HARVARD | BUSINESS | SCHOOL

Voter Registration Costs and Disenfranchisement: Experimental Evidence from France

Céline Braconnier
Jean-Yves Dormagen
Vincent Pons

Working Paper 16-098

Voter Registration Costs and Disenfranchisement: Experimental Evidence from France

Céline Braconnier
Université de Cergy-Pontoise

Jean-Yves Dormagen
Université Montpellier 1

Vincent Pons
Harvard Business School

Working Paper 16-098

Voter Registration Costs and Disenfranchisement: Experimental Evidence from France

Céline Braconnier Jean-Yves Dormagen Vincent Pons*

February 2016†

Abstract

A large-scale randomized experiment conducted during the 2012 French presidential and parliamentary elections shows that voter registration requirements have significant effects on turnout, disproportionately discouraging marginalized citizens. We assigned 20,500 apartments to one control or six treatment groups that received canvassing visits providing either information about registration or help to register at home. While both types of visits increased registration, the home registration visits had a higher impact than the information-only visits, indicating that both information costs and administrative barriers impede registration. Home registration did not reduce turnout among those who would have registered anyway. On the contrary, citizens registered due to the visits became more interested in the elections and 93 percent voted at least once in 2012. Overall, these results suggest that easing registration requirements could substantially enhance political participation, improve representation of marginalized groups, and increase the average level of competence and informedness of the population.

*Université de Cergy-Pontoise; Université Montpellier 1; Harvard Business School

†We are grateful to Daron Acemoglu, Stephen Ansolabehere, Abhijit Banerjee, Adam Berinksy, Esther Duflo, Alan Gerber, Jens Hainmueller, Daniel Hidalgo, Benjamin Olken, Daniel Posner, James Snyder, seminar participants at Yale, MIT, LSE, Stanford GSB, HBS, Bocconi, Warwick, TSE, Crest, UCSD, Northwestern Kellogg, Stockholm IIES, Sciences Po, INSEAD, and conference participants at APSA, EPSA, WPSA, NYU-CESS, and CASP for suggestions that have improved the paper. We thank Caroline Le Pennec and Ghislain Gabalda for the outstanding research assistance they provided throughout the entire project and Aude Soubiron for her assistance in the administration of the interventions in the cities surrounding Bordeaux. We thank the town hall administration of each of the ten cities included in the experiment for their generous collaboration and are indebted to all canvassers who administered the interventions, including students from the Ecole Normale Supérieure, the University Cergy-Pontoise, the IEP of Bordeaux and the University of Montpellier 1, the NGO of retired workers of the MGEN, the NGO "Tous Citoyens," the NGO "RAJ-LR," local units of the Socialist Party in Cergy, Sevran and Carcassonne and the local unit of the Front de Gauche in the 20th arrondissement of Paris. We gratefully acknowledge funding from the Russell Sage Foundation, MIT France, the Tobin Project, the city of Montpellier, the University of Montpellier 1, and the University of Cergy-Pontoise.

1 Introduction

Elections in established democracies regularly attract less than half of the voting-age population (Blais, 2010), raising concerns not only for the equal representation of all citizens, but also for the overall legitimacy and stability of the democratic regimes. Participation is unequal (Wolfinger and Rosenstone, 1980) and tends to be more so when it is lower (Rosenstone and Hansen, 1993), threatening the representativeness of elected officials and public policies. Low and unequal turnout may be largely driven by the cost of participating (e.g, Piven and Cloward, 1988). This paper studies the impact of a specific type of costs: voter registration costs.

In most democracies, voter registration is automatic and done by the state. But in a few others, such as the United States and France, registration is self-initiated: citizens who wish to vote must register first, and they need to go through this process again each time they move. Many citizens are not registered (US Census Bureau, 2012; Insee Premiere, 2012) or are registered at an old address (Braconnier and Dormagen, 2007; Braconnier, Dormagen, Gabalda and Niel, 2015). One view is that self-initiated registration does not matter much because these citizens fail to register due to their low interest in voting, and most of them would abstain if they were registered. Another view holds that self-initiated registration is largely responsible for low participation, because registration costs (in terms of time it takes and the information it requires) can be much higher than the cost of voting itself, and they occur at a time when interest in the election is far from its peak. To the extent that registering is more difficult or costly for some citizens than others, self-initiated registration may also be responsible for unequal participation.

To disentangle both views, a natural empirical test would be to check the extent to which voter turnout is affected by changes in the registration costs. The first view predicts a minimal impact, the second a substantial one.

Observational studies first ran this test using variation in voter registration laws. Gosnell (1930) identifies such laws as one of the most important institutional factors explaining lower

turnout rates in US, compared to Europe (also see Powell, 1986; Jackman, 1987). Converse (1972) exploits temporal rather than spatial variation and notes that the introduction of voter registration laws at the turn of the 19th century in the US coincided with a large drop in participation. However, other authors point out that other concomitant trends may have contributed to this drop (e.g, Rusk, 1970). This controversy illustrates an important limit of observational studies, namely the difficulty of separating the impact of voter registration laws from other factors, including other institutional variations. In addition, the adoption of different registration rules by different countries, states or counties might reflect unobserved motives correlated with participation.[1] These limits may help explain why different studies reach opposite conclusions on the effects of laws which, since the 1960s, have relaxed voter registration requirements. Some studies find little or no effect on turnout and inequality (e.g, Brown and Wedeking, 2006; Nagler, 1991). Others find strong effects (up to 10 percentage points) on voter turnout of "motor voter" provisions (Knack, 1995), registration deadlines closer to the election (e.g, Vonnahme, 2012), or election-day registration (Knack, 2001). These studies further report that less stringent requirements deacrease inequality in the electorate by bringing in younger and less educated citizens as well as frequent movers (e.g, Rosenstone and Wolfinger, 1978; Highton, 1997), although this alters the electorate's overall demographic composition and partisan balance only minimally.[2]

To isolate the causal impact of voter registration costs from correlated factors, we designed a large field experiment that facilitated registration for a random group of households. Prior to the 2012 French presidential and parliamentary elections, we conducted in-depth preparatory field work in ten cities to identify households likely to include unregistered citizens. We then randomly assigned these 20,500 households to one control or six treatment groups. Treatment households received home canvassing visits providing either informa-

[1] The 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 turnout rates among non-registrants, conditional on being registered (Erikson, 1981; Timpone, 1998). But being registered or not may be correlated with unobserved factors that strongly predict turnout.

[2] On the consequences of higher or universal turnout on electoral outcomes, also see the studies that compare the preferences of voters and non-voters (e.g, Citrin, Schickler and Sides, 2003).

tion about registration or help to register at home. Depending on the city, the visits were conducted by non-partisan students, NGO members, or members of political parties. Treatments further varied the timing of the visits (early, during the two to three months before the registration deadline; or late, during the last month before the deadline) and their frequency (once or twice). 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 on 1,500 respondents after the elections.

Our method draws on a large experimental literature pioneered by Gerber and Green (2000). While many studies evaluate the impact of door-to-door canvassing on voter turnout (e.g, Green, Gerber and Nickerson, 2003; Arceneaux and Nickerson, 2010; Pons and Liegey, 2016) and vote shares (e.g, Barton, Castillo and Petrie, 2014; Pons, 2016), fewer experiments study voter registration (Nickerson, 2007; Bennion and Nickerson, 2011; John, MacDonald and Sanders, 2015). Perhaps closest to this paper is a US experiment, Nickerson (2015), which reports that voter registration drives substantially increase registration, but that only a small fraction of this impact translates into increased voter turnout.

Our experiment extends the existing literature in a number of important directions. The experimental variation we introduced between the different treatment groups and the data we collected were motivated by six central questions, which all relate to the impact of voter registration costs, and participation costs more generally.

First, we ask whether the number of registrations increases when registration is simplified. Reducing the costs may simply facilitate the registration of people who would have registered regardless. Instead, we find that the visits increased new registrations by 29 percent on average. In addition, the impact of our treatments is proportional to the extent to which they facilitate registration. This reinforces the conclusion that a large fraction of citizens fail to register not out of apathy (not wanting to participate), but because it is too costly.

Second, the random variation we introduced in the content of the visits brings the first

experimental evidence on the respective impact of two obstacles inherent to self-initiated registration: information costs and bureaucratic barriers. Information-only visits increased registration, but visits that offered to register people at home had a higher impact, 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.

Third, we measure the extent to which increased registration translates into increased participation. Using 135,000 turnout observations, we find that 93 percent of the citizens registered due to the visits voted at least once in 2012. This striking result means that self-initiated registration excludes a large number of citizens who are prepared to vote, conditional on getting registered. To better characterize their propensity to vote, we exploit a unique feature of our setting: the fact that four successive electoral rounds of very different salience took place within only two months in 2012 in France. Looking at citizens registered due to the visits, their participation in the presidential elections was higher than in the less salient parliamentary elections and it decreased more in-between than other citizens' did, suggesting that two conditions need to be met for unregistered citizens to participate: reduced registration costs and high electoral salience. Comparing across groups, we find that the propensity to vote of the marginal registrant decreases as registration is made easier.

Fourth, we ask whether the inclusion of new citizens comes at the cost of disengaging those who would have registered regardless of the reduced cost. Most of the literature on registration implicitly assumes that one's propensity to vote does not depend on the obstacles one has to overcome to get registered. However, Erikson (1981) hypothesized that "the prospective voter who undergoes the cost of registration may be more likely to vote than if registration were free in order to 'protect' the sunk cost of the registration investment." We provide the first experimental evidence on this hypothesis: we test whether home registration decreased the participation of those who would have registered on their own at the town hall otherwise. Two treatment groups were designed to isolate this effect

by creating symmetric groups of newly registered citizens but with varying rate of home registration, following a strategy inspired from Karlan and Zinman (2009). We do not find any disengagement effect.

Fifth, we check if instead of disengaging citizens who would have registered regardless, the visits empowered those who would have remained unregistered. Several studies observe that political participation enhances individuals' informedness about politics (e.g, Leighley, 1991). We provide experimental evidence – the first, to our knowledge – of this causal relationship. By the time of the postelectoral survey, political interest and information were larger in the treatment groups than in the control group, suggesting that citizens registered due to the visits became more interested and attentive to the elections as a result of being able to participate in them.

Finally, we investigate the effect of voter registration costs on the equal representation of all groups by checking how citizens registered due to the visits differ from other registered citizens. To address this question, our survey collected a wealth of individual-level sociodemographic information unparalleled in other experiments. We combine this data with information available from the voter rolls for all registered citizens and find that citizens registered due to the visits differ systematically from other citizens. In our sample, they are more likely to be immigrants, young, less educated, and their political preferences are slightly more to the left. This suggests that self-initiated registration might skew electoral outcomes away from being accurate representations of the citizenry and their interests.

The remainder of the paper is organized as follows. Section 2 provides more background information on the experimental setting and design. We evaluate the impact of the visits on registration and turnout in Sections 3 and 4, on politicization in Section 5, and on the composition of the electorate in Section 6. Section 7 concludes with a discussion.

2 Experimental setting and design

2.1 The setting

In France, it is the responsibility of citizens to register and re-register each time they move.[3] To register, one must file an application, submitting a form, an ID, and proof of address such as a recent utility bill. Most people register in person at the town hall, although the application can be brought to the town hall by a third party, mailed in, or in some cities, completed online. Nine percent of eligible citizens registered for the first time or updated their registration status in 2011, before 31 December, the registration deadline for the French 2012 elections (Insee, 2012). Nonetheless, 7 percent of all people living in metropolitan France who were eligible to register remained unregistered (Insee Premiere, 2012) and around 15 percent were "misregistered" at an old address, making voting relatively more costly to them (Braconnier, Dormagen, Gabalda and Niel, 2015).[4]

Seventy-nine percent of registered voters 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 percent) and François Hollande was elected president with 52 percent of the vote.

Similarly to the presidential elections, the general elections consist of two rounds, unless a candidate obtains more than 50 percent of the votes in the first round. They took place on June 10 and 17. Fewer voters (57 and 55 percent) participated in these elections than either the presidential elections or the previous general elections (Figure A1, in the attached Online Appendix). The Parti Socialiste won in 57 percent of the constituencies.

[3] There is only one exception to this rule: since 1997, teenagers who turn 18 are, in principle, automatically registered. However, as any other citizen, they need to re-register when they move away from the address where they lived at 18.

[4] Until they get struck from the lists, misregistered citizens can continue voting in the polling station corresponding to their previous address, but this requires traveling back or applying for a proxy vote.

2.2 Experimental design

This study took place in ten cities, ranging in size from 10,000 inhabitants to more than 200,000 (Figure A2).[5] The main criteria for selection of the cities were the availability of groups of people willing to take part in the experiment as unpaid canvassers and the logistical and financial support that the municipality could provide. In each city, we selected precincts characterized by relatively lower turnout rates at previous elections, and thus likely to host many unregistered and misregistered citizens. Within these 44 precincts, in-depth preparatory field work identified apartments in which unregistered and misregistered citizens were likely to reside by systematically comparing names found on the mailboxes with the list of registered citizens as of January 2011 (more details in Online Appendix A). Overall, the experimental sample contains 20,502 apartments, located at 4,118 addresses.

One fourth of these apartments were allocated to the control group and three fourths to the treatment group, after randomization at the address level and stratification by precinct and number of registered citizens at each address. Treatment apartments received registration visits carried out by 230 canvassers belonging to three groups: students, NGO members, and party activists.[6] Each precinct was covered by a different group of canvassers. All canvassers received an identical one-day training, based on role plays. In a randomly selected third of the treatment apartments, canvassers encouraged people to register and provided information about the process (hereafter, the canvassing group); after a conversation of one to five minutes, they distributed a leaflet that summarized this information (an example can be found in Figure A3). In a second third, 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

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

[6] 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.

themselves. The remaining apartments 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 (see Figure 1). Half of the canvassing and home registration apartments were visited early, two to three months before the registration deadline, whereas the other half were visited late, during the last month before the deadline. Half of the two-visits apartments received an early canvassing visit and a late home registration visit, whereas the other half received two home registration visits. On average, 46.2 percent of the apartments visited only once opened their door, and 65.1 percent of the apartments visited twice opened their door at least once.[7]

[Figure 1 about here]

With a total of six different treatment groups and one control group, our experiment may seem overly intricate. As will become evident in the following sections, however, all this instrumental variation was carefully designed to address the six questions laid out in the introduction, and thereby to understand the full scope of consequences of voter registration costs and effects of facilitating registration. We first estimate the impact of the visits on registration itself and disentangle different factors: information, logistical costs, and timing.

3 Impact on registration

3.1 Data

We identify the citizens who registered in 2011 by comparing the January 2011 and January 2012 administrative voter lists. We locate their apartment based on their listed address and by matching their last name or marital name with the names initially found

[7] Table A1 presents summary statistics on the sample population, including sociodemographic characteristics collected through a postelectoral survey (see Section 7), and verifies balance across treatment arms. We 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. Out of 70 differences, four are significant at the 5 percent level, and six at the 10 percent level, which is in line with what should be expected.

on the mailboxes. In addition to the voter lists, we collected the registration date, previous registration status, and previous city of registration, if any, for all citizens who registered in 2011.

3.2 Overall impact

To assess the impact of the visits on registration, we would ideally measure changes in the individual registration status of citizens initially unregistered or misregistered. However, there is no systematic list of these citizens. Instead, we use the number of new registrations in each apartment as the outcome. Based on information collected by the canvassers during the visits, we estimate that the average apartment contained 0.92 unregistered and misregistered citizens (see Online Appendix B for more details). As shown in Figure 2, in the control group, 0.17 (18 percent) of them registered in 2011. The number of new registrations was higher in each treatment group, from 0.18 in the group which received an early canvassing visit to 0.26 in the group which received two home registration visits.

[Figure 2 about here]

To investigate the statistical significance of the differences shown in Figure 2 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. 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 1. The number of new registrations in the average control apartment was 0.168. In Panel A, the six treatment groups are pooled together. On average, the visits increased the number of new registrations by 0.049 (29 percent). This effect is statistically significant at the 1 percent level and robust to the inclusion of apartment and building controls. Disentangling by initial registration status, we find that the visits increased registration of citizens originally unregistered, registered in another city, and registered at another address in the same city, by 47 percent, 18 percent, and 32 percent respectively (Table A2).

[Table 1 about here]

3.3 Effect of information, logistical costs, and timing

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 – lack of information about the process and administrative cost of registering – and to examine whether these obstacles are reinforced by procrastination. We now study more closely the respective importance of these three impediments to registration and the extent to which the visits alleviated them.

First, early and late canvassing visits increased the number of new registrations by 0.014 (8 percent) and 0.031 (18 percent) respectively, for an average of 0.022 (13 percent), significant at the 5 percent level (Table 1, Panel B). This suggests that imperfect information prevents some eligible citizens from registering to vote. In addition to providing information, the canvassing visits may also have served as a reminder of civic duty. However, additional evidence supports the view that, to a large extent, increased information explains the impact: 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 do not know which documents are 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.

Second, in addition to providing information, home registration visits offered people the opportunity to register at home. Early and late home registration visits increased the number of new registrations by 0.032 (19 percent) and 0.054 (32 percent) respectively. Their average effect of 0.043 (26 percent) nearly doubled the effect of canvassing visits, a difference significant at the 10 percent level. This suggests that conditional on available information, the administrative cost of registering also impedes registration.

Third, 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 percent level. The sign of this difference might be surprising at first, since early visits left more time to register. A possible interpretation is that early visits also left more time to procrastinate and, eventually, to forget the discussion with the canvassers. People who have to register may indeed be particularly prone to procrastinate, as they have to pay the cost now and will only get the benefit (voting) later. 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. An alternative interpretation is that the visits were complementary to the media campaign, whose intensity increased as both the registration deadline and Election Day came closer. In either case, our finding suggests that later registration deadlines, which allow later registration drives, will also produce higher registration rates as a result.

We now investigate the extent to which the large impact of the visits on registration

translated into increased turnout at the 2012 elections.

4 Impact on turnout

4.1 Data

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 attendance sheets at the 2012 French presidential and general elections and digitized them. Thanks to this administrative data, we measure the actual voting behavior of all registered citizens in the sample addresses. Altogether, our analysis is based on approximately 135,000 individual turnout observations.

4.2 Voter turnout in the control and treatment groups

Figure 3 shows the participation rates of newly registered citizens in the control and treatment groups, as well as the participation of citizens who were previously registered (prior to 2011) and who live in the sample addresses. Turnout was very high at the presidential elections overall, and much lower at the general elections. Newly registered citizens in the control and treatment groups were more likely to participate at each electoral round than previously registered citizens. 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 [2]:

$$V_{i,b} = \alpha + \gamma N_{i,b} + \sum_{t=1}^{6} \delta_t T_b^t \times N_{i,b} + \epsilon_{i,b} \qquad (2)$$

where $V_{i,b}$ and $N_{i,b}$ are dummies equal to 1 if, respectively, i participated in the election and if she is a newly registered citizen. Previously registered citizens are the omitted category.

[Figure 3 about here]

The results are shown in Table 2. Panel A pools the six treatment groups together. The

difference between the participation of newly registered citizens in the treatment groups and in the control group is small and 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 percent level. However, the fraction of newly registered citizens who voted at least once in 2012 is not significantly different in the control and treatment groups (column 6).

[Table 2 about here]

Turnout differences between newly registered citizens in the control and treatment groups are potentially the sum of a selection and a treatment effect. The selection effect is that citizens who registered as a result of the visits (henceforth the "compliers") may participate less than the citizens who would have registered regardless of whether or not they received a visit (the "always-takers"). The treatment effect is that the visits themselves may have affected participation, even for people who would have registered anyway. We now disentangle the two effects.

4.3 Average turnout of the citizens registered due to the visits

We first focus on the selection effect of the visits. One possible view is that information and registration costs are small and similar for everyone, so that the registration process selects all interested citizens and only excludes citizens with very low interest in voting. We should then expect compliers to vote much less than the always-takers. Another view, however, holds that the cost of registering is in general higher than the cost of voting, so that many citizens modestly interested in the elections but prepared to vote fail to register. In addition, to the extent that information and registration costs vary across citizens, the registration process may also exclude citizens who have a high interest in the elections but face an unusually high registration cost. Then, we may expect high participation rates among compliers. The model included in Online Appendix C provides a more formal exposition of

both views.

The difficulty is that newly registered citizens in the treatment groups include both compliers and always-takers, and we do not know which category any particular individual belongs to. As a result, we do not directly observe turnout differences between compliers and always-takers. We can, however, infer them from the turnout differences between the newly registered citizens in the control and treatment groups. Denote by V_g the average turnout of newly registered citizens in group g ($g = 0$ in the control group and $g = T$ in the treatment groups); by $V_{A,g}$ and $V_{C,g}$ the average turnout of always-takers and compliers in group g; and by $P_{C,g}$ the proportion of compliers among all newly registered citizens in group g. By definition, the control group only includes always-takers. Thus, $V_0 = V_{A,0}$. In the treatment groups, instead, $V_T = V_{A,T}\left(1 - P_{C,T}\right) + V_{C,T}P_{C,T}$. Let us assume for now that the visits did not have any treatment effect, so that the participation of always-takers was identical in the control and treatment groups: $V_{A,0} = V_{A,T} = V_A$. Then we get $V_{C,T} - V_A = \frac{1}{P_{C,T}}\left(V_T - V_0\right)$: the difference between the participation of compliers and always-takers can be computed by scaling by a factor $\frac{1}{P_{C,T}}$ the difference between the participation of newly registered citizens in the treatment and control groups.

Pooling all treatment groups together, from Table 1, column 2, we get $P_{C,T} = \frac{0.048}{0.168+0.048}$. Therefore, $\frac{1}{P_{C,T}} = \left(\frac{0.168+0.048}{0.048}\right) = 4.5$. In addition, from Table 2, Panel A, column 5, we have that, averaging over the four electoral rounds, $V_T - V_0 = -0.022$. We infer that on average the compliers were only 9.9 percentage points ($V_{C,T} - V_A = 4.5 \times 0.022 = 9.9$) less likely to participate in the 2012 elections than the always-takers. Since the always-takers' average participation was 69.5 percent ($0.517 + 0.119$), we obtain that the compliers' average participation was 59.6 percent. Using the same method and the estimates reported in column 6, we find that 93.0 percent of the compliers participated in at least one of the four rounds. This fraction is strikingly high, and only a nonsignificant 1.1 percentage points lower than the always-takers.

We now consider the compliers selected by each treatment separately, using results re-

ported in Table 2, Panel B. On average, voter turnout of newly registered citizens was lower in all treatment groups, compared to the control group (column 5). 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 statistically significant in both the "Early Home registration" and "Late Home registration" groups. The participation of newly registered citizens in the home registration groups was also lower compared to the canvassing groups, a difference significant at the 10 percent level. We infer from the estimated δ's that the propensity to vote of compliers selected by home registration visits was 52.8 percent, or 16.7 percentage points lower than the always-takers, on average.[8] However, the fraction who participated in at least one of the four rounds remained very high, at 91.5 percent (column 6).

We verify that these results are not driven by compositional effects: they are robust to comparing compliers and always-takers who share the same initial registration status (see Online Appendix D and Table A3).

4.4 Turnout as a function of election salience

To better characterize the propensity to vote of the compliers, who were registered due to the visits, we now investigate the extent to which their choice to vote depends on electoral salience. We estimate the drop in their participation between the highly salient presidential elections and the less salient parliamentary elections and compare it with the average turnout decline among always-takers. Formally, we run seemingly unrelated regressions of Equation [2] using participation at each round as a different outcome, and we compute the point estimates and standard errors of linear combinations of the coefficients. Consider, for instance, the previously registered citizens, who are the omitted category in Equation [2]. The percent decline in their turnout between the presidential and the general elections is

[8] 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 2, Panel B, column 5) and $\frac{1}{p_{C,T}} = \left(\frac{0.168+0.043}{0.043}\right) = 4.9$ (Table 1, Panel B, column 2).

$\frac{1/2(\alpha_{G1}+\alpha_{G2})-1/2(\alpha_{P1}+\alpha_{P2})}{1/2(\alpha_{P1}+\alpha_{P2})}$ where α_{P1}, α_{P2}, α_{G1} and α_{G2} are the estimated constants for each round. The results are presented in Table A4. In the control group, the turnout decline between the presidential and general elections was significantly stronger among newly registered citizens (42.8 percent) than among previously registered citizens (38.4 percent). In addition, the turnout decline was larger among newly registered citizens in the treatment groups (45.3 percent on average) than in the control group, 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 percent level.

Overall, these findings suggest that facilitating registration, in particular offering home registration in addition to information provision, does select voters who are slightly less likely to participate, and whose participation depends more on the salience of the elections. However, the selection effect of the visits is strikingly small: 93.0 percent of citizens registered due to the visits – nearly as high a rate as among citizens who registered on their own – voted at least once during elections that took place several months after the canvassers' visits. We infer that absent any visit, compliers' failure to register is driven less by lack of interest in the elections than by registration costs that are too high.

4.5 Get-out-the-vote effect

We now relax the assumption that the participation of always-takers was identical in the control and treatment groups and examine whether the visits affect participation independently of registration, including for people who would have registered anyway. First, the visits might have had a mobilization effect similar to that documented by the large get-out-the-vote literature (Gerber and Green, 2000, 2015). We isolate this effect by considering citizens whose turnout could only have been affected by it: citizens living in the sample apartments who registered before 2011 or in 2011, but before the visits started. We estimate Equation [4] on this sample:

$$V_{i,b} = \alpha + \sum_{t=1}^{6} \beta_t T_b^t + X_{i,b}^{'} \lambda + \sum_{s} \delta_b^s + \epsilon_{i,b} \qquad (4)$$

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

The interventions did not significantly affect the participation of citizens who had registered prior to the visits at any of the four rounds, their average participation, or the likelihood that they participated at least once (Table A6).

We conclude that the visits did not have any get-out-the-vote effect. Interestingly, this null result also points to the absence of significant within-household spillover effects between compliers and their household members.

4.6 Disengagement effect

Let us now investigate if the visits had not a positive, but a negative treatment impact on participation, in particular for citizens that were registered at home and would have registered on their own at the town hall otherwise. Citizens who make an effort to register on their own might get more involved politically, increasing their electoral participation. Home registration may have decreased their participation by reducing this engagement effect. Several factors might underlie this (dis)-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). 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). 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 reaffirmed 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).

It is difficult to isolate the disengagement effect of the visits since 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 by 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 4 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 (Table A7). 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.

[Figure 4 about here]

We estimate the following model:

$$NV_{i,b} = \alpha + \beta T_b^{EH\&LH} + \epsilon_{i,b} \qquad (5)$$

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 3 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 percent 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 that the way in which one gets registered (at the town hall or at home) affects one's subsequent participation, or that home registration disengages citizens who would have registered on their own otherwise. Instead, the next section shows that the visits had actually the opposite effect of empowering the compliers.

[Table 3 about here]

5 Impact on politicization

5.1 Data

A difference repeatedly found between voters and nonvoters is that the latter are less interested and informed (e.g, Palfrey and Poole, 1987). Given this difference, some authors raise the concern that institutions facilitating participation might bring in voters who are unlikely to cast a well-considered ballot and might add noise to the final results (e.g, Jakee

and Sun, 2006). But interest and informedness are not necessarily fixed: citizens induced to register and vote might also become more interested in the campaign, as they know that they will be able to vote, and in the results of elections in which they participate (e.g, Leighley, 1991). To test this hypothesis, we administered a postelectoral survey door-to-door to a sample of 1,500 respondents. Respondents were surveyed at their apartment 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.[9]

To 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 Kling, Liebman and Katz (2007).

5.2 Results

As can be seen in Figure 5, 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 percent 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 percent levels respectively (Table A8). The effect is positive for all but one of the 12 sub-indices, and it is significant for four of them: the ability to locate one's political preferences on the left-right axis; to locate prominent local and national politicians on this axis; 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

[9]More information about the sampling frame of the postelectoral survey is available in Online Appendix E.

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). By contrast, 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.

[Figure 5 about here]

As a result of these effects, the overall level of political interest and competence of newly registered citizens in the treatment groups was not lower than in the control group by the time of the postelectoral survey, suggesting that compliers registered due to the visits were not less politicized than the always-takers. These findings lend support to the view that inducing citizens to become active voters can increase their political interest and competence.

The finding that facilitating registration can dramatically increase turnout and interest in the elections is particularly important in a context where abstention steadily increases and threatens the legitimacy of elected governments. Perhaps equally central to the functioning and stability of democracy is the equal representation of all groups. While the introduction of new voting technologies or the adoption of compulsory voting have been found to increase equality of representation at the same time that they enhance participation (Fujiwara, 2015; Fowler, 2013), evidence from get-out-the-vote studies point to the opposite effect: on average, existing mobilization interventions actually widen disparities in participation by mobilizing underrepresented citizens less than high-propensity individuals (Enos, Fowler and Vavreck, 2014). We now examine the extent to which the registration visits affected the social makeup of the electoral rolls and the distribution of political preferences among registered voters.

6 Impact on electorate's composition and its preferences

6.1 Data

In addition to the data on political interest and competence exploited in Section 5, the postelectoral survey includes information on respondents' sociodemographic characteristics, political preferences, and choices of candidates at the presidential and parliamentary elections. We complete the analysis using data from the voter rolls available for all registered citizens, but on three characteristics only: age, gender, and place of birth.

6.2 Impact on the composition of the electorate

As mentioned above, the postelectoral survey was only administered to individuals who were initially unregistered or misregistered. Let us emphasize some of the most salient traits among the sociodemographic characteristics summarized in Table A1, Panel C. The average respondent is young (36 years old, which is more than 10 years younger than the average French adult), and has relatively low education: 42 percent do not have any diploma or have less than an end-of-high-school diploma, which is less than the overall adult population. Ten percent – slightly more than the overall adult population – are unemployed, and 27 percent are inactive. More than half live in social housing and 42 percent earn less than the minimum wage (1100 euros a month). Twenty-four percent were born outside of France, 22 percent are binationals, and 40 percent speak a language other than French with family members. Finally, half of the respondents have lived in the city for more than 10 years, and 17 percent arrived less than two years ago.

To identify the variables which best predict registration and the extent to which their influence was affected by the visits, we estimate the following OLS model:

$$I_{i,b} = \alpha + \beta T_b + \sum_k \gamma_k Z^k_{i,b} + \sum_k \delta_k Z^k_{i,b} \times T_b + \epsilon_{i,b} \qquad (6)$$

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.

[Figure 6 about here]

In the control group, all other things being equal, males and single persons are significantly less likely to register. In line with the resource model of political participation (Brady, Verba and Schlozman, 1995), we also find that the likelihood to be registered is lower among citizens with time constraints (in this case, because they return from work after the town hall's closing time); 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. Quite strikingly, the negative influence of several 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 A9).[10]

[10]Table A9 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). 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 tests to the choice of the outcome variable. The results are robust to using registration anywhere or the standardized average of participation at the presidential and parliamentary elections as the outcome, not registration at the current address (which excludes registration at another address in the city).

Next we turn to comparing the compliers and always-takers with previously registered citizens, using the voter rolls data. We run two simple selection equations. 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 characteristics. The results are shown in Table 4, column 1. We find that newly registered citizens are younger, more likely to be born further away from the city where they live, and more likely to be immigrants than previously registered citizens. Next we include all newly registered citizens in the sample and regress the treatment dummy on the same characteristics (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.

[Table 4 about here]

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. Let us now finally examine whether these citizens also have different political preferences.

6.3 Impact on the preferences of the electorate

The vote shares obtained by left-wing candidates were 67 percent, 74 percent, 69 percent and 75 percent 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 percent, 90 percent, 91 percent, and 95 percent 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 subject to 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.

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. The procedure is detailed in Online Appendix F, and the results presented in Table A10. 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 Discussion and Conclusion

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. These obstacles decrease registration and voting disproportionately for some segments of the population, including younger and less educated citizens, as well as immigrants.

Self-initiated registration could theoretically serve to select more interested and compe-

tent voters, and to increase their political involvement. And indeed, 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 salience of the election. Still, the most striking finding of our experiment resides in the fact that a large majority of those registered as a result of our visits took part in the Spring 2012 elections. In fact, 93 percent participated in either the presidential or the parliamentary elections. Moreover, we do not find any evidence for a disengagement effect of home registration. Quite the contrary, 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 or a few days before the elections, when electoral campaigns are most intense, would facilitate the transmission of information to unregistered citizens and could decrease procrastination: in our experiment, late visits were more effective than early ones. Our results further imply that registration rules that 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 (Brennan Center for Justice, 2009). Door-to-door enrollment 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 widespread. Unlike door-to-door enrollment, these

techniques do not involve any personal contact and thus might have different, and perhaps negative, treatment effects on participation. Those registered may be disengaged as a result of not playing any part in the registration process, and some may not even realize that they are registered and eligible to vote. 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 that offered home registration to the largest group of citizens offered it twice, once during the two to three months before the registration deadline, then again during the last month before the deadline. We estimate that this intervention increased overall participation from 64.7 to 68.6 percent in the first round and 65.6 to 69.3 percent in the second round of the presidential election, and from 41.2 to 42.1 percent and 39.4 to 41.2 percent in the corresponding general elections.[11] These estimates are lower bounds of the increased turnout that would result from making registration universal. Were it universal, the large number of citizens who did not use the possibility of getting registered at home 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 large. Indeed, the debriefing meetings we held with the canvassers revealed that only a slim minority of respondents who did not register with them invoked the rejection of elections and voting as their motivation. Their choice is likely explained by another factor: the trust they had to show toward the canvassers. Accepting the offer of home registration implied entrusting strangers with copies of electricity bills, ID cards or passports, and trusting them to file the registration application before the deadline. Canvassing is much less developed in France than in the US (Pons, 2016) and

[11] To derive these estimates, we proceed in several steps. 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 (more details in Online Appendix G).

there is no tradition of voter registration drives. The respondents in our sample were thus offered a service that they were unfamiliar with. An automatic registration procedure led by the state would naturally not face such trust issues.

Our findings also suggest that, beyond enhancing participation, implementing an automatic voter registration policy would likely increase the social and ethnic representativeness of the electoral rolls and the actual vote. Would this transformation 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. The vote choice of citizens at the margin of registering may depend relatively more than other citizens on the context (here, favorable to the left), similarly as their level of participation depends relatively more on the salience of the election. 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

While our sample was not randomly selected, it includes multiple regions, municipalities of varying size (from 10,000 inhabitants to the capital, Paris) and wealth, and different types of canvassers, enhancing the external validity of our findings. In addition, the sample precincts were selected for their large fractions of unregistered citizens and should thus be quite representative of French areas that would be the most affected by changes in the registration system.

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 much lower impact on turnout (Nickerson, 2015). There are two complementary interpretations of these different findings. The first is that unregistered citizens in the US have a lower propensity to vote 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. In France, it remains high, due in particular to the early registration deadline.

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 percent, versus 74 percent 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 (e.g, Garrigou, 1992). An important reason why the cost to register still generates such important effects might be that, differently from the cost of voting itself, each person pays it separately. All citizens vote on the same day, very visibly, and it is not a task that one can put off. Only a minority of citizens have to register every year, they do it inconspicuously and on different dates – so there is less social pressure to complete the task and more opportunity to procrastinate it. Removing avoidable costs may increase the likelihood citizens will overcome these other obstacles and participate – a lesson that might extend beyond voter registration to other prerequisites to voting, such as acquiring a valid voter ID.

References

Arceneaux, Kevin and David W. Nickerson. 2010. "Comparing Negative and Positive Campaign Messages: Evidence From Two Field Experiments." *American Politics Research* 38(1):54–83.

Barton, Jared, Marco Castillo and Ragan Petrie. 2014. "What Persuades Voters? A Field Experiment on Political Campaigning." *The Economic Journal* 124(574):293–326.

Bénabou, Roland and Jean Tirole. 2006. "Incentives and Prosocial Behavior." *American Economic Review* 95(5):1652–1678.

Bennion, Elizabeth A. and David W. Nickerson. 2011. "The Cost of Convenience: An Experiment Showing E-Mail Outreach Decreases Voter Registration." *Political Research Quarterly* 64(4):858–869.

Blais, André. 2010. "Political participation." *Comparing democracies* 3:165–183.

Braconnier, Celine and Jean-Yves Dormagen. 2007. *La démocratie de l'abstention.* Paris: Gallimard.

Braconnier, Celine, Jean-Yves Dormagen, Ghislain Gabalda and Xavier Niel. 2015. "La mal-inscription en France. Son ampleur et ses effets sur la participation à la présidentielle et aux législatives de 2012." *Revue Française de Sociologie* .

Brady, Henry E, Sidney Verba and Kay Lehman Schlozman. 1995. "Beyond SES : A resource model of political participation." *American Political Science Review* 89(2):271–294.

Brennan Center for Justice. 2009. *Expanding Democracy: Voter Registration Around the World.*

Brown, Robert D and Justing Wedeking. 2006. "People Who Have Their Tickets But Do Not Use Them: Motor Voter, Registration, and Turnout Revisited." *American Politics Research* 34(4):479–504.

Citrin, Jack, Eric Schickler and John Sides. 2003. "What of if Everyone Voted ? Simulating the Impact of Increased Turnout in Senate Elections." *American Journal of Political Science* 47(1):75–90.

Converse, Philip E. 1972. Change in the American Electorate. In *The human meaning of social change*. New York: Russell Sage pp. 263–337.

Enos, Ryan D, Anthony Fowler and Lynn Vavreck. 2014. "Increasing Inequality: The Effect of GOTV Mobilization on the Composition of the Electorate." *Journal of Politics* 76(1):273–288.

Erikson, Robert S. 1981. "Why Do People Vote? Because They Are Registered." *American Politics Research* 9(3):259–276.

Fowler, Anthony. 2013. "Electoral and Policy Consequences of Voter Turnout: Evidence from Compulsory Voting in Australia." *Quarterly Journal of Political Science* 8(2):159–182.

Fujiwara, Thomas. 2015. "Voting Technology, Political Responsiveness, and Infant Health: Evidence From Brazil." *Econometrica* 83(2):423–464.

Garrigou, Alain. 1992. *Le Vote et la vertu, comment les Français sont devenus électeurs*. Paris: Presses de Sciences Po.

Gaxie, Daniel. 1978. *Le cens caché. Inégalités culturelles et ségrégation politique*. Paris: Seuil.

Gerber, Alan S. and Donald P. Green. 2000. "The Effects of Canvassing, Telephone Calls, and Direct Mail on Voter Turnout: A field experiment." *American Political Science Review* 94(3):653–663.

Gerber, Alan S. and Donald P. Green. 2015. *Get out the vote*. Brookings Institution Press.

Gosnell, Harold Foote. 1930. *Why Europe Votes.* The University of Chicago Press.

Green, Donald P., Alan S. Gerber and David W. Nickerson. 2003. "Getting Out the Vote in Local Elections: Results from Six Door-to-Door Canvassing Experiments." *Journal of Politics* 65(4):1083–1096.

Greenwald, Anthony G., Catherine G. Carnot, Rebecca Beach and Barbara Young. 1987. "Increasing voting behavior by asking people if they expect to vote." *Journal of Applied Psychology* 72(2):315–318.

Highton, Benjamin. 1997. "Easy Registration and Voter Turnout." *Journal of Politics* 59(02):565.

Insee. 2012. *Les inscriptions sur les listes électorales 2011.*

Insee Premiere. 2012. *L'inscription et la participation électorales en 2012. Qui est inscrit et qui vote.* Insee Première 1411.

Jackman, Robert W. 1987. "Political institutions and voter turnout in industrial democracies." *American Political Science Review* 81(2):405–424.

Jakee, Keith and Guang Zhen Sun. 2006. "Is compulsory voting more democratic?" *Public Choice* 129(1-2):61–75.

John, Peter, Elizabeth MacDonald and Michael Sanders. 2015. "Targeting voter registration with incentives: A randomized controlled trial of a lottery in a London borough." *Electoral Studies* 40:170–175.

Karlan, Dean and Jonathan Zinman. 2009. "Observing unobservables: Identifying information asymmetries with a consumer credit field experiment." *Econometrica* 77(6):1993–2008.

Kling, Jeffrey R., Jeffrey B. Liebman and Lawrence F. Katz. 2007. "Experimental analysis of neighborhood effects." *Econometrica* 75(1):83–119.

Knack, Stephen. 1995. "Does Motor Voter Work? Evidence from State-Level Data." *Journal of Politics* 57(3):796–811.

Knack, Stephen. 2001. "Election-day Registration : The Second Wave." *American Politics Research* 29(1):65–78.

Leighley, Jan. 1991. "Participation as a Stimulus of Political Conceptualization." *Journal of Politics* 53(1):198–211.

Nagler, Jonathan. 1991. "The effect of registration laws and education on US voter turnout." *American Political Science Review* 85(4):1393–1405.

Nickerson, David W. 2007. "Does Email Boost Turnout ?" *Quarterly Journal of Political Science* 2(4):369–379.

Nickerson, David W. 2015. "Do Voter Registration Drives Increase Participation? For Whom and When?" *Journal of Politics* 77(1):88–101.

Palfrey, Thomas R. and Keith T. Poole. 1987. "The Relationship between Information, Ideology, and Voting Behavior." *American Journal of Political Science* 31(3):511–530.

Piven, Frances Fox and Richard A. Cloward. 1988. *Why Americans don't vote*. Pantheon.

Pons, Vincent. 2016. "Will a Five-Minute Discussion Change your Mind? A Countrywide Experiment on Voter Choice in France." *Harvard Business School Working Paper, No. 16-079* .

Pons, Vincent and Guillaume Liegey. 2016. "Increasing the Electoral Participation of Immigrants - Experiment Evidence from France." *Harvard Business School Working Paper, No. 16-094* .

Powell, Bingham G. 1986. "American voter turnout in comparative perspective." *American Political Science Review* 80(1):17–43.

Rosenstone, Steven J. and John Mark Hansen. 1993. *Mobilization, Participation and Democracy in America.* New York: Macmillan.

Rosenstone, Steven J. and Raymond E. Wolfinger. 1978. "The Effect of Registration Laws on Voter Turnout." *American Political Science Review* 72(1):22–45.

Rusk, Jerrold G. 1970. "The effect of the Australian ballot reform on split ticket voting: 1876-1908." *American Political Science Review* 66(4):1220–1238.

Ryan, Rrichard M. and Edward L. Deci. 2000. "Self-determination theory and the facilitation of intrinsic motivation, social development, and well-being." *American psychologist* 55(1):68.

Timpone, Richard J. 1998. "Structure, behavior, and voter turnout in the United States." *American Political Science Review* 92(1):145–158.

US Census Bureau. 2012. *Voting and Registration in the Election of November 2012– Detailed Tables. Table 4a. Reported Voting and Registration, for States: November 2012.*

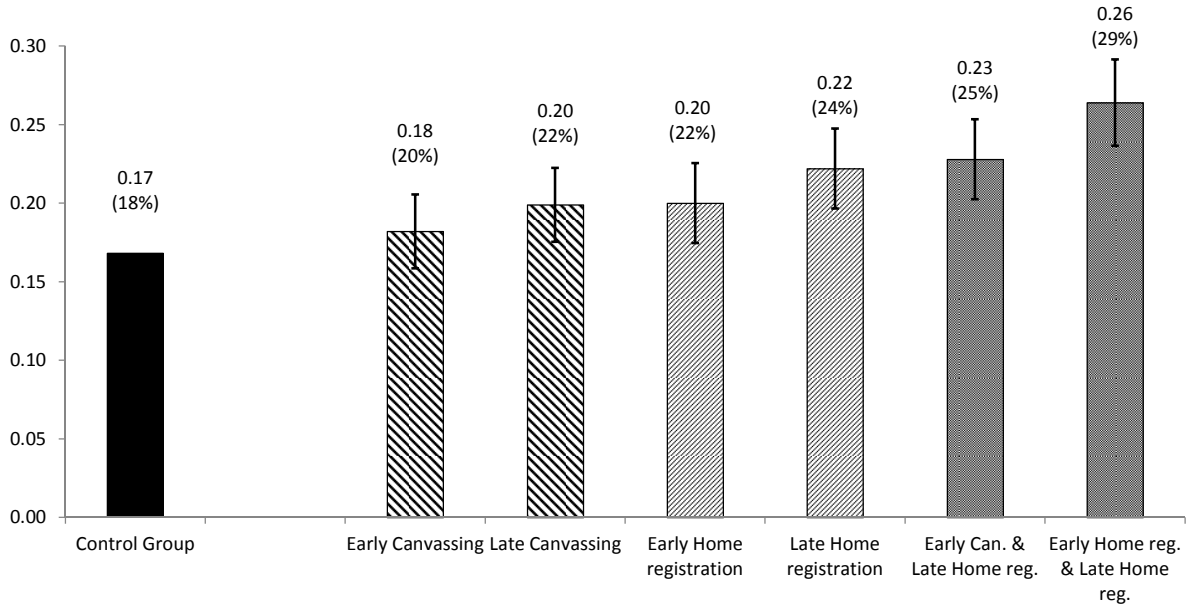
Vonnahme, Greg. 2012. "Registration Deadlines and Turnout in Context." *Political Behavior* 34(4):765–779.

Wolfinger, Raymond E. and Steven J. Rosenstone. 1980. *Who votes?* New Haven: Yale University Press.

Figure 1. 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 2. Impact on the number of new registrations among initially unregistered and misregistered citizens



Notes: We show the average number of new registrations in apartments of the control group and each treatment group, and the 95% confidence interval of the difference between the treatment groups and the control group. 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=20458.

Figure 3. Electoral participation by registration status and treatment group

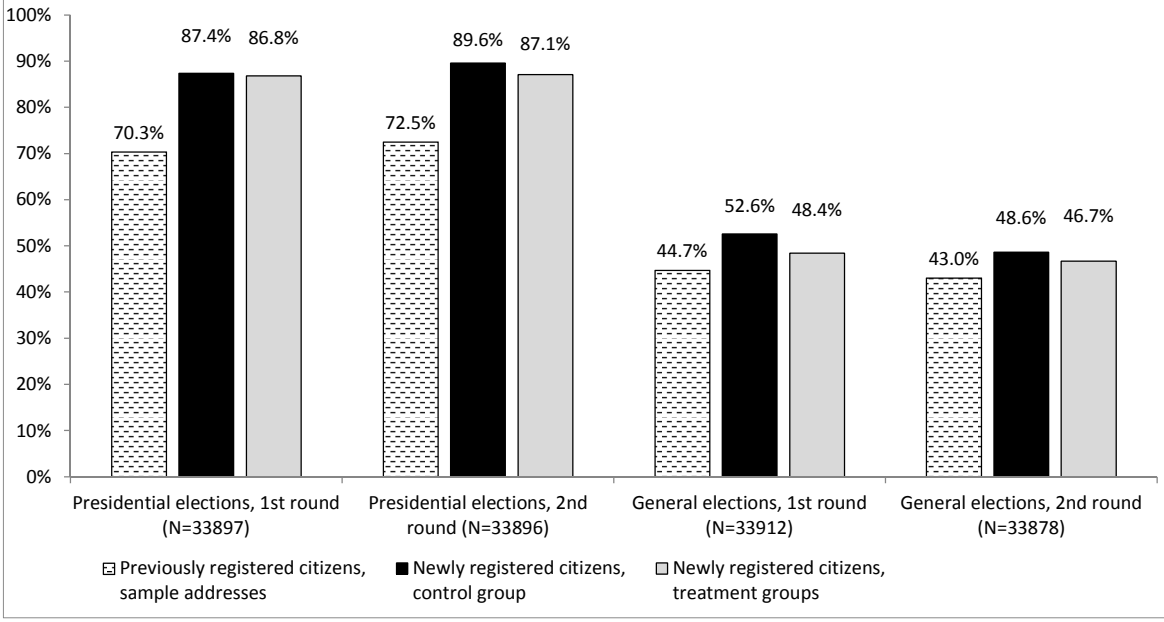
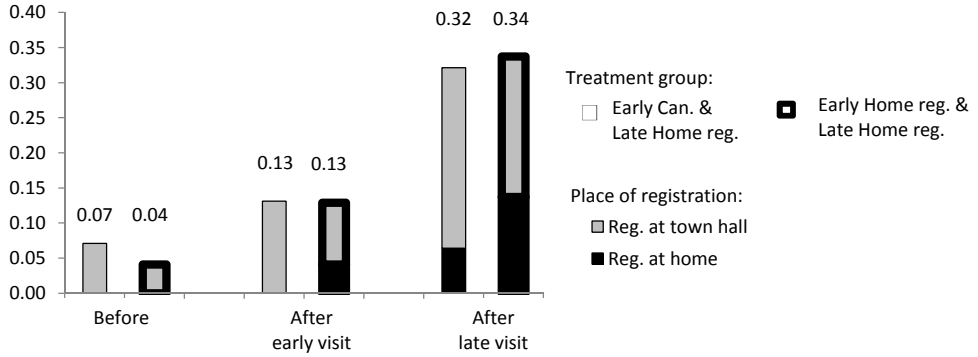
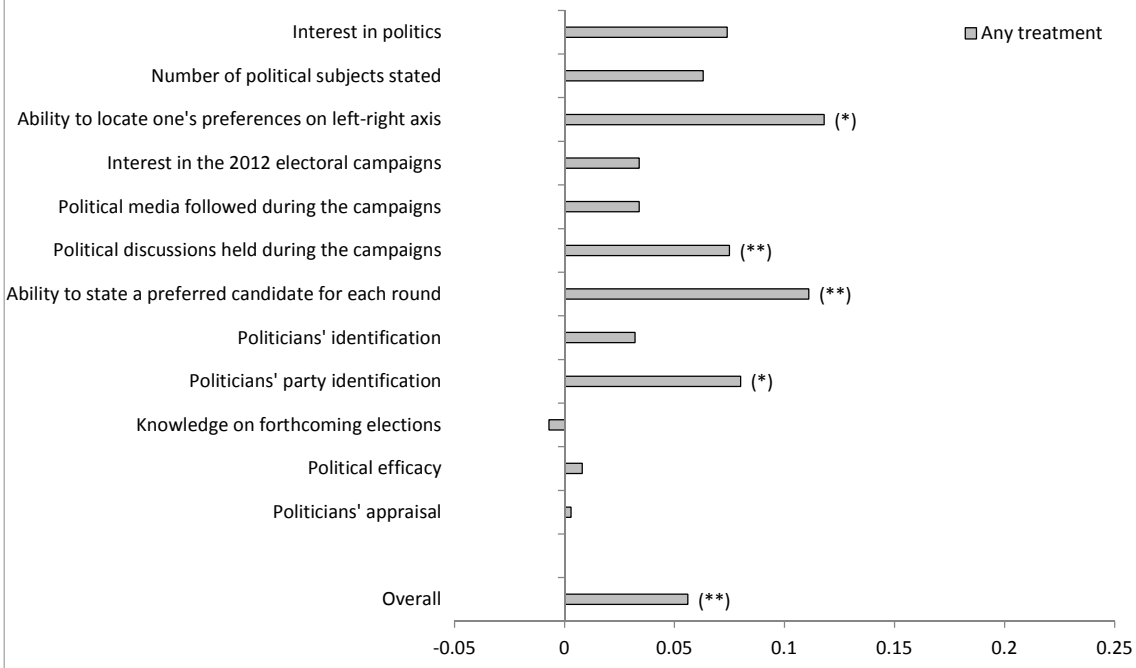


Figure 4. Controlling for the selection effect of home registration



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 between the late visit and the registration deadline. N=1399.

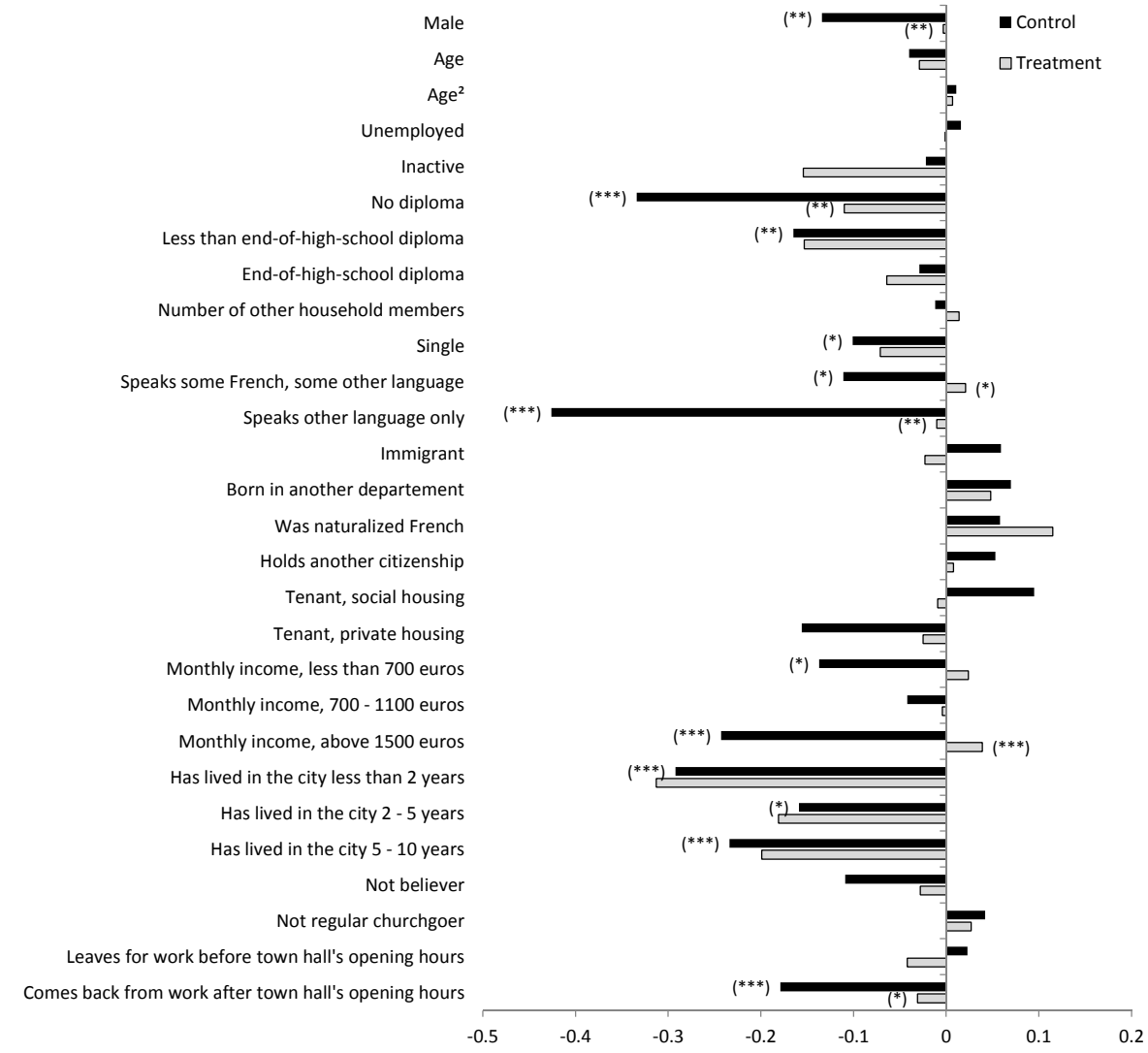
Figure 5. Impact on the level of politicization



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*=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' 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.

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



Notes: We show the effect of individual characteristics on the likelihood that the respondent is registered in her city, separately for the control group and all treatment groups pooled together. All characteristics 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 difference of 1 member is represented by 1 point). 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, and 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. Standard errors adjusted for clustering at the building level. *N*=1012.

Table 1: Impact on the number of new registrations

	(1)	(2)
	Number of new registrations	
Panel A. All treatments pooled together		
Any treatment	0.049***	0.048***
	(0.009)	(0.008)
Strata fixed effects	Yes	Yes
Apartment & Building controls	No	Yes
Observations	20458	20458
R-squared	0.02	0.03
Mean in Control Group	0.168	0.168
Panel B. Each treatment included separately		
Early Canvassing (EC)	0.015	0.014
	(0.013)	(0.012)
Late Canvassing (LC)	0.033***	0.031**
	(0.012)	(0.012)
Early Home registration (EH)	0.033**	0.032**
	(0.014)	(0.013)
Late Home registration (LH)	0.052***	0.054***
	(0.014)	(0.013)
Early Canvassing & Late Home registration (EC&LH)	0.062***	0.060***
	(0.013)	(0.013)
Early Home registration & Late Home registration (EH&LH)	0.098***	0.096***
	(0.014)	(0.014)
Strata fixed effects	Yes	Yes
Apartment & Building controls	No	Yes
Observations	20458	20458
R-squared	0.03	0.03
Mean in Control Group	0.168	0.168
Linear combinations of estimates:		
Average effect of Canvassing	0.024**	0.022**
1/2 (EC + LC)	(0.010)	(0.010)
Average effect of Home registration	0.043***	0.043***
1/2 (EH + LH)	(0.011)	(0.011)
Difference between average effect of Home reg. and Can.	0.019*	0.021*
1/2 (EH + LH) - 1/2 (EC + LC)	(0.011)	(0.011)
Difference between average effect of Late visit and Early visit	0.019*	0.020*
1/2 (LH + LC) - 1/2 (EH + EC)	(0.011)	(0.011)

Notes: Unit of observation is the apartment. We include all newly registered citizens in the sample apartments. Controls include: number of mailboxes in the building and number of last names found on the mailbox of the apartment that were absent from the 2011 voter rolls. Clustered standard errors in parentheses.
***, **, * indicate significance at 1, 5 and 10%.

Table 2: Electoral participation of citizens by registration status and treatment group

	(1)	(2)	(3)	(4)	(5)	(6)
	Presidential elections		General elections		Average on	One vote
	1st round	2nd round	1st round	2nd round	all rounds	at least
Panel A. All treatments pooled together						
Newly registered x Any treatment	-0.006	-0.025**	-0.042**	-0.019	-0.022*	-0.002
	(0.012)	(0.011)	(0.019)	(0.019)	(0.011)	(0.008)
Newly registered	0.171***	0.171***	0.079***	0.056***	0.119***	0.154***
	(0.011)	(0.010)	(0.017)	(0.017)	(0.010)	(0.007)
Constant	0.703***	0.725***	0.447***	0.430***	0.577***	0.786***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)
Observations	33897	33896	33912	33878	33789	33789
R-squared	0.02	0.02	0.00	0.00	0.01	0.02
Panel B. Each treatment included separately						
Newly registered x Early Canvassing (EC)	-0.009	-0.010	-0.026	0.009	-0.008	-0.004
	(0.017)	(0.015)	(0.027)	(0.027)	(0.016)	(0.012)
Newly registered x Late Canvassing (LC)	-0.002	-0.024	-0.022	-0.008	-0.014	0.009
	(0.017)	(0.017)	(0.032)	(0.029)	(0.017)	(0.011)
Newly registered x Early Home registration (EH)	0.006	-0.058***	-0.040	-0.024	-0.028*	-0.009
	(0.017)	(0.018)	(0.027)	(0.025)	(0.016)	(0.012)
Newly registered x Late Home registration (LH)	-0.011	-0.030*	-0.065**	-0.059**	-0.040**	-0.001
	(0.018)	(0.017)	(0.026)	(0.027)	(0.016)	(0.012)
Newly registered x Early Can. & Late Home reg. (EC&LH)	-0.018	-0.013	-0.033	-0.025	-0.021	-0.008
	(0.018)	(0.017)	(0.028)	(0.026)	(0.016)	(0.014)
Newly registered x Early Home reg. & Late Home reg. (EH&LH)	-0.002	-0.012	-0.060**	-0.003	-0.019	0.000
	(0.016)	(0.015)	(0.027)	(0.028)	(0.016)	(0.011)
Newly registered	0.171***	0.171***	0.079***	0.056***	0.119***	0.154***
	(0.011)	(0.010)	(0.017)	(0.017)	(0.010)	(0.007)
Constant	0.703***	0.725***	0.447***	0.430***	0.577***	0.786***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)
Observations	33897	33896	33912	33878	33789	33789
R-squared	0.02	0.02	0.00	0.00	0.01	0.02
Linear combinations of estimates:						
Av. difference between newly registered in Canvassing gr. and control	-0.006	-0.017	-0.024	0.000	-0.011	0.002
1/2 (EC + LC)	(0.014)	(0.013)	(0.024)	(0.023)	(0.014)	(0.009)
Av. difference between newly registered in Home registration gr. and control	-0.003	-0.044***	-0.053**	-0.041*	-0.034**	-0.005
1/2 (EH + LH)	(0.014)	(0.014)	(0.023)	(0.022)	(0.013)	(0.010)
Av. difference between newly registered in Home reg. gr. and Can. gr.	0.003	-0.027*	-0.029	-0.042**	-0.024*	-0.008
1/2 (EH + LH) - 1/2 (EC + LC)	(0.014)	(0.014)	(0.022)	(0.021)	(0.013)	(0.010)

Notes: Unit of observation is the individual participation at a given electoral round. We include all previously registered citizens (registered before 2011) and newly registered (registered in 2011) in the sample addresses. Previously registered citizens are the reference group. 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. Column 6: "One vote at least" is equal to 1 if the individual participated in any of the four rounds. Clustered standard errors in parentheses.

***, **, * indicate significance at 1, 5 and 10%.

Table 3: 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 registratior	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: Unit of observation is the apartment. We 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". Clustered standard errors in parentheses.

***, **, * indicate significance at 1, 5 and 10%.

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: Unit of observation is the registered citizen. 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. Clustered standard errors in parentheses.

***, **, * indicate significance at 1, 5 and 10%.