}{\sigma_b}\right)\left(\frac{c'-c}{\sigma_c}\right)\right) < -\frac{1}{2(1-\rho^2)}\left(\left(\frac{c'-\mu_c}{\sigma_c}\right)^2 - \left(\frac{c-\mu_c}{\sigma_c}\right)^2 - 2\rho\left(\frac{b-\mu_b}{\sigma_b}\right)\left(\frac{c'-c}{\sigma_c}\right)\right)$$

$$\Leftrightarrow -2\rho\left(\frac{b'-\mu_b}{\sigma_b}\right)\left(\frac{c'-c}{\sigma_c}\right) > -2\rho\left(\frac{b-\mu_b}{\sigma_b}\right)\left(\frac{c'-c}{\sigma_c}\right)$$

$$\Leftrightarrow (b'-b)(c'-c) > 0$$

Thus, $-f(b,c)$ satisfies log-increasing differences in b and c.

Theorem 1

F first-order stochastically dominates G if and only if $F(b) \leq G(b)$ for all b.

Proof of Theorem 1

Define $H(b) = F(b) - G(b)$ for all b.

Proof that F first-order stochastically dominates $G \Rightarrow F(b) \leq G(b)$ for all b. Suppose towards contradiction that $\exists b*$ such that $H(b*) > 0$. Define $z(b) = \mathbf{1}_{\{b \geq b*\}}$. Then, $\int_{-\infty}^{\infty} z(b)h(b)db = \int_{b*}^{\infty} h(b)db = -H(b*) < 0$, from the definition of H and the assumption that $H(b*) > 0$, and $\int_{-\infty}^{\infty} z(b)h(b)db > 0$, from the fact that $z(b)$ is weakly increasing and the assumption that F first-order stochastically dominates G. This finishes the proof by contradiction.

Proof that $F(b) \leq G(b)$ for all $b \Rightarrow F$ first-order stochastically dominates G. Take any weakly increasing z that is differentiable everywhere. Then, by integration by parts, $\int_{-\infty}^{\infty} z(b)h(b)db = [z(b)H(b)]_{-\infty}^{\infty} - \int_{-\infty}^{\infty} z'(b)H(b)db = -\int_{-\infty}^{\infty} z'(b)H(b)db \geq 0$ since z is weakly increasing and $H(b) \leq 0$ for all b. This shows that F first-order stochastically dominates G.

Theorem 2

If F dominates G in the Monotone Likelihood Ratio sense, then F also first-order stochastically dominates G.

Proof of Theorem 2

If F dominates G in the Monotone Likelihood Ratio sense, then $\frac{g(b')}{g(b)} \leq \frac{f(b')}{f(b)}$ for any $b' > b$.

Since f and g are density functions, $\int_{-\infty}^{\infty} f(b)db = \int_{-\infty}^{\infty} g(b)db = 1$ and $\int_{-\infty}^{\infty} (f-g)(b)db = 0$. Thus, there exists $b*$ such that $f(b*) = g(b*)$.[27]

$\frac{g(b')}{g(b)} < \frac{f(b')}{f(b)}$ for any $b' > b$ implies $\int_{-\infty}^{x} g(b)db \geq \frac{g(x)}{f(x)} \int_{-\infty}^{x} f(b)db$ for any x.

We can further show that $\int_{-\infty}^{x} g(b)db \geq \int_{-\infty}^{x} f(b)db$ for any x:

- for any $x \leq b*$, $\frac{g(x)}{f(x)} \geq \frac{g(b*)}{f(b*)} = 1$ so that $\int_{-\infty}^{x} g(b)db \geq \frac{g(x)}{f(x)} \int_{-\infty}^{x} f(b)db \geq \int_{-\infty}^{x} f(b)db$
- for any $x > b*$, $\frac{g(x)}{f(x)} \leq \frac{g(b*)}{f(b*)} = 1$. $g(x) \leq f(x)$ for any $x > b*$ implies $\int_{x}^{\infty} g(b)db < \int_{x}^{\infty} f(b)db$ for any $x > b*$. Since $\int_{-\infty}^{\infty} f(b)db = \int_{-\infty}^{\infty} g(b)db = 1$, this implies $\int_{-\infty}^{x} g(b)db \geq \int_{-\infty}^{x} f(b)db$ for any $x > b*$

So, $G(x) \geq F(x)$ for any x: using *Theorem 1*, this shows that F first-order stochastically dominates G.

Claim 1

Under *Conditions ID* and *R1*, compliers have lower benefits of voting on average than always-takers: $E[b_i \mid i \text{ is complier}] \leq E[b_i \mid i \text{ is always-taker}]$.

Proof of Claim 1

[27] This implicitly assumes the continuity of f and g. However, the proof holds even without this assumption.

$E[b_i \mid i \text{ is complier}] = \frac{\int_{-\infty}^{\infty} b \int_{g(b)}^{g(b)/\lambda} f(b,c)dc\ db}{\int_{-\infty}^{\infty} \int_{g(b)}^{g(b)/\lambda} f(b,c)dc\ db}$ and $E[b_i \mid i \text{ is always-taker}] = \frac{\int_{-\infty}^{\infty} b \int_{-\infty}^{g(b)} f(b,c)dc\ db}{\int_{-\infty}^{\infty} \int_{-\infty}^{g(b)} f(b,c)dc\ db}$.

$E[b_i \mid i \text{ is complier}]$ and $E[b_i \mid i \text{ is always-taker}]$ can be rewritten as $E[b_i \mid i \text{ is complier}] = \int_{-\infty}^{\infty} bh(b)db$ and $E[b_i \mid i \text{ is always-taker}] = \int_{-\infty}^{\infty} bk(b)db$ with $h(b) = \frac{\int_{g(b)}^{g(b)/\lambda} f(b,c)dc}{\int_{-\infty}^{\infty} \int_{g(b)}^{g(b)/\lambda} f(b,c)dc\ db}$ and $k(b) = \frac{\int_{-\infty}^{g(b)} f(b,c)dc}{\int_{-\infty}^{\infty} \int_{-\infty}^{g(b)} f(b,c)dc\ db}$.

h and k are two density functions: since $f(b,c) > 0$ for any b and c, $h(b) > 0$ and $k(b) > 0$ for any $b \in]-\infty, \infty[$. Moreover, $\int_{-\infty}^{\infty} h(b)db = \int_{-\infty}^{\infty} k(b)db = 1$. We call H and K the distribution functions corresponding to h and k.

We now show that $\frac{h(b')}{h(b)} \leq \frac{k(b')}{k(b)}$ for any $b' > b$.

$\frac{h(b')}{h(b)} \leq \frac{k(b')}{k(b)}$ for any $b' > b \Leftrightarrow \frac{\int_{g(b')}^{g(b')/\lambda} f(b',c)dc}{\int_{-\infty}^{g(b')} f(b',c)dc} \leq \frac{\int_{g(b)}^{g(b)/\lambda} f(b,c)dc}{\int_{-\infty}^{g(b)} f(b,c)dc}$ for any $b' > b$, which we show in two steps

First, we show that $\frac{\int_{g(b')}^{g(b')/\lambda} f(b',c)dc}{\int_{-\infty}^{g(b')} f(b',c)dc} \leq \frac{\int_{g(b')}^{g(b')/\lambda} f(b,c)dc}{\int_{-\infty}^{g(b')} f(b,c)dc}$ for any $b' > b$ using *Condition ID.*

Take $b' > b$. Since $-f(b,c)$ satisfies log-increasing differences: $\frac{f(b',c')}{f(b',c)} \leq \frac{f(b,c')}{f(b,c)}$ for any $c \in]-\infty, g(b')]$ and $c' \in [g(b'), g(b')/\lambda]$. Therefore, $\frac{\int_{g(b')}^{g(b')/\lambda} f(b',c)dc}{f(b',c)} \leq \frac{\int_{g(b')}^{g(b')/\lambda} f(b,c)dc}{f(b,c)}$ or $\frac{f(b',c)}{\int_{g(b')}^{g(b')/\lambda} f(b',c)dc} \geq \frac{f(b,c)}{\int_{g(b')}^{g(b')/\lambda} f(b,c)dc}$ for any $c \in]-\infty, g(b')]$ and $\frac{\int_{-\infty}^{g(b')} f(b',c)dc}{\int_{g(b')}^{g(b')/\lambda} f(b',c)dc} \geq \frac{\int_{-\infty}^{g(b')} f(b,c)dc}{\int_{g(b')}^{g(b')/\lambda} f(b,c)dc}$ or $\frac{\int_{g(b')}^{g(b')/\lambda} f(b',c)dc}{\int_{-\infty}^{g(b')} f(b',c)dc} \leq \frac{\int_{g(b')}^{g(b')/\lambda} f(b,c)dc}{\int_{-\infty}^{g(b')} f(b,c)dc}$.

Second, we show that $\frac{\int_{g(b')}^{g(b')/\lambda} f(b,c)dc}{\int_{-\infty}^{g(b')} f(b,c)dc} \leq \frac{\int_{g(b)}^{g(b)/\lambda} f(b,c)dc}{\int_{-\infty}^{g(b)} f(b,c)dc}$ for any b and $b' > b$ using *Condition R1.*

Consider any $b' > b$. We show $\frac{\int_{g(b')}^{g(b')/\lambda} f(b,c)dc}{\int_{-\infty}^{g(b')} f(b,c)dc} \leq \frac{\int_{g(b)}^{g(b)/\lambda} f(b,c)dc}{\int_{-\infty}^{g(b)} f(b,c)dc}$ or $\frac{\int_{g(b')}^{g(b')/\lambda} f(b,c)dc}{F(g(b')|b)} \leq \frac{\int_{g(b)}^{g(b)/\lambda} f(b,c)dc}{F(g(b)|b)}$ by showing that $z\left(\widetilde{\lambda}\right) = \frac{\int_{g(b')}^{g(b')\widetilde{\lambda}} f(c|b)dc}{F(g(b')|b)} - \frac{\int_{g(b)}^{g(b)\widetilde{\lambda}} f(c|b)dc}{F(g(b)|b)}$ decreases in $\widetilde{\lambda}$ for any $\widetilde{\lambda} \in [1, 1/\lambda]$ and that $z(1) = 0$.

$z(1) = 0$ is immediate.

We prove that $z\left(\widetilde{\lambda}\right)$ decreases in $\widetilde{\lambda}$ for any $\widetilde{\lambda} \in [1, 1/\lambda]$ by iteration.

First, $z'(1) \leq 0$. Indeed, $z'(1) = \lim_{\varepsilon > 0, \varepsilon \to 0} \frac{z(1+\varepsilon) - z(1)}{\varepsilon}$.

For any $\varepsilon > 0$, $\frac{z(1+\varepsilon)-z(1)}{\varepsilon} \leq 0 \Leftrightarrow \frac{1}{\varepsilon}\left(\frac{\int_{g(b')}^{g(b')(1+\varepsilon)} f(c|b)dc}{F(g(b')|b)} - \frac{\int_{g(b)}^{g(b)(1+\varepsilon)} f(c|b)dc}{F(g(b)|b)}\right) \leq 0$.

As $\varepsilon \to 0$, $\frac{1}{\varepsilon}\left(\frac{\int_{g(b')}^{g(b')(1+\varepsilon)} f(c|b)dc}{F(g(b')|b)} - \frac{\int_{g(b)}^{g(b)(1+\varepsilon)} f(c|b)dc}{F(g(b)|b)}\right) \to \frac{g(b')f(g(b')|b)}{F(g(b')|b)} - \frac{g(b)f(g(b)|b)}{F(g(b)|b)}$.

From *Condition R1*, we have that $\frac{g(b')f\left(g(b')|b\right)}{F(g(b')|b)} < \frac{g(b)f(g(b)|b)}{F(g(b)|b)}$. Therefore, $z'(1) \leq 0$

We now show that if $z\left(\widetilde{\lambda}\right)$decreases in $\widetilde{\lambda}$ for any $\widetilde{\lambda} \in \left[1, \widetilde{\widetilde{\lambda}}\right[$, we also have $z'\left(\widetilde{\widetilde{\lambda}}\right) < 0$, where $\widetilde{\widetilde{\lambda}} \leq 1/\lambda$.

Since $z\left(\widetilde{\lambda}\right)$ decreases in $\widetilde{\lambda}$ for any $\widetilde{\lambda} \in \left[1, \widetilde{\widetilde{\lambda}}\right[$, $z\left(\widetilde{\widetilde{\lambda}}\right) \leq 0$: $\frac{\int_{g(b')}^{g(b')\widetilde{\widetilde{\lambda}}} f(c|b)dc}{F(g(b')|b)} \leq \frac{\int_{g(b)}^{g(b)\widetilde{\widetilde{\lambda}}} f(c|b)dc}{F(g(b)|b)}$, which implies $\frac{F\left(g(b')\widetilde{\widetilde{\lambda}}|b\right)}{F(g(b')|b)} = \frac{F\left(g(b')|b\right)+\int_{g(b')}^{g(b')\widetilde{\widetilde{\lambda}}} f(c|b)dc}{F(g(b')|b)} \leq \frac{F(g(b)|b)+\int_{g(b)}^{g(b)\widetilde{\widetilde{\lambda}}} f(c|b)dc}{F(g(b)|b)} = \frac{F\left(g(b)\widetilde{\widetilde{\lambda}}|b\right)}{F(g(b)|b)}$.

By definition, $z'\left(\widetilde{\widetilde{\lambda}}\right) = \lim_{\varepsilon>0,\varepsilon\to 0} \frac{z\left(\widetilde{\widetilde{\lambda}}+\varepsilon\right)-z\left(\widetilde{\widetilde{\lambda}}\right)}{\varepsilon}$.

For any $\varepsilon > 0$, $\frac{z\left(\widetilde{\widetilde{\lambda}}+\varepsilon\right)-z\left(\widetilde{\widetilde{\lambda}}\right)}{\varepsilon} = \frac{1}{\varepsilon}\frac{\int_{g(b')\widetilde{\widetilde{\lambda}}}^{g(b')\left(\widetilde{\widetilde{\lambda}}+\varepsilon\right)} f(c|b)dc}{F(g(b')|b)} - \frac{1}{\varepsilon}\frac{\int_{g(b)\widetilde{\widetilde{\lambda}}}^{g(b)\left(\widetilde{\widetilde{\lambda}}+\varepsilon\right)} f(c|b)dc}{F(g(b)|b)} = \frac{1}{\varepsilon}\frac{\int_{g(b')\widetilde{\widetilde{\lambda}}}^{g(b')\left(\widetilde{\widetilde{\lambda}}+\varepsilon\right)} f(c|b)dc}{F\left(g(b')\widetilde{\widetilde{\lambda}}|b\right)}\frac{F\left(g(b')\widetilde{\widetilde{\lambda}}|b\right)}{F(g(b')|b)} - \frac{1}{\varepsilon}\frac{\int_{g(b)\widetilde{\widetilde{\lambda}}}^{g(b)\left(\widetilde{\widetilde{\lambda}}+\varepsilon\right)} f(c|b)dc}{F\left(g(b)\widetilde{\widetilde{\lambda}}|b\right)}\frac{F\left(g(b)\widetilde{\widetilde{\lambda}}|b\right)}{F(g(b)|b)}$.

Since $\frac{F\left(g(b')\widetilde{\widetilde{\lambda}}|b\right)}{F(g(b')|b)} \leq \frac{F\left(g(b)\widetilde{\widetilde{\lambda}}|b\right)}{F(g(b)|b)}$, it is enough to show that $\frac{1}{\varepsilon}\frac{\int_{g(b')\widetilde{\widetilde{\lambda}}}^{g(b')\left(\widetilde{\widetilde{\lambda}}+\varepsilon\right)} f(c|b)dc}{F\left(g(b')\widetilde{\widetilde{\lambda}}|b\right)} - \frac{1}{\varepsilon}\frac{\int_{g(b)\widetilde{\widetilde{\lambda}}}^{g(b)\left(\widetilde{\widetilde{\lambda}}+\varepsilon\right)} f(c|b)dc}{F\left(g(b)\widetilde{\widetilde{\lambda}}|b\right)} \leq 0$ to show that $\frac{z\left(\widetilde{\widetilde{\lambda}}+\varepsilon\right)-z\left(\widetilde{\widetilde{\lambda}}\right)}{\varepsilon} \leq 0$.

As $\varepsilon \to 0$, $\frac{1}{\varepsilon}\frac{\int_{g(b')\widetilde{\widetilde{\lambda}}}^{g(b')\left(\widetilde{\widetilde{\lambda}}+\varepsilon\right)} f(c|b)dc}{F\left(g(b')\widetilde{\widetilde{\lambda}}|b\right)} - \frac{1}{\varepsilon}\frac{\int_{g(b)\widetilde{\widetilde{\lambda}}}^{g(b)\left(\widetilde{\widetilde{\lambda}}+\varepsilon\right)} f(c|b)dc}{F\left(g(b)\widetilde{\widetilde{\lambda}}|b\right)} \to \frac{g(b')f\left(g(b')\widetilde{\widetilde{\lambda}}|b\right)}{F\left(g(b')\widetilde{\widetilde{\lambda}}|b\right)} - \frac{g(b)f\left(g(b)\widetilde{\widetilde{\lambda}}|b\right)}{F\left(bg(b)\widetilde{\widetilde{\lambda}}|b\right)}$.

From *Condition R1*, we have that $\frac{g(b')f\left(g(b')\widetilde{\widetilde{\lambda}}|b\right)}{F\left(g(b')\widetilde{\widetilde{\lambda}}|b\right)} < \frac{g(b)f\left(g(b)\widetilde{\widetilde{\lambda}}|b\right)}{F\left(g(b)\widetilde{\widetilde{\lambda}}|b\right)}$. Therefore, $z'\left(\widetilde{\widetilde{\lambda}}\right) \leq 0$.

This finishes the proof that $z\left(\widetilde{\lambda}\right)$ decreases in $\widetilde{\lambda}$ for any $\widetilde{\lambda} \in [1, 1/\lambda]$ and, combined with $z(1) = 0$, that $\frac{\int_{g(b')}^{g(b')/\lambda} f(b,c)dc}{\int_{-\infty}^{g(b')} f(b,c)dc} \leq \frac{\int_{g(b)}^{g(b)/\lambda} f(b,c)dc}{\int_{-\infty}^{g(b)} f(b,c)dc}$.

Since $\frac{h(b')}{h(b)} \leq \frac{k(b')}{k(b)}$ for any $b' > b$, we can apply *Theorem 2* to h and k: K first-order stochastically dominates H. By definition of the first-order stochastic dominance, this implies that for every weakly increasing function $z(b)$, $\int_{-\infty}^{\infty} z(b)h(b)db \leq \int_{-\infty}^{\infty} z(b)k(b)db$. In particular, for the identity function $z(b) = b$, we get $E[b_i \mid i \text{ is complier}] \leq E[b_i \mid i \text{ is always-taker}]$ Q.E.D.

Claim 2

Under *Conditions ID* and *R1*, compliers face a higher registration cost on average than always-takers: $E[c_i \mid i \text{ is complier}] > E[c_i \mid i \text{ is always-taker}]$.

Proof of Claim 2

$E[c_i \mid i \text{ is always-taker}] = \frac{\int_{-\infty}^{\infty} c \int_{g^{-1}(c)}^{\infty} f(b,c)db\ dc}{\int_{-\infty}^{\infty} \int_{g^{-1}(c)}^{\infty} f(b,c)db\ dc}$ and $E[c_i \mid i \text{ is complier}] = \frac{\int_{-\infty}^{\infty} c \int_{g^{-1}(\lambda c)}^{g^{-1}(c)} f(b,c)db\ dc}{\int_{-\infty}^{\infty} \int_{g^{-1}(\lambda c)}^{g^{-1}(c)} f(b,c)db\ dc}$

The proof follows the same steps and is symmetric to the proof of *Claim 1.*

First, we write $E[c_i \mid i \text{ is complier}] = \int_{-\infty}^{\infty} ch(c)dc$ and $E[c_i \mid i \text{ is always-taker}] = \int_{-\infty}^{\infty} ck(c)dc$.

We then show that H first-order stochastically dominates K, using *Theorem 2*, which concludes.

Claim 3

Under *Conditions ID* and *R1*, compliers have a lower propensity to vote on average than always-takers: $E[v(b_i) \mid i \text{ is complier}] \leq E[v(b_i) \mid i \text{ is always-taker}]$.

Proof of Claim 3

$E[v(b_i) \mid i \text{ is always-taker}] = \frac{\int_{-\infty}^{\infty} v(b) \int_{-\infty}^{g(b)} f(b,c)dc\ db}{\int_{-\infty}^{\infty} \int_{-\infty}^{g(b)} f(b,c)dc\ db} = \int_{-\infty}^{\infty} v(b)k(b)db$

and $E[v(b_i) \mid i \text{ is complier}] = \frac{\int_{-\infty}^{\infty} v(b) \int_{g(b)}^{g(b)/\lambda} f(b,c)dc\ db}{\int_{-\infty}^{\infty} \int_{g(b)}^{g(b)/\lambda} f(b,c)dc\ db} = \int_{-\infty}^{\infty} v(b)h(b)db,$

where $h(b)$ and $k(b)$ are defined as in the proof of *Claim 2*:

$h(b) = \frac{\int_{g(b)}^{g(b)/\lambda} f(b,c)dc}{\int_{-\infty}^{\infty} \int_{g(b)}^{g(b)/\lambda} f(b,c)dc\ db}$ and $k(b) = \frac{\int_{-\infty}^{g(b)} f(b,c)dc}{\int_{-\infty}^{\infty} \int_{-\infty}^{g(b)} f(b,c)dc\ db}$.

$v(b) = 1 - F_{\varepsilon}(-b)$ is increasing since $F_{\varepsilon}(.)$ is increasing.

Since K first-order stochastically dominates H (as proved in the proof of *Claim 2*), we thus get $\int_{-\infty}^{\infty} v(b)h(b)db \leq \int_{-\infty}^{\infty} v(b)k(b)db$, ie $E[v(b_i) \mid i \text{ is complier}] < E[v(b_i) \mid i \text{ is always-taker}]$.

Claim 4

Under *Conditions ID* and *R1*, compliers who vote have lower expected benefits of voting on average than always-takers: $E[b_i \mid i \text{ is complier}, i \text{ votes}] \leq E[b_i \mid i \text{ is always-taker}, i \text{ votes}]$.

Proof of Claim 4

$E[b_i \mid i \text{ is always-taker}, i \text{ votes}] = \frac{\int_{-\infty}^{\infty} b \int_{-\infty}^{g(b)} v(b)f(b,c)dc\ db}{\int_{-\infty}^{\infty} \int_{-\infty}^{g(b)} v(b)f(b,c)dc\ db}$ and

$E[b_i \mid i \text{ is complier}, i \text{ votes}] = \frac{\int_{-\infty}^{\infty} b \int_{g(b)}^{g(b)/\lambda} v(b)f(b,c)dc\ db}{\int_{-\infty}^{\infty} \int_{g(b)}^{g(b)/\lambda} v(b)f(b,c)dc\ db}$.

We write $w(b,c) \equiv v(b)f(b,c)$. Since $-f(b,c)$ satisfies log-increasing differences in b and c, $-w(b,c)$ satisfies log-increasing differences as well : $\frac{w(b',c')}{w(b',c)} = \frac{f(b',c')}{f(b',c)} < \frac{f(b,c')}{f(b,c)} = \frac{w(b,c')}{w(b,c)}$ for any $b' > b$ and $c' > c$.

Therefore, substituting $w(b,c)$ to $f(b,c)$ in the proof of *Claim 1*, we get $\frac{\int_{-\infty}^{\infty} b \int_{g(b)}^{g(b)/\lambda} v(b) f(b,c) dc\ db}{\int_{-\infty}^{\infty} \int_{g(b)}^{g(b)/\lambda} v(b) f(b,c) dc\ db} < \frac{\int_{-\infty}^{\infty} b \int_{-\infty}^{g(b)} v(b) f(b,c) dc\ db}{\int_{-\infty}^{\infty} \int_{-\infty}^{g(b)} v(b) f(b,c) dc\ db}$, ie $E[b_i \mid i \text{ is complier}, i \text{ votes}] < E[b_i \mid i \text{ is always-taker}, i \text{ votes}]$.

Claim 5

Under *Conditions ID*, *R1* and *R2*, for a given share of compliers and unchanged conditional densities $f(c \mid b)$, an increase in the compliers' propensity to vote, generated by an increase in the relative number of compliers with a higher b, is concomitant to an increase in their benefits of voting and registration cost.

Proof of Claim 5

Absent the condition *R2*, we have that, for any b, $z(b') \geq z(b)$ for any b' and b such that $g(b') \geq {}^{g(b)}/_{\lambda}$. Indeed, the conditional expectation is then taken on a separated support, "higher" for b' than b. For b' and b such that $g(b') < {}^{g(b)}/_{\lambda}$, $z(b') - z(b)$ is driven by two opposite effects. The support is still "higher" for b' which tends to make the difference positive. However, for any $\left(\widetilde{c}, \widetilde{\widetilde{c}}\right) \in [g(b'), {}^{g(b)}/_{\lambda}[$ with $\widetilde{\widetilde{c}} > \widetilde{c}$ the relative weight of compliers facing the higher cost $\widetilde{\widetilde{c}}$ is higher for those who have the lower degree of politicization b, which tends to make the difference negative: $\frac{f\left(b', \widetilde{\widetilde{c}}\right)}{f\left(b', \widetilde{c}\right)} \leq \frac{f\left(b, \widetilde{\widetilde{c}}\right)}{f\left(b, \widetilde{c}\right)}$ since $-f(b,c)$ satisfies log-increasing differences. The closer b' to b, the bigger the importance of this second effect relative to the first one. *Condition R2* ensures that the second effect never outweighs the first one so that $z(b') \geq z(b)$ even for b' close to b.

Indeed, we construct a new density $f_2(b,c)$ based on the density $f(b,c)$ and such that, among compliers characterized by a given b, the shape of the conditional density of C given $B = b$ is unchanged: for any b and any $(c, c') \in [b, {}^{b}/_{\lambda}]^2$: $\frac{f_2\left(c' \mid b\right)}{f_2(c \mid b)} = \frac{f\left(c' \mid b\right)}{f(c \mid b)}$.

$\frac{f_2\left(c' \mid b\right)}{f_2(c \mid b)} = \frac{f\left(c' \mid b\right)}{f(c \mid b)}$ for any b and any $(c, c') \in [b, {}^{b}/_{\lambda}]^2$ is equivalent to $\frac{f_2\left(b, c'\right)}{f_2(b, c)} = \frac{f\left(b, c'\right)}{f(b, c)}$ for any b and any $(c, c') \in [b, {}^{b}/_{\lambda}]^2$.

This requires $f_2(b,c) = f(b,c)\, h(b)$ for any b and $c \in [g(b), {}^{g(b)}/_{\lambda}]$, for some function $h(b)$ positive.

For b and c such that $c \notin [b, {}^{b}/_{\lambda}]$, we set $f_2(b,c) = f(b,c)$ otherwise.

h must be positive, so that $f_2(b,c) \geq 0$ for any b and c, a condition to qualify as a density.

Further, h must satisfy $\int_{-\infty}^{\infty} h(b) \left(\int_{g(b)}^{g(b)/\lambda} f(b,c)\, dc \right) db = \int_{-\infty}^{\infty} \int_{g(b)}^{g(b)/\lambda} f(b,c)\, dc\ db$ for the fraction of compliers to be unchanged.

This also satisfies the second condition for $f_2(b,c)$ to qualify as a density: $\int_{-\infty}^{\infty} \int_{-\infty}^{\infty} f_2(b,c)\, dc\ db =$

$\int_{-\infty}^{\infty}\int_{-\infty}^{\infty} f(b,c)\,dc\,db + \int_{-\infty}^{\infty} h(b)\left(\int_{g(b)}^{g(b)/\lambda} f(b,c)\,dc\right)db - \int_{-\infty}^{\infty}\int_{g(b)}^{g(b)/\lambda} f(b,c)\,dc\,db = 1$

Finally, to obtain that the expected participation of the compliers is higher under the joint density $f_2(b,c)$ than the joint density $f(b,c)$, we impose that $h(b)$ be increasing as a sufficient (but not necessary) condition. We show below that this condition is indeed sufficient:

$E_{f_2}[v(b_i) \mid i \text{ is complier}] = \int_{-\infty}^{\infty} v(b)\frac{\int_{g(b)}^{g(b)/\lambda} f_2(b,c)dc}{\int_{-\infty}^{\infty}\int_{g(b)}^{g(b)/\lambda} f_2(b,c)dc\;db}db = \int_{-\infty}^{\infty} v(b)k(b)db$ and $E_f[v(b_i) \mid i \text{ is complier}] = \int_{-\infty}^{\infty} v(b)\frac{\int_{g(b)}^{g(b)/\lambda} f(b,c)dc}{\int_{-\infty}^{\infty}\int_{g(b)}^{g(b)/\lambda} f(b,c)dc\;db}db = \int_{-\infty}^{\infty} v(b)l(b)db$

With h increasing in b, we get that for any $b' > b$, $\frac{k(b')}{k(b)} = \frac{h(b')}{h(b)}\frac{\int_{g(b')}^{g(b')/\lambda} f(b',c)dc}{\int_{g(b)}^{g(b)/\lambda} f(b,c)dc} = \frac{h(b')}{h(b)}\frac{l(b')}{l(b)} \geq \frac{l(b')}{l(b)}$

Applying *Theorem 2* to the density functions k and l, we thus get that K first-order stochastically dominates L, and $E_{f_2}[v(b_i) \mid i \text{ is complier}] \geq E_f[v(b_i) \mid i \text{ is complier}]$

Now, for any $h(b)$ satisfying the conditions listed above, we sign the difference between the compliers' benefits of voting, benefits of voting conditional on voting and registration cost when the joint density is $f(b,c)$ or $f_2(b,c)$.

First, $E_{f_2}[b_i \mid i \text{ is complier}] = \int_{-\infty}^{\infty} b\frac{\int_{g(b)}^{g(b)/\lambda} f_2(b,c)dc}{\int_{-\infty}^{\infty}\int_{g(b)}^{g(b)/\lambda} f_2(b,c)dc\;db}db \geq \int_{-\infty}^{\infty} b\frac{\int_{g(b)}^{g(b)/\lambda} f(b,c)dc}{\int_{-\infty}^{\infty}\int_{g(b)}^{g(b)/\lambda} f(b,c)dc\;db}db = E_f[b_i \mid i \text{ is complier}]$ comes directly from the fact that K first-order stochastically dominates L.

Second, $E_{f_2}[b_i \mid i \text{ is complier}, i \text{ votes}] = \int_{-\infty}^{\infty} b\frac{\int_{g(b)}^{g(b)/\lambda} v(b)f_2(b,c)dc}{\int_{-\infty}^{\infty}\int_{g(b)}^{g(b)/\lambda} v(b)f_2(b,c)dc\;db}db \geq \int_{-\infty}^{\infty} b\frac{\int_{g(b)}^{g(b)/\lambda} v(b)f(b,c)dc}{\int_{-\infty}^{\infty}\int_{g(b)}^{g(b)/\lambda} v(b)f(b,c)dc\;db}db = E_f[b_i \mid i \text{ is complier}, i \text{ votes}]$.

The proof of this is identical to the proof above, rewriting $k(b) = \frac{\int_{g(b)}^{g(b)/\lambda} v(b)f_2(b,c)dc}{\int_{-\infty}^{\infty}\int_{g(b)}^{g(b)/\lambda} v(b)f_2(b,c)dc\;db}$ and $l(b) = \frac{\int_{g(b)}^{g(b)/\lambda} v(b)f(b,c)dc}{\int_{-\infty}^{\infty}\int_{g(b)}^{g(b)/\lambda} v(b)f(b,c)dc\;db}$.

Third, $E_{f_2}[c_i \mid i \text{ is complier}] \geq E_f[c_i \mid i \text{ is complier}]$.

These two objects can be written as:

$E_f[c_i \mid i \text{ is complier}] = \int_{-\infty}^{\infty} E_f[c_i \mid i \text{ is complier}, b_i = b]\frac{\int_{g(b)}^{g(b)/\lambda} f(b,c)dc}{\int_{-\infty}^{\infty}\int_{g(b)}^{g(b)/\lambda} f(b,c)dc\;db}db$

and $E_{f_2}[c_i \mid i \text{ is complier}] = \int_{-\infty}^{\infty} E_{f_2}[c_i \mid i \text{ is complier}, b_i = b]\frac{\int_{g(b)}^{g(b)/\lambda} f_2(b,c)dc}{\int_{-\infty}^{\infty}\int_{g(b)}^{g(b)/\lambda} f_2(b,c)dc\;db}db$.

But $E_{f_2}[c_i \mid i \text{ is complier}, b_i = b] = \frac{\int_{g(b)}^{g(b)/\lambda} cf_2(b,c)dc}{\int_{g(b)}^{g(b)/\lambda} f_2(b,c)dc} = \frac{h(b)}{h(b)}\frac{\int_{g(b)}^{g(b)/\lambda} cf(b,c)dc}{\int_{g(b)}^{g(b)/\lambda} f(b,c)dc} = E_f[c_i \mid i \text{ is complier}, b_i = b]$.

Moreover, $z(b) \equiv E_f[c_i \mid i \text{ is complier}, b_i = b]$ is increasing in b by assumption (*Condition R2*).

Writing again, $l(b) = \frac{\int_{g(b)}^{g(b)/\lambda} f(b,c)dc}{\int_{-\infty}^{\infty}\int_{g(b)}^{g(b)/\lambda} f(b,c)dc\ db}$ and $k(b) = \frac{\int_{g(b)}^{g(b)/\lambda} f_2(b,c)dc}{\int_{-\infty}^{\infty}\int_{g(b)}^{g(b)/\lambda} f_2(b,c)dc\ db}$, we have $\frac{k(b')}{k(b)} \geq \frac{l(b')}{l(b)}$ for any $b' > b$: K dominates L in the MLR sense. Thus, by *Theorem 2*, K first-order stochastically dominates L. Since $z(b)$ is increasing in b, we get $\int_{-\infty}^{\infty} z(b)\frac{\int_{g(b)}^{g(b)/\lambda} f_2(b,c)dc}{\int_{-\infty}^{\infty}\int_{g(b)}^{g(b)/\lambda} f_2(b,c)dc\ db}db \geq \int_{-\infty}^{\infty} z(b)\frac{\int_{g(b)}^{g(b)/\lambda} f(b,c)dc}{\int_{-\infty}^{\infty}\int_{g(b)}^{g(b)/\lambda} f(b,c)dc\ db}db$ Q.E.D.

Claim 6

Claim 6: Under *Conditions ID* and *R1* and for any $\lambda' < \lambda < 1$, λ' visits select compliers with a higher registration cost, lower benefits of voting, lower propensity to vote, and lower benefits of voting conditional on voting than λ visits.

Proof of Claim 6

The λ' visits select additional compliers characterized by $g(b_i)/\lambda \leq c_i < g(b_i)/\lambda'$.

The additional compliers have lower benefits of voting: $\frac{\int_{-\infty}^{\infty} b\int_{g(b)/\lambda}^{g(b)/\lambda'} f(b,c)dc\ db}{\int_{-\infty}^{\infty}\int_{b/\lambda}^{b/\lambda'} f(b,c)dc\ db} < \frac{\int_{-\infty}^{\infty} b\int_{g(b)}^{g(b)/\lambda} f(b,c)dc\ db}{\int_{-\infty}^{\infty}\int_{g(b)}^{g(b)/\lambda} f(b,c)dc\ db}$. The proof is derived in the same way as the proof of *Claim 1*.

They face a higher registration cost: $\frac{\int_{-\infty}^{\infty} c\int_{g^{-1}(\lambda' c)}^{g^{-1}(\lambda c)} f(b,c)db\ dc}{\int_{-\infty}^{\infty}\int_{g^{-1}(\lambda' c)}^{g^{-1}(\lambda c)} f(b,c)db\ dc} > \frac{\int_{-\infty}^{\infty} c\int_{g^{-1}(c)}^{g^{-1}(\lambda c)} f(b,c)db\ dc}{\int_{-\infty}^{\infty}\int_{g^{-1}(c)}^{g^{-1}(\lambda c)} f(b,c)db\ dc}$. The proof is derived in the same way as the proof of *Claim 2*.

They have a lower turnout: $\frac{\int_{-\infty}^{\infty} v(b)\int_{g(b)/\lambda}^{g(b)/\lambda'} f(b,c)dc\ db}{\int_{-\infty}^{\infty}\int_{g(b)/\lambda}^{g(b)/\lambda'} f(b,c)dc\ db} < \frac{\int_{-\infty}^{\infty} v(b)\int_{g(b)}^{g(b)/\lambda} f(b,c)dc\ db}{\int_{-\infty}^{\infty}\int_{g(b)}^{g(b)/\lambda} f(b,c)dc\ db}$. The proof is derived in the same way as the proof of *Claim 3*.

Finally, they have lower expected benefits of voting conditional on voting: $\frac{\int_{-\infty}^{\infty} b\int_{g(b)/\lambda}^{g(b)/\lambda'} v(b)f(b,c)dc\ db}{\int_{-\infty}^{\infty}\int_{g(b)/\lambda}^{g(b)/\lambda'} v(b)f(b,c)dc\ db} < \frac{\int_{-\infty}^{\infty} b\int_{g(b)}^{g(b)/\lambda} v(b)f(b,c)dc\ db}{\int_{-\infty}^{\infty}\int_{g(b)}^{g(b)/\lambda} v(b)f(b,c)dc\ db}$. The proof is derived in the same way as the proof of *Claim 4*.

Claim 7

All previous results hold in the extended version of the model where a registered citizen's actual propensity to vote is $w(b_i)$, with $w(b') - v(b') \leq w(b) - v(b)$ and $w(b') \geq w(b)$ for any $b' \geq b$.

Proof of Claim 7

Claims 1 and *2* are unaffected, since the selection process of compliers and always-takers is unchanged: we assume that at the registration stage, individual i still anticipates that she will vote if $b_i + \varepsilon_i \geq 0$.

The proofs of *Claims 3, 4, 5* and 6 can be redone, substituting $w(b)$ to $v(b)$. They rely on relations of first-order stochastic dominance between distribution functions. and thus hold for any weakly increasing function of b, be it v or w.

Claim 8

The difference between compliers and always-takers' predicted turnout is lower once the mobilization effect is taken into account.

Proof of Claim 8

Claim 8 can be restated as

$\int_{-\infty}^{\infty} w(b)k(b)db - \int_{-\infty}^{\infty} w(b)h(b)db \leq \int_{-\infty}^{\infty} v(b)k(b)db - \int_{-\infty}^{\infty} v(b)h(b)db$

$\Leftrightarrow \int_{-\infty}^{\infty} [v(b) - w(b)]\, k(b)db \geq \int_{-\infty}^{\infty} [v(b) - w(b)]\, h(b)db$

where, as before, $h(b) = \frac{\int_{g(b)}^{g(b)/\lambda} f(b,c)dc}{\int_{-\infty}^{\infty}\int_{g(b)}^{g(b)/\lambda} f(b,c)dc\ db}$ and $k(b) = \frac{\int_{-\infty}^{g(b)} f(b,c)dc}{\int_{-\infty}^{\infty}\int_{-\infty}^{g(b)} f(b,c)dc\ db}$.

The proof comes immediately from the fact that K first-order stochastically dominates H and $v(b) - w(b)$ increases in b (by assumption).

Microfounding the assumption that $w(b) - v(b)$ decreases with b

We discuss how this assumption can be grounded in a more fundamental assumption about the way the campaign affects the perceived benefits of voting b_i: suppose that a registered citizen votes if $m(b_i) + \varepsilon_i \geq 0$. Then, the propensity to vote of an individual with politicization b is $w(b) = v\,(m(b))$. Under what condition on m do we have $z(b) \equiv w(b) - v(b)$ decrease in b?

Since $v(b) = 1 - F_\varepsilon\,(-b)$, we have $v'(b) = f_\varepsilon(-b)$. Therefore,

$z'(b) \leq 0 \Leftrightarrow v'\,(m(b))\, m'(b) - v'(b) \leq 0 \Leftrightarrow m'(b) \leq \frac{f_\varepsilon(-b)}{f_\varepsilon(-m(b))}$

If f_ε is increasing on $]-\infty, 0]$ (a condition fulfilled by many usual density functions, including the normal density), this condition is satisfied for any $b \geq 0$ by any function m such that $m(b) \geq b$ and $x(b) \equiv \frac{m(b)}{b}$ decreases: the mobilization increases each citizen's perceived benefits of voting, but less so for citizens with a higher b. Indeed, then, we have $x'(b) \leq 0$ and $m(b) \geq b \Rightarrow m'(b) \leq \frac{m(b)}{b} \leq 1$ and $m(b) \geq b$ and f_ε increasing on $]-\infty, 0] \Rightarrow 1 \leq \frac{f_\varepsilon(-b)}{f_\varepsilon(-m(b))}$.

Claim 9

For any k, if *Conditions* ID_k, $R1_k$ and $R2_k$ hold, all results established for unregistered citizens hold for misregistered citizens facing an additional cost k of voting at their previous address.

Proof of Claim 9

We first prove that $g_k(b)$ is strictly increasing in b for any k.

$g_k(b) = g(b) - g(b-k) = \int_{-b}^{\infty}(b+\varepsilon) f_\varepsilon(\varepsilon)d\varepsilon - \int_{-b+k}^{\infty}(b-k+\varepsilon) f_\varepsilon(\varepsilon)d\varepsilon = \int_{-b}^{-b+k}(b+\varepsilon) f_\varepsilon(\varepsilon)d\varepsilon + k\int_{-b+k}^{\infty} f_\varepsilon(\varepsilon)d\varepsilon$.

$g_k'(b) = -kf_\varepsilon(-b+k) + \int_{-b}^{-b+k} f_\varepsilon(\varepsilon)d\varepsilon + kf_\varepsilon(-b+k) = \int_{-b}^{-b+k} f_\varepsilon(\varepsilon)d\varepsilon > 0$: $g_k(b)$ is strictly increasing in b for any k.

Considering any k, since $g_k(b)$ is strictly increasing, it can be substituted to $g(b)$ in the proofs above.

1.9.6 Estimate of the effect of the intervention "Early Home registration & Late Home registration" on overall turnout

To estimate the effect of the intervention "Early Home registration & Late Home registration" on overall turnout, we first compute the fraction of citizens initially unregistered and misregistered who live in apartments that opened their door to canvassers at least once: 69.9 percent (see Appendix 3). The effect of the intervention on citizens who live in apartments which actually opened their door should thus be scaled by $1/0.699 = 1.43$. We infer from the estimates presented in Table 2 that the intervention increased electoral participation from 16.1 percent to 29.2 percent at the first round of the presidential election. Indeed, the average apartment hosts 0.92 initially unregistered and misregistered citizens. Since 0.148 votes were cast by initially unregistered and misregistered citizens in the average control apartment at the first round of the presidential elections, we infer that their participation was $\frac{0.148}{0.92} = 0.161$. The effect of the "Early Home registration & Late Home registration" intervention in apartments which opened their door at least once was: $0.084 \times 1.43 = 0.120$. Thus, the participation of initially unregistered and misregistered citizens living in these apartments was $\frac{0.148+0.120}{0.92} = 0.292$. Similarly, the intervention increased electoral participation from 16.4 percent to 29.3 percent at the second round of the presidential elections, and from 9.8 percent to 14.5 percent and 8.9 percent to 15.6 percent at the general elections among these citizens. We assume that the effect would be the same among citizens living in apartments which did not

open their door at either the first or second visit.

The above figures represent increased participation at the polling station closest to each citizen's place of residence. However, a fraction of the citizens who remained misregistered at the end of the registration period participated in the elections by travelling back to their previous address or by voting by proxy. We do not observe their participation rate, but can estimate it based on the observed participation of their counterparts: citizens who are registered here but live elsewhere (as signaled by the fact that their name was not found on any mailbox). The implicit assumptions here are that the participation of misregistered citizens who move out is similar to the participation of those who move in, and that the participation rate of misregistered compliers would have been identical to the participation rate of other misregistered citizens had they not registered. The latter assumption is valid to the extent that the decision to register, by misregistered compliers, signals a higher cost of voting at the previous address (predicting lower participation) as much as a higher interest in the elections (predicting higher participation). The participation of citizens who are registered here but live elsewhere was 58.4 percent and 61.3 percent at the presidential elections, and 37.2 percent and 35.9 percent at the general elections. The fraction of citizens who were initially unregistered or misregistered and are still misregistered is 57 percent in the control group and 50 percent in the group "Early Home registration & Late Home registration". We infer that this intervention increased electoral participation among citizens initially unregistered or misregistered from 49.4 percent to 58.4 percent and 51.3 percent to 60 percent at the presidential elections, and from 31 percent to 33 percent and 29.4 percent to 33.5 percent at the general elections.

As a final step, we have to factor in the participation of well-registered citizens: 76.6 percent and 78.5 percent at the presidential elections, and 49.1 percent and 47.2 percent at the general elections. Taking into account the relative shares of the different categories of citizens in the areas covered by this study, we conclude that the "Early Home registration & Late Home registration" increased overall participation from 64.7 percent to 68.6 percent and 65.6 percent to 69.3 percent at the presidential elections, and from 41.2 percent to 42.1 percent and 39.4 percent to 41.2 percent at the general elections.

Table A1: Impact of the interventions on the gross number of new registrations, with addresses at the unit of observation

	(1)
Early Canvassing	0.12
	(0.08)
Late Canvassing	0.21
	(0.08)***
Early Home registration	0.27
	(0.08)***
Late Home registration	0.3
	(0.08)***
Early Canvassing + Late Home registration	0.34
	(0.08)***
Early Home registration + Late Home registration	0.53
	(0.08)***
Strata fixed effects	Yes
Building controls	Yes
Observations	4105
R-squared	0.61
Mean in Control Group	1.13
Linear combinations of estimates:	
Average effect of all interventions 1/6 (EC + LC + EH + LH + EC&LH + EH&LH)	0.295
	(0.050)***
Average effect of Canvassing 1/2 (EC + LC)	0.167
	(0.061)***
Average effect of Home registration 1/2 (EH + LH)	0.281
	(0.061)***
Difference between average effect of Home reg. and Can. 1/2 (EH + LH) - 1/2 (EC + LC)	0.114
	(0.062)*
Difference between average effect of Late visit and Early visit 1/2 (LH + LC) - 1/2 (EH + EC)	0.059
	(0.061)

Notes: Clustered standard errors are in parentheses. ***, **, * indicate significance at 1, 5 and 10%. We take the address as the unit of observation and include all newly registered citizens in the sample addresses.
We finally report point estimates and standard errors of linear combinations of the coefficients.

Table A2: Impact of the interventions on the NET number of new registrations

	(1)
Early Canvassing	0.025
	(0.015)*
Late Canvassing	0.043
	(0.014)***
Early Home registration	0.06
	(0.015)***
Late Home registration	0.067
	(0.015)***
Early Canvassing & Late Home	0.071
registration	(0.014)***
Early Home registration & Late	0.107
Home registration	(0.015)***
Strata fixed effects	Yes
Apartment & Building controls	Yes
Observations	20458
R-squared	0.03
Mean in Control Group	0.09
Linear combinations of estimates:	
Average effect of all interventions	0.062
1/6 (EC + LC + EH + LH + EC&LH + EH&LH)	(0.010)***
Average effect of Canvassing	0.034
1/2 (EC + LC)	(0.012)***
Average effect of Home registration	0.064
1/2 (EH + LH)	(0.012)***
Difference between average effect of Home reg. and Can.	0.03
1/2 (EH + LH) - 1/2 (EC + LC)	(0.012)**
Difference between average effect of Late visit and Early visit	0.012
1/2 (LH + LC) - 1/2 (EH + EC)	(0.012)

Notes: Clustered standard errors are in parentheses. ***, **, * indicate significance at 1, 5 and 10%. We take the apartment as the unit of observation and include all newly registered citizens and all citizens dropped from the voter rolls in the sample apartments. We report point estimates and standard errors of linear combinations of the coefficients.

Table A3: Impact of the interventions on the number of new registrations, by initial registration status

	(1) All newly registered	(2) Not registered before	(3) Registered in another city before	(4) Registered at another address in this city before	(5) "Automatically" registered
Early Canvassing	0.014	0.01	-0.005	0.003	0.004
	(0.012)	(0.007)	(0.008)	(0.004)	(0.003)
Late Canvassing	0.031	0.006	0.012	0.01	0.004
	(0.012)**	(0.006)	(0.009)	(0.005)*	(0.003)
Early Home registration	0.032	0.012	0.01	0.007	0.004
	(0.013)**	(0.006)*	(0.009)	(0.005)	(0.003)
Late Home registration	0.054	0.022	0.02	0.008	0.004
	(0.013)***	(0.007)***	(0.008)**	(0.005)*	(0.003)
Early Canvassing & Late Home	0.06	0.035	0.015	0.007	0.005
registration	(0.013)***	(0.007)***	(0.009)*	(0.005)	(0.004)
Early Home registration & Late	0.096	0.047	0.032	0.013	0.002
Home registration	(0.014)***	(0.007)***	(0.009)***	(0.005)**	(0.003)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes
Apartment & Building controls	Yes	Yes	Yes	Yes	Yes
Observations	20458	20458	20458	20458	20458
R-squared	0.03	0.02	0.04	0.02	0.02
Mean in Control Group	0.168	0.047	0.079	0.025	0.013
Linear combinations of estimates:					
Average effect of all interventions	0.048	0.022	0.014	0.008	0.004
1/6 (EC + LC + EH + LH + EC&LH + EH&LH)	(0.008)***	(0.004)***	(0.006)**	(0.003)***	(0.002)*
Average effect of Canvassing	0.022	0.008	0.004	0.006	0.004
1/2 (EC + LC)	(0.010)**	(0.005)	(0.007)	(0.004)*	(0.003)*
Average effect of Home registration	0.043	0.017	0.015	0.007	0.004
1/2 (EH + LH)	(0.011)***	(0.005)***	(0.007)**	(0.004)*	(0.002)
Difference between average effect of Home reg. and Can.	0.021	0.009	0.012	0.001	0
1/2 (EH + LH) - 1/2 (EC + LC)	(0.011)*	(0.006)	(0.007)*	(0.004)	(0.003)
Difference between average effect of Late visit and Early visit	0.02	0.003	0.013	0.004	0
1/2 (LH + LC) - 1/2 (EH + EC)	(0.011)*	(0.006)	(0.007)*	(0.004)	(0.003)

Notes : Clustered standard errors are in parentheses. ***, **, * indicate significance at 1, 5 and 10%. We take the apartment as the unit of observation and include all newly registered citizens in the sample apartments (column 1), those who were not registered before (column 2), those who were registered in another city before (column 3), those who were registered at another address in the same city before (column 4) and those who were "automatically" registered (column 5).
We finally report point estimates and standard errors of linear combinations of the coefficients.

Table A4: Characteristics of newly registered citizens in apartments which opened their door for a late home registration visit

	(1)	(2)	(3)	(4)	(5)
	Individual characteristics			Apartment and building characteristics	
	Gender	Age	Born abroad	Number of names of citizens not registered	Number of mailboxes
Early Canvassing + Late Home registration	0.015	0.129	0.017	-0.049	-2.682
	(0.038)	(1.474)	(0.060)	(0.074)	(3.467)
Early Home registration + Late Home registration	0.005	0.372	-0.080	-0.045	-0.500
	(0.038)	(1.343)	(0.053)	(0.081)	(3.245)
Constant (Late Home registration)	0.444	37.066	0.384	1.375	19.884
	(0.025)***	(0.970)***	(0.038)***	(0.056)***	(2.626)***
Observations	692	692	688	692	692
R-squared	0.00	0.00	0.01	0.00	0.00

Notes: Clustered standard errors are in parentheses. ***, **, * indicate significance at 1, 5 and 10%. The sample includes all newly registered citizens living in apartments which opened their door at the late visit in the treatment groups "Late Home registration", "Early Canvassing & Late Home registration" and "Early Home registration & Late Home registration".
We consider individual characteristics (columns 1 through 3) as well as the number of names of citizens not registered initially found on the mailbox corresponding to the person's apartment and the total number of mailboxes and baseline registration rate at her address.

Table A5: Electoral participation of citizens by registration status, treatment group, and previous registration status

	(1)	(2)	(3)	(4)	(5)
	Presidential elections		General elections		Average on
	1st round	2nd round	1st round	2nd round	all rounds
Newly reg., previously not reg. x Any treatment (1)	0.012	-0.023	-0.061	-0.005	-0.020
	(0.021)	(0.019)	(0.034)*	(0.034)	(0.020)
Newly reg., previously reg. in another city x Any treatment (2)	-0.048	-0.040	-0.041	-0.038	-0.040
	(0.013)***	(0.015)***	(0.030)	(0.029)	(0.016)**
Newly reg., previously reg. at another address in this city x Any treatment (3	-0.001	-0.005	-0.029	-0.069	-0.023
	(0.030)	(0.025)	(0.047)	(0.046)	(0.027)
Newly reg., automatically reg. x Any treatment (4)	0.034	-0.019	-0.043	-0.016	-0.010
	(0.045)	(0.041)	(0.043)	(0.042)	(0.030)
Newly reg., previously not reg.	0.111	0.122	0.010	0.006	0.063
	(0.019)***	(0.017)***	(0.031)	(0.031)	(0.019)***
Newly reg., previously reg. in another city	0.182	0.153	0.098	0.062	0.123
	(0.011)***	(0.012)***	(0.026)***	(0.025)**	(0.014)***
Newly reg., previously reg. at another address in this city	0.129	0.128	0.134	0.155	0.133
	(0.027)***	(0.022)***	(0.041)***	(0.040)***	(0.024)***
Newly reg., automatically reg.	-0.108	-0.040	-0.154	-0.181	-0.121
	(0.039)***	(0.036)	(0.038)***	(0.037)***	(0.026)***
Previously reg., name not on mailbox	-0.184	-0.172	-0.117	-0.109	-0.145
	(0.007)***	(0.007)***	(0.008)***	(0.008)***	(0.006)***
Constant	0.764	0.782	0.485	0.466	0.624
	(0.004)***	(0.004)***	(0.005)***	(0.005)***	(0.004)***
Observations	33773	33772	33788	33754	33665
R-squared	0.05	0.05	0.02	0.01	0.04
Linear combinations of estimates:					
Av. difference between newly reg. in treatment gr. and control, controlling for	-0.011	-0.027	-0.046	-0.028	-0.027
previous reg. status (Weighted average of (1), (2), (3) and (4))	(0.011)	(0.011)**	(0.019)**	(0.019)	(0.011)**

Notes : Clustered standard errors are in parentheses. ***, **, * indicate significance at 1, 5 and 10%. We take the individual participation at a given electoral round as the unit of observation and include all previously registered citizens (registered before 2011) and all newly registered citizens (registered in 2011). For the former, we control fo whether the name was found on a mailbox at the corresponding address or not, as a proxy for the quality of registration (well- or mis-registered). Well- previously registered citizens are the omitted category. The sample size is slightly smaller than in Table 3 since we drop a few newly registered citizens whose previous registration status is unknown.

We estimate differences in the propensity to vote of newly registered citizens in the control and the treatment groups for each electoral round separately (columns 1 through 4) and for their average (column 5).

Newly registered citizens are included separately, according to their former registration status. We report the point estimates and standard errors of the weighted average difference between newly registered citizens in treatment and control groups, controlling for previous registration status.

Table A6: Number of votes cast over the 4 electoral rounds by registration status and treatment group

	(1) 0 vote out of 4	(2) 1 vote out of 4	(3) 2 votes out of 4	(4) 3 votes out of 4	(5) 4 votes out of 4
Newly reg. x Early Canvassing (EC)	0.004	0.018	-0.011	-0.017	0.006
	(0.012)	(0.013)	(0.023)	(0.021)	(0.025)
Newly reg. x Late Canvassing (LC)	-0.009	0.017	0.006	0.029	-0.042
	(0.011)	(0.015)	(0.028)	(0.023)	(0.028)
Newly reg. x Early Home registration (EH)	0.009	0.030	0.004	-0.021	-0.022
	(0.012)	(0.015)**	(0.023)	(0.022)	(0.025)
Newly reg. x Late Home registration (LH)	0.001	0.037	0.024	-0.004	-0.059
	(0.012)	(0.014)***	(0.023)	(0.021)	(0.025)**
Newly reg. x Early Can. & Late Home reg. (EC&LH)	0.008	0.014	0.005	-0.002	-0.025
	(0.014)	(0.012)	(0.024)	(0.022)	(0.026)
Newly reg. x Early Home reg. & Late Home reg. (EH&LH)	0.000	0.022	0.001	0.006	-0.030
	(0.011)	(0.013)*	(0.024)	(0.021)	(0.026)
Newly reg.	-0.093	-0.027	0.064	0.043	0.014
	(0.008)***	(0.008)***	(0.015)***	(0.014)***	-0.017
Previously reg., name not on mailbox	0.186	-0.007	-0.046	-0.048	-0.084
	(0.007)***	(0.004)*	(0.006)***	(0.005)***	(0.007)***
Constant	0.153	0.085	0.230	0.177	0.355
	(0.003)***	(0.002)***	(0.004)***	(0.003)***	(0.005)***
Observations	33789	33789	33789	33789	33789
R-squared	0.06	0.00	0.01	0.01	0.01
Linear combinations of estimates:					
Av. difference between newly registered in treatment gr. and control	0.002	0.023	0.005	-0.002	-0.029
1/6 (EC + LC + EH + LH + EC&LH + EH&LH)	(0.008)	(0.009)***	(0.017)	(0.015)	(0.019)
Av. difference between newly registered in Canvassing gr. and control	-0.002	0.017	-0.003	0.006	-0.018
1/2 (EC + LC)	(0.009)	(0.011)	(0.021)	(0.018)	(0.022)
Av. difference between newly registered in Home registration gr. and control	0.005	0.033	0.014	-0.013	-0.04
1/2 (EH + LH)	(0.010)	(0.012)***	(0.020)	(0.018)	(0.022)*
Av. difference between newly registered in Home reg. gr. and Can. gr.	0.008	0.016	0.017	-0.019	-0.022
1/2 (EH + LH) - 1/2 (EC + LC)	(0.010)	(0.012)	(0.019)	(0.017)	(0.020)

Notes : Clustered standard errors are in parentheses. ***, **, * indicate significance at 1, 5 and 10%. We take a dummy indicating whether the individual casted a total of 0 (or 1, 2, 3, 4) ballots at the 2012 elections as the outcome and include all previously registered citizens (registered before 2011) and newly registered (registered in 2011). For the former, we control for whether the name was found on a mailbox at the corresponding address or not, as a proxy for the quality of registration (well- or mis-registered).
We report point estimates and standard errors of linear combinations of the coefficients.

Chapter 2

Increasing the Electoral Participation of Immigrants - Experimental Evidence from France[1]

2.1 Introduction

The number of first and later generation immigrants continues to increase among the population of Europe (Eurostat 2011). As an increasing number of migrants settle down and become full-rights citizens, the question of their integration has gained dramatic importance (Givens 2007). Recent surveys show that immigrants have a low sense of national belonging. In France, only 47 percent of naturalized immigrants say that they strongly identify as French, which is all the more striking as they have spent sixteen years in the country at the time of naturalization on average (Haut Conseil à l'Intégration 2011). Similarly, only 63 percent of descendants born in France of two immigrants say that they strongly feel French, compared to 88 percent of the mainstream population (Simon 2012). This low sense of national belonging is reflected in immigrants' political participation, a

[1]This chapter is coauthored with Guillaume Liégey. We are very thankful to the Centre Maurice Halbwachs for providing access to the Trajectories and Origins survey, to www.meilleursagents.com for providing housing price data at the address level for all addresses of our sample and to the George and Obie Shultz Fund and MIT France for their generous funding. Esther Duflo, Benjamin Olken, Daniel Posner, Abhijit Banerjee, James Snyder, Todd Roggers, Stephen Ansolabehere, Rafaela Dancygier, Alan Gerber, and Benjamin Marx gave invaluable feedback about the study - we are grateful to them.

standard indicator of involvement in the collective decisions: immigrants and their descendants are much less likely to register on voter rolls and to participate in the elections than other citizens (Insee Premiere 2012).

Immigrants' low sense of belonging and political participation affects both their ability to improve their well-being by advancing political claims and social cohesion in the receiving society (Bloemraad et al. 2008). Growing tensions between an estranged youth of migrant origin and the police and other institutions have recently manifested themselves in major suburban revolts in several European countries (Lagrange and Oberti 2006; Koff and Duprez 2009). To address immigrants' low sense of belonging, it is important to understand the causes. Immigrants' low socioeconomic status, their lack of efforts or aptitude to integrate, and their rejection by the mainstream population are the three most common explanatory theories. We propose a fourth one: the paucity of outreach efforts targeting immigrants and emphasizing that they are full-rights citizens. This theory relies on the idea that integrating immigrants requires extending a hand to them, beyond simply fighting against their rejection and stigmatization.

To test this theory, one would ideally want to implement an intervention extending a hand to randomly selected immigrants and test whether their sense of belonging increases more than that of non-treated immigrants. However, this strategy is costly, as it relies on conducting large-scale baseline and endline surveys. Additionally, attitudinal questions have well-known weaknesses. We provide a more indirect but still powerful test of the theory using a civic outcome that is both a product and a strong indicator of sense of belonging, and that can be measured at smaller cost and with administrative data instead of self-reports: political participation. Elections are an important opportunity to build a sense of belonging and invite people to be full-fledged members of their civic community. In the four weeks leading to the French 2010 regional elections, members of the Socialist party went door-to-door canvassing in eight cities of the region surrounding Paris to encourage registered citizens to vote. Each of the 1,350 addresses in our sample of 23,836 citizens[2] was randomly allocated to the treatment group, which received the visits, or to the control group, which did not receive any visit. In the control group, immigrants' turnout did not greatly differ from native-born citizens, once other characteristics are controlled for. This provides some evidence that immigrants' socioeconomic status does affect their participation and their sense of belonging. However, canvassers' visits increased the turnout of immigrants by 3.9 percentage points

[2]Although most buildings were multi-apartment buildings, some individual houses are also included in the sample.

(13 percent) in the first round without significantly affecting native-born citizens. Our interpretation is as follows: the canvassers' visits enhanced a feeling of inclusion in the national community, which was initially lower among immigrants. As further support for this interpretation, we find that the impact of the visits on voter turnout is of similar size for very different groups of citizens of migrant origin: first generation immigrants from the three most common origins (Maghreb, Sub-Saharan Africa, and Asia), the second generation, and citizens born in French territories overseas. This is all the more striking as, using out-of-the-sample data (Trajectories and Origins 2008), we find that these subgroups differ substantially in terms of the strength of their connections with their country of origin and their degree of objective integration in French society. What they share, however, is a low subjective sense of national belonging. Finally, a series of robustness checks provides additional evidence that the results are not driven by other differences between immigrants and the mainstream population.

Unsurprisingly, the effect of the visits diminishes over time: one year later, at the 2011 cantonal elections, the voter turnout of immigrants in the treatment group was no longer higher than in the control group. This suggests that, among other policies, repeated and more intensive outreach efforts to immigrants are needed to durably increase their political participation and their integration.

To our knowledge, this study is the first that draws on heterogeneous effects of a voter mobilization intervention to assess the determinants of the political behavior of a subgroup of citizens. It builds on a large literature that compares the registration and voter turnout of immigrants and ethnic minorities to the mainstream population while controlling for other factors (e.g., Verba, Schlozman, and Brady 1995; Jackson 2003; Bevelander and Pendakur 2008; Maxwell 2010). It also fits in the "get-out-the-vote" experimental literature launched by Gerber and Green (2000), which has recently demonstrated the effectiveness of door-to-door canvassing to mobilize voters of ethnic minorities (e.g., Michelson 2005, 2006).

Our findings and conclusions might better extend to other European countries, with a recent history of immigration and integration policies similar to France (Joppke 2007), than to classic immigration countries, such as the United States. However, the largely different insights obtained when comparing voter turnout levels between different groups and comparing the effects of an identical encouragement to participate suggest a general methodological lesson. Exploring the heterogeneous effects of voter mobilization interventions usefully completes observational studies to assess the influence of factors such as immigrant origin and race on electoral participation and

integration. A generalization of this approach might be as productive as the current effort to refine multivariate regressions by controlling for an increasing set of explanatory variables.

The rest of the paper is organized as follows. Section 2 discusses existing evidence and theories on the integration of immigrants. Section 3 describes the context of the experiment and its design. Section 4 briefly outlines the data used in the analysis. Sections 5 presents the results and our interpretation. Section 6 discusses alternative interpretations. Section 7 concludes with a discussion of the validity of our findings for other countries in and outside of Europe and of the insights that can be derived from systematically exploring heterogeneous effects of voter mobilization interventions.

2.2 Existing Evidence and Theories on the Integration of Immigrants

2.2.1 Immigrants in France

France is the second largest European country in terms of foreign-born population. The first immigrants came from other European countries, but following World War II, the majority of immigrants were non-European, primarily from former French colonies in Maghreb, Sub-Saharan Africa, and Asia. Until recently, most immigration was motivated by people seeking work; however, work immigration to France was officially stopped after 1974 so that today's immigration is mostly fueled by family reunification (Weil 2005).

Only French citizens can vote. This study thus focuses on first generation immigrants who hold French citizenship[3], as well as people born in the DOM-TOM (the French territories overseas, including Réunion, Martinique, Guadeloupe, and Guyane) who migrated to Metropolitan France, descendants of two parents born abroad or in the DOM-TOM (the second generation) and descendants of one parent born abroad or in the DOM-TOM and one parent born in France (the generation 2.5). These citizens of migrant origin are compared to the mainstream population: citizens born in Metropolitan France of parents born in Metropolitan France.

[3]We use the United Nations definition of immigrants as people born abroad (United Nations 2006). This definition differs from the French definition of immigrant: a person born abroad without the French citizenship (INSEE 2005), which excludes 1.8 million people born French citizens abroad who migrated back to France: repatriates from former colonies such as the "pieds-noirs" from Algeria, and expatriates.

2.2.2 Competing Explanations for the Low Integration of Immigrants

Immigrants' low sense of national belonging has been explained thus far by their low socioeconomic status, their lack of efforts or aptitude to integrate, and their rejection by the mainstream population. We briefly review these three Theories and outline a fourth Theory, centered on the paucity of outreach efforts extending a hand to immigrants.

Theory 1: Socioeconomic Status

A first theory is that immigrants' low sense of belonging reflects compositional differences in terms of standard demographic, socioeconomic, and contextual factors, which also influence other citizens. In sum, the socioeconomic dimension of integration is the most important, it promotes participation and a sense of belonging (Marshall 1950)[4].

Extensive evidence shows that immigrants are more likely than the mainstream population to be uneducated, unemployed, and, conditional on being employed, to have a low-paying job. As a consequence, their median income is lower, they face a higher risk of poverty, and they are often segregated in suburbs dominated by social housing (e.g., Cour des comptes 2004; Beauchemin, Hamel, and Simon 2010; Eurostat 2011). Immigrants' descendants are still disadvantaged compared to the mainstream population and their situation is sometimes even worse than that of their parents (Maxwell 2009). It is only when immigrants' descendants have a mixed background, in the generation 2.5, that their disadvantage sometimes disappears.

To the extent that the socioeconomic dimension of integration leads its subjective aspect, improving immigrants' socioeconomic status will increase their sense of national belonging. Policies that try to improve immigrants' economic situation either provide specific aid to migrants, such as hostels for migrant workers, or they target areas and subgroups of the population where immigrants are overrepresented. Examples of French policies that fall into the latter category include special resources for education allocated to the "ZEP" (Zones d'Education Prioritaires, or Priority Education Areas) and subsidies and tax breaks provided to the "ZUS" (Zones Urbaines Sensibles, or Sensitive Urban Areas) (Calvès 2004).

We can assume that socioeconomic status and other standard demographic and contextual

[4]We define integration as the process through which immigrants become an integrated part of the collectivity by obtaining equal access to services and opportunities (including education, housing, and employment), and by adopting a common set of values and attitudes, in particular a sense of belonging to the national community conducive to political participation.

factors play some role in explaining immigrants' low sense of belonging. The relevant empirical question is whether they explain it entirely.

Theory 2: Lack of Efforts or Aptitude to Integrate

Beyond the socioeconomic status of immigrants, integration is often described as a two-way process, which requires cooperation by both immigrants and natives. Failure to adapt to this process by the former or the latter is highlighted as the reason behind immigrants' low sense of belonging by Theories 2 and 3, respectively. Theory 2 blames immigrants' lack of efforts or aptitude to integrate. Theory 3 blames the rejection of immigrants by French natives. While these Theories are analytically opposite, the mechanisms they describe can be reinforcing in practice.

Supporting Theory 2, qualitative studies have documented the existence of a strong Muslim communitarianism in some French suburbs, leading to the contestation of French values such as secularism and gender equality (e.g., Kepel, Arslan, and Zouheir 2011). More generally, the low frequency of inter-ethnic marriage and the connections that many immigrants and their descendants maintain with their country of origin are often interpreted as a sign that their desire to integrate into French society is low (e.g., Haut Conseil à l'Intégration 2011)[5]. Some politicians have repeatedly questioned the desire of some groups of immigrants to assimilate (Weil 2005) and close to half of the mainstream population believe that people of immigrant origin do not give themselves the means to integrate (Brouard and Tiberj 2011). Beyond lack of efforts to integrate, the lack of aptitude is also a possible factor, either due to language barriers or to the spatial concentration of immigrants in disadvantaged neighborhoods. Accordingly, several European countries have recently enforced mandatory courses with language instruction and civic education, and sought to increase diversity in the attribution of social housing (Joppke 2007; Haut Conseil à l'Intégration 2011).

[5]In a vision that sees identity as a zero-sum game, the lack of allegiance to France is inferred from maintained allegiance to another country or ethnic group (Simon 2012). Evidence that indirectly supports this view includes the relatively less stringent residence, language, and fee requirements to be naturalized in France compared to other European countries (Goodman 2010), which might lead to instrumental acquisition of French citizenship; the increased prevalence of dual citizenship and the important shares that still define their origin according to a different country (Simon 2012); the strength of the links maintained by some immigrants and their descendants with their countries of origin, including trips back to this country, investment, and remittances (Beauchemin, Lagrange, and Safi 2010).

Theory 3: Rejection by the Mainstream Population

Opposite to Theory 2, the third explanation of immigrants' low sense of belonging attributes it to their rejection by the natives. In France, 59 percent of immigrants believe that above all, it is the French society that does not give people of different origin the means to integrate (Brouard and Tiberj 2011). 45 percent of naturalized immigrants and 36 percent of their descendants feel that natives do not regard them as being French (Simon 2012).

The rejection of immigrants and their descendants often originates in racism[6]. This rejection takes two distinct forms - isolated acts of discrimination, and organized political opposition - which produce different consequences for immigrants' integration. First, in the current context of high unemployment, prejudice against immigrants in Europe has increased (e.g., Strabac and Listhaug 2008; Kaya and Karakoc 2012) and evidence of discrimination on the job and housing markets as well as anti-immigrant violence is abundant (e.g., Hubbuck and Carter 1980; Witte 1995; Pettigrew 1998; Klink and Wagner 1999; Amadieu 2004; Beauchemin, Hamel, and Simon 2010; Adida, Laitin, and Valfort 2010). These isolated acts of discrimination can estrange immigrants from the national community. They have resulted in the generalization of antidiscrimination laws (e.g., Council Directive 2000/43/EC), and the creation of agencies responsible for overseeing the application of these laws (e.g., the Commission for Racial Equality in Britain or the HALDE in France).

Second, the anti-foreigner sentiment has also found a political expression: it has been captured by far-right anti-immigration parties, such as the Front National in France or the Freiheitliche Partei Österreichs in Austria (Semyonov, Raijman, and Gorodzeisky 2006) which have been pushing, sometimes successfully, for formal legislative barriers to integration. For instance, several European countries have recently strengthened the language and country-knowledge requirements for settlement, naturalization, and immigration (Joppke 2007). Similarly, ID controls by the police have been generalized in several countries and they often disproportionately target people of migrant origin (Jobard et al. 2012), possibly contributing to a feeling of estrangement.

[6]While mainstream French population is predominately White, 70 percent of immigrants were born in a non-EU-27 country (Weil 2005; Eurostat 2011). Except for the repatriates and returned expatriates, the large majority of citizens with a migrant origin thus belong to visible ethnic minorities: Arabs, Blacks (from Sub-Saharan Africa and the DOM-TOM), and Asians.

Theory 4: Lack of Outreach Efforts Extending a Hand to Immigrants

Following Theory 3, the responsibility of the receiving societies in the integration process is often restricted to preventing immigrants' rejection. As Joppke (2007) puts it, "in shifting the burden of adjustment from the migrant to the receiving society, antidiscrimination is society's distinct share in the two-way process of immigrant integration".

Beyond preventing discrimination, we argue that receiving societies could foster immigrants' integration by increasing their outreach efforts, which extend a hand to immigrants, present them as full-rights citizens, and highlight their contribution to the national history and culture. The current paucity of such outreach efforts could contribute to immigrants' low sense of belonging.

Policies extending a hand to immigrants include allowing non-European immigrants to vote in local elections (as is the case in Belgium and the Netherlands); highlighting the role played by immigration in the country's history and citing examples of successful immigrants during the naturalization ceremony; introducing national holidays related to immigration, such as St Patrick's Day or Columbus Day in the United States; celebrating immigrants' contributions to the common national history through museums such as the French Cité nationale de l'histoire de l'immigration; teaching major languages spoken by immigrants at school; making administrative procedures available not only in the national language, but also in other languages[7]; and recruiting more immigrants as public officials. In addition, more financial means could be granted to existing initiatives fostering social and intercultural mediation, such as the French "agents in charge of local developments for integration" ("agents de développement locaux pour l'intégration") and local NGOs of "intermediation women" ("femmes-relais"), which welcome their neighbors and help them through administrative procedures (Madelin 2007). The difficulty when studying these policies is that their outcomes are not entirely clear and difficult to measure.

We address this difficulty by studying a clearer outcome: political behavior. Elections are an important moment to build a sense of belonging and to invite people to be a full-fledged part of the civic community. We show that outreach efforts conducted in the context of an electoral campaign are successful at increasing immigrants' participation. We argue that this effect is obtained by increasing their sense of national belonging, thus raising their expressive benefits of voting[8].

[7]In the United States, voter registration forms are typically available in many different languages. In France, they are only available in French.

[8]The expressive dimension of voting is the object of a vast literature, which argues that voting expresses allegiance to and efficacy in the political system, compliance with the norms of the country, including civic duty, and interest

2.3 The Experiment

2.3.1 The Context of the Experiment

The intervention took place during the campaign for the regional elections in Ile-de-France, the region including and surrounding Paris[9]. Regional elections are list polls with two rounds. The head of the list that receives the highest share of votes at the second round becomes president of the region. This list also receives 25 percent of the seats at the regional assembly, while the remaining 75 percent are divided proportionally between all lists that made it to the second round.

At the national level, the 2010 elections were characterized by a relatively low turnout (46 percent and 51 percent of the registered citizens for the first and second rounds which took place on March 14th and 21st) and by a large victory of the Socialist party, which won 21 of the 22 metropolitan regions. In Ile-de-France the participation was also low. The Socialist party gathered 25 percent of the votes at the first round and merged its list with two other left-wing lists for the second round. The merged list received 57 percent of the votes, enabling Jean-Paul Huchon, the Socialist candidate and incumbent, to be reelected as president of the region[10]. Cantonal elections took place one year later, in March 2011[11]. Four of the eight cities, which accounted for 70 percent of registered citizens in our sample, participated in these elections. Similarly to the 2010 elections, they were characterized by a low turnout (45 percent in both rounds) and by the overall domination of left-wing parties. Individual turnout data was again collected at these elections to compute the middle run impact of the intervention.

in the collective debates and decisions (e.g., Campbell et al. 1960; Wolfinger and Rosenstone 1980; Conway 1991; Teixeira 1992; Rosenstone and Hansen 1993).

[9]French regions were created in 1982 to decentralize some of the power concentrated in Paris. Their attributions include social housing, high schools, transportation, environment, town and country planning, aids to firms, continuous training, and research. Each region is headed by a president and has a regional assembly, both elected for six years.

[10]Despite this clear-cut victory, the outcome of the election in Ile-de-France had been uncertain during most of the campaign: on the left of the political spectrum, before the first round, "the Greens" emerged as a powerful third force and threatened to receive a higher share of the votes than the Socialist party. It eventually received 17 percent of the votes. On the right side of the political spectrum, Valérie Pécresse, the Minister for Higher Education and Research minister at that time, was heading the list of the "UMP", the party of President Nicolas Sarkozy, which held more than half the seats at the National Assembly.

[11]The general councils of departments (the administrative unit below the region) whose members are chosen by these elections, have fewer competences than the regional councils. They are responsible for middle schools, solidarity programs, leisure, and town and country planning, among a few other attributions. Council members are elected for six years; every three years, half of the cantons of each department are called to the polls.

2.3.2 The Intervention

The door-to-door canvassing visits took place in the evenings and on Saturdays during the 4 weeks before the 2010 election. Canvassers knocked on doors in groups of two. They came from various socioeconomic backgrounds and ethnic origins, but with an underrepresentation of the poor working class and ethnic minorities. They were active members of the Socialist party and were not compensated for their participation in the experiment. Only a few of them had preexisting experience of door-to-door canvassing and all received a training including role-playing.

The canvassers knocked at all doors of the buildings in the treatment group. When the door opened, the discussion lasted for one to five minutes in most cases. Canvassers gave basic information about the election day, the location and opening times of the poll office, and past and planned projects of the region in the corresponding neighborhood. They further encouraged people to vote, and to choose the list of the Socialist party. At the end of the discussion, they gave their interlocutor a leaflet summarizing the program of this list. When no one opened, this leaflet was left at the door.

We can be confident that the control group did not receive any visits from canvassers of other political parties. Until the massive door-to-door campaign led by the Socialist party for the 2012 presidential elections, French political parties did not use door-to-door canvassing as a major campaign strategy. Instead, it was limited to small-scale local initiatives. In this particular case, neither the canvassers nor the households visited reported other visits.

2.3.3 Sampling Frame

A total of 1,350 buildings hosting 23,836 citizens registered on the voter rolls were included in the experiment. The buildings are located in eight cities, which were chosen based on two criterions: low political turnout at previous elections, and interest of the Socialist party's local unit in the experiment[12]. In each city, the sample is restricted to the polling stations characterized by the lowest historical electoral participation.

The sample population primarily lives in the "banlieues", suburban neighborhoods which face an important set of interconnected economic and social challenges, including poverty, housing decay,

[12]Cities in the experiment are: Sevran, Villetaneuse, Pierrefitte (in the department 93, Seine-Saint-Denis), Montrouge, Bagneux, Malakoff (in the department 92, Hauts-de-Seine), Domont (in the department 95, Val d'Oise), and the 11th arrondissement of Paris.

low employment rates, high criminality, and poor educational achievement. These neighborhoods are marked by increased internal tensions, notably between the youth and the police, and a widening gap with the rest of the country, including low voter turnout. Overall, the sample is neither representative of Ile-de-France nor of the entire country, but it is quite representative of these banlieues.

2.3.4 Experimental Design

Each building in the sample was allocated randomly to the control group, which did not receive any intervention, or the treatment group, which received the visits of canvassers. All citizens living in a given building thus belonged to the same group by design[13]. Before randomly allocating the buildings between the treatment and control groups, we stratified them by street and size to ensure balance of the two groups[14].

Between the first and second rounds, in one city, canvassers continued to cover treatment group buildings that they had not covered before the first round[15].

2.4 The data

2.4.1 Experimental Data

Voter rolls

Voter rolls for the 8 cities included in the sample indicate the polling station, address, gender, place and date of birth of all registered citizens[16].

Monitoring spreadsheets

[13]Conducting the randomization at the apartment level would have increased our statistical power, but it was infeasible. Indeed, most registered citizens do not indicate their apartment number on the voter rolls.

[14]In fact, the streets included in our sample differ from one another on several dimensions correlated to our outcome, including past participation and vote shares historically obtained by the Socialist party. Moreover, the size of a building, proxied by the number of registered citizens living in it, is a good indicator of socioeconomic status: in the areas included in the sample, big buildings often contain social housing, and households living there are poorer, on average, than those living in residential areas.

[15]Some buildings were cross-randomized to receive a second visit between the two rounds, creating four groups: buildings which received either no visit, a visit before the first round only, a visit between the two rounds only, or two visits. Unfortunately, only 84 buildings hosting 2,145 registered citizens could be integrated in this second randomization, since the two rounds were separated by one week only. This very small sample limits the precision of the comparisons we can draw between the impact of one vs. two visits, and one visit before the first round vs. one visit between the two rounds; thus, we do not report these estimates.

[16]Importantly, race is unknown, due to the ban of ethnic statistics in France (Conseil Constitutionnel 2007).

Canvassers were asked to report the date and number of doors knocked and opened for each building covered[17]. Overall, they knocked at 9,070 doors and 4,432 (48.9 percent) opened[18].

Individual turnout

In France, each voter who participates at an election signs an attendance sheet. These sheets are available for consultation by any registered French citizen up to ten days after the election. This administrative data enables us to measure the actual voting behavior of all registered citizens in our sample without any bias, unlike survey reports, which are often unreliable when it comes to voter turnout (Ansolabehere and Hersh 2011). We collected individual turnout data for the two rounds of the 2010 regional elections and the two rounds of the 2011 cantonal elections.

Post-electoral survey

A post-electoral survey was administered over the phone on a subsample of registered citizens whose phone number could be found in the phonebook. All respondents were surveyed within two months after the regional elections. The questionnaire was administered in five minutes or less and included questions about socioeconomic status and political competence.

892 (24 percent) of the people called responded, among whom 839 completed the entire survey. The pool of people who were called, and the pool of respondents, was not randomly drawn from the entire pool of registered voters: their participation is higher, and they live disproportionately in Sevran (46 percent, compared to 31 percent for the entire sample)[19].

Additional sources of data

Using Google Maps, we measure and control for the distance between a person's home and polling station. In addition to this, we obtained housing price data at the building level from the real estate company www.MeilleursAgents.com, which we use as a proxy for socioeconomic status. Finally, we identified all sample buildings included in a ZUS or a ZRU ("Zone de Redynamisation Urbaine")[20]. ZRU are ZUS that benefit from additional fiscal exemptions.

[17]To avoid imposing too many constraints, we did not ask the canvassers to report which pair covered which building.

[18]The ratio of opened doors varies from 38.2 percent in Montrouge to 55.4 percent in Sevran. Although it is difficult to precisely evaluate the relative importance of the different reasons why doors do not open, the major reason by far is that no one was at home during the canvassers' visit. Other reasons include children being temporarily alone at home and distrust.

[19]Conversely, Montrouge and Villetaneuse are underrepresented among the respondents to the survey due to the specific order in which the surveying team received the lists of phone numbers to call in the different cities. They were asked to conduct 900 surveys and stopped when they achieved this goal.

[20]The atlas of ZUS and ZRU is available at http://sig.ville.gouv.fr/Atlas/ZUS/.

2.4.2 Identification of the Immigrants and Their Descendants

We identify the first generation immigrants and people born in the DOM-TOM based on the information on place of birth available in the voter rolls. Using city level census data (Recensement de la population 2009), we find that, in the sample cities, 75 percent of the foreign-born were born with a foreign citizenship and 25 percent were born French[21]. We are unable to separate these two groups of immigrants, since we only know the place of birth (and not the citizenship at birth) of registered citizens.

The voter rolls further enable us to reconstruct households and identify descendants born in France of immigrants. We assume that citizens sharing the same last name (either as their last name or marital name) and living at the same address belong to the same household, and that two individuals of the same household who were born more than 15 years apart are from different generations[22]. This method allows us to identify the subset of immigrants' descendants registered to vote who live with their parents and whose parents are registered. It misses those who do not live with their parents or whose parents are not French citizens or not naturalized. We count someone who lives with at least one citizen of an older generation as second generation if all the older generation citizens were born abroad or in the DOM-TOM; and as generation 2.5 if one of these older generation citizens was born abroad or in the DOM-TOM, but another one was born in France.

2.4.3 The Survey "Trajectories and origins"

The survey "Trajectories and Origins" was conducted by INSEE and INED between September 2008 and February 2009 on a representative sample of people living in Metropolitan France and aged between 18 and 60 years old. Its comprehensive questionnaire was designed to better understand the social trajectories and living conditions of immigrants and their descendants by comparing them to the mainstream population (Trajectories and Origins 2008; Beauchemin, Hamel, and Simon 2010).

We use this survey data to compute statistics on outcomes such as sense of belonging that are

[21] The situation of the latter group is intermediate: mostly White, like members of the non-migrant mainstream population, they were born abroad and migrated to their current country of residence, like other immigrants.

[22] This technique is not perfect: in a few cases, for very common names, we might wrongly allocate two people with the same name to the same household when they actually live in the same building but in two different apartments. More importantly, we misallocate household members who share neither their last name nor their marital name to different households. Respondents to our post-electoral survey reported living with 1.1 other registered voters, on average, (for an average household size of 2.1), when we estimate their household size to be of 1.9 registered voters, on average. This small difference shows that our method, although imperfect, is relatively satisfying.

relevant to the interpretation of our experimental results. We conduct these analyses on a sample of 4,560 respondents as close as possible to our experimental sample: French citizens who live in Ile-de-France and report that they are registered to vote.

2.4.4 Verifying Randomization

Table 1 presents summary statistics separately for the control and treatment groups. We 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, registered citizens in the two groups are extremely similar, as was expected. Out of 51 differences shown in Table 1, only 4 are significantly different from zero at the 10 percent level, and only 3 are significant at the 5 percent level[23].

Slightly more than half of the registered citizens in our sample live in cities of the department 93 that are famous for high crime rate and underprivileged neighborhoods. Overall, more than a third live in a ZUS. The average housing price is nonetheless relatively high, due to the proximity of Paris. Buildings with multiple apartments dominate individual houses by far, and people can count, on average, 49 other registered citizens living at the same address.

The average registered citizen has only to walk 270 meters to reach his polling station. He is 44 years old and lives with one other registered citizen. 45 percent of the registered citizens are males, 22 percent were born abroad, 8 percent in the DOM-TOM, 8 percent can be identified as the second generation and 1 percent as the generation 2.5. Among the foreign-born, 87 percent were born in Maghreb, Sub-Saharan Africa, and Asia (mostly in former French colonies).

The rates of reply and survey completion of the post-electoral survey in the control and treatment groups are identical. Among the respondents, 45 percent do not have the baccalaureate (French end-of-high-school diploma). 59 percent are employed workers and 11 percent (more than the national average) report being unemployed. Intermediate jobs and employees largely dominate other types of activities.

[23]There are slightly more males in the treatment group. Among respondents to the post-electoral survey, the treatment group contains relatively more students than the control group, and the social occupations are distributed unevenly between the two groups.

2.5 Main results

2.5.1 Comparing Turnout *Levels*

We first measure the difference in voter turnout levels between immigrants and native-born citizens by estimating the following OLS regression:

$$Y_{i,b} = \alpha_1 + \theta_1 I_{i,b} + \epsilon_{i,b} \quad (1)$$

where $Y_{i,b}$ is turnout of individual i living in building b and $I_{i,b}$ is a dummy equal to 1 if the person is an immigrant. We restrict the sample to citizens in the control group to avoid contaminating the estimation with the effect of the treatment. In this and in all remaining regressions, we adjust the standard errors for clustering at the building level. We test the null hypothesis that $\theta_1 = 0$.

Part of the turnout differences between immigrants and native-born citizens can be driven by differences in their demographic and socioeconomic characteristics or the neighborhoods where they live. Indeed, as shown in Table 3, immigrants are more likely than native-borns to live in a ZUS (47 percent vs. 31 percent), they are poorer and older. Higher shares of immigrants are males, and they live in bigger households[24].

To account for such differences, we estimate specifications of the form in Equation [2]:

$$Y_{i,b} = \alpha_2 + \theta_2 I_{i,b} + X_b^{'}\gamma_2 + Z_{i,b}^{'}\delta_2 + \epsilon_{i,b} \quad (2)$$

where X_b is a vector of building characteristics (its stratum, which captures the city and street; its housing price, size, distance to the polling station, and whether it is located in a ZUS), and $Z_{i,b}$ is a vector of individual characteristics (age, gender, household size, whether the individual was born in the same city, in the same department, and in the same region as the ones where he currently lives)[25]. Again, we test the null hypothesis that $\theta_2 = 0$.

Finally, the group of native-born citizens is heterogeneous: in addition to members of the

[24]Table 3 also lists differences on variables measured by the post-electoral survey. Although we cannot control for them in the regressions run on the entire sample, they are informative. We comment on these variables in Section 6.

[25]These variables capture people's demographics and the context well, but they proxy only imperfectly for their socioeconomic status, which is an equally important determinant of turnout (e.g., Campbell et al. 1960; Verba and Nie 1972; Leighley and Nagler 1992; Rosenstone and Hansen 1993). Income is proxied at the building level by housing price, and we only observe education, occupation, and employment status for the respondents to the post-electoral survey and thus cannot control for these variables in the regression.

mainstream population, it includes people born in the DOM-TOM, the second generation and the generation 2.5. Thus, in addition to $I_{i,b}$, we finally include three additional dummies for the three other groups of citizens of migrant origin: citizens born in the DOM-TOM, the second generation, and the generation 2.5.

2.5.2 Low Turnout of Both Immigrants and Native-Born Citizens Absent the Canvassing Visits

We first estimate Equations [1] and [2] using voter turnout at the first round of the 2010 regional elections as the outcome (Table 2, Panel A). Only 34.2 percent of the registered citizens in our sample participated in this election. This turnout is significantly lower than the regional average (43.8 percent), consistent with the choice of polling stations with a relatively low turnout history.

Absent any control, the participation of immigrants does not significantly differ from native-born citizens (column 1). Controlling for building characteristics, it is higher (column 2), but this difference disappears when we add individual controls (column 3).

We then include additional dummies for the other groups of citizens of migrant origin, so that each of these groups as well as immigrants are compared to members of the mainstream population (columns 4 to 6). Controlling for building and individual characteristics, we find that the participation of immigrants is lower than the mainstream population by 5.2 percentage points, a difference significant at the 1 percent level (column 6). Turnout of citizens born in the DOM-TOM is even lower, by 9.3 percentage points. Finally, absent any control, the second generation had the lowest turnout: 15.3 percentage points below the mainstream population (column 4). This difference, however, is mostly driven by their younger age and other characteristics: controlling for them, it is no longer statistically significant (column 6). Similarly, the voter turnout of generation 2.5 is not significantly different from the mainstream population.

Most of these patterns hold for the second round of the 2010 elections (Table 2, Panel B), where turnout was, on average, slightly higher (37.8 percent for a regional average of 47.1 percent), with one major difference: all building and individual characteristics equal, the participation of immigrants at the second round is not significantly different from members of the mainstream population (column 6). In other words, the increase in turnout between the first and second rounds is higher for the former than for the latter.

As shown in Panels C and D of Table 2, even fewer people participated in the 2011 cantonal elections (26.2 percent and 29.1 percent at the first and second rounds)[26]. As in 2010, all building and individual characteristics equal, and compared to the mainstream population, the participation of immigrants is significantly lower for the first round only; the participation of citizens born in the DOM-TOM is lowest and significantly lower for both rounds; and the participation of the second generation and of generation 2.5 is not significantly different for either round (Table 2, Panels C and D).

These patterns are similar to preexisting findings, which show that the lower electoral participation of immigrants mostly reflects the influence of other standard predictors of turnout (e.g., Maxwell 2010)[27]. They tend to support the idea that immigrants' low turnout and their low sense of belonging reflect their low socioeconomic status (Theory 1).

2.5.3 Comparing the *Effects* of the Intervention on Turnout

We next turn to estimating the impact of receiving a visit by canvassers on electoral participation with the following OLS regression:

$$Y_{i,b} = \alpha_3 + \beta_3 Visited_b + X_b^{'}\gamma_3 + Z_{i,b}^{'}\delta_3 + \epsilon_{i,b} \quad (3)$$

where $Visited_b$ is a dummy variable equal to 1 if building b received the visit of canvassers. $Visited_b$ is instrumented with T_b, a dummy equal to 1 if the building was allocated to the treatment group[28].

Beyond the average treatment effects, we estimate the treatment effects separately for immigrants and native-born citizens with the following equation:

$$Y_{i,b} = \alpha_4 + \beta_4 Visited_b + \theta_4 I_{i,b} + \lambda_4 Visited_b \times I_{i,b} + X_b^{'}\gamma_4 + Z_{i,b}^{'}\delta_4 + \epsilon_{i,b} \quad (4)$$

[26]This difference is partly, but not entirely, accounted for by the restriction of the sample to the 4 cities where cantonal elections were held: turnout was already relatively lower in these cities than in the rest of the sample for the regional elections.

[27]Maxwell uses turnout data for three 2004 French elections and finds that, controlling for socioeconomic characteristics and for the type of neighborhood, immigrants' electoral participation is not different from citizens born in Metropolitan France and that it is higher than people born in the DOM-TOM.

[28]Due to a lack of time, the canvassers did not cover some buildings in the treatment group, and a few buildings in the control group were covered by mistake, with a first stage of 0.86. These two sources of difference between treatment group and actual treatment received are not particularly interesting, so that the "intention to treat" effect does not have any interest per se in this case and we only report the "treatment-on-the-treated" effect.

where $Visited_b$ and $Visited_b \times I_{i,b}$ are instrumented by T_b and $T_b \times I_{i,b}$. In this specification, β_4 and $\beta_4 + \lambda_4$ estimate the impact of receiving the visit of canvassers for native-borns and immigrants respectively.

The above specification might mistakenly attribute heterogeneous treatment effects to being an immigrant, which really come from differences in other characteristics correlated to it. We thus consider a last specification, in which we allow for heterogeneity in the treatment effects by other dimensions than place of birth. We run the following equation:

$$Y_{i,b} = \alpha_5 + \beta_5 Visited_b + \theta_5 I_{i,b} + \lambda_5 Visited_b \times I_{i,b} + W_{i,b}^{'} \rho_5 + Visited_b \times W_{i,b}^{'} \tau_5 + X_b^{'} \gamma_5 + Z_{i,b}^{'} \delta_5 + \epsilon_{i,b} \quad (5)$$

where $W_{i,b}$ is the vector of characteristics along which we allow for heterogeneity in the treatment effects. $Visited_b$, $Visited_b \times I_{i,b}$ and $Visited_b \times W_{i,b}^{'}$ are instrumented by T_b, $T_b \times I_{i,b}$ and $T_b \times W_{i,b}^{'}$.

2.5.4 Interpretation of the Magnitude of β_3, β_4 and $\beta_4 + \lambda_4$

Our estimates of the impact of the intervention should account for the fact that canvassers were not able to interact with all citizens living in the treated buildings, but only with those living in apartments which opened their door. Moreover, when comparing the impact obtained on different groups of citizens, we need to distinguish whether the difference results from various behavioral responses, or from a different door-opening rate.

β_3 estimates the average impact of the visits on citizens living in a treated building. It is equal to $\beta_3^{'}$, the impact on citizens living in apartments which opened their door times the proportion of such citizens among all citizens living in treated buildings[29]. This proportion is not necessarily equal to the proportion of doors opened since the relative household size of households that opened and did not open their door might differ. In the post-electoral survey, members of larger households were more likely to say that their household did not receive the canvassers' visit during the campaign. If anything, the fraction of opened doors (48.9%) is thus an upper bound on the proportion of citizens living in households who opened their door. We can therefore derive an upper bound for $\beta_3^{'}$: $\beta_3^{'} \geq \frac{\beta_3}{0.49}$. Moreover, $\beta_3^{'}$ is a lower bound for $\beta_3^{"}$, the impact of the visits on the people who

[29] According to the results of the post-electoral survey, the visits did not increase the share of people who recall finding a leaflet on their door. Therefore, the impact of leaving a leaflet on doors that did not open is allegedly very small.

were present at the time of the visit and interacted with the canvassers, by difference with members of the household who were absent and were only affected through within-household spillovers[30]: $\beta_3^{"} \geq \beta_3^{'} \geq \frac{\beta_3}{0.49}$.

The same reasoning applies to the interpretation of β_4, the average impact of the visits on native-born citizens living in a treated building and $\beta_4 + \lambda_4$, the impact on immigrants. The door-opening rate of households with immigrants (or with native-born citizens) is unknown, but it can be estimated as follows: for each building, we compute the proportion of doors that opened and the proportion of households with at least one immigrant. We then regress the first on the second and find that, in buildings with 100 percent of immigrants, 59 percent of doors would have opened. Therefore,$(\beta_4 + \lambda_4)^{"} \geq (\beta_4 + \lambda_4)^{'} \geq \frac{\beta_4 + \lambda_4}{0.59}$.

2.5.5 Differential Impact of Canvassing on Immigrants

The results of Equation [3] are presented in Table 4. We find that door-to-door canvassing did not significantly increase overall participation, neither in the first round (Panel A) nor in the second round (Panel B) of the 2010 elections[31]. This finding is robust to the inclusion of individual and building controls.

We then run Equation [4] to estimate the treatment effects separately for native-born citizens and immigrants. The results are shown in Table 5. The intervention had a large impact on the latter: it increased their participation in the first round of the 2010 regional elections by 3.9 (-0.5 + 4.4) percentage points (Panel A, column 2). This estimate is statistically significant at 5 percent. It is robust to the inclusion of strata fixed effects and of building and individual control variables (column 4). Conversely, the impact on native-born citizens is very small, negative, and not significant at the standard levels.

Taking into account the estimated door opening rate of households with immigrants, canvassing increased the first round participation of immigrants who live in an apartment which opened its door by 6.6 percentage points ($^{3.9}/_{0.59}$). Since their participation was 33.8 percent in the control group, we conclude that the intervention mobilized approximately one in ten foreign-born abstentionists

[30] We cannot measure the size of these within-household spillovers, as canvassers kept track of the door-opening rate for each building, but did not maintain a list of apartments that opened their door and of the people with whom they spoke. This would have required costly preliminary work, to draw maps with apartment doors and numbers in all buildings and to match inhabitants with their apartment number.

[31] The sample is slightly smaller in Panel B because we drop the strata in which buildings were covered a second time after the first round to ensure that our results can be interpreted as the impact of a single visit.

among those that were actually reached by the canvassers.

Panel B shows the same estimates, but for the second round of the regional elections. Again, the impact of canvassing on immigrants' turnout is positive, but its magnitude is lower (2.6 percentage points when including the control variables), and it is not statistically significant. This difference between the first and the second rounds might reflect the fact that, absent any visit, a relatively larger number of immigrants only vote at the second round, as discussed in Section 5.2. A fraction of those mobilized by the visits to participate in the first round probably belong to this group of mild abstentionists.

The heterogeneous effect of canvassing on turnout at the first round and, to a lesser extent, at the second round, might mistakenly attribute to being an immigrant a result which actually originates in other characteristics correlated to this variable. First, perhaps what matters is not the fact that immigrants were born outside of France, but simply that they were born outside of the region Ile-de-France. It could be that the intervention affected all people born outside of the region by helping them to bridge a knowledge or identity gap. To test this hypothesis, we allow for heterogeneity in the treatment effect by place of birth. However, the inclusion of three dummies interacted with $Visited_b$ that indicate birth in a different city, department, or region, does not alter our estimate of the differential impact of canvassing on immigrants (Table 6, Panel A, column 2).

In columns 3 to 6 of Table 6, Panel A, we allow the treatment effect to be heterogeneous along other dimensions on which immigrants differ from native-born citizens. None of them are associated with statistically significant heterogeneous effects. On the contrary, the differential effect obtained on immigrants is remarkably consistent across all specifications, both in magnitude (between 4.9 and 6 percentage points) and in statistical significance. In column 7, we allow for heterogeneity by all these dimensions simultaneously, and measure a consistent differential effect of 5.7 percentage points, significant at the 5 percent level. The estimate of the differential effect of canvassing on the participation of foreign-born citizens in the second round is also consistent with allowing for heterogeneity of the treatment effect along these dimensions, although smaller and at the limit of statistical significance (Panel B).

These findings are at odds with the expectation that the effect of door-to-door canvassing depends on subjects' baseline propensity to vote (Niven 2004; Arceneaux and Nickerson 2009), which is similar among immigrants and native-born citizens. Our interpretation is as follows: absent the intervention, the comparably low participation of immigrants and native-born citizens, after

controlling for observable characteristics, reflects different unobserved determinants. In particular, the low turnout of immigrants is in part the result of their low sense of national belonging. The canvassers were seen as representatives of one of the two major national political organs; by going all the way to visit their targets and ask them to vote, they enhanced a feeling of inclusion in the national community. This mechanism only affected immigrants, and not the native-born citizens who already had a high sense of belonging, even absent the visits.

2.5.6 Lack of Any Impact of Canvassing One Year Later

We next turn to evaluating the impact of canvassing one year later, at the 2011 cantonal elections. Our sample is now restricted to the four cities in which cantonal elections were held in 2011: Montrouge, Pierrefitte-sur-Seine, Sevran and Villetaneuse. These four cities account for 68 percent of the entire sample.

The results are shown in Table 7. We find that the impact of the canvassers' visits on voter turnout at the cantonal elections is close to zero and not significant for either immigrants or native-born citizens: the effect of the short interaction that mobilized some immigrants for the elections immediately following the intervention rapidly decays over time.

This result differs from Gerber, Green, and Shachar (2003), who find a persistent effect of voter mobilization interventions, but it is not inconsistent with our interpretation of the impact on turnout at the 2010 regional elections: in the absence of repeated and more intensive outreach efforts, it is not surprising that the canvassers' visits did not increase immigrants' sense of national belonging in the long-run.

2.5.7 Tests of additional implications of the theory

We now test additional implications of the theory to build further confidence that the short-run impact of the visits on immigrants' turnout was obtained by increasing their sense of belonging. If this interpretation is accurate, the intervention should have increased turnout of all groups of citizens with a low sense of national belonging, and they should have left all groups of citizens with a high sense of belonging unaffected. To identify the groups that fall in these two categories, we use data from the Trajectories and Origins survey for respondents living in Ile-de-France.

We first find that the sense of national belonging is lower for immigrants of all three broad

origins, Maghreb, Sub-Saharan Africa, and Asia, compared to the mainstream population (Table 8, column 19). If our interpretation of the mechanism underlying the visits' impact is correct, we should thus expect the visits to have increased the participation of all three subgroups. We test this hypothesis by running Equation [4] and separating immigrants by broad origin (Table 9). Columns 1 and 3 are the same as columns 2 and 4 of Table 5 and were included for reference only. As expected, the impact of canvassing on turnout at the first and second rounds is large and of similar size for citizens born in Maghreb, Sub-Saharan Africa, and Asia. It is only significant for the first (and largest) group, due to the small sample size.

We next consider others groups of citizens of migrant origin. We find that the sense of national belonging is also low among citizens born in the DOM-TOM and among the second generation. On the contrary, the generation 2.5 are nearly as likely as the mainstream population to strongly identify as French (Table 8, column 19). Accordingly, we expect the visits to have increased the participation of citizens born in the DOM-TOM and of the second generation, and to have left the generation 2.5 unaffected. We check these predictions in Table 10 and find that the canvassing visits increased turnout among citizens born in the DOM-TOM by 2.7 (-1.7 + 4.4) percentage points in the first round, and 2.6 (-1.2 + 3.4) percentage points in the second round - an impact significant only for the first round (Column 2 of Panels A and B). Conversely, the impact on the participation of the second generation is negligible and not significant for the first round, but high (5.1 = -2 - 0.3 + 7.4 percentage points) and significant at 10 percent for the second round (Column 3 of Panels A and B). Finally, the visits did not affect significantly the generation 2.5. The preciseness of these analyses is constrained by the small size of these subgroups. However, the evidence converges towards the conclusion that beyond immigrants, the door-to-door canvassing visits increased the voter turnout of other groups of citizens of migrant origin who had a low sense of national belonging without affecting those who had a high sense of belonging.

These results are unlikely to be explained by an immigrant-related factor different from the low sense of belonging. Indeed, except for this shared characteristic, differences across these different groups of citizens of migrant origin are large, notably in terms of socioeconomic integration and strength of the links maintained with their country of origin. First, among foreign-born citizens, immigrants born in Asia differ from those born in Maghreb and Sub-Saharan African on several important dimensions. Many immigrants born in Southeast Asian countries, who account for the bulk of Asian immigrants, arrived in France in exile between 1977 and 1985. As a result, they

maintain much weaker relations with their country of origin[32]. In addition, they are less likely to have a religion, or to see religion as very important, and they more likely to use a contraception method and to say that less than half of their friends share their origin (Table 8, columns 9, 10, 12 and 13). Finally, they are less likely to be Muslims[33], and their children succeed better in school, even compared with children of the mainstream population (Brinbaum, Moguérou, and Primon 2010). Second, citizens born in the DOM-TOM are relatively more familiar with the French language and religious and cultural practices than immigrants: Caribbeans have been exposed to French culture since they were colonized in the seventeenth century, and they obtained full citizenship rights in 1946, when Guadeloupe, Guyane, and Martinique became French departments (Anselin 1990). Third, citizens of the second generation have weaker ties with their country of origin than their parents: they are less likely to hold dual citizenship, to go back to their country of origin at least once a year, to maintain regular contacts with people living outside of Metropolitan France, to provide financial help to such people, or to own property (land, house, or apartment) outside of Metropolitan France (Table 8, Panel A). Moreover, the second generation are more socially integrated in French society than their parents, and their values are closer to the mainstream population: they are much more likely to say that less than half of their friends share their origin; they are more likely to have no religion or to see religion as not very important and to use a contraception method (Table 8, Panel B).

Interestingly, citizens born in Magreb, Sub-Saharan Africa, Asia, and in the DOM-TOM, and the second generation not only report a low sense of national belonging, but most of them are visible ethnic minorities, and they are all equally likely to report not being seen as French (Table 8, column 18). This rejection by French society probably enters into their low sense of belonging. The effect of our intervention, however, was not obtained by simply being non-discriminatory, and even less by fighting existing stigmatization. What distinguishes our intervention from the ordinary experience of immigrants is not the lack of discrimination, but a positive outreach effort.

[32]They are only 10.1 percent to hold dual citizenship, compared to 50.8 percent and 27.6 percent for immigrants from Maghreb and Sub-Saharan Africa (Table 8, column 2). They are also much less likely to go back to their country of origin at least once a year, to provide financial help to people living outside of France, or to own property outside of France.

[33]Muslims are often seen as the most "unassimilable" group of immigrants. They have repeatedly been stigmatized by conservative parties and newspapers, and more vehemently by the radical right party, Front National, in debates questioning the compatibility of Muslim faith with French values (Modood 2005; Klausen 2005; Koopmans et al. 2005).

2.6 Alternative interpretations

2.6.1 Instrumental vs. Expressive Benefits of Voting

The fact that the intervention increased turnout of all groups of citizens with a low sense of national belonging reinforces confidence in the validity of our interpretation. We next turn to considering alternative interpretations of the short-run impact of the visits on immigrants' turnout more closely.

First, the heterogeneous effects of the intervention could result from the treatment administered to citizens of migrant origin being actually different[34]. But ethnic minorities were underrepresented among the canvassers; canvassers did not target immigrants and they were not encouraged to deliver a specific message to them. The wide-spread belief among French people, and left-wing activists more specifically, that no distinction should be made on the basis of ethnicity[35] makes it unlikely that they adapted their message in that way, and we did not notice any adaptation during the canvassing sessions in which we took part.

Second, even if the treatment administered to the immigrants and the native-born citizens was identical, one could hypothesize that it was used by the former as a coordination mechanism to organize as a group and advance political claims. Instead of increasing the sense of belonging and expressive benefits of voting, the visits might have increased instrumental benefits. However, the great diversity of immigrant origins observed in French cities in general, and in our sample in particular, makes their coordinated mobilization and organization as a political force relatively difficult (Cutts et al. 2007). In our sample, it is never the case that any of the three major broad origins accounts for more than 50 percent of the total immigrants of the city, except in Paris' 11th neighborhood, as shown in Figure 1. Other obstacles to the local political mobilization of immigrants include the French republican discourse, which considers minority identities to be an illegitimate basis for political claims, and electoral rules favoring winning parties (Dancygier 2010). It is unlikely that our intervention was strong enough to help immigrants overcome these obstacles.

[34]In the United States, for instance, "get-out-the-vote" and registration campaigns conducted in the 60's, after the abolition of laws preventing the participation of the Black, were specifically targeting Blacks and had a dramatic effect on their participation (Campbell and Feagin 1984; Vogl 2012). More recently, mobilization campaigns targeting Latinos produced similar effects (Barreto 2005).

[35]See for instance the campaign "against ethnic statistics" published in 2007 by the antiracist organization, SOS Racisme, which counted many politicians of the Socialist party amongst its signatories.

2.6.2 Cultural Explanations

The discussions with the canvassers might have resonated differently for those with political views closest to the ideological platform of the Socialist party. As other European progressive parties, this party is more interested in attracting immigrant voters and it is perceived as more sensitive to their interests than right-wing parties (Givens and Luedtke 2005). The Socialist party promotes immigrant naturalization, antidiscrimination policies, and the right to vote at local elections for non-naturalized immigrants. Unsurprisingly then, French citizens of migrant origin are generally more to the left (Brouard and Tiberj 2011). But the other voters in the neighborhoods included in the sample also vote predominantly for the left: the left-wing candidate won more than 50 percent of the votes cast at the second round of the regional election in all cities, and more than 60 percent in 7 of the 8 cities, and people born outside of France only account for one in five registered citizens who did not abstain. Among respondents to the post-electoral survey, native-born citizens and immigrants were almost equally likely to say that they were satisfied or very satisfied with the government's politics. Finally, among those who voted and disclosed whom they had voted for, native-born voters were almost equally likely to have voted for the Socialist Party at the first or second round (Table 3).

Our results could further be explained by some immigrants' relatively lesser experience of democracy. Some immigrants were born in countries where the degree of democracy is minimal, and part of their socialization took place in their country of origin. These immigrants might have perceived the canvassers' visits as a sign that the French take elections seriously and that one's vote actually matters. If this explanation has any merit, we should expect the impact of the intervention to be proportional to the level of democracy in each immigrant's country of origin. We test this prediction by running the following OLS regression:

$$Y_{i,b} = \alpha_6 + \beta_6 Visited_b + \theta_6 I_{i,b} + \lambda_6 Visited_b \times I_{i,b} + \kappa_6 I_{i,b} \times Democracy_{i,b} + \lambda_6 Visited_b \times I_{i,b} \times Democracy_{i,b} + X_b^{'}\gamma_6 + Z_{i,b}^{'}\delta_6 + \mu_{i,b} \quad (6)$$

where $Democracy_{i,b}$ indicates the "level" of democracy in the individual's country of birth. $Visited_b \times I_{i,b} \times Democracy_{i,b}$ is instrumented by $T_b \times I_{i,b} \times Democracy_{i,b}$. λ_6 measures the additional turnout increase induced by canvassing for people born in a country with a stronger democracy. We use different measures of *Democracy*: the level of democracy in the individual's country of birth when he was 18 ("Polity 1"), and in 2010 ("Polity 2"); the overall score of democracy of country of birth in 2011 ("EIU 1"), the quality of the electoral process and the pluralism of parties

in country of birth in 2011 ("EIU 2"), and the degree of electoral participation in the country of birth in 2011 ("EIU 3")[36].

In none of these specifications does the impact of canvassing vary with the initial level of democracy in the immigrant's country of birth (Table 11). Their participation, absent the intervention, does not vary by these measures of democracy either, except for the level of democracy in their country of birth today, as computed by the Polity IV project.

2.6.3 Education

Another difference between immigrants and the mainstream population could drive the results. Immigrants and their descendants could be relatively less educated and, as a result, less informed about French politics and political institutions, and less interested in the regional elections. The discussions with the canvassers might have increased their information and competence, when it was originally higher for other citizens, explaining the heterogeneous results[37]. We cannot test this explanation directly, since we only know the level of education of the respondents to the post-electoral survey. However, we do not find much supportive evidence for this explanation: among respondents to the post-electoral survey, immigrants are more likely than native-born citizens to have no diploma (20.4 percent vs. 11.8 percent), but they are equally likely to hold a high-school diploma (Table 3). They are only slightly less likely to identify the newly elected president of the region, to be able to name an attribution of the region, and to have an opinion on the government, and only one of these differences is statistically significant at 10 percent. Finally, among the active voters, immigrants are almost equally likely to be able to describe their electoral choice at the first and second rounds.

2.6.4 Understanding the Lack of Effect on the Mainstream Population

While we argue that a specific attitude, shared by citizens of migrant origin, explains the heterogeneous results, a reverse interpretation is in theory possible. Perhaps what deserves an explanation is not the higher impact of door-to-door canvassing among immigrants, but rather the lack of impact

[36]The two first measures are built using the composite index provided by the Polity IV project and derived from the coded values of authority characteristic component variables (Marshall, Jaggers, and Gurr 2010). The three latter indices are selected among all indices provided by the Economist Intelligence Unit (Economist Intelligence Unit 2011). The overall score of democracy aggregates the two last indices as well as other indices.

[37]The influence of education on the impact of canvassing could, however, also go the other way: people with more education might be better able to process the information given to them by the canvassers.

among the mainstream population. The impact of the visits on immigrants could simply result from the standard mechanisms through which canvassing was found to operate in the United States, such as reemphasizing the civic norm or increasing the saliency of the elections (Gerber and Rogers 2009; Nickerson and Rogers 2010). Conversely, the lack of impact on the mainstream population might reflect an attitude characteristic of native-born citizens living in the French banlieues. A possible argument goes as follows: mainstream citizens who live in disadvantaged neighborhoods are those who could not afford to move to a better neighborhood. They feel outcast and no longer believe that their situation can improve, making them unreceptive to the canvassers' visits.

However, the effect of the visits was null across a relatively large variety of demographic and socioeconomic profiles of mainstream citizens, which makes this interpretation unlikely. Further, many citizens of migrant origin, in particular the second generation, have been repeatedly described as distrustful of the institutions and despaired - but we show that they were affected by the intervention. This suggests another interpretation of the lack of effect on the mainstream population: the traditional channels through which canvassing operates might have been ineffective in this context because identification with the activists was difficult. Unlike most American activists, the canvassers who took part in the experiment were official party members[38]. Partisanship is an important part of their identity and takes up a significant share of their time, including outside of electoral campaigning. Some of the canvassers are elected as city councilmen and others are active in neighborhood councils. The people who received the canvassers' visits might thus have less easily identified with them. They might have associated them to distrusted politicians, nullifying the traditional effects of canvassing, except for the sense-of-belonging-enhancing mechanism. This explanation should be tested by future comparative work. More generally, the lack of impact of canvassing on French mainstream population remains an important topic for continued research.

2.7 Discussion

2.7.1 Effectiveness of Outreach Efforts Towards Immigrants

A same intervention (a visit by two canvassers) affected citizens of migrant origin and the mainstream population differently. The differential impact on immigrants' electoral participation is

[38]French parties have a limited culture of opening themselves to non-members or new members, and the distance between voting and being active in a campaign is wider in France than in the United States (Terra Nova 2009).

striking: it was obtained in a context characterized by economic scarcity and lack of immigrant electoral power, which usually generates immigrant-state conflict (Dancygier 2010). Moreover, the visits also affected immigrants' descendants, who have been involved in tensions with the police and in notorious urban riots.

These results undermine the idea that immigrants are unwilling to increase their integration and participation in the receiving societies. They suggest that extending a hand to immigrants is an effective way to enhance their sense of belonging and their participation in the institutions. We conclude that, beyond immigrants' stigmatization and rejection, the paucity of such outreach efforts is an important cause of their low sense of belonging.

However, the absence of any lasting impact on the 2011 elections demonstrates that a one-time visit cannot permanently enhance immigrants' electoral participation. This probably requires repeated interactions and more substantial interventions. How much of the outreach efforts towards immigrants should be borne by the state or by nongovernmental and private actors is questionable and should be tested. One can expect that outreach efforts will be all the more effective if their contributors are perceived as representatives of official institutions, whether they are public officials or members of well-known organizations as was the case in this study.

The findings of this study could be specific to France and its assimilationist model. This model, rooted in a universalist conception of the nation (Renan 1882), grants immigrants' and their descendants citizenship based on the length of their occupancy through *jus soli*, but it expects them to assimilate in the French society by overcoming racial or cultural differences. This might make outreach efforts towards immigrants particularly relevant. On the contrary, extending a hand to them might be less important in countries with a multiculturalist model, such as the United States, Britain, or Netherlands, which allow immigrants to root their participation in society within their cultural, religious, and ethnic communities (Brubaker 1992; Kymlicka and Norman 1994; Howard 2009; Koopmans et al. 2005; Koopmans 2010). Furthermore, outreach efforts towards immigrants might be simply meaningless in countries such as Germany and Austria where citizenship has traditionnally had an ethnic rather than a civic base, which de facto excludes most immigrants.

However, the existing classification of integration and citizenship models has recently been criticized as implausible: in Europe, immigration policy and immigrant integration are increasingly ruled by European Community laws, and the policies on immigrant integration currently converge across states. For instance, the Netherlands have significantly departed from their multiculturalist

policies and Germany has adopted a new nationality law in 2000, making it easier for immigrants and their children to acquire German citizenship (Haut Conseil à l'Intégration 2006; Korteweg 2006; Joppke 2004, 2007).

Today, the more relevant distinction might be between the classic immigration countries, such as the United States, Canada, Australia, or New Zealand, which were built around immigrants and have traditionnally emphasized the positive influence of immigration (e.g., Portes and Rumbaut 2006), and European countries, where immigration was long inexistent or confined to work immigration from within the continent. In this context, immigrants' influence is often perceived negatively, making their integration inherently more difficult. This gives us reasons to believe that outreach efforts such as the ones described in this study would be particularly useful to enhance immigrants' integration all across Europe, independent of the remaining specificities of each country's integration model. On the other hand, how much our conclusions extend to the classic immigration countries remains questionable: despite the much more important role that immigration has played in the foundation of these countries, some scholars argue that they face increasing difficulties integrating the most recent waves of immigrants (e.g., Huntington 2004). As in France and in Europe, immigrants in these countries participate less in the elections than the mainstream population (for the United States, see for instance Terrazas 2011; Jimenez 2011; and United States Census Bureau 2012). As a consequence, the best-practice diffusion in terms of citizenship tests and related civic integration policies is currently taking place within all western states, and not limited to Europe. Similarly, both European countries and these classic immigration nations could benefit from sharing best practices in terms of outreach efforts.

2.7.2 Comparing Turnout Effects Rather Than Levels

In this study, the effect of door-to-door canvassing is very different for immigrants and native-born citizens, although their turnout levels are similar. This suggests an important and more general methodological lesson.

Most studies of the electoral participation of immigrants or ethnic minorities rely on multivariate regressions of turnout levels on a variety of socioeconomic, demographic, and contextual variables, including dummies for immigrant and race. The influence of these two variables is inferred from the significance and size of the corresponding coefficients. Small or insignificant coefficients are interpreted as evidence that the determinants of the electoral participation of immigrants or ethnic

minorities are the same as for other citizens.

Our findings show that this conclusion might be misinformed: while the immigrant dummy affects turnout levels only marginally, it is central to the interpretation of the effects of the intervention. This can be understood as follows: the variables that can be controlled for in observational studies affect participation through a series of characteristics - for instance, middle age proxies for a series of resources, costs, attitudes, and perspectives on politics that all enter into people's decision to participate (Wolfinger and Rosenstone 1980). In this study, the neighborhood and building variables that we observe and control for probably capture some determinants that are identical for all citizens living in a given type of neighborhood, such as economic resources or ethnic diversity[39], as well as other determinants that are specific to some subgroups, such as the paucity of outreach efforts towards immigrants. Our regression of turnout levels misses the influence of this factor.

As an attempt to address this issue, electoral scholars have incorporated an increasing set of independent variables in their regressions, including some that are immigrant-related, such as coming from a repressive regime, anti-immigrant legislation, language proficiency, or percent of life spent outside of the country (e.g. Uhlaner, Cain, and Kiewiet 1989; Cho 1999; Ramakrishnan and Espenshade 2001; Xu 2005). But this approach also has its limits: attitudes such as sense of national belonging are costly and difficult to measure[40]. Moreover, it is unclear how a factor such "paucity of outreach efforts" which, our findings suggest, plays an important role in determining immigrants' turnout, could be measured and controlled for in a satisfying way in such regressions.

This suggests that exploring heterogeneous effects of voter mobilization interventions could usefully complete existing observational studies to assess the influence of factors, such as being an immigrant or race, on voter turnout. The approach used in this paper could be generalized by comparing the effects of voter mobilization interventions with different types of messages on the electoral participation of the mainstream population on one hand, and of immigrants or ethnic minorities on the other hand. Future work conducted along these lines will benefit from systematic cross-country comparisons, building on the handful of recent studies which, like this one, administer voter mobilization interventions, originally tested in the United States, in radically new contexts (e.g., John and Brannan 2008; Banerjee et al. 2011; Aker, Collier and Vicente 2011).

[39]Ethnic diversity has been repeatedly shown to negatively affect the provision of local public goods (e.g. Algan, Hémet and Laitin 2012) and could thus be expected to be one of the factors explaining low turnout in the French banlieues.

[40]For a paper regressing political participation of different ethnic groups on various attitudes, see for instance Leighley and Vedlitz (1999).

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Figure 1. Shares of immigrants by origin and city

	Paris 11e	Bagneux	Domont	Malakoff	Montrouge	Pierrefite-sur-Seine	Sevran	Villetaneuse	Total
Number of registered citizens	1223	2404	1165	2782	3997	1502	7313	3437	23823
% of immigrants	11.3%	20.1%	13.3%	15.0%	15.3%	26.8%	30.5%	26.6%	22.5%
% of immigrants born in									
Maghreb	55.8%	40.1%	33.5%	48.4%	42.7%	43.8%	32.0%	40.0%	38.2%
Sub-Saharan Africa	21.7%	36.8%	32.9%	18.5%	20.8%	21.9%	31.0%	33.3%	28.9%
Asia	8.0%	10.1%	9.7%	13.7%	11.1%	20.6%	29.0%	13.1%	19.6%

Table 1: Verifying the randomization

	Control group		Treatment group		*P-value* Treatment = Control	Number of obs.
	Mean	SD	Mean	SD		
Panel A. Building characteristics						
City						
Located in Paris (11th arr.)	0.056	*0.230*	0.046	*0.210*	0.542	23836
Located in Bagneux	0.098	*0.297*	0.104	*0.305*	0.665	23836
Located in Domont	0.049	*0.216*	0.049	*0.215*	0.941	23836
Located in Malakoff	0.116	*0.320*	0.118	*0.322*	0.935	23836
Located in Montrouge	0.171	*0.376*	0.164	*0.371*	0.834	23836
Located in Pierrefitte	0.065	*0.246*	0.061	*0.240*	0.685	23836
Located in Sevran	0.299	*0.458*	0.315	*0.464*	0.443	23836
Located in Villetaneuse	0.146	*0.353*	0.143	*0.350*	0.899	23836
Number of registered citizens in the building	54.7	*49.0*	45.9	*43.2*	0.183	23836
Based in a ZUS	0.344	*0.475*	0.353	*0.478*	0.683	23836
Based in a ZRU	0.298	*0.458*	0.313	*0.464*	0.45	23836
Housing price	3449	*1423*	3398	*1401*	0.561	23836
Distance to the polling station	0.272	*0.243*	0.268	*0.248*	0.757	23836
Panel B. Individual characteristics (whole sample)						
Gender	0.449	*0.497*	0.461	*0.498*	0.061	23836
Age	44.2	*17.9*	44.2	*17.9*	0.883	23836
Household size	2.2	*1.6*	2.2	*1.4*	0.595	23836
Immigrant	0.219	*0.414*	0.230	*0.421*	0.2	23823
Born in Maghreb	0.382	*0.486*	0.382	*0.486*	0.993	5351
Born in Sub-Saharan Africa	0.291	*0.454*	0.287	*0.453*	0.85	5351
Born in Asia	0.191	*0.393*	0.202	*0.402*	0.5	5351
Born in South America	0.043	*0.202*	0.038	*0.192*	0.517	5351
Born in Western Europe	0.041	*0.198*	0.042	*0.201*	0.819	5351
Born in Eastern Europe	0.030	*0.171*	0.028	*0.165*	0.719	5351
Born in Middle East	0.022	*0.148*	0.020	*0.139*	0.626	5351
Born in North America	0.001	*0.038*	0.001	*0.034*	0.827	5351
Born in the DOM-TOM	0.082	*0.274*	0.079	*0.269*	0.561	23823
Second generation	0.080	*0.271*	0.079	*0.270*	0.872	23836
Generation 2.5	0.010	*0.100*	0.011	*0.105*	0.523	23836
Born in this city	0.038	*0.191*	0.033	*0.179*	0.49	23823
Born in this department	0.232	*0.422*	0.226	*0.418*	0.579	23823
Born in this region	0.520	*0.500*	0.504	*0.500*	0.141	23823
Panel C. Individual characteristics (postelectoral survey)						
Called for a survey	0.154	*0.361*	0.162	*0.369*	0.399	23836
Survey conducted	0.242	*0.428*	0.232	*0.422*	0.464	3766
Education						
No diploma	0.144	*0.352*	0.137	*0.344*	0.773	817
Diploma below end-of-high-school	0.322	*0.468*	0.287	*0.453*	0.387	817
End-of-high-school diploma	0.227	*0.419*	0.254	*0.436*	0.444	817
Higher education diploma	0.307	*0.462*	0.322	*0.468*	0.621	817
Employed workers	0.588	*0.493*	0.583	*0.494*	0.873	804
Unemployed workers	0.109	*0.312*	0.077	*0.266*	0.141	804
Student	0.070	*0.256*	0.105	*0.307*	0.084	804
Retired	0.179	*0.384*	0.169	*0.375*	0.716	804
Other inactivity	0.053	*0.225*	0.066	*0.249*	0.457	804
Social category						
Category 2 (farmers)	0.033	*0.178*	0.025	*0.158*	0.526	892
Category 3 (craftsmen, shopkeepers)	0.085	*0.279*	0.129	*0.336*	0.05	892
Category 4 (intermediate jobs)	0.279	*0.449*	0.236	*0.425*	0.172	892
Category 5 (employees)	0.314	*0.465*	0.268	*0.443*	0.12	892
Category 6 (workers)	0.076	*0.266*	0.079	*0.269*	0.879	892
Category 8 (no activity)	0.096	*0.295*	0.145	*0.353*	0.041	892
No opinion on the government	0.194	*0.396*	0.205	*0.404*	0.739	748
Satisfied with the government's policies	0.114	*0.318*	0.148	*0.356*	0.23	599

Notes : The unit of observation is the building in Panel A, the individual in Panel B, and the respondent to the post-electoral survey in Panel C. In Panels B and C, standard errors are adjusted for clustering at the building level.

Table 2: 1st and 2nd round participation in the control group (2010 and 2011)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A.			1st round, 2010			
Immigrant	-0.005	0.039	-0.019	-0.031	0.023	-0.052
	(0.015)	(0.014)***	(0.015)	(0.016)*	(0.015)	(0.017)***
Born in DOM-TOM				-0.091	-0.026	-0.093
				(0.019)***	(0.018)	(0.020)***
Second generation				-0.153	-0.089	-0.013
				(0.021)***	(0.019)***	(0.018)
Generation 2.5				-0.049	-0.070	0.018
				(0.042)	(0.044)	(0.045)
Constant	0.343			0.369		
	(0.013)***			(0.014)***		
Building controls and strata fixed effects	No	Yes	Yes	No	Yes	Yes
Individual controls	No	No	Yes	No	No	Yes
Observations	12269	12269	12269	12269	12269	12269
R-squared	0.00	0.08	0.11	0.01	0.08	0.11
Mean in Control Group	0.342	0.342	0.342	0.342	0.342	0.342
Panel B.			2nd round, 2010			
Immigrant	0.026	0.066	0.004	0.003	0.051	-0.022
	(0.014)*	(0.013)***	(0.014)	(0.016)	(0.015)***	(0.017)
Born in DOM-TOM				-0.065	-0.007	-0.072
				(0.021)***	(0.020)	(0.022)***
Second generation				-0.153	-0.092	-0.020
				(0.023)***	(0.021)***	(0.022)
Generation 2.5				-0.076	-0.089	-0.006
				(0.046)	(0.047)*	(0.047)
Constant	0.372			0.396		
	(0.013)***			(0.014)***		
Building controls and strata fixed effects	No	Yes	Yes	No	Yes	Yes
Individual controls	No	No	Yes	No	No	Yes
Observations	12269	12269	12269	12269	12269	12269
R-squared	0.00	0.07	0.10	0.01	0.07	0.10
Mean in Control Group	0.378	0.378	0.378	0.378	0.378	0.378

Table 2: 1st and 2nd round participation in the control group (2010 and 2011) (cont.)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel C.			1st round, 2011			
Immigrant	0.005	0.032	-0.026	-0.025	0.008	-0.078
	(0.017)	(0.015)**	(0.016)*	(0.020)	(0.018)	(0.019)***
Born in DOM-TOM				-0.091	-0.044	-0.123
				(0.023)***	(0.021)**	(0.022)***
Second generation				-0.137	-0.098	-0.018
				(0.029)***	(0.026)***	(0.023)
Generation 2.5				-0.120	-0.138	-0.039
				(0.050)**	(0.048)***	(0.050)
Constant	0.261			0.291		
	(0.016)***			(0.018)***		
Building controls and strata fixed effects	No	Yes	Yes	No	Yes	Yes
Individual controls	No	No	Yes	No	No	Yes
Observations	7924	7924	7924	7924	7924	7924
R-squared	0.00	0.08	0.13	0.01	0.08	0.13
Mean in Control Group	0.262	0.262	0.262	0.262	0.262	0.262
Panel D.			2nd round, 2011			
Immigrant	0.042	0.062	0.011	0.015	0.042	-0.027
	(0.016)**	(0.013)***	(0.016)	(0.019)	(0.015)***	(0.019)
Born in DOM-TOM				-0.071	-0.031	-0.091
				(0.025)***	(0.021)	(0.024)***
Second Generation				-0.128	-0.085	-0.016
				(0.028)***	(0.022)***	(0.019)
Generation 2.5				-0.097	-0.118	-0.031
				(0.056)*	(0.050)**	(0.053)
Constant	0.281			0.307		
	(0.017)***			(0.020)***		
Building controls and strata fixed effects	No	Yes	Yes	No	Yes	Yes
Individual controls	No	No	Yes	No	No	Yes
Observations	7921	7921	7921	7921	7921	7921
R-squared	0.00	0.08	0.12	0.01	0.08	0.12
Mean in Control Group	0.291	0.291	0.291	0.291	0.291	0.291

Notes: Clustered standard errors are in parentheses. ***, **, * indicate significance at 1, 5 and 10%.
We count someone who lives with at least one citizen of an older generation as Second Generation if all the older generation citizens were born abroad or in the DOM-TOM; and as Generation 2.5 if one of these older generation citizens was born abroad or in the DOM-TOM, but another one was born in France.
Building controls include: housing price, size, distance to the polling station, and whether it is located in a ZUS.
Individual controls include: age, gender, household size, whether the individual was born in the same city, in the same department, and in the same region as the ones where he currently lives.

Table 3: Differences between the characteristics of native-born citizens and immigrants

	Native-born citizens		Immigrants		*P-value* Treatment = Control	Number of obs.
	Mean	SD	Mean	SD		
Panel A. Building characteristics						
City						
Located in Paris (11th arr.)	0.059	*0.235*	0.026	*0.159*	0.031	23823
Located in Bagneux	0.104	*0.305*	0.090	*0.287*	0.206	23823
Located in Domont	0.055	*0.227*	0.029	*0.168*	0.033	23823
Located in Malakoff	0.128	*0.334*	0.078	*0.268*	0.002	23823
Located in Montrouge	0.183	*0.387*	0.114	*0.318*	0.002	23823
Located in Pierrefitte	0.060	*0.237*	0.075	*0.264*	0.116	23823
Located in Sevran	0.275	*0.447*	0.417	*0.493*	0.000	23823
Located in Villetaneuse	0.137	*0.343*	0.171	*0.376*	0.021	23823
Number of registered citizens in the building	51.2	*47.0*	48.0	*44.7*	0.093	23823
Based in a ZUS	0.314	*0.464*	0.469	*0.499*	0.000	23823
Based in a ZRU	0.275	*0.446*	0.413	*0.492*	0.000	23823
Housing price	3539	*1445*	3031	*1215*	0.000	23823
Distance to the polling station	0.271	*0.242*	0.265	*0.254*	0.661	23823
Panel B. Individual characteristics (whole sample)						
Gender	0.443	*0.497*	0.493	*0.500*	0.000	23823
Age	42.8	*18.4*	49.1	*15.0*	0.000	23823
Household size	2.2	*1.5*	2.4	*1.5*	0.000	23823
Panel C. Individual characteristics (postelectoral survey)						
Called for a survey	0.156	0.363	0.163	0.370	0.403	23823
Survey conducted	0.227	0.419	0.270	0.444	0.013	3762
Education						
No diploma	0.118	0.323	0.204	0.404	0.004	816
End-of-high-school diploma and higher	0.550	0.498	0.569	0.496	0.614	816
Active and unemployed	0.197	0.398	0.233	0.424	0.302	594
Voted for Socialist Party at 1st round	0.478	0.501	0.493	0.504	0.835	249
Voted for Socialist Party at 2nd round	0.769	0.422	0.848	0.361	0.110	287
Satisfied with the government's politic	0.136	0.343	0.114	0.319	0.408	599
Is able to tell his choice at the 1st round	0.805	0.398	0.754	0.434	0.379	243
Is able to tell his choice at the 2nd round	0.767	0.424	0.747	0.437	0.722	297
Is able to identify the president of the region	0.420	0.494	0.363	0.482	0.094	849
Is able to name an attribution of the region	0.331	0.471	0.279	0.450	0.147	850
Does not have any opinion on the government	0.189	0.392	0.224	0.418	0.280	747

Notes: The unit of observation is the building in Panel A, the individual in Panel B, and the respondent to the post-electoral survey in Panel C. In Panels B and C, standard errors are adjusted for clustering at the building level.

Table 4: Overall impact on 1st and 2nd round participation (2010)

	(1)	(2)	(3)
Panel A.	1st round, 2010		
Visited	0.005	0.001	0.000
	(0.011)	(0.008)	(0.009)
Building controls and strata fixed effects	No	Yes	Yes
Individual controls	No	No	Yes
Observations	23773	23773	23760
R-squared	0.00	0.06	0.09
Panel B.	2nd round, 2010		
Visited	-0.001	0.002	0.001
	(0.012)	(0.009)	(0.009)
Building controls and strata fixed effects	No	Yes	Yes
Individual controls	No	No	Yes
Observations	18825	18825	18819
R-squared	0.00	0.06	0.09

Notes: Clustered standard errors are in parentheses. ***, **, * indicate significance at 1, 5 and 10%. Visited is instrumented with T.
Individual and building controls as in Table 2.

Table 5: Impact on 1st and 2nd round participation for immigrants and native-born citizens (2010)

	(1)	(2)	(3)	(4)
Panel A.		1st round, 2010		
Visited	0.005	-0.005	0.000	-0.012
	(0.011)	(0.012)	(0.009)	(0.009)
Immigrant * Visited		0.044		0.052
		(0.020)**		(0.018)***
Immigrant		-0.006		-0.021
		(0.015)		(0.014)
Building controls and strata fixed effects	No	No	Yes	Yes
Individual controls	No	No	Yes	Yes
Observations	23773	23760	23760	23760
R-squared	0.00	0.00	0.09	0.09
Panel B.		2nd round, 2010		
Visited	-0.001	-0.008	0.001	-0.008
	(0.012)	(0.013)	(0.009)	(0.010)
Immigrant * Visited		0.028		0.034
		(0.022)		(0.021)
Immigrant		0.025		-0.002
		(0.016)		(0.015)
Building controls and strata fixed effects	No	No	Yes	Yes
Individual controls	No	No	Yes	Yes
Observations	18825	18819	18819	18819
R-squared	0.00	0.00	0.09	0.09

Notes: Clustered standard errors are in parentheses. ***, **, * indicate significance at 1, 5 and 10%.
Visited is instrumented with T and Immigrant * Visited with Immigrant * T.
Individual and building controls as in Table 2.

Table 6: Impact on 1st and 2nd round participation for immigrants and native-born citizens, allowing for heterogeneous treatment effects along other dimensions (2010)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Heterogenous effects allowed along		Born in same area or not	Housing price	Age	Gender	ZUS	All ((2) - (6))
Panel A.				1st round, 2010			
Visited	-0.012	-0.020	-0.016	-0.009	-0.017	-0.020	-0.062
	(0.009)	(0.015)	(0.023)	(0.021)	(0.012)	(0.012)	(0.041)
Immigrant * Visited	0.052	0.060	0.053	0.053	0.052	0.049	0.057
	(0.018)***	(0.022)***	(0.019)***	(0.018)***	(0.018)***	(0.018)***	(0.022)**
Immigrant	-0.021	-0.024	-0.021	-0.021	-0.020	-0.019	-0.023
	(0.014)	(0.015)	(0.014)	(0.014)	(0.014)	(0.014)	(0.015)
Building controls and strata fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23760	23760	23760	23760	23760	23760	23760
R-squared	0.09	0.10	0.09	0.10	0.09	0.10	0.10
Panel B.				2nd round, 2010			
Visited	-0.008	-0.013	-0.012	0.011	-0.017	-0.013	-0.038
	(0.010)	(0.016)	(0.027)	(0.022)	(0.012)	(0.014)	(0.063)
Immigrant * Visited	0.034	0.040	0.035	0.038	0.033	0.033	0.038
	(0.021)	(0.024)*	(0.021)	(0.022)*	(0.021)	(0.021)	(0.024)
Immigrant	-0.002	-0.005	-0.002	-0.003	-0.001	-0.001	-0.004
	(0.015)	(0.016)	(0.015)	(0.015)	(0.015)	(0.015)	(0.016)
Building controls and strata fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18819	18819	18819	18819	18819	18819	18819
R-squared	0.09	0.09	0.09	0.09	0.09	0.09	0.09

Notes: See Table 5.

Table 7: Impact on the 2011 cantonal elections 1st and 2nd round participation for immigrants and native-born citizens

	(1)	(2)	(3)	(4)
Panel A.		1st round, 2011		
Visited	-0.001	-0.004	0.003	-0.001
	(0.013)	(0.017)	(0.009)	(0.011)
Immigrant * Visited		0.010		0.015
		(0.026)		(0.022)
Immigrant		0.005		-0.027
		(0.018)		(0.015)*
Building controls and strata fixed effects	No	No	Yes	Yes
Individual controls	No	No	Yes	Yes
Observations	15426	15415	15415	15415
R-squared	0.00	0.00	0.11	0.11
Panel B.		2nd round, 2011		
Visited	-0.016	-0.023	-0.003	-0.010
	(0.014)	(0.017)	(0.011)	(0.012)
Immigrant * Visited		0.023		0.028
		(0.022)		(0.020)
Immigrant		0.032		0.006
		(0.018)*		(0.016)
Building controls and strata fixed effects	No	No	Yes	Yes
Individual controls	No	No	Yes	Yes
Observations	12672	12667	12667	12667
R-squared	0.00	0.00	0.09	0.09

Notes: See Table 5.

Table 8. Connections outside of France and social integration

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Share of the population surveyed	Panel A. Connections outside of Metropolitan France (with DOM or foreign country) Holds dual citizenship	Follows media of country/DOM of origin	Goes back to country of origin at least once a year	Has regular contacts with ppl outside of Met. France	Provides financial help outside of Met. France	Owns some property outside of Met. France
Immigrant	12.7%	41.9%	51.1%	32.3%	52.6%	15.4%	14.1%
born in Maghreb	*37.2%*	*50.8%*	*46.5%*	*39.4%*	*50.6%*	*11.5%*	*12.7%*
born in Subsaharan Africa	*22.2%*	*27.6%*	*60.2%*	*16.7%*	*48.3%*	*27.0%*	*19.0%*
born in Asia	*16.4%*	*10.1%*	*50.2%*	*10.0%*	*55.8%*	*8.5%*	*6.0%*
Born in DOM	2.6%	0.2%	73.6%	22.9%	65.4%	8.1%	14.6%
Second Generation	12.2%	30.0%	49.5%	N/A	37.7%	4.6%	6.0%
Generation 2.5	12.0%	8.9%	22.1%	N/A	28.3%	2.0%	4.5%
Mainstream population	60.4%	0.0%	N/A	N/A	18.5%	1.7%	0.7%

	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Panel B. Values and social integration Speaks French with children	Has a religion	Sees religion as very important	Ideal number of children in a family	Uses a contraception method	Less than half friends with same origin	Panel C. Discriminations Often asked about his origins	Has experienced discriminations	Has experienced racism
Immigrant	98.7%	78.3%	31.7%	2.9	42.5%	51.1%	22.3%	6.1%	33.8%
born in Maghreb	*98.7%*	*89.6%*	*39.6%*	*2.8*	*40.2%*	*46.3%*	*15.9%*	*3.7%*	*35.4%*
born in Subsaharan Africa	*99.1%*	*86.2%*	*47.4%*	*3.2*	*34.0%*	*31.8%*	*30.7%*	*18.5%*	*49.2%*
born in Asia	*97.5%*	*58.2%*	*16.5%*	*2.7*	*42.2%*	*59.3%*	*27.7%*	*1.8%*	*25.3%*
Born in DOM	98.1%	83.4%	35.0%	2.7	40.4%	42.9%	28.9%	4.2%	48.1%
Second Generation	98.3%	74.7%	26.5%	2.8	47.9%	65.2%	31.5%	5.6%	48.4%
Generation 2.5	98.9%	56.5%	8.1%	2.4	65.6%	59.6%	16.4%	3.6%	25.8%
Mainstream population	98.6%	52.2%	8.5%	2.5	57.8%	19.2%	6.9%	2.2%	24.8%

	(17)	(18)	(19)	(20)
	Panel D. Sense of national belonging Strongly feels at home in France	Strongly disagrees that seen as French	Strongly feels French	Rather feels French
Immigrant	65.1%	18.9%	60.4%	27.9%
born in Maghreb	*60.1%*	*17.8%*	*65.3%*	*24.9%*
born in Subsaharan Africa	*69.4%*	*27.4%*	*66.0%*	*21.6%*
born in Asia	*50.9%*	*21.4%*	*33.5%*	*50.4%*
Born in DOM	51.8%	18.8%	71.7%	18.5%
Second Generation	66.3%	14.9%	63.9%	28.5%
Generation 2.5	84.1%	2.8%	88.0%	8.8%
Mainstream population	79.7%	0.4%	90.0%	7.7%

*Notes: S*tatistics computed based on the survey Trajectories and Origins conducted by INED and INSEE in 2008-2009. We use the individual weights provided in the database to build a sample representative of all French citizens living in the region Ile-de-France. The Second Generation are people born in France of two parents born abroad or in a DOM. The Generation 2.5 are people born in France from one parent born in France and one parent born abroad or in a DOM. The sample is restricted to immigrants with children living in France, in Column 8; women younger than 55 who are in a couple, in Column 12; and respondents who say that they have seen friends in the past 15 days, in Column 13.

Table 9: Impact on 1st and 2nd round participation for immigrants of different origins and native-born citizens (2010)

	(1)	(2)	(3)	(4)
	1st round, 2010		2nd round, 2010	
Visited	-0.012	-0.012	-0.008	-0.008
	(0.009)	(0.009)	(0.010)	(0.010)
Immigrant * Visited	0.052		0.034	
	(0.018)***		(0.021)	
Immigrant	-0.021		-0.002	
	(0.014)		(0.015)	
Born in Western country * Visited		0.013		0.015
		(0.100)		(0.088)
Born in SubSaharian Africa * Visited		0.049		0.037
		(0.030)		(0.033)
Born in Maghreb * Visited		0.073		0.059
		(0.026)***		(0.025)**
Born in Asia * Visited		0.045		0.025
		(0.038)		(0.042)
Other immigrant * Visited		0.014		-0.040
		(0.058)		(0.068)
Born in Western country		-0.009		0.011
		(0.047)		(0.048)
Born in Sub-Saharan Africa		0.050		0.039
		(0.023)**		(0.024)
Born in Maghreb		-0.050		-0.016
		(0.017)***		(0.019)
Born in Asia		-0.065		-0.033
		(0.026)**		(0.028)
Other immigrant		-0.042		-0.009
		(0.038)		(0.046)
Building controls and strata fixed effects	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Observations	23760	23760	18819	18819
R-squared	0.09	0.10	0.09	0.09

Notes: Clustered standard errors are in parentheses. ***, **, * indicate significance at 1, 5 and 10%.
Visited is instrumented with T and Immigrant * Visited with Immigrant * T. Similarly for Born in Western country * Visited, Born in Sub-Saharan Africa * Visited, etc.
Individual and building controls as in Table 2.

Table 10: Impact on 1st and 2nd round participation for citizens of migrant origin and the mainstream population (2010)

	(1)	(2)	(3)
Panel A.	1st round, 2010		
Visited	-0.012	-0.017	-0.019
	(0.009)	(0.010)*	(0.010)*
Immigrant * Visited	0.052	0.058	0.059
	(0.018)***	(0.019)***	(0.020)***
Born in DOM * Visited		0.044	0.046
		(0.026)*	(0.026)*
Child * Visited			0.011
			(0.025)
Second generation * Visited			0.010
			(0.035)
Generation 2.5 * Visited			-0.089
			(0.072)
Immigrant	-0.021	-0.047	-0.047
	(0.014)	(0.015)***	(0.015)***
Born in DOM		-0.090	-0.091
		(0.017)***	(0.018)***
Child			-0.038
			(0.019)*
Second generation			0.006
			(0.024)
Generation 2.5			0.042
			(0.049)
Building controls and strata fixed effects	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes
Observations	23760	23760	23760
R-squared	0.09	0.10	0.10
Panel B.	2nd round, 2010		
Visited	-0.008	-0.012	-0.020
	(0.010)	(0.011)	(0.012)*
Immigrant * Visited	0.034	0.039	0.048
	(0.021)	(0.022)*	(0.024)**
Born in DOM * Visited		0.034	0.043
		(0.032)	(0.032)
Child * Visited			-0.003
			(0.026)
Second generation * Visited			0.074
			(0.038)*
Generation 2.5 * Visited			-0.003
			(0.083)
Immigrant	-0.002	-0.023	-0.026
	(0.015)	(0.017)	(0.018)
Born in DOM		-0.071	-0.075
		(0.021)***	(0.022)***
Child			-0.031
			(0.020)
Second generation			-0.021
			(0.028)
Generation 2.5			0.002
			(0.055)
Building controls and strata fixed effects	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes
Observations	18819	18819	18819
R-squared	0.09	0.09	0.09

Notes: Clustered standard errors are in parentheses. ***, **, * indicate significance at 1, 5 and 10%.
We count someone who lives with at least one citizen of an older generation as Second Generation if all the older generation citizens were born abroad or in the DOM-TOM; and as Generation 2.5 if one of these older generation citizens was born abroad or in the DOM-TOM, but another one was born in France.
Visited is instrumented with T and Immigrant * Visited with Immigrant * T. Similarly for Born in DOM * Visited, Child * Visited, etc.
Individual and building controls as in Table 2.

Table 11: Impact on 1st and 2nd round participation for immigrants from countries with different levels of democracy and for native-born citizens (2010)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A.			1st round, 2010			
Visited	-0.012	-0.010	-0.012	-0.012	-0.012	-0.012
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Immigrant * Visited	0.052	0.051	0.062	0.074	0.077	0.086
	(0.018)***	(0.017)***	(0.023)***	(0.045)	(0.027)***	(0.045)*
Immigrant * Visited * Democracy		0.001	-0.002	-0.005	-0.006	-0.010
		(0.006)	(0.005)	(0.010)	(0.006)	(0.012)
Immigrant	-0.021	-0.008	-0.054	-0.009	-0.032	-0.032
	(0.014)	(0.014)	(0.017)***	(0.027)	(0.018)*	(0.029)
Democracy		0.001	0.008	-0.002	0.003	0.003
		(0.003)	(0.003)**	(0.006)	(0.004)	(0.008)
Democracy measure		Polity 1	Polity 2	EIU 1	EIU 2	EIU 3
Building controls and strata fixed effe	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23760	22802	23359	23721	23721	23721
R-squared	0.09	0.10	0.10	0.10	0.09	0.09
Panel B.			2nd round, 2010			
Visited	-0.008	-0.005	-0.007	-0.008	-0.008	-0.008
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Immigrant * Visited	0.034	0.035	0.040	0.011	0.044	0.000
	(0.021)	(0.022)	(0.028)	(0.047)	(0.030)	(0.049)
Immigrant * Visited * Democracy		0.000	-0.002	0.005	-0.002	0.009
		(0.007)	(0.006)	(0.011)	(0.006)	(0.013)
Immigrant	-0.002	0.010	-0.034	-0.007	-0.029	0.006
	(0.015)	(0.017)	(0.020)*	(0.034)	(0.022)	(0.032)
Democracy		0.002	0.008	0.001	0.006	-0.002
		(0.004)	(0.004)**	(0.007)	(0.004)	(0.008)
Democracy measure		Polity 1	Polity 2	EIU 1	EIU 2	EIU 3
Building controls and strata fixed effe	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18819	18082	18513	18791	18791	18791
R-squared	0.09	0.10	0.09	0.09	0.09	0.09

Notes: Clustered standard errors are in parentheses. ***, **, * indicate significance at 1, 5 and 10%.
Visited is instrumented with T, Immigrant * Visited with Immigrant * T and Immigrant * Visited * Democracy with Immigrant * Treatment * Democracy.
We use the following measures of democracy: the level of democracy in the individual's country of birth when he was 18 ("Polity 1"), and in 2010 ("Polity 2"); the overall score of democracy of country of birth in 2011 ("EIU 1"), the quality of the electoral process and the pluralism of parties in country of birth in 2011 ("EIU 2"), and the degree of electoral participation in the country of birth in 2011 ("EIU 3")
Individual and building controls as in Table 2.

Chapter 3

Does Door-to-door Canvassing Affect Vote Shares? Evidence from a Countrywide Field Experiment in France[1]

3.1 Introduction

Door-to-door canvassing is an effective method to mobilize voters and increase turnout. This result was first established by the early study of Gosnell (1926) and the 1998 New Haven field experiment conducted by Gerber and Green (2000). Since then, it has been replicated in many contexts. At times of declining turnout, establishing the great impact of canvassing on participation was important in its own right. Students of political behavior have further investigated the mechanisms underlying this result, such as information provision, social pressure, and the formation of a voting plan (e.g., Nickerson 2008; Gerber, Green, and Larimer 2008; Nickerson and Rogers 2010). Political parties and non-partisan organizations in the United States were quick to apply these findings. Since

[1]I am grateful to Esther Duflo, Benjamin Olken, Daniel Posner, Alan Gerber, Todd Rogers, Danny Hidalgo, and Jens Hainmueller for suggestions that have improved the paper.

the presidential elections of 2004, they have devoted increasing resources to door-to-door canvassing (Bergan et al. 2005). Today, they continue to optimize the scripts delivered by canvassers according to the latest scientific results (Issenberg 2012).

A natural question, then, is how much door-to-door canvassing affects vote shares and electoral outcomes. This question is not only of interest to campaign analysts and candidates. First, scholars who seek to isolate the contribution of different political, economic or demographic factors to recent electoral results should account for the own impact of voter outreach efforts. Second, assessing the mechanisms underlying the effect of door-to-door canvassing on vote shares gives the opportunity to better understand the forces shaping political preferences and vote choice, a central question in political science (e.g., Lazarsfeld, Berelson, and Gaudet 1944; Berelson, Lazarsfeld, and McPhee 1954; Campbell et al. 1960; Brader 2005; Chong and Druckman 2007; Hillygus and Shield 2008; Lenz 2012; Wlezien and Erikson 2002; Lodge, Steenberger and Brau 1995). When door-to-door canvassing is non-partisan, its effect on vote shares depends not only on the fractions of left-wing and right-wing nonvoters, which a vast literature suggests are relatively close (e.g., Citrin et al. 2003; Brunell and DiNardo 2004; Bernhagen and Marsh 2007; Rubenson et al. 2007), but also on their relative receptivity to voter mobilization and on the extent to which increased information can change people's political orientation (e.g., Lau and Redlawsk 2006; Fowler and Margolis 2013). When door-to-door canvassing is partisan, other mechanisms can contribute to its impact on vote shares. Political activists often target nonvoters deemed likely to prefer their party, but they also target consistent voters deemed undecided or leaning towards other candidates. Persuasive messages conveyed to these voters might affect both their beliefs and their preferences (DellaVigna and Gentzkow 2010). First, door-to-door canvassing might change voters' beliefs by signaling quality or by providing information about the candidate and his or her program. In the latter case, the sign of the effect might depend on the fit between the message conveyed and voters' preexisting views. If this fit is too dim or if partisan affiliations are too strong, voters might be reinforced in their preexisting choice and polarized (Arceneaux and Kolodny 2009; Nicholson 2012), or cross-pressured and demobilized (Fiorina 1976; Foos and de Rooij 2013). Second, the interaction with canvassers can also work as a peripheral factor that affects voters' choice without changing their beliefs (e.g., Petty and Cacioppo 1996), similar to how a picture of an attractive female on a lender's mailer can increase loan takeup (Bertrand et al. 2010).[2]

[2]As another example related more closely to this project, see also the finding by Landry et al. (2006) that more

Given the bulk of evidence accumulated on the effect of door-to-door canvassing on turnout, it is striking to see how little robust evidence there is on these mechanisms and on the overall effect of door-to-door canvassing on vote shares. The reason is simple: data limitation. Individual participation can be measured at the individual level using free administrative records. Differently, there is no administrative data on the vote choice of individual voters. Instead, researchers have to rely on post-election surveys. A large literature finds a positive correlation between contact with partisan campaigning and self-reported support for the candidate (e.g., Rosenstone and Hansen 1993; Iyengar and Simon 2000; Vavreck, Spiliotes, and Fowler 2002; but see Kramer 1970), but the fact that campaigns strategically target potential supporters (e.g., Huckfelt and Sprague 1992; Wielhouwer 2003) makes interpreting the results difficult. To address this issue, recent projects study the experimental or quasi-experimental administration of door-to-door canvassing and other voter outreach methods, including mailings, phone calls, door hangers, television ads, and online advertisements. Some studies find substantial effects on vote choice (e.g., Huber and Arceneaux 2007; Arceneaux 2007; Arceneaux and Nickerson 2010; Barton, Casillo, and Petrie 2011; Gerber et al. 2011; Rogers and Nickerson 2013) while other studies find no effect (e.g., Cardy 2005; Nickerson 2005; Bailey, Hopkins, and Rogers 2013; Broockman and Green 2013). Unfortunately, for all its merits, randomization does not eliminate traditional survey biases. In phone surveys, non-response rates are often as high as 75 percent, and there is ample evidence that questions on political behavior are particularly prone to misreporting, including over reporting 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 (Cardy 2005; Bailey, Hopkins, and Rogers 2013). An alternative is to run the analysis not at the individual level, but at an aggregate level at which administrative records of vote shares are available. A few studies exploit quasi-experimental variations in the media coverage of precincts or counties and find strong effects of the media and of campaign advertisements on voting behavior (e.g., Simon and Stern 1955; DellaVigna and Kaplan 2007). It is more difficult, however, to find ex-post credible sources of exogenous variation in the areas targeted by campaign activists, and ex-ante precinct-level randomization of personal-contact campaigning is logistically demanding. Indeed, it requires a large number of precincts – and a very large number of individuals – to secure sufficient statistical power (Arceneaux 2005). Researchers studying the effect of direct mail (Gerber 2004; Rogers and Middleton 2012), robotic phone calls (Shaw et al. 2012), or radio

attractive female solicitors raise significantly more funds in a door-to-door fund-raising campaign.

ads (Panagopoulos and Green 2008) have managed to secure a sufficiently large sample to run precinct-randomized experiments. To this date, this has proved impossible in the case of door-to-door canvassing, which requires significantly more human resources than these techniques to contact the same number of voters. But there are reasons to believe that face-to-face personal interactions with voters – the distinctive feature of door-to-door canvassing – might affect their vote choice in a way different from other types of contacts. After all, their effect on the decision to vote is itself radically different (e.g., Gerber and Green 2000).

The author's involvement in François Hollande's field campaign for the 2012 French presidential elections provided a unique opportunity to run the first well-powered precinct-randomized evaluation of the impact of door-to-door canvassing on vote shares. This paper reports the results of this countrywide field experiment. From 1 February to 6 May 2012, the second round of the French presidential elections, an estimated 80,000 left-wing activists knocked on 5 million doors to encourage people to vote for the candidate of the Parti Socialiste, making this campaign the largest door-to-door effort in Europe to date. From the start of the campaign, the priority was given to the mobilization of left-wing nonvoters. Using results of past elections, 22,500 precincts were identified for their relatively higher fraction of left-wing nonvoters. 80 percent of these precincts and 17.1 million citizens were included in the treatment group and a subset of them was assigned to the canvassers. The remaining 20 percent were included in the control group.

I evaluate the effects of this door-to-door effort using the daily reports entered by canvassers on the campaign web platform, official election results at the precinct level, and an anonymous post-electoral online survey to which 1,972 canvassers responded. Although left-wing nonvoters had been the main target of the campaign, surprisingly, door-to-door canvassing did not affect voter turnout. However, it increased François Hollande's vote share by 3.1 percentage points and 2.7 percentage points at the first and second rounds of the presidential elections. These effects take into account the imperfect compliance of the canvassers with the lists of precincts that they had been assigned, and they are significant at the 5 and 10 percent levels respectively. They were obtained by decreasing the vote share of Marine Le Pen, the candidate of the far-right Front National, at the first round and by decreasing the vote share of Nicolas Sarkozy, the incumbent, at the second round. Overall, door-to-door canvassing contributed to approximately one fourth of Hollande's victory margin at the second round. The effect on vote shares persisted at the parliamentary elections, which took place one month after the presidential elections. On average, door-to-door canvassing increased the

vote share obtained by PS candidates at the second round by 0.5 percentage points. This effect is by no means negligible: the victory margin of the elected MPs was lower than 0.5 percentage points in 33 (6 percent) constituencies. The results were likely driven by the persuasion of medium and high-propensity voters, who voted at both the presidential and the parliamentary elections, rather than by the mobilization of left nonvoters and the demobilization of opponents. Overall, the results suggest that the mobilization effect of voter outreach methods might not extend to elections of very high salience, but that personal contact can have a large persuasion effect and can be an effective way to reconnect with disgruntled voters and reduce the attractiveness of extremist parties.

The remainder of the paper is organized as follows. Section 2 provides more background information on the French 2012 elections and on François Hollande's door-to-door campaign. Section 3 describes the experimental sample. Section 4 evaluates the overall impact of the door-to-door canvassing visits on voter turnout and vote shares at the presidential and the parliamentary elections. Section 5 concludes with a discussion of the results.

3.2 Setting

3.2.1 The 2012 French presidential and general elections

French presidential elections have two rounds. The two candidates who get the highest vote shares at the first round qualify for the second round. 79 percent of the registered citizens participated in the first round of the French presidential elections, on April 22nd 2012.[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 candidate in the center, divided his vote share by more than two (9.1 percent compared to 18.6 percent), and the vote share of the far-left candidates 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 at the second round, on May 6th, was high again (80 percent) and François Hollande

[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 are computed in this way. Since the door-to-door canvassing campaign started after the registration deadline of 31 December 2011, it could not affect the number of registered citizens.

was elected President with 51.6 percent of the votes.

Similarly to the presidential elections, the parliamentary elections consist of two rounds, unless a candidate obtains more than 50 percent of the votes at the first round. Unlike the presidential elections, all candidates who obtain a number of votes higher than 12.5 percent of registered citizens at the first round can compete in the second round. In practice, two or three candidates usually compete in the second round. The 2012 parliamentary elections took place on June 10^{th} and 17^{th}. Fewer voters (57 and 55 percent) participated in these elections than in the presidential elections and in the previous parliamentary elections, confirming the lower salience of the parliamentary elections and the declining trend of turnout (Figure 2). The PS candidates won in 57 percent of the constituencies.

3.2.2 From 2008 to 2012: transposing door-to-door canvassing a la Obama to France

In 2008, the successful campaign of Barack Obama generated unusual levels of public attention and enthusiasm across France. Several prominent French politicians and think tanks sought to adapt American electoral and campaign innovations to suit the French political context. As a first step, the PS abandoned the designation of its candidate to the presidential elections by sole party members in favor of open-primary elections, which were deemed to have contributed to the success of the once challenger Barack Obama (Terra Nova 2008, 2009).

A second step was the rationalization and modernization of electoral tactics and strategies. Among the many differences between French and American campaigns, two prominent ones are funding and the distribution of media access. Hollande's 2012 presidential campaign spent 29 million dollars, 38 times less than Barack Obama's 1,107 million dollars (Journal Officiel de la République française 2012; Federal Election Commission 2013). The bulk of Obama's money was spent on radio and television advertising. Instead, from the official start of the 2012 French presidential campaign, on 20 March 2012, to the first round, all French radio and TV channels were mandated to cover equally the campaign of each of the twelve candidates. Similarly, between the two rounds, they had to give equal coverage to Nicolas Sarkozy and François Hollande: in France, candidates do not compete with TV ads. If anything, the small budget and the impossibility of buying TV ads should lead candidates and their staff to put even more emphasis on the recruitment of a large

number of volunteers and to be particularly strategic when selecting field campaign methods. On the contrary, until recently, French electoral 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 method of campaigning. Although door-to-door canvassing had once been an important campaigning method, it had progressively been replaced by other more impersonal techniques, such as handing out flyers at markets and subway stations exits, or putting leaflets in mailboxes (Liegey, Muller, and Pons 2013). Only in a few cities did door-to-door canvassing remain a frequently used technique (Lefebvre 2005).

Similarly as in the United States, academic research played an important role in the renewed importance given to door-to-door canvassing. The first French randomized evaluation of a door-to-door canvassing get-out-the-vote effort (Pons and Liegey 2013) convinced the PS to scale up the method for the 2012 presidential election.[4]

3.2.3 François Hollande's 2012 door-to-door canvassing campaign

At the end of December 2011, François Hollande and his team decided to make door-to-door canvassing a priority of the campaign. Similarly as during the 2008 Obama campaign, the candidate's websites would be used as a means to recruit field activists. In effect, however, the online anonymous survey administered after the presidential elections to canvassers who had taken part in the campaign found that the fraction recruited through this channel was low (Table 1). French political parties have a stronger membership base than American parties. This proved to be a double-edged sword. On one hand, it provided a large network of highly motivated volunteers: 87 percent of the 1,972 respondents took part in three or more sessions of door-to-door canvassing, and 38 percent in more than ten sessions. On the other hand, many of these volunteers saw militancy as an important part of their identity and they were reluctant or at best unaccustomed to welcome newer and more intermittent supporters. As a result, by the end of the campaign, only 12 percent of the activists were sympathizers involved in a campaign for the first time, while 80 percent were members of the PS. Relatedly, two thirds of the canvassers were more than 46 years old, reflecting the skewed age pyramid of PS members. These figures show a stark contrast with the average American campaign volunteer who is much less likely to be an active party member and much younger.

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

The field organizers coordinating the volunteers were themselves recruited primarily among the heads of PS' local units, and most of the départements' coordinators had preexisting responsibilities within the PS. The volunteers, field organizers, and départements' coordinators were all voluntary, but fifteen paid regional coordinators assisted in the national coordination of the campaign. Finally, 150 unpaid national trainers with mixed backgrounds were recruited. Some had comprehensive field experience as PS members, while others had never been involved in any partisan activity. All, however, were highly motivated and highly educated. This team of trainers proved the most important asset to launch the campaign: every Saturday, they were sent to the local headquarters of the campaign across France to train field organizers and launch or amplify the scope of the door-to-door canvassing sessions. The trainings revolved around role playing and taught field organizers how to train and coordinate volunteers. 59 percent of the respondents to the post-electoral survey attended a training. These trainings addressed a real need: only 22 percent of the respondents had frequently done door-to-door canvassing before the campaign.

The trainings emphasized a simple message: this campaign was about door-to-door canvassing, and nothing else. Simplicity, it was thought, was key to achieve a radical change in campaign methods. The emphasis put on door-to-door canvassing was also evident in the campaign material sent to each département: next to leaflets, that can be given to voters through various methods, canvassers received door-hangers dedicated to the door-to-door canvassing campaign, and to be put on the handles of doors which failed to open. As a result, the scope of the campaign was without comparison with previous door-to-door efforts of any party.

The scope of the door-to-door effort can be estimated thanks to the reports made by the canvassers throughout the campaign. After each canvassing session, any participant registered on the field campaign's web platform "TousHollande Terrain" could report the number of doors knocked and opened, and provide qualitative feedback. The website enabled each volunteer or field organizer to follow the progress of the field campaign in his area and to compare it with other areas. At the national level, the départements with the fastest progresses turned red while those left behind remained white. Figure 3 shows snapshots of the maps that the départements' coordinators and field organizers could see in the five last weeks of the campaign. This system was supposed to create emulation, with some success. But the most important objective was to monitor the progress of the campaign and identify areas in need of additional support in a timely manner. In many cities, however, field organizers and canvassers never registered on the campaign platform. Even when

the campaign activists were registered on the platform, they only reported a fraction of all doors knocked. With the help of the fifteen regional coordinators of the campaign, it was estimated that 36 percent of doors knocked were actually reported on the website, for a total of approximately 5 million doors knocked.

Figure 4 plots the number of doors knocked over time. As is clear from this picture, the scale of the campaign was very low until six weeks before the first round of the presidential elections. It then increased gradually and reached its peak on the Saturday 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.

In addition to the aforementioned trainings, a large and enthusiastic media coverage[5] and the endorsement of door-to-door canvassing by many local and national figures of the PS contributed to motivating the canvassers.[6] The activists' own perception of door-to-door canvassing was equally important. At the end of the campaign, less than 1 percent said they would not do it again, against 61 percent who deemed it a really good method. For 28 percent, the main reason to like the method was that it was an enriching experience. For 31 percent it was its effectiveness, and 32 percent saw door-to-door canvassing as a good way to spread the ideas and values of the left. Before assessing whether they were correct, let me describe in more details the areas that were covered by the canvassers and the randomization process.

3.3 Sampling frame

3.3.1 Definition of 3,500 territories

Before the start of the door-to-door campaign, I first split the entire country into 3,539 territories, each defined by a set of one or several municipalities sharing a common zip code. In each département, zip codes with less than 5,000 registered citizens were subsumed under a unique territory.

[5]The door-to-door canvassing campaign was covered by 210 articles in French newspapers, 50 French television and radio reports, and 20 articles or TV and radio reports in foreign countries. In the United States, news reports included headlines such as "In France, Using Lessons From Obama Campaign" (*New York Times*, Steven Erlanger, April 21), "The American Connection" (*Slate*, Sasha Issenberg, April 20), "Décision 2012: A presidential election without political ads" (*NBC "Rock Center" footage*, Ted Koppel, April 18) and "Hollande uses Obama tactics to increase vote" (*Financial Times*, Hugh Carnegie, April 13).

[6]The former presidential candidate Ségolène Royal, the presidential candidate François Hollande and many of his future ministries personally knocked on doors (Liegey, Muller, and Pons 2013).

Any new activist joining the campaign through its websites was asked to provide his zip code and was automatically allocated to the corresponding territory.

From the start of the campaign, priority was given to the mobilization of left-wing nonvoters. I assumed that the vote choices of active voters were a good indicator of the preferences of nonvoters living in the same territory and used the following proxy for the fraction of left-wing nonvoters in each territory: $\widetilde{LA}$, the product of the fraction of nonvoters by the left vote share among active voters, each taken from the results of the second round of the 2007 presidential elections. In each territory, I set the target number of doors proportionally to this proxy. Overall, the goal was to knock at 5 million doors, or roughly 15 percent of all French housings.

3.3.2 Identification of priority precincts

In the United States, most states make individual turnout history publicly available. Political parties and private companies such as Catalist, LLC have long collected these databases and merged them with databases of registered Democrats, Republicans, and independents and, occasionally, with consumer databases (Ansolabehere and Hersh 2010; Issenberg 2012). "Voter IDs" complement these data: campaign activists ask their interlocutors at the door or on the phone how likely they are to vote and whether they lean towards their candidate or towards the opponent. In other countries besides the US, such individual-level profiling of voters is usually unavailable. In France, individual turnout information is not recorded electronically: voters who cast a ballot sign turnout sheets, which are only available for consultation until ten days after the election. People cannot register as supporters of a specific party. Finally, the campaign technology, less advanced than in the United States, does not allow the activists to record individual voter information. Therefore, instead of targeting individual voters, political campaigns have to target geographical areas.

To identify priority areas within each territory, I relied on the official results of the 2007 presidential elections at the precinct level, made publicly available by the French Ministry of the Interior. In addition, I used voter rolls that had been collected by the PS to organize its 2011 primary elections. These voter rolls indicated the address and precinct of each registered citizen and could thus be used to draw the geographical boundaries of precincts. In territories where the voter rolls were available for all or most municipalities, I ranked precincts according to $\widetilde{LA}$, the proxy for the number of left-wing nonvoters.[7] In territories where the voter rolls had not been collected by the

[7] Some municipalities redrew the boundaries of their precincts between 2007 and 2011, to accommodate for changes

PS or were missing for too many municipalities, I did not know the geographical boundaries of the precincts. Canvassers would most likely not have this information either. Thus, municipalities, and not precincts, were ranked according to $\widetilde{LA}$.

It is important to note that any precinct 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 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.

3.3.3 Randomization

Before conducting the randomization, I grouped precincts (or municipalities) of each territory in strata of five: the five precincts with the highest $\widetilde{LA}$ were allocated to the first stratum, the five precincts ranked immediately below were allocated to the second stratum, and so on until the last stratum, composed of the five or less remaining precincts. I only kept a subset of strata, those with the highest $\widetilde{LA}$, in the experimental sample.[8]

When there is no obvious reason to favor some units over others, the random choice of units receiving an intervention is costless. Differently, in this project, it was thought to be in the interest of the campaign to cover areas with the highest possible $\widetilde{LA}$. Randomization would thus come at a cost. To combine the two objectives of maximizing vote shares and learning from the campaign, only 20 percent of the precincts in the experimental sample were allocated to the control group. Specifically, in strata with five precincts, four were randomly selected to be included in the treatment group and the remaining precinct was included in the control group. In strata with strictly less

in the number of registered citizens. In these precincts, I thus had to estimate $\widetilde{LA}$ in a different way. First, I used the sample of cities where precincts' boundaries had not changed and regressed $\widetilde{LA}$ on precinct characteristics that could be inferred from the 2011 voters rolls: the average number of registered citizens per address (as a proxy for social housing), this variable squared, the share of individual houses, small buildings and big buildings, the average age, this variable squared, the proportion of registered citizens below 25 years old and the proportion of registered citizens above 65. Second, I used these estimated coefficients to predict $\widetilde{LA}$ in precincts with changed boundaries. This procedure was crude, granted, as many determinants of $\widetilde{LA}$ were unobserved. But any error in estimating $\widetilde{LA}$ only affected the selection of the experimental sample, not the internal validity of the results.

[8]The exact number of strata included in each territory was decided based on the target number of doors in the territory, the number of registered citizens in each stratum, and under the assumption that each door would host 1.4 registered citizens on average. The latter ratio was obtained by dividing the total number of registered citizens, 46.0 million, by the total number of housings, 33.2 million (Insee-SOeS 2012). A few territories where the targeting was conducted at the municipality level counted only one municipality. Then, canvassers were asked to cover this municipality, but the territory was dropped from the experimental sample.

than five precincts, each precinct had an 80 percent chance to be included in the treatment group and a 20 percent chance to be included in the control group.

Importantly, once the randomization was conducted, I only assigned a subset of the treatment precincts, those with the highest $\widetilde{LA}$, to the canvassers. This ensured that the canvassers would cover the most priority areas and that the total size of the precincts that they were assigned corresponded to the number of doors they had to knock. In each territory, the exact number of assigned precincts depended on the number of registered citizens per precinct and on the target number of doors in the territory.

Table 2 emphasizes the distinction between precincts *included in the treatment group* and, within this set, precincts *assigned to canvassers*. As shown in Panel A, 17,986 precincts (80%) were included in the treatment group and 4,466 (20%) were included in the control group. As shown in Panel B, 9,521 of the 17,986 treatment precincts were assigned to the canvassers, and 8,465 were not. Treatment precincts assigned to the canvassers had a total of 7.2 million registered citizens, or approximately 5.2 million doors. Treatment precincts not assigned to canvassers and control precincts had a total of 9.8 million registered citizens, or approximately 7.1 million doors.

To preserve the integrity of the randomization, treatment precincts that were not assigned to the canvassers are maintained in the treatment group when estimating the impact of door-to-door canvassing. To account for the fact that they were not assigned to the canvassers, I present treatment-on-the-treated estimates that instrument "assigned" with the treatment group, rather than intent-to-treat estimates.

3.3.4 Final sample

In each territory, volunteers and field organizers could download the list of precincts that they were assigned from their personal account on the campaign's platform. For strategic reasons, it was impossible to otherwise access to this list. However, as mentioned earlier, in some territories, no volunteer or field organizer ever registered on the platform. As a result, canvassers in these territories were not aware of and could not follow the list of assigned precincts. To avoid introducing noise in the analysis, I exclude the corresponding territories.

In addition, a fraction of municipalities located in territories where the randomization was conducted at the precinct level redrew the boundaries of their precincts and added or dropped precincts between 2011 and 2012 to accommodate for changes in the number of registered citizens.

I identify these municipalities as those where the number of precincts changed between the two dates and exclude them from the analysis.

In the end, the analysis is based on a sample of 6,615 precincts. 3,195 are located in territories where the randomization was conducted at the precinct level, and 3,420 in territories where the randomization was conducted at the municipality level.

3.3.5 Imperfect compliance

Even in the territories where some volunteers and field organizers did register on the platform, and could thus download the list of assigned precincts, compliance with this list was not perfect.

To foster compliance and facilitate the logistical organization of the canvassing sessions, in addition to the list of assigned precincts in their territory, canvassers could also download lists of corresponding addresses, with the number of registered citizens in each address as a proxy for its size.[9] It was emphasized that these precincts and addresses hosted a disproportionately large number of left-wing nonvoters, the main target of the campaign. However, in territories where the number of canvassers was too small, they could not possibly cover all assigned addresses. In addition, canvassers often covered other addresses: 29 percent of the respondents to the post-electoral survey state that they never heard of the list of priority precincts provided by the campaign; 16 percent that they did not use this list at all, or only very little; and 11 percent that they used this list only partially (Table 1). This is not too surprising. As mentioned earlier, the local units of the PS have a culture of relative autonomy, and the resources available to coordinate the campaign made it impossible to monitor all canvassing sessions on a day-to-day basis. Specific reasons to cover other areas than those indicated by us included their proximity to canvassers' home or workplace, their perceived importance by the canvassers, and past campaigning habits.

It is important to estimate the degree to which the canvassers followed the lists of assigned precincts and municipalities: failure to do so would bias results towards zero. Fortunately, when canvassers entered a report on the campaign's platform, they could indicate the precinct that they had covered. 55 percent of the reports, accounting for 48 percent of doors knocked, include this information (Table 2, Panel C). 37.4 percent of all doors knocked were located in precincts assigned to canvassers; 14.1 percent in precincts that were not assigned to them but that were

[9] Unfortunately, I could only create these lists in territories where the targeting and randomization had been done at the precinct, not the municipality level.

included in the experimental sample; and 48.5 percent in precincts that were not included in the experimental sample. Let us assume that these detailed reports are representative of the extent to which canvassers followed the lists of assigned areas, and that canvassers never knocked at any door more than once. These assumptions are conservative: if anything, they should lead us to overestimate compliance. Based on these assumptions, I obtain that the canvassers knocked on $0.374 * 5 = 1.9$ million doors in assigned precincts and $0.141 * 5 = 0.7$ million doors in precincts that they had not been assigned but that were included in the experimental sample. Since these two groups had initially 5.2 and 7.1 million doors, I infer that the canvassers covered $f_a = \frac{1.9}{5.2} = 35.9$ percent of doors in assigned precincts; and $f_{\bar{a}} = \frac{0.7}{7.1} = 10.0$ percent of doors in precincts that they had not been assigned but that were included in the experimental sample. Therefore, to estimate the effects of the campaign in precincts that were covered by canvassers and would not have been covered if they had not been assigned to them, I scale up the raw estimates by a factor of $\frac{1}{f_a - f_{\bar{a}}} = \frac{1}{0.359 - 0.100} = 3.9$.

3.4 Results

3.4.1 Verifying randomization

Randomization ensures that all observable and unobservable characteristics should be symmetrically distributed between treatment and control precincts. Table 3 verifies this for a series of observed characteristics. It presents summary statistics for precincts in the sample, 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 extremely similar. Out of 54 differences shown in Table 3, 7 are significantly different from zero at the 10 percent level, 3 are significantly different from zero at the 5 percent level, and 1 is significantly different from zero at the 1 percent level, as could be expected.

The average precinct counted 919 registered citizens, with an estimated 8.7 percent left-wing nonvoters at the 2007 presidential elections. All 22 French regions as well as the French territories overseas (DOM-TOM) were represented in the sample. Five regions accounted for more than 50 percent of the entire sample: Ile-de-France, the most populated region where Paris is located,

Rhône-Alpes, the second most populated region, Bretagne, Pays-de-la-Loire, and Lorraine. In addition to this data available at the precinct level, I collected census data at the municipality level from the French Census Office, the Institut national de la statistique et des études économiques (INSEE 2013). The municipality of the average precinct counted 53,000 citizens. 14 percent of precincts were located in rural areas, 36 percent in center cities, 10 percent in isolated cities, and 40 percent in suburbs. In the municipality of the average precinct, 48 percent of the inhabitants were men, 56 percent were less than 45 years old, and the mobility rate (the share of the population who arrived in the municipality over the five past years) was 24 percent. The working population accounted for 72 percent of all people aged 15 to 64, and the unemployed for 12 percent of the working population. Finally, the INSEE distinguishes eight socio-professional groups among people above 15 years old: farmers (PCS1), craftsmen, storekeepers and businessmen (PCS2), executives and intellectual occupations (PCS3), intermediate occupations (PCS4), employees (PCS5), laborers (PCS6), retired people (PCS7), and other inactive people (PCS8). On average, inactive people accounted for 44 percent of all people above 15 years old and PC3, PCS4, PCS5, and PCS6 for 8 percent, 14 percent, 17 percent, and 14 percent respectively.

3.4.2 Effects on voter turnout

I first examine the impact of the door-to-door canvassing campaign on voter turnout. I plot electoral participation in control and treatment precincts in Figure 5, and find that it was almost identical in both groups, and just below the national average: 79.2 and 79.8 percent at the first and second rounds of the presidential elections, and 56.1 percent and 55.2 percent at the parliamentary elections.

To investigate the statistical significance of the differences shown in Figure 5 more systematically, I estimate the following OLS regression:

$$P_i = \alpha + \beta A_i + X_i^{'}\lambda + \sum_s \delta_i^s + c_i \qquad (1)$$

where P_i is electoral participation among registered citizens in precinct i, A_i is a dummy equal to 1 if the precinct was assigned to the canvassers, X_i is a vector of precincts characteristics and δ_i^s are strata fixed effects. A_i is instrumented with T_i, a dummy equal to 1 if the precinct was included in the treatment group. X_i includes $\widetilde{LA}$ (the proxy for the fraction of left-wing nonvoters in 2007), the number of registered citizens in the municipality, this variable squared, the share of working

population within people aged 15 to 64, and the share of unemployed people among the working population. The key coefficient of interest is β, which indicates the differential participation in precincts that were assigned to canvassers. In this and all other regressions, I adjust standard errors for clustering at the level of randomization (precinct or municipality).

The results from Equation [1] are presented in Table 4. Panel A uses voter turnout at the first round (columns 1 and 2), at the second round (columns 3 and 4), and averaged over the two rounds (columns 5 and 6) of the presidential elections as the outcome. Door-to-door canvassing had no significant effect on voter turnout at either the first or the second round: the estimated βs are very small and statistically not significant. These estimates are robust to the inclusion of the control variables.

Panel B uses voter turnout at the parliamentary elections as the outcome. The sample is restricted to constituencies where there was a PS candidate at the first round (columns 1 and 2) and to constituencies where there was a second round with more than one candidate[10] and with a PS candidate (columns 3 through 6). Again, I find no significant effect on voter turnout.

3.4.3 Effects on vote shares

Vote shares of François Hollande at the presidential elections

I now examine the impact of door-to-door canvassing on François Hollande's vote shares at the presidential elections and on PS candidates' vote shares at the parliamentary elections. Figure 6 shows the vote shares obtained by these candidates in control and treatment precincts. In all rounds, the share is higher than the national average. In addition, it is higher by 0.3 to 0.5 percentage points in treatment precincts compared to control precincts. To investigate the impact on vote shares more systematically, I estimate OLS specifications of the form in Equation [2]:

$$S_i = \alpha + \beta A_i + X_i^{'}\lambda + \sum_s \delta_i^s + \epsilon_i \qquad (2)$$

where S_i is the PS vote share in precinct i, and A_i is once again instrumented with T_i. β indicates the differential PS vote share in precincts that were assigned to canvassers. Table 5 presents the results both for the presidential elections (Panel A) and for the parliamentary elections (Panel B).

[10] When two candidates qualify for the second round, it is sometimes the case that one of the two steps aside, so that a second round is held but with only one candidate.

In areas assigned to canvassers, door-to-door canvassing increased François Hollande's vote share by 0.8 percentage points at the first round of the presidential elections (Panel A, column 1). This estimate is significant at the 1 percent level and robust to the inclusion of the control variables (column 2). The effect on Hollande's second round vote share was 0.7 percentage points, an effect significant at the 10 percent level (column 3). When control variables are included, this estimate is slightly lower and no longer significant (column 4).

As discussed in Section 3.4, the canvassers complied imperfectly with the precincts to which they had been assigned. To take into account this imperfect compliance, I apply the multiplier of 3.9 computed in Section 3.4 and obtain effects of 3.1 percentage points and 2.7 percentage points at the first and second rounds. This measures the impact of door-to-door canvassing for addresses that were covered by canvassers and would not have been covered had they not been assigned.

Assuming that the impact was identical in the other addresses covered, and since the canvassers covered 15 percent of all French housings, I estimate that the door-to-door canvassing campaign increased François Hollande's national vote share by 0.5 percentage points at the first round of the presidential elections. The effect on Hollande's vote share at the second round was 0.4 percentage points; it was mechanically mirrored by a negative effect of the same size on the vote share of the other candidate, Nicolas Sarkozy. Thus, in total, door-to-door canvassing increased Hollande's victory margin by 0.8 percentage points. Since he won with 51.6 percent of the votes, against 48.4 for Nicolas Sarkozy, the effect of door-to-door canvassing accounted for approximately one fourth of the victory margin.

Vote shares of PS candidates at the parliamentary elections

The effect of door-to-door canvassing persisted in the parliamentary elections. As shown in Table 5, Panel B, the effect on PS candidates' vote shares in assigned areas was 0.5 percentage points at the first round (column 1) and 0.9 percentage points at the second round (column 3). Due to smaller sample sizes, although these estimates are comparable in magnitude to the presidential elections, they are only marginally significant. The second round effect is significant at the 10 percent level. When including control variables, however, it is no longer statistically significant (column 4).

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 0.5 percentage points, on average, at the second round of the parliamentary elections. In 94 percent of the constituencies,

the PS candidate was opposed against a single other candidate at the second round. In these constituencies, the 0.5 percentage points average increase in PS vote share mechanically amounts to an increase in the difference between the vote shares of the two candidates of 1 percentage point. This is by no means negligible, considering that the victory margin of the winner was less than 1 percentage point in 6 percent of all constituencies, and that PS candidates won by a margin of less than 1 percentage point in 5 percent of all constituencies in which they won.

Vote shares of non-PS candidates

The positive effect of door-to-door canvassing on the vote shares obtained by PS candidates also means that it decreased the vote shares of candidates of other parties. To assess the extent to which the different parties were affected, I estimate OLS specifications of the form in Equation [2], using the vote shares obtained by other candidates instead of the PS candidate as the outcome S_i. Table 6 performs this analysis for the first round of the presidential elections (Panel A) and for the first round of the parliamentary elections (Panel B).[11]

Columns 1 and 2 are identical to columns 1 and 2 of Table 5, and they are included for reference only. Compared to the national averages (see Figure 1), in the control group, the center and right-wing parties obtained fewer votes in both elections. The effect of door-to-door canvassing on vote shares of the far-left, other left, and center candidates at the first round of the presidential elections was close to 0 (Panel A, columns 3 through 8). The effect on the vote shares of the right candidates Nicolas Sarkozy and Nicolas Dupont-Aignan was negative (-0.3 percentage points) but not significant (columns 9 and 10). The effect on the vote shares of the far-right candidate, Marine Le Pen, was negative (-0.4 percentage points) and significant at the 10 percent level (column 11). When including the control variables, the estimate is slightly lower and not significant (column 12).

I find similar results for the first round of the parliamentary elections (Panel B). Once again, the center and right-wing candidates obtained fewer votes in the precincts of the sample than the national average. The door-to-door canvassing campaign did not significantly affect the vote shares of candidates of any orientation, except for the far-right, who were mostly FN candidates: on average, it decreased the vote shares of these candidates by 0.6 percentage points, an effect

[11] Since there were only two candidates at the second round of the presidential elections, the effect on the second candidate, Nicolas Sarkozy, is by definition exactly opposite to the effect on François Hollande. Similarly, there were only two candidates left in the second round of the parliamentary elections in most constituencies. As a result, I lack statistical power to estimate the effects on vote shares where three candidates competed in the second round.

significant at the 1 percent level (column 11). I obtain a similar point estimate, significant at the 5 percent level, when including the control variables (column 12).

After taking into account the imperfect compliance of the canvassers with the precincts that they had been assigned, the effects measured for the far-right candidates correspond to effects of -1.6 and -2.3 percentage points in addresses that were covered by canvassers and would not have been covered had they not been assigned.

3.5 Discussion and conclusion

3.5.1 Interpretation of the results

A persuasion effect

This project investigated the impact of door-to-door canvassing on electoral participation and vote shares in the context of the 2012 French presidential elections. An estimated 80,000 left-wing activists knocked at 5 million doors to encourage people to vote for the candidate of the Parti Socialiste, François Hollande. Although left-wing nonvoters had been the main target of the campaign, surprisingly, door-to-door canvassing did not affect voter turnout. The scale of the experiment and the related statistical power allow ruling out even small positive or negative effects. However, door-to-door canvassing increased François Hollande's vote share by 3.1 percentage points and 2.7 percentage points at the first and second rounds. This contributed to approximately one fourth of Hollande's victory margin at the second round. At the same time, door-to-door canvassing decreased the vote share obtained by Marine Le Pen, the candidate of Front National, at the first round, and the vote share obtained by Nicolas Sarkozy, the incumbent, at the second round.

There are two possible interpretations of the results. The first interpretation is that the intervention mobilized left-wing nonvoters and cross-pressured and demobilized an equal number of (far) right-wing voters (Fiorina 1976). However, since the effects observed at the presidential elections persisted in the parliamentary elections, this interpretation can only hold if door-to-door canvassing mobilized nonvoters who participated both in the presidential and in the parliamentary elections. This seems unlikely. Indeed, the door-to-door canvassing campaign was of much lower intensity for the parliamentary elections than for the presidential elections.[12] But absent any renewed con-

[12]The low intensity of the door-to-door canvassing effort that took place for the parliamentary elections is the result of several factors. First, in many constituencies, the parliamentary election was less contested than the presidential

tact, we should not expect chronic nonvoters mobilized for high-salience elections to also vote at much lower-salience elections. Quite the contrary, Arceneaux and Nickerson (2009) find that the type of people that can get mobilized to vote depends on the saliency of the election and that only moderate-propensity voters – and not chronic nonvoters – can be mobilized in low salience elections.

An alternative and more plausible interpretation is that the effect of door-to-door canvassing on Hollande's vote share was obtained by persuading voters leaning towards other candidates, and notably towards Marine Le Pen. Under this interpretation, the persistence of the effects at the second round of the presidential elections suggests that the majority of these persuaded voters would have voted for Nicolas Sarkozy at the second round, absent the door-to-door canvassing campaign. Similarly, the persistent effect observed at the parliamentary elections suggests that these voters were equally likely to participate in these elections as other voters who had participated in the presidential elections, and that their vote choice was consistent in the two elections. Among voters living in apartments that opened their door to the canvassers, what fraction was persuaded? Since 48 percent of the doors knocked at by canvassers opened, I scale the previous estimates by $\frac{1}{0.48}$ and find that 6.5 percent and 5.6 percent of the voters living in apartments that opened their door were persuaded to vote for François Hollande at the first and second rounds of the presidential elections.[13] More specifically, at the first round, 3.3 percent of the voters living in apartments which opened their door would have voted for Marine Le Pen absent door-to-door canvassing, but voted for François Hollande instead. This represents approximately 20 percent of the electorate of Marine Le Pen.

How could left-wing canvassers persuade far-right voters?

The fact that the persuasion effect was largest among Marine Le Pen's electorate might be surprising at first: the localization of the FN on the far-right makes it the party most distant to the PS in the one-dimensional representation of the political spectrum. However, the distance that voters switching from the FN to the PS had to cover should not be overestimated.

In France as in most countries in Western Europe and in the US, the left-right split has not

elections. Second, the national coordination and support for door-to-door canvassing ceased immediately after the presidential elections, as they were funded by the presidential campaign. Third, the general sense was that the party that had won the presidential race would also win the parliamentary elections. This resulted in a lower sense of urgency among PS campaign activists.

[13] I assume that, on average, doors that opened hosted as many registered citizens as doors that did not, and I count as "treated" all citizens living in an apartment that opened its door, regardless of whether they interacted personally with the canvassers or not.

one, but two dimensions, sociocultural and economic, which overlap only imperfectly (e.g., Lipset 1959; Fleishman 1988; Heath, Evans, and Martin 1994; Knutsen 1995; Evans, Heath, and Lalljee 1996). The sociocultural dimension has gained importance since the 1970s (Inglehart 1971, 1977, 1981, 1990; Clark and Lipset 1991; Clark, Lipset, and Rempel 1993; Inglehart and Abramson 1994; Nieuwbeerta 1996; Layman and Carmines 1997). On this first dimension, the platforms of the FN and the PS, and the attitudes of their respective electorates, 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 2011, 2013). But the economic dimension still matters (e.g., Van der Waal, Achterberg, and Houtman 2007). On this second dimension, the distance between the FN and the PS, which traditionally promotes state interventionism against economic liberalism, is much smaller. After the austerity turn taken by the socialist government of François Mitterrand in 1983, large numbers of working-class left-wing voters started voting for the FN as a way to protest and voice their disappointment (Mayer 2003). They contributed to the first electoral successes of the FN, although their identification with the party and their electoral stability remained low: among the FN electorate at the 1988 presidential elections, only one third said that the party they felt the closest to was the FN, while 20 percent mentioned that they preferred a left-wing party (Mayer and Perrineau 1992). In today's context of sluggish economic growth and high unemployment, the distrust of the political system and of traditional parties has spread, particularly among blue-collar workers, employees, and other groups exposed to unemployment and precariousness.[14] These groups of people used to vote for left-wing candidates predominantly, but an increasing number are inclined to vote for the FN (e.g., Evans 2000). Its appeal originates in an anti-elite populism directed against traditional parties of both left and right, the corrupt political establishment, and the privileged few; in a simplified vision of the world, which presents immigration as the source of all economic and social difficulties; and in the social tone that Marine Le Pen gave to her platform after she succeeded her father as the leader of the party 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. In sum, in addition to a *core* of strongly affiliated supporters who fully adhere to its values, the FN has long attracted and continues to attract *swing* dissatisfied voters who

[14]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, 48 percent trust neither the right nor the left to rule the country and 66 percent do not trust political parties (Cevipof 2012).

come both from the right and from the left, many of whom hold "left" preferences on economic issues (Perrineau 1995; Mayer 2011). The (relative) proximity between the FN and the PS on this dimension explains why 23 percent of voters who voted for Marine Le Pen in the first round of the 2012 presidential election and who voted in the second round then voted for François Hollande instead of Nicolas Sarkozy (Cevipof 2012). This is all the more striking as, on the sociocultural dimension, Sarkozy's positioning was closer to the FN than it had ever been: his campaign put a large emphasis on issues such as immigration and insecurity which FN voters rank among the most important (Mayer 2013).

It is likely that door-to-door canvassing persuaded fewer FN core supporters than swing supporters – in other words, that the door-to-door effort enabled the PS to reconnect with dissatisfied voters, notably among those who had once voted left. I provide an indirect test of this hypothesis by comparing the effect in areas with different proportions of FN core and swing supporters. Although I cannot directly measure these proportions, the results of the 2002 and 2007 elections enable me to construct a proxy. The 2002 presidential elections marked the culmination of the FN's electoral ascent (see Figure 7). On April 21, Jean-Marie Le Pen obtained 16.9 percent of the votes and qualified for the second round. Conversely, in 2007, the scores of the FN attained historic lows: after the "earthquake of 21 April 2002", the electorate repolarized around Nicolas Sarkozy on the right and Ségolène Royal on the left. Jean-Marie Le Pen only obtained 10.4 percent of the votes at the presidential elections, his lowest score since his first participation in the presidential elections in 1974. A few weeks later, FN candidates obtained only 4.3 percent of all votes at the parliamentary elections, their lowest score since 1981. Numerically and demographically, the people who voted for the FN at the 2007 elections formed the hard core of his electorate (Mayer 2011). On the contrary, the electorate of the FN at the 2002 elections was composed of both core and swing supporters. Similarly, both core and swing supporters voted for the FN at the 2012 elections, giving it its highest score at the presidential elections and its second to highest score at the parliamentary elections.

At the municipality level, the 2007 FN vote share is thus a good indicator of the size of its core electorate. Symmetrically, for a given 2007 FN vote share, a higher 2002 FN vote share indicates a larger fraction of swing supporters. If the effect of door-to-door canvassing was obtained by persuading FN swing supporters, as I hypothesize, this effect should be lower in municipalities with a higher 2007 FN vote share, and larger in municipalities with a higher 2002 FN vote share. To

test this prediction, I estimate the following model:

$$S_i = \alpha + \beta A_i + \beta_{2002} A_i \times \mathbf{1}_{FN_{i,2002} \geq \overline{FN}_{2002}} + \beta_{2007} A_i \times \mathbf{1}_{FN_{i,2007} \geq \overline{FN}_{2007}} + \\ + \delta_{2002} \mathbf{1}_{FN_{i,2002} \geq \overline{FN}_{2002}} + \delta_{2007} \mathbf{1}_{FN_{i,2007} \geq \overline{FN}_{2007}} + X_i' \lambda + \textstyle\sum_s \delta_i^s + \epsilon_i \qquad (3)$$

where S_i is the vote share obtained by the PS or the FN candidate in precinct i and $\mathbf{1}_{FN_{i,2002} \geq \overline{FN}_{2002}}$ and $\mathbf{1}_{FN_{i,2007} \geq \overline{FN}_{2007}}$ are dummies equal to 1 if the precinct is located in a municipality where the 2002 and 2007 FN vote shares, respectively, were higher than the sample median. When the model is estimated for the presidential (parliamentary) elections, $\mathbf{1}_{FN_{i,2002} \geq \overline{FN}_{2002}}$ and $\mathbf{1}_{FN_{i,2007} \geq \overline{FN}_{2007}}$ are computed based on the 2002 and 2007 presidential (parliamentary) elections. A_i is once again instrumented with T_i, $A_i \times \mathbf{1}_{FN_{i,2002} \geq \overline{FN}_{2002}}$ is instrumented with $T_i \times \mathbf{1}_{FN_{i,2002} \geq \overline{FN}_{2002}}$ and $A_i \times \mathbf{1}_{FN_{i,2007} \geq \overline{FN}_{2007}}$ is instrumented with $T_i \times \mathbf{1}_{FN_{i,2007} \geq \overline{FN}_{2007}}$. The key coefficients of interest are β_{2002} and β_{2007}, which indicate the differential effect of canvassing in precincts located in municipalities where the 2002 and 2007 FN vote shares were higher than the median.

Table 7 performs this analysis. The data provides some support for the hypothesis. At the first round of the presidential elections (Panel A), the negative effect of door-to-door canvassing on Marine Le Pen's vote share was larger by 1.3 percentage points in municipalities where the 2002 FN vote share was higher than the median (column 1). On the contrary, the effect was lower in municipalities where the 2007 FN vote share was higher than the median: in a municipality where the 2002 and 2007 FN vote shares were both higher than the median, the effect was approximately zero (-0.005 - 0.013 + 0.018 = 0). These estimates are significant at the 1 percent level, and they are robust to the inclusion of control variables (column 2). However, I do not find any evidence of heterogeneity of the effect of door-to-door canvassing on Hollande's vote share along past vote shares of the FN (columns 3 and 4). Turning to the parliamentary elections (Panel B), there is some evidence that the effect of door-to-door canvassing on both the far-right and PS vote shares was higher where the 2002 FN vote share was high, and that it was lower where the 2007 FN vote share was high, as expected. However, these estimates are not statistically significant.

What exactly persuaded FN voters to vote for the PS instead? To the extent that many of them had voted for left wing parties in the past and had then switched to the FN, it is likely that they were persuaded by the signal sent by the canvassers' presence more than by their specific arguments. Door-to-door canvassing showed that Hollande and his supporters were willing to bridge the gap

with disgruntled voters; it contrasted with the idea that the political world is solely populated by the privileged few seeking their own personal interest; and it comforted the image of the PS as a modern and innovative party, which had successfully organized open primaries and, once again, found new ways to reinvigorate democracy. This interpretation resonates with Barton, Castillo, and Petrie's (2011) finding that a candidate's presence matters more than the political message delivered. It should be tested by future research.

3.5.2 External validity

I conclude by discussing the external validity of the findings. They were obtained on a large sample, spanning all French regions and encompassing areas of very different types, from rural villages to the capital, Paris. But to what extent are the results informative about other types of elections and other national contexts?

The lack of impact of door-to-door canvassing on turnout differs from most previous field experiments. Let us examine possible interpretations of this difference. First, this campaign was partisan, unlike the first experimental studies of door-to-door canvassing (e.g., Gerber and Green 2000). But subsequent papers found that partisan door-to-door canvassing can mobilize voters too, and that it can be as effective as non-partisan door-to-door canvassing (e.g., Nickerson, Friedrichs, and King 2006).

Second, this campaign took place in France, when the bulk of the existing literature is about the US. But recent papers show that door-to-door canvassing can effectively mobilize voters in other countries as well, including China (Guan and Green 2006) and England (John and Brannan 2008). Are French nonvoters less receptive to door-to-door canvassing than voters in these countries, perhaps because they find it too intrusive? There does not seem to be much empirical support for this claim. Instead, Pons and Liegey (2013) find that door-to-door canvassing increased turnout at the 2010 French regional elections, with a large impact on immigrants. The enthusiastic media coverage of the 2012 door-to-door canvassing campaign, the positive feedback provided by the respondents to the post-electoral survey, and my own field experience all support the view that canvassing is not at odds with French culture.

Third, French presidential elections are characterized by very high salience and turnout. The 2012 presidential elections were no exception, with a participation of more than 80 percent of registered citizens at the second round. This could well be the decisive factor: high baseline participation

might have placed a ceiling on the marginal effect that the canvassers could have obtained in increasing participation. In support of this claim, while some previous studies found a significant impact of door-to-door canvassing in the context of high-salience elections (e.g., Gerber and Green 2008; Middleton and Green 2008), others did not find any impact (Bennion 2005; Michelson 2005). In addition, to my knowledge, previous randomized evaluations of door-to-door canvassing were all conducted in the context of elections of lower salience. In particular, voter turnout is much lower in the US than in this study, including at presidential elections. Arceneaux and Nickerson (2009)'s review shows that the voters' baseline propensity to vote conditions the effectiveness of door-to-door canvassing. In the context of high-turnout elections, campaigns mobilize low-propensity voters. Following this logic, during the very high salience French 2012 presidential elections, where the level of political awareness and encouragements to vote by friends and family members reached its peak, there might have been no one left to mobilize. Registered citizens who did not participate in these elections might be of a type that cannot get mobilized by a short interaction, either because they were absent on election day or "misregistered",[15] making voting too costly for them; or because they were too alienated from the political system. If this interpretation is correct, we can expect the null result on participation to extend to other elections of very high salience and turnout.

I finally turn to discussing the external validity of the impact of door-to-door canvassing on vote shares. The relatively large size of this effect could be related to two dimensions, which should be further investigated by continued research. First, if it is true that canvassers persuaded voters by sending a signal of quality rather than by specific arguments, then the impact on vote shares might have been enhanced by the fact that most voters contacted by the campaign had never interacted with canvassers before. As more parties engage in door-to-door canvassing campaigns, their persuasion effect might dampen out. Second, the persuasion effect might vary negatively with the intensity of voters' partisan affiliations. In France, the diversity of political parties and platforms results in weaker partisan affiliations and more frequent changes in vote choice than in bipartisan contexts, such as in the US. This might have contributed to the strong persuasion effect of door-to-door canvassing.

[15] Misregistered citizens are citizens who stay registered at a previous address and can only vote by proxy or by travelling back, making voting more costly (Braconnier, Dormagen, and Pons 2013).

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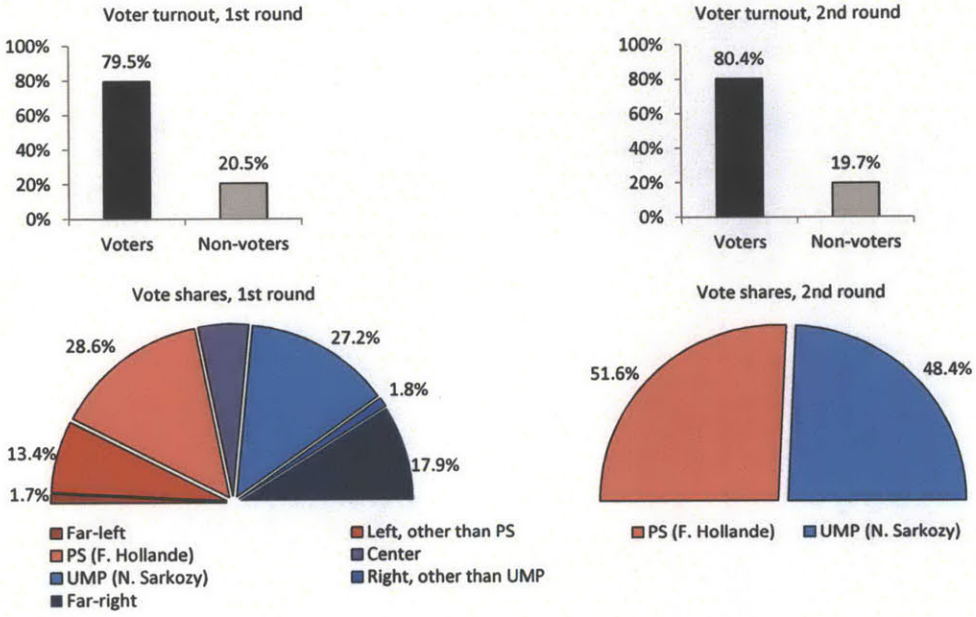
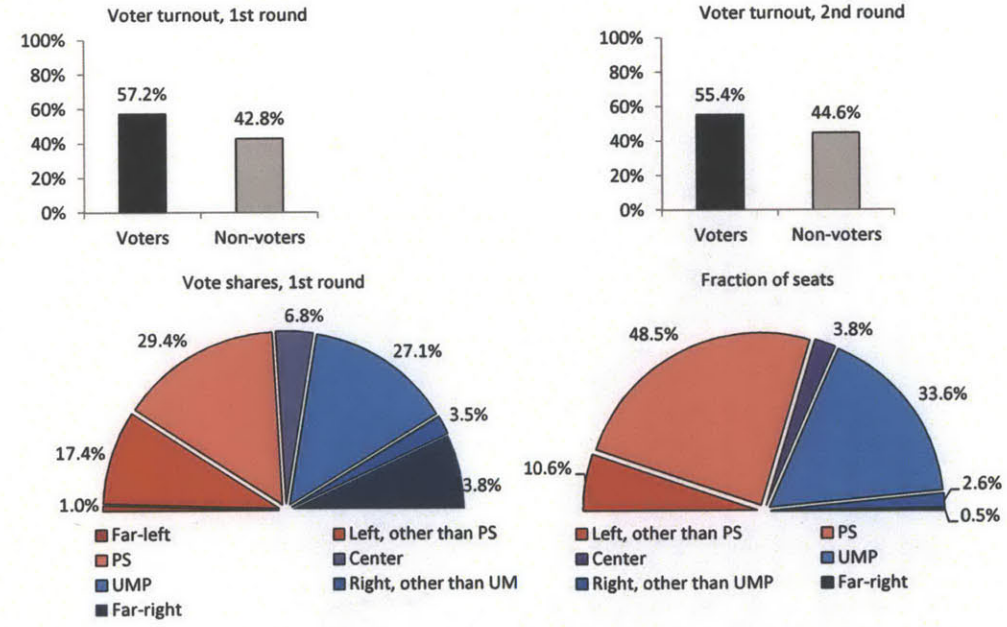
Figure 1. Results of the 2012 elections

Presidential elections

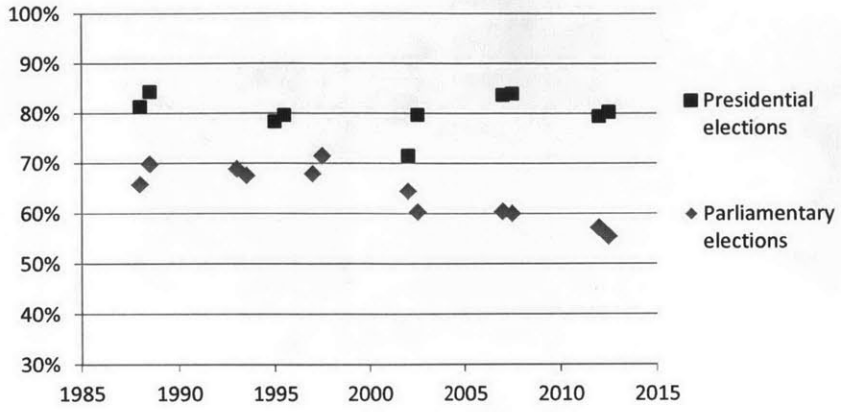


Parliamentary elections



Source: French Ministry of the Interior
Notes: At the first round of the presidential elections, the far-left candidates were Philippe Poutou (Nouveau Parti Anticapitaliste) and Nathalie Arthaud (Lutte Ouvrière). The left candidates other than PS 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 UMP was Nicolas Dupont-Aignan (Debout la République).

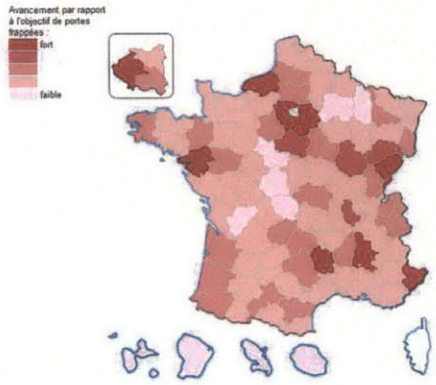
Figure 2. Turnout at French presidential and parliamentary elections, 1988-2012



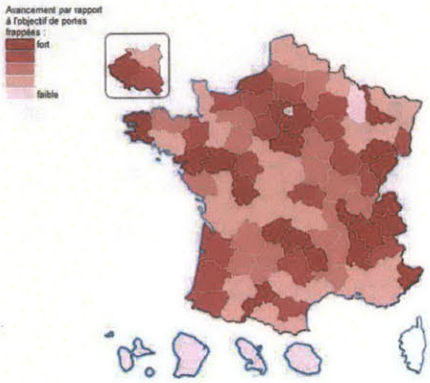
Source: French Ministry of the Interior
Notes: French turnout rates are computed using the number of registered citizens (rather than the number of eligible citizens) as the denominator.

Figure 3. Weekly completion of the target objectives, by département

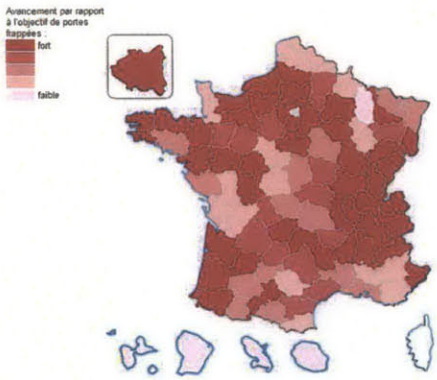
April 6th



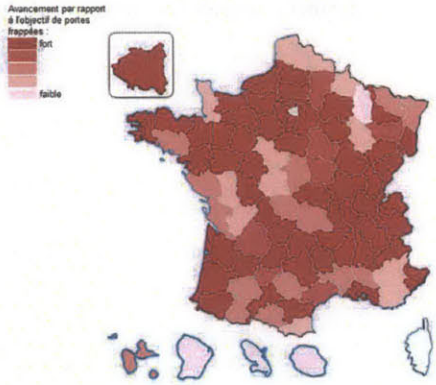
April 13th



April 20th



April 27th



May 4th

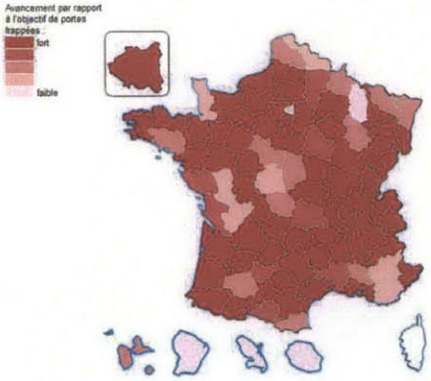
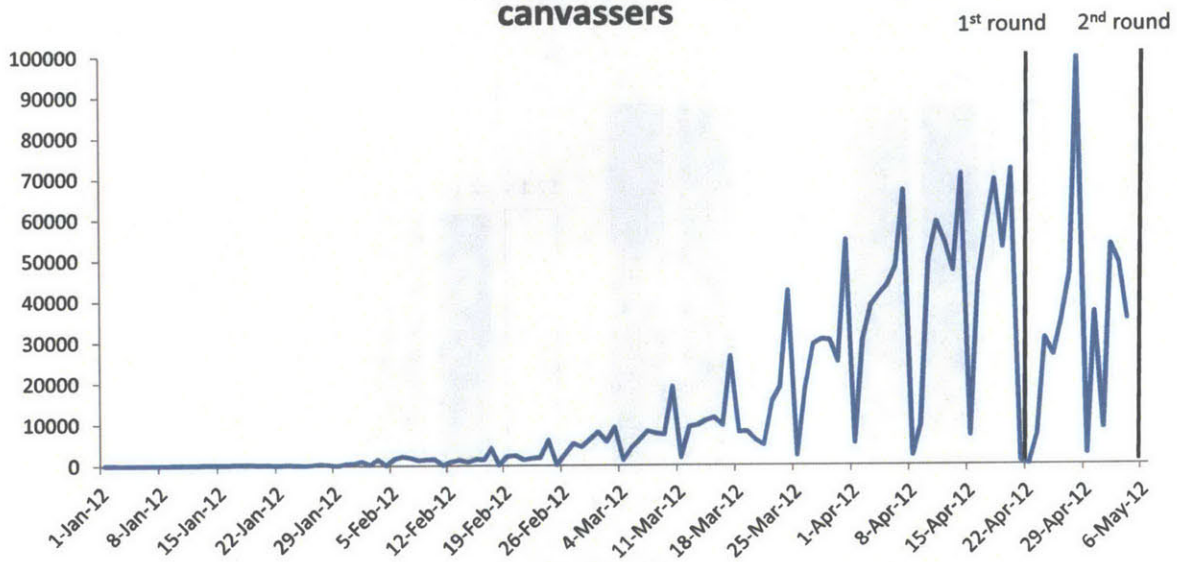
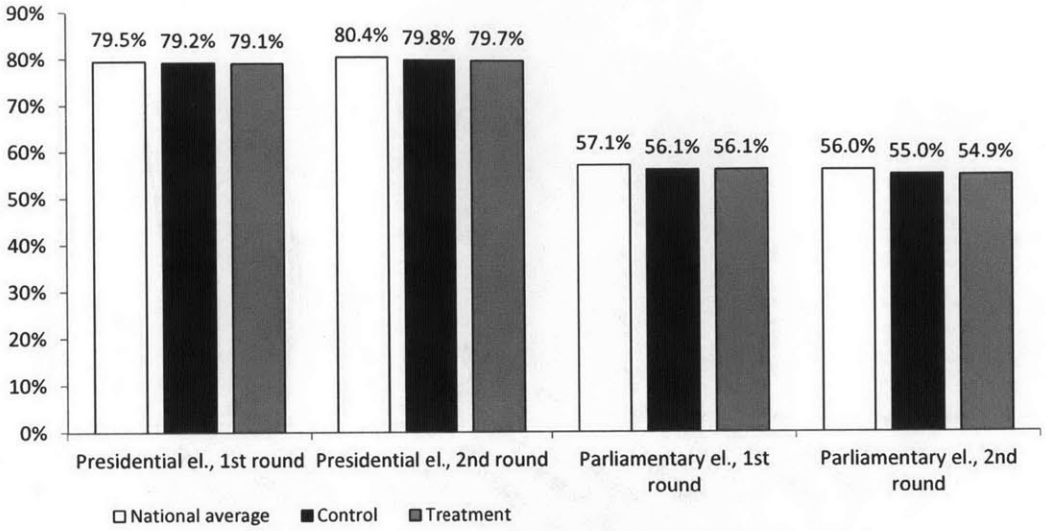


Figure 4. Daily number of doors knocked, as reported by canvassers



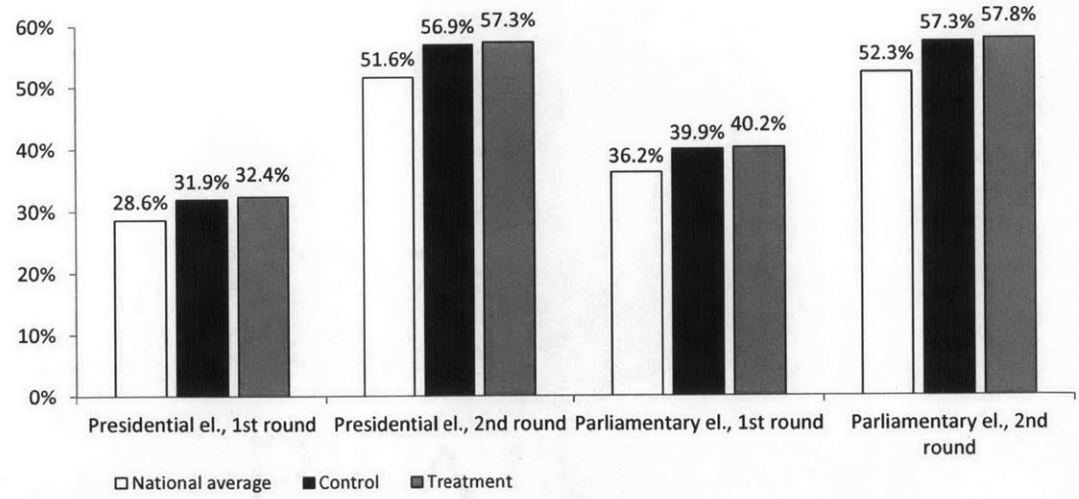
Notes: We plot the number of doors knocked by canvassers and reported by them on the campaign's platform.

Figure 5. Voter turnout



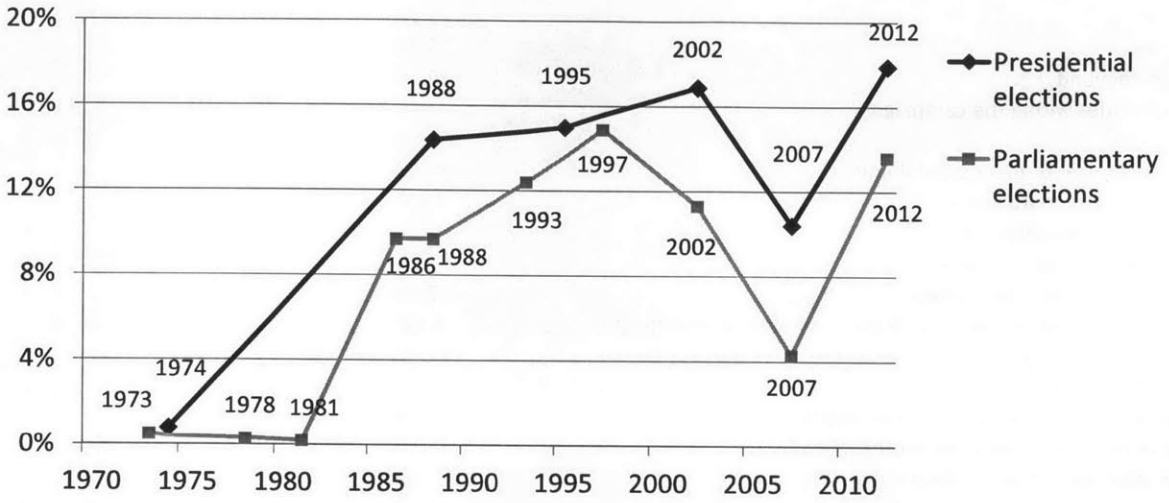
Notes: N is 6615 for the 1st and 2nd rounds of the presidential elections. For the 1st round of the parliamentary elections, the sample is restricted to constituencies where there was a PS candidate, and N is 5520. For the 2nd round of the parliamentary elections, the sample is restricted to constituencies where there was a second round with more than one candidate and where there was a PS candidate and N is 4893. I control for strata fixed effects.

Figure 6. PS Vote shares



Notes: N is 6615 for the 1st and 2nd rounds of the presidential elections. For the 1st round of the parliamentary elections, the sample is restricted to constituencies where there was a PS candidate, and N is 5520. For the 2nd round of the parliamentary elections, the sample is restricted to constituencies where there was a second round with more than one candidate and where there was a PS candidate and N is 4893. I control for strata fixed effects.

Figure 7. Front National's vote shares at Presidential and Parliamentary elections, 1973 - 2012



Source: French Ministry of the Interior

Notes: I only plot the vote shares obtained at the first round of the presidential and parliamentary elections. The Front National's candidate qualified for the second round of the presidential elections of 2002, and Front National's candidates at the parliamentary elections frequently qualify for the second round.

Table 1: Profile of the canvassers and feedback on the campaign (post-electoral survey)

Panel A. Profile of the canvassers	
Age	
29 or less	11.2%
30 - 45	23.1%
46 - 59	36.3%
60 and beyond	29.5%
Responsibilities within the campaign	
Volunteer	58.8%
Field organizer or head of local unit	37.1%
Département coordinator	4.2%
Relationship to Socialist Party	
Member for 5 years or more	52.0%
Member for less than 5 years	27.3%
Sympathiser and had previously been involved in a campaign	8.3%
Sympathiser and is involved in a campaign for the first time	12.4%
Previous field 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%
Panel B. Involvement in the campaign	
Attended a training session on door-to-door canvassing	59.0%
Number of door-to-door sessions taken part to	
1 to 2	13.3%
3 to 10	48.5%
More than 10	38.2%
Type of areas most covered	
Big cities (more than 100 000 in habitants)	25.4%
Middle-size cities (10 000 - 100 000)	47.2%
Rural areas (<10 000)	27.4%
Main context in which did door-to-door canvassing	
Frequently, with my local unit.	83.9%
For special national events	16.1%
Did you (or your local unit) use the list of prioritary 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 prioritary polling stations or cities.	43.5%
Did you use the guides and tools provided by the campaign?	
No	33.8%
Sometimes	43.7%
Yes, most of the time.	22.5%
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%

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

Table 1 (cont.): Profile of the canvassers and feedback on the campaign (post-electoral survey)

Panel C. 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 it, but not more than other campaign methods.	38.5%
A really good method, it should be one of the main methods used.	60.8%
If you like door-to-door canvassing, why so?	
It is good to take part to a large national action.	7.0%
It is effective.	31.3%
It is fun.	2.6%
It is a good way to spread the ideas and values of the left.	31.6%
It is an enriching experience.	27.5%
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%
Overall, how did you like the web platform "toushollande terrain"?	
1 (= Useless)	1.6%
2	7.6%
3	28.3%
4	41.4%
5 (= Excellent)	21.1%

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

Table 2: Sampling frame

Panel A. Randomization	(1) Treatment	(2) Control	(3) Total
# territories randomized at the polling station level	-	-	1,428
# precincts	6,064	1,495	7,559
	80.2%	*19.8%*	*100.0%*
# territories randomized at the municipality level	-	-	1,832
# municipalities	7,732	1,951	9,683
	79.9%	*20.1%*	*100.0%*
# precincts	11,922	2,971	14,893
	80.1%	*19.9%*	*100.0%*
Total # territories	-	-	3,260
Total # precincts	17,986	4,466	22,452
	80.1%	*19.9%*	*100.0%*
Total # registered citizens	13,690,901	3,363,444	17,054,345
	80.3%	*19.7%*	*100.0%*
Panel B. Assignment to canvassers	**Assigned**	**Not assigned**	**Total**
Initial assignment			
# treatment precincts	9,521	8,465	17,986
# control precincts	-	4,466	4,466
# registered citizens	7,243,727	9,810,618	17,054,345
# doors	5,223,580	7,074,611	12,298,192
Panel C. Reports by canvassers	**Assigned**	**Not assigned**	**Total**
% reports indicating the precinct covered	-	-	54.5%
% doors located in precincts initially assigned (or not assigned) to canvassers	37.4%	14.1%	51.5%

Table 3: Summary statistics

	Control group		Treatment group		*P-value* Treatment = Control	Number of obs.
	Mean	SD	Mean	SD		
Panel A. Electoral outcomes						
Number of registered citizens	918	269	919	255	0.961	6,615
Estimated share of left-wing nonvoters in 2007	0.086	0.034	0.087	0.033	0.413	6,615
Panel B. Location of the precinct						
Population of the municipality	56,127	252,745	52,361	239,859	0.644	6,615
0 to 1999	0.164	0.370	0.151	0.358	0.362	6,615
2000 to 4999	0.197	0.398	0.180	0.385	0.415	6,615
5000 to 9999	0.199	0.399	0.196	0.397	0.892	6,615
10000 to 19999	0.172	0.378	0.191	0.393	0.391	6,615
20000 and more	0.269	0.443	0.282	0.450	0.511	6,615
Rural or urban context						
Rural	0.152	0.360	0.133	0.339	0.163	6,615
Urban, center city	0.367	0.482	0.362	0.481	0.838	6,615
Urban, isolated city	0.079	0.270	0.105	0.307	0.081	6,615
Urban, suburbs	0.401	0.490	0.400	0.490	0.966	6,615
Region						
Ile-de-France	0.172	0.378	0.160	0.367	0.488	6,615
Champagne-Ardenne	0.026	0.160	0.017	0.129	0.360	6,615
Picardie	0.031	0.174	0.047	0.211	0.100	6,615
Haute-Normandie	0.049	0.217	0.047	0.211	0.800	6,615
Centre	0.049	0.215	0.049	0.217	0.932	6,615
Basse-Normandie	0.020	0.139	0.020	0.141	0.937	6,615
Bourgogne	0.024	0.153	0.034	0.182	0.184	6,615
Nord-Pas-de-Calais	0.024	0.153	0.032	0.177	0.370	6,615
Lorraine	0.049	0.215	0.055	0.229	0.566	6,615
Alsace	0.017	0.130	0.019	0.135	0.770	6,615
Franche-Comté	0.014	0.118	0.020	0.141	0.240	6,615
Pays-de-la-Loire	0.063	0.242	0.063	0.244	0.945	6,615
Bretagne	0.113	0.317	0.083	0.275	0.132	6,615
Poitou-Charentes	0.016	0.127	0.016	0.124	0.857	6,615
Aquitaine	0.047	0.212	0.050	0.218	0.778	6,615
Midi-Pyrénées	0.035	0.185	0.041	0.198	0.522	6,615
Limousin	0.016	0.124	0.015	0.121	0.859	6,615
Rhône-Alpes	0.134	0.341	0.123	0.329	0.539	6,615
Auvergne	0.031	0.174	0.030	0.170	0.809	6,615
Languedoc-Roussillon	0.033	0.179	0.035	0.183	0.839	6,615
Provence-Alpes-Côte-d'Azur	0.026	0.158	0.031	0.173	0.479	6,615
Corse	0.002	0.050	0.004	0.059	0.548	6,615
DOM-TOM	0.008	0.090	0.010	0.099	0.723	6,615

Notes : For each variable, we 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 precinct. Standard errors are adjusted for clustering at the level of randomization (precinct, or municipality).

Table 3 (cont.): Summary statistics

	Control group		Treatment group		*P-value* Treatment = Control	Number of obs.
	Mean	SD	Mean	SD		
Panel C. Sociodemographic characteristics of the population of the municipality						
Share of men	0.482	0.017	0.481	0.016	0.426	6,615
Share of the population with age						
0 - 14	0.183	0.031	0.184	0.031	0.790	6,615
15 - 29	0.181	0.042	0.182	0.042	0.609	6,615
30 - 44	0.196	0.027	0.195	0.027	0.417	6,615
45 - 59	0.204	0.023	0.203	0.023	0.471	6,615
60 - 74	0.141	0.031	0.143	0.032	0.330	6,615
75 and older	0.094	0.038	0.094	0.036	0.615	6,615
Mobility rate	0.247	0.052	0.242	0.055	0.061	6,615
Within population of 15 - 64						
Share of working population	0.722	0.044	0.717	0.045	0.042	6,615
Share of unemployed (among working population	0.118	0.047	0.125	0.051	0.004	6,615
Socioprofessional group, within population above 15						
PCS1 (Farmers)	0.006	0.014	0.006	0.011	0.086	6,615
PCS2 (Craftsmen, storekeepers, businessmen)	0.032	0.013	0.031	0.012	0.104	6,615
PCS3 (Executives, intellectual occupations)	0.081	0.054	0.078	0.054	0.249	6,615
PCS4 (Intermediate occupations)	0.141	0.038	0.138	0.038	0.107	6,615
PCS5 (Employees)	0.169	0.029	0.168	0.030	0.534	6,615
PCS6 (Laborers)	0.138	0.047	0.141	0.049	0.096	6,615
PCS7 (Retired)	0.273	0.070	0.273	0.071	0.940	6,615
PCS8 (Other inactive)	0.160	0.043	0.165	0.044	0.018	6,615
Median income	19,486	3,661	19,297	3,992	0.265	6,552

Notes : For each variable, we 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 precinct. Standard errors are adjusted for clustering at the level of randomization (precinct, or municipality).

Table 4: Impact on voter turnout

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A.			Presidential elections			
	1st round		2nd round		Average of 1st and 2nd rounds	
Assigned to canvassers	-0.002	0.001	-0.002	0.001	-0.002	0.001
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Strata fixed effects	x	x	x	x	x	x
Control variables		x		x		x
Observations	6615	6615	6615	6615	6615	6615
R-squared	0.78	0.84	0.74	0.81	0.76	0.83
Mean in Control Group	0.792	0.792	0.798	0.798	0.795	0.795
Panel B.			Parliamentary elections			
	1st round		2nd round		Average of 1st and 2nd rounds	
Assigned to canvassers	0.000	0.003	-0.002	0.001	-0.001	0.002
	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)
Strata fixed effects	x	x	x	x	x	x
Control variables		x		x		x
Observations	5520	5520	4893	4893	4893	4893
R-squared	0.73	0.79	0.72	0.78	0.73	0.79
Mean in Control Group	0.561	0.561	0.550	0.550	0.557	0.557

Notes : The unit of observation is the precinct. Standard errors are adjusted for clustering at the level of randomization (precinct, or municipality). ***, **, * indicate significance at 1, 5 and 10%. "Assigned to canvassers" is instrumented by the treatment assignment.
In Panel B, the sample is restricted to constituencies where there was a PS candidate at the 1st round (columns 1 and 2) and to constituencies where there was a second round with more than one candidate and where there was a PS candidate (columns 3 through 6).
The control variables include the proxy for the fraction of left-wing nonvoters, the municipality's population, this variable squared, the share of working population within the population aged 15 to 64 year old and the share of unemployed population among the working population. In addition, in Panel B, the control variables include the number of candidates at the 1st round (columns 1 and 2), the number of candidates at the 2nd round (columns 3 and 4) and the numbers of candidates at the 1st and 2nd rounds (columns 5 and 6).

Table 5: Impact on PS vote shares

Panel A.	(1)	(2)	(3)	(4)	(5)	(6)
	Presidential elections					
	1st round		2nd round		Average of 1st and 2nd rounds	
Assigned to canvassers	0.008	0.007	0.007	0.005	0.008	0.006
	(0.003)***	(0.003)**	(0.004)*	(0.003)	(0.003)**	(0.003)**
Strata fixed effects	x	x	x	x	x	x
Control variables		x		x		x
Observations	6615	6615	6615	6615	6615	6615
R-squared	0.77	0.81	0.76	0.81	0.77	0.82
Mean in Control Group	0.319	0.319	0.569	0.569	0.444	0.444
Panel B.	Parliamentary elections					
	1st round		2nd round		Average of 1st and 2nd rounds	
Assigned to canvassers	0.005	0.004	0.009	0.006	0.008	0.006
	(0.005)	(0.005)	(0.005)*	(0.005)	(0.004)*	(0.004)
Strata fixed effects	x	x	x	x	x	x
Control variables		x		x		x
Observations	5520	5520	4893	4893	4893	4893
R-squared	0.79	0.80	0.77	0.80	0.77	0.80
Mean in Control Group	0.399	0.399	0.573	0.573	0.484	0.484

Notes : The unit of observation is the precinct. Standard errors are adjusted for clustering at the level of randomization (precinct, or municipality). ***, **, * indicate significance at 1, 5 and 10%. "Assigned to canvassers" is instrumented by the treatment assignment.
In Panel B, the sample is restricted to constituencies where there was a PS candidate at the 1st round (columns 1 and 2) and to constituencies where there was a second round with more than one candidate and where there was a PS candidate (columns 3 through 6).
The control variables include the proxy for the fraction of left-wing nonvoters, the municipality's population, this variable squared, the share of working population within the population aged 15 to 64 year old and the share of unemployed population among the working population. In addition, in Panel B, the control variables include the number of candidates at the 1st round (columns 1 and 2), the number of candidates at the 2nd round (columns 3 and 4) and the numbers of candidates at the 1st and 2nd rounds (columns 5 and 6).

Table 6: Impact on all parties' vote shares

Panel A.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Presidential elections, 1st round											
	PS		Far-left		Left, other than PS		Center		Right		Far-right	
Assigned to canvassers	0.008	0.007	0.000	0.000	0.001	0.000	-0.001	-0.001	-0.003	-0.003	-0.004	-0.003
	(0.003)***	(0.003)**	(0.000)	(0.000)	(0.002)	(0.002)	(0.001)	(0.001)	(0.003)	(0.003)	(0.002)*	(0.002)
Strata fixed effects	x	x	x	x	x	x	x	x	x	x	x	x
Control variables		x		x		x		x		x		x
Observations	6615	6615	6615	6615	6615	6615	6615	6615	6615	6615	6615	6615
R-squared	0.77	0.81	0.44	0.44	0.71	0.72	0.72	0.74	0.70	0.74	0.78	0.78
Mean in Control Group	0.319	0.319	0.018	0.018	0.147	0.147	0.086	0.086	0.252	0.252	0.175	0.175
Panel B.												
	Parliamentary elections, 1st round											
	PS		Far-left		Left, other than PS		Center		Right		Far-right	
Assigned to canvassers	0.005	0.004	0.000	0.000	0.002	0.001	0.000	0.000	-0.002	-0.001	-0.006	-0.005
	(0.005)	(0.005)	(0.001)	(0.001)	(0.003)	(0.003)	(0.002)	(0.002)	(0.004)	(0.004)	(0.002)***	(0.002)**
Strata fixed effects	x	x	x	x	x	x	x	x	x	x	x	x
Control variables		x		x		x		x		x		x
Observations	5520	5520	5520	5520	5520	5520	5520	5520	5520	5520	5520	5520
R-squared	0.79	0.80	0.60	0.60	0.86	0.86	0.90	0.90	0.87	0.88	0.80	0.80
Mean in Control Group	0.399	0.399	0.011	0.011	0.130	0.130	0.055	0.055	0.267	0.267	0.131	0.131

Notes : The unit of observation is the precinct. Standard errors are adjusted for clustering at the level of randomization (precinct, or municipality). ***, **, * indicate significance at 1, 5 and 10%. "Assigned to canvassers" is instrumented by the treatment assignment.

In Panel B, the sample is restricted to constituencies where there was a PS candidate at the 1st round of the parliamentary elections.

The control variables include the proxy for the fraction of left-wing nonvoters, the municipality's population, this variable squared, the share of working population within the population aged 15 to 64 year old and the share of unemployed population among the working population. In addition, in Panel B, the control variables include the number of candidates at the 1st round of the parliamentary elections.

At the first round of the presidential elections, the far-left candidates were Philippe Poutou (Nouveau Parti Anticapitaliste) and Nathalie Arthaud (Lutte Ouvrière). The left candidates other than PS were Jean-Luc Mélenchon (Front de Gauche) and Eva Joly (Europe Ecologie les Verts). The center candidate was François Bayrou (Mouvement démocrate). The right candidates were Nicolas Sarkozy (Union pour la Majorité Présidentielle) and Nicolas Dupont-Aignan (Debout la République). The far-right candidate was Marine Le Pen (Front National).

Table 7: Heterogeneity of the impact along past vote shares of the Front National

Panel A.	(3)	(4)	(1)	(2)
	Presidential elections, 1st round			
	Far-right		PS	
Assigned to canvassers	-0.004	-0.005	0.007	0.007
	(0.003)*	(0.003)*	(0.004)*	(0.004)*
Assigned x high far-right vote share in 2007	0.015	0.018	0.002	-0.001
	(0.005)***	(0.005)***	(0.009)	(0.007)
Assigned x high far-right vote share in 2002	-0.013	-0.013	-0.001	0.000
	(0.005)***	(0.005)***	(0.009)	(0.007)
Strata fixed effects	x	x	x	x
Control for high far-right vote share in 2002 and 2007	x	x	x	x
Control variables		x		x
Observations	6614	6614	6614	6614
R-squared	0.79	0.79	0.77	0.81
Mean in Control Group	0.175	0.175	0.319	0.319
Panel B.	Parliamentary elections, 1st round			
	Far-right		PS	
Assigned to canvassers	-0.004	-0.005	0.001	0.002
	(0.003)*	(0.003)*	(0.009)	(0.008)
Assigned x high far-right vote share in 2007	0.005	0.005	-0.003	-0.006
	(0.006)	(0.006)	(0.012)	(0.011)
Assigned x high far-right vote share in 2002	-0.007	-0.004	0.013	0.011
	(0.006)	(0.006)	(0.012)	(0.011)
Strata fixed effects	x	x	x	x
Control for high far-right vote share in 2002 and 2007	x	x	x	x
Control variables		x		x
Observations	5519	5519	5519	5519
R-squared	0.81	0.81	0.79	0.80
Mean in Control Group	0.131	0.131	0.399	0.399

Notes : The unit of observation is the precinct. Standard errors are adjusted for clustering at the level of randomization (precinct, or municipality). ***, **, * indicate significance at 1, 5 and 10%. "Assigned to canvassers" is instrumented by the treatment assignment. "Assigned x high far-right vote share in 2007" is instrumented by Treatment x high far-right vote share in 2007 and "Assigned x high far-right vote share in 2002" is instrumented by Treatment x high far-right vote share in 2002.

In Panel B, the sample is restricted to constituencies where there was a PS candidate at the 1st round of the parliamentary elections.

The control variables include the proxy for the fraction of left-wing nonvoters, the municipality's population, this variable squared, the share of working population within the population aged 15 to 64 year old and the share of unemployed population among the working population. In addition, in Panel B, the control variables include the number of candidates at the 1st round of the parliamentary elections.