Voter Registration Costs and Disenfranchisement: Experimental Evidence from France

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Abstract

In many countries (including the US) citizens must register before voting. This paper 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.

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 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.[1] 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

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[1]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.

the elections. In the control group, 18.3% of the initially unregistered 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.

2 Literature review

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.

3 Setting

3.1 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.

3.2 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.[2]

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[2]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.

3.3 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, 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.[3] 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.

4 Sample population and data

4.1 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.[4] 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

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[3] 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.

[4] 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.

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 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.[5] 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.

4.2 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,

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[5]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.

misregistered and unregistered citizens, 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.

4.3   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.

4.4   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.[6]

Panel C of Table 1 presents summary statistics for the respondents to the survey. The average respondent

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

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.

5 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.[7] Third, we study what 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?

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[7]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.

5.1 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.

5.2 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.

5.3 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

13

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

5.4 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

15

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: $-f_k(b, c)$ satisfies 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.[8] This has an important consequence 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.

6 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.

6.1 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.[9] 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

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

[9]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.

higher in all treatment groups. To investigate the statistical significance of the differences shown in Figure 4 more systematically, we 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.[10] 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.[11] 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 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

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[10]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.

[11]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.

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[12] 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.[13]

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.[14]

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 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

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[12]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.

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

[14]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.

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.[15]

6.2   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.[16] 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 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

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[15]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.

[16]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.

significantly increased voter turnout, except for the "Early Canvassing" visits.

6.3 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:

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

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).[17]

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 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?

6.4   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-

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[17]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).

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.

7 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.

7.1 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.[18]

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.

7.2 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.

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

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[18]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.

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).

7.3   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.

7.4 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, $\overline{V_0} = \overline{V_A}$, $\overline{V_T} = \overline{V_A}\left(1 - p_C\right) + \overline{V_C}p_C$. 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.[19]

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[19]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,

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.[20]

7.5 ... 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 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.[21]

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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).

[20]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).

[21]The detailed results by initial registration status are available in Table A7 of the Appendix. Based on these results, we

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.

7.6 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).[22]

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

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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.

[22]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.

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 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.

8   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.[23] 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 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.

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[23]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.

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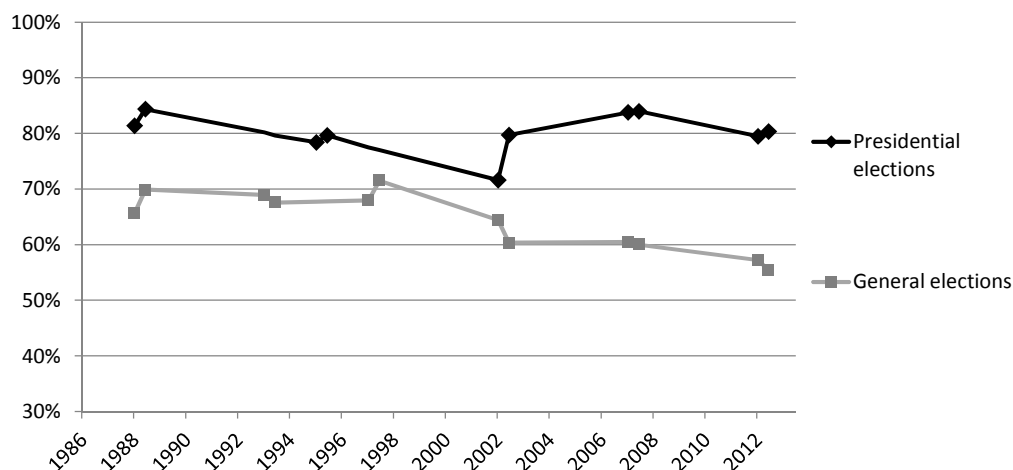
Source: French Ministry of the Interior
Notes: These official turnout figures use the number of registered citizens (not the number of eligible citizens) as the denominator.

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

■ Always-takers

▨ Compliers

3a. Uniform benefits of voting

$g(b_i)$

Always-takers   Compliers

$g(b)$

c_i

$g(b)$ $g(b)/\lambda$

3b. Uniform registration costs

$g(b_i)$

Always-takers

Compliers

c

λc

c_i

c

3c. General case

Always-takers

$g(b_i)$ $g(b)=c$

Compliers

$g(b)=\lambda c$

c_i
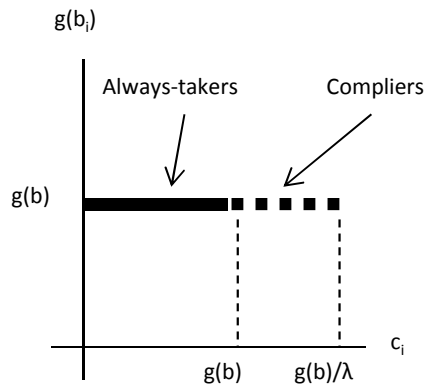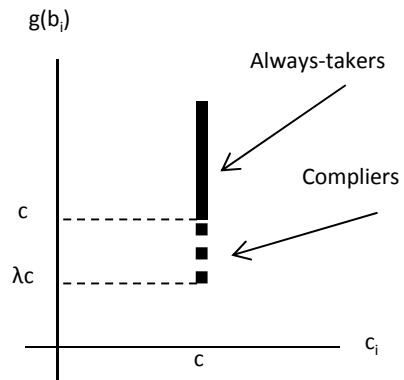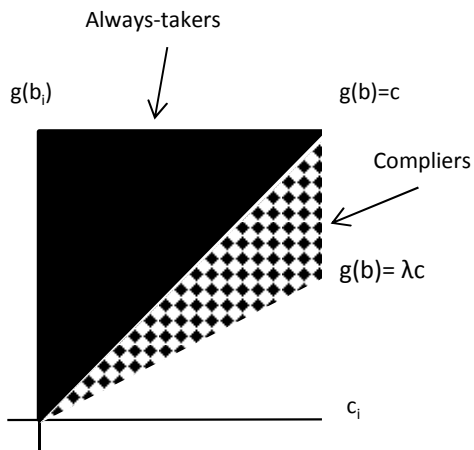
3d. Compliers with higher benefits of voting than always-takers

$g(b_i)$ $g(b)=c$

Always-takers

Compliers

$g(b)=\lambda c$

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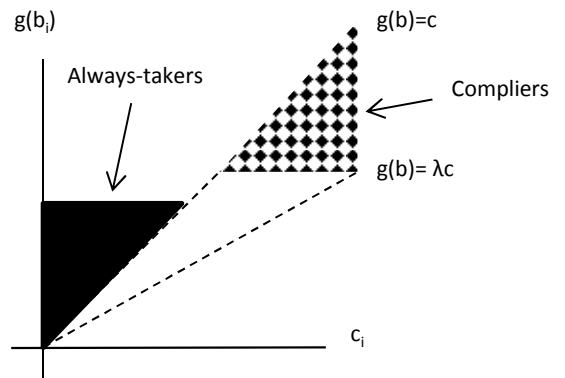
Figure 4. Impact on the number of new registrations among initially unregistered and misregistered citizens

0.30

0.26
(29%)

0.23
(25%)

0.22
(24%)

0.20
(22%)

0.20
(22%)

0.18
(20%)

0.17
(18%)

0.25

0.20

0.15

0.10

0.05

0.00

Control Group | Early Canvassing | Late Canvassing | Early Home registration | Late Home registration | Early Can. & Late Home reg. | Early Home reg. & Late Home reg.
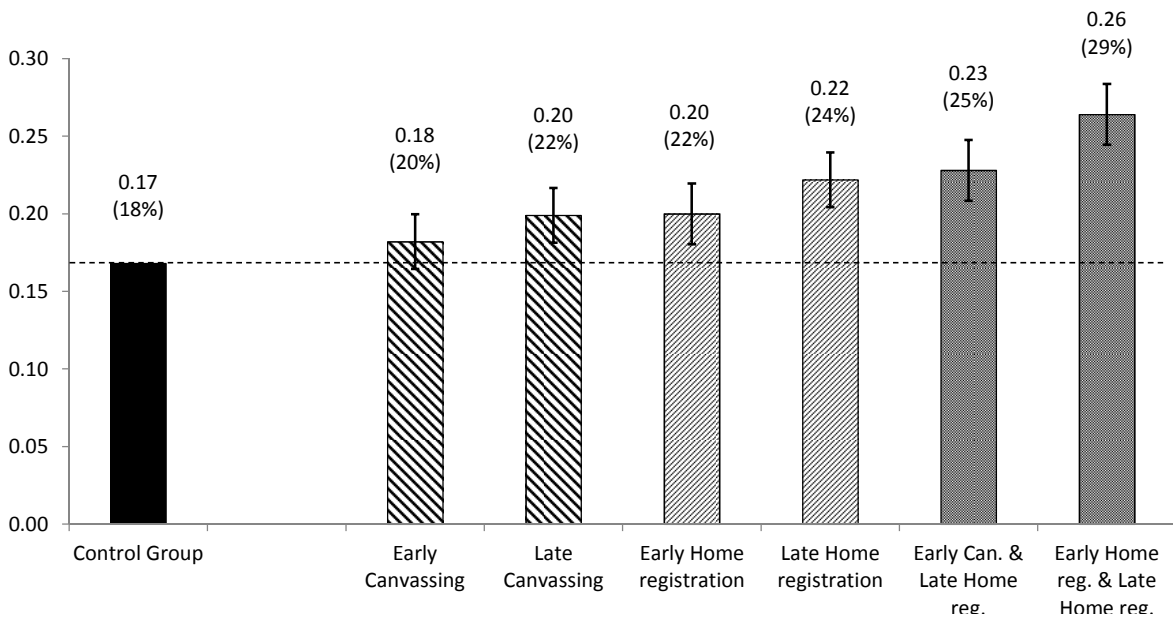
Notes: We show the number of new registrations in the average apartment of the control group and each treatment group. We also show the 95% confidence interval of the difference between the treatment groups and the control group and, in each group, we estimate the fraction of initially unregistered and 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

0.18
(19%)

0.20

0.15
(17%)

0.15
(16%)

0.14
(15%)

0.14
(15%)

0.13
(14%)

0.12
(13%)

0.15

0.10

0.05

0.00

Control Group | Early Canvassing | Late Canvassing | Early Home registration | Late Home registration | Early Can. & Late Home reg. | Early Home reg. & Late Home reg.
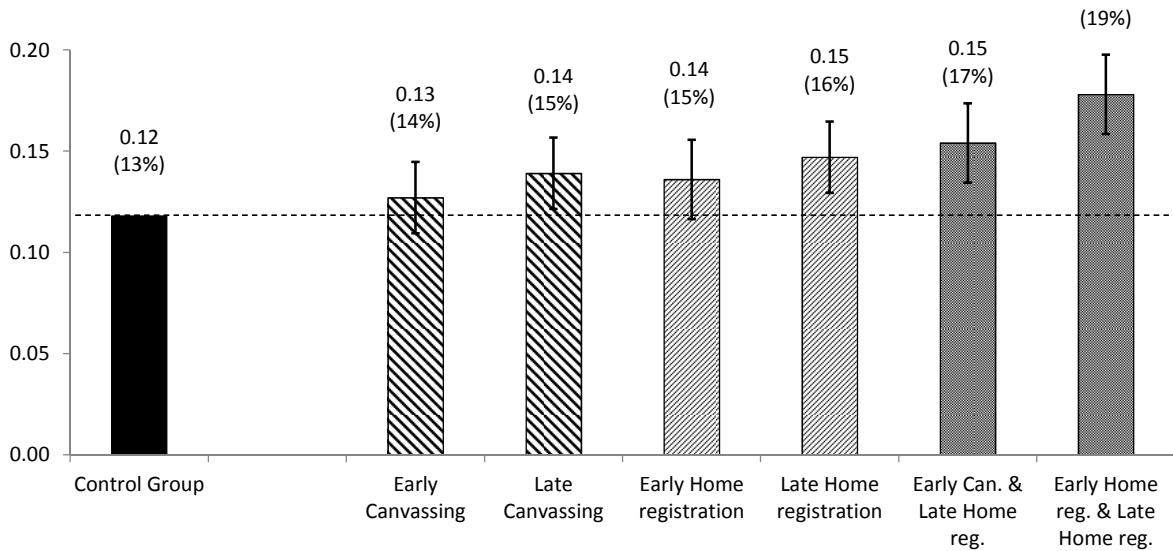
Notes: We show the number of votes cast on average at the four electoral rounds by initially unregistered and misregistered citizens in the average apartment of the control group and each treatment group. We also show the 95% confidence interval of the difference between the treatment groups and the 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
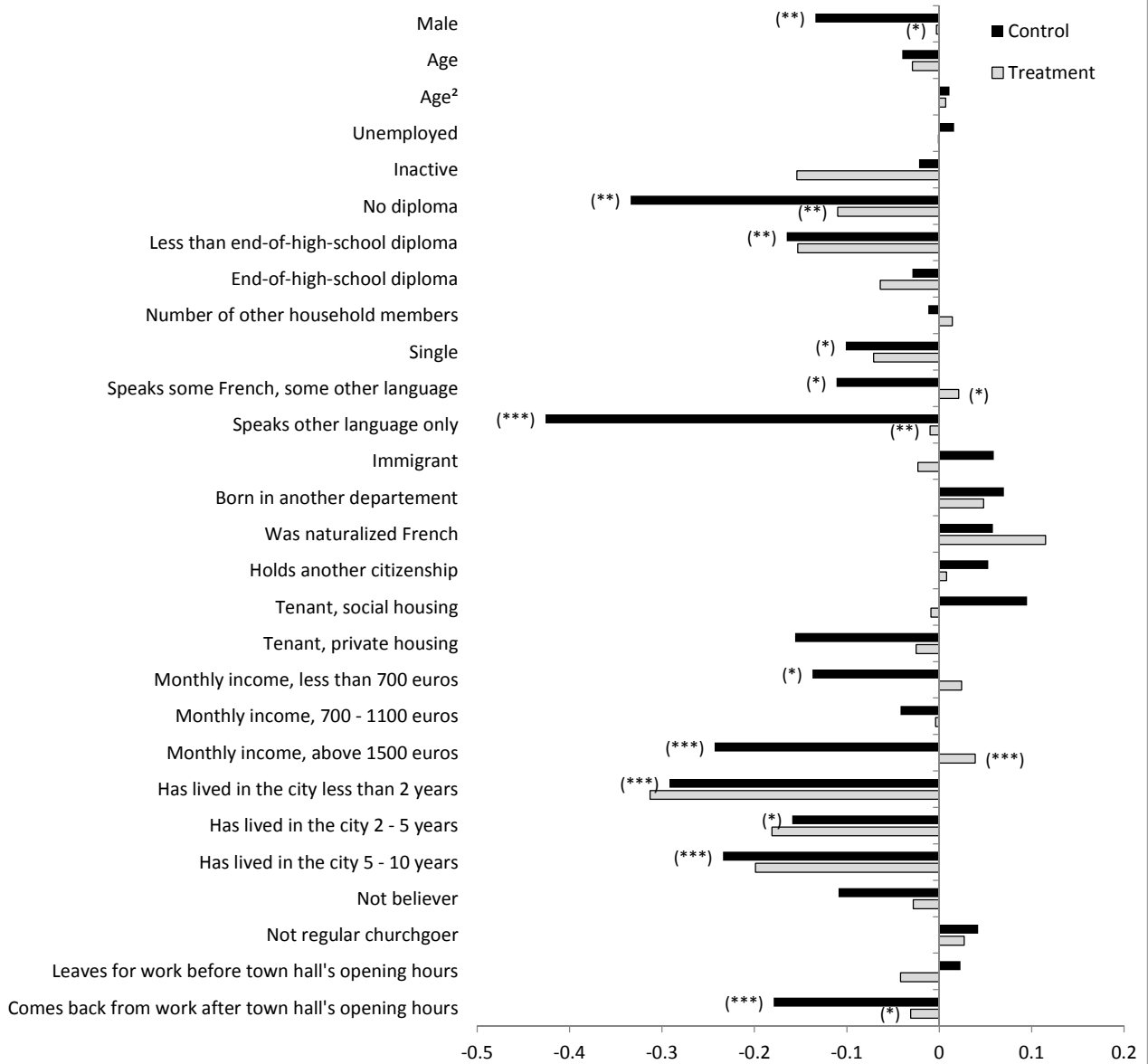
Notes: We report the point estimate of the effect of various individual characteristics and their interaction with a treatment dummy on a dummy equal to 1 if the respondent is registered in his city and 0 otherwise. All treatment groups are pooled together. All independent variables are dummies, except 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
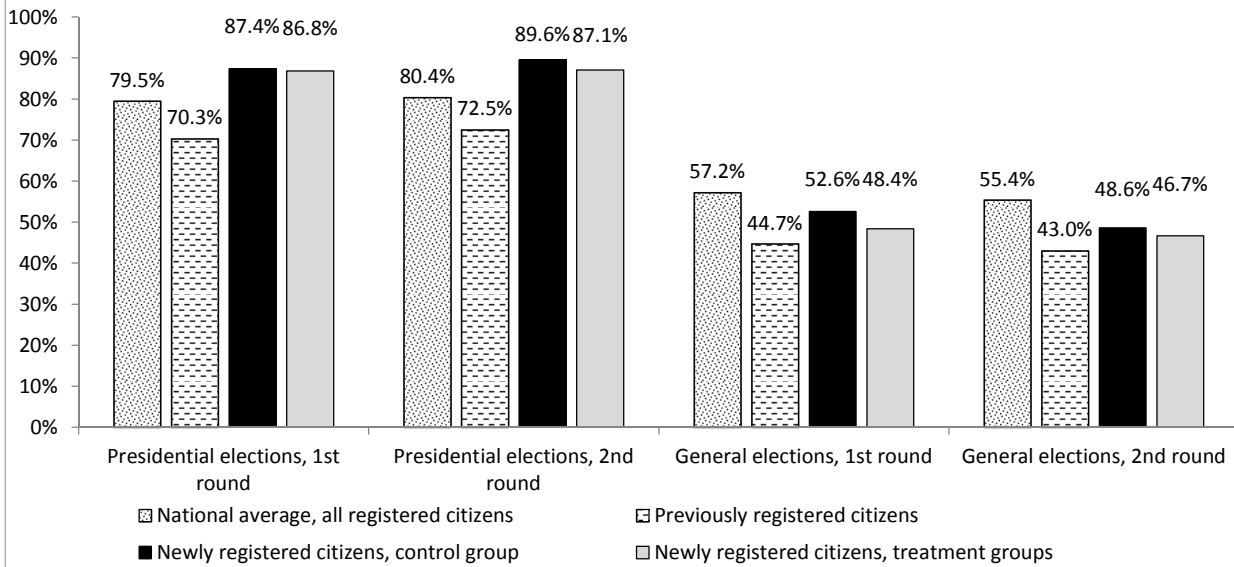
Notes: We report the national average and the point estimates of the voter turnout at each electoral round of the following categories: previously registered citizens and newly registered citizens in the control and treatment groups. We also show the 95% confidence interval of the difference between 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
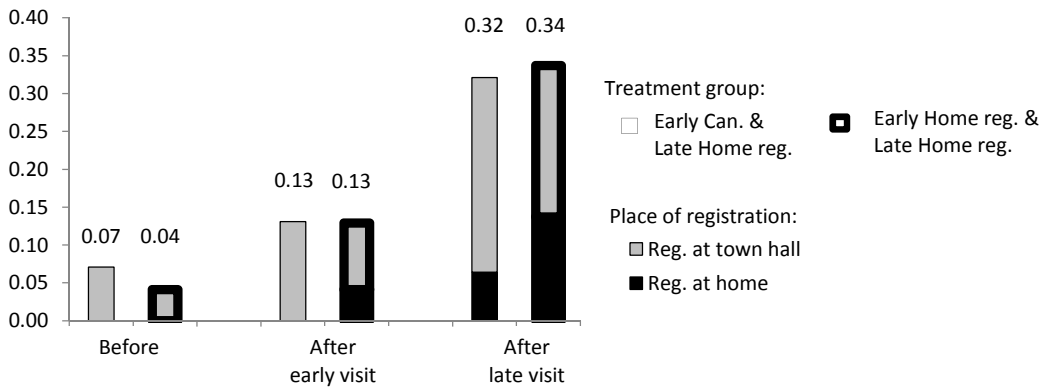
Notes: We show the average number of new registrations made at home and at the town hall in the apartments of the groups "Early Canvassing and Late Home registration" and "Early Home registration and Late Home registration" which opened their door for the late visit. The numbers of new registrations 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
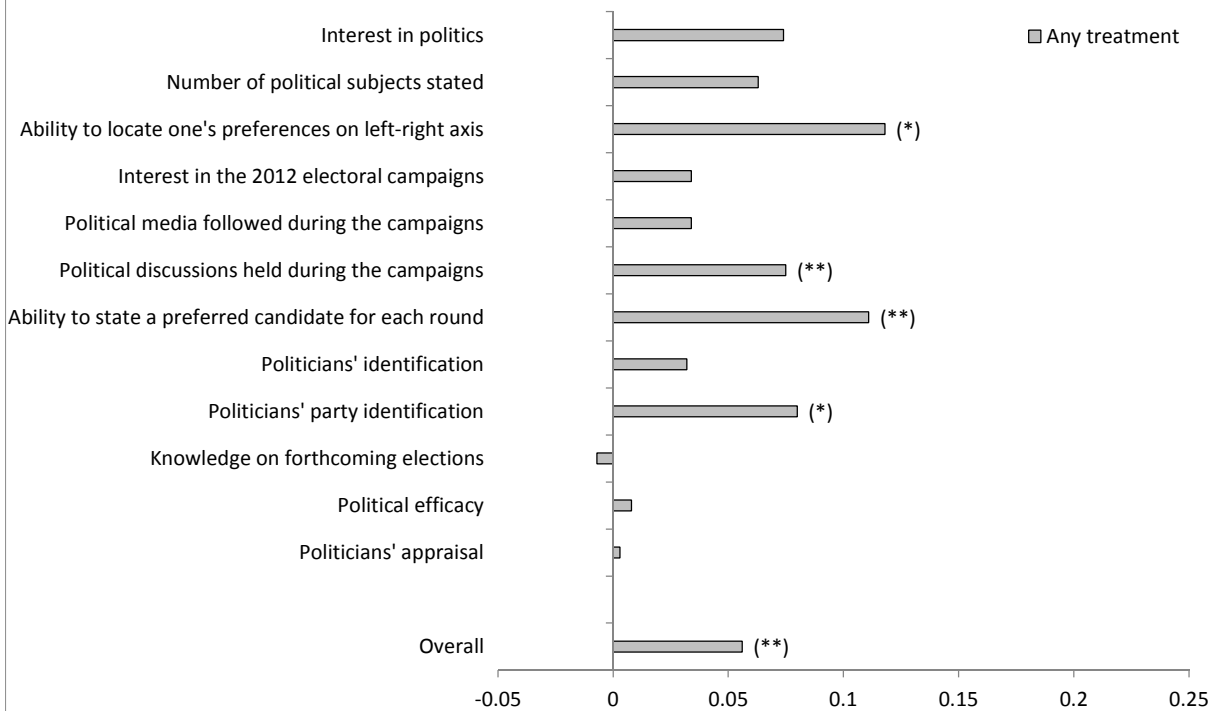
Notes: All outcomes are summary indices defined to be the equally weighted average of z-scores of their components. For each outcome, we plot the point estimate of the difference between the control group and any treatment group. ***, **, * indicate significance at 1, 5 and 10%. We control for a series 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*	Test: joint significance of treatment dummies		Number of obs.
					Treatment = Control			
	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 - Door-to-door canvassing group	statistic	0.53	1.18	0.95	0.72
	p-value	0.979	0.246	0.539	0.855
Two visits group - Home registration group	statistic	1.36	1.49	0.95	0.80
	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)
Age²	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 address	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 1/6 (EC + LC + EH + LH + EC&LH + EH&LH)	-0.006	-0.025	-0.041	-0.018	-0.022
	(0.012)	(0.011)**	(0.019)**	(0.019)	(0.011)*
Av. difference between newly registered in Canvassing gr. and control 1/2 (EC + LC)	-0.006	-0.017	-0.024	0.000	-0.011
	(0.014)	(0.013)	(0.024)	(0.023)	(0.014)
Av. difference between newly registered in Home registration gr. and contro 1/2 (EH + LH)	-0.003	-0.044	-0.053	-0.041	-0.034
	(0.014)	(0.014)***	(0.023)**	(0.022)*	(0.013)**
Av. difference between newly registered in Home reg. gr. and Can. gr. 1/2 (EH + LH) - 1/2 (EC + LC)	0.003	-0.027	-0.029	-0.042	-0.024
	(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.