

INCREASING THE ELECTORAL PARTICIPATION OF IMMIGRANTS: EXPERIMENTAL EVIDENCE FROM FRANCE*

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Improving the political participation of immigrants could advance their interests and foster their integration into receiving countries. In this study, 23,800 citizens were randomly assigned to receive visits from political activists during the lead-up to the 2010 French regional elections. Treatment increased the turnout of immigrants without having any statistically significant effect on non-immigrants, while turnout was roughly equal in the control group. A post-electoral survey reveals that immigrants initially had less political information, which could explain the heterogeneous impact. Although the effect decays over subsequent elections, our findings suggest that voter outreach efforts can successfully increase immigrants' political participation, even when they do not specifically target their communities and concerns.

As the number of first and later-generation immigrants continues to increase among the population of the US and Europe (Segal *et al.*, 2010; Eurostat, 2011; Homeland Security, 2012), the question of their integration gains ever more importance. Immigrants are more likely to be uneducated and unemployed than other citizens; their median income is lower and they are often segregated in suburbs dominated by social housing (Beauchemin *et al.*, 2010; Eurostat, 2011; Alba and Foner, 2015). Immigrants' children remain disadvantaged and their situation is sometimes even worse than that of their parents (Maxwell, 2009). Perhaps reflecting these poor economic conditions, immigrants' sense of national belonging remains low. In France, for instance, less than half of naturalised immigrants and only 63% of their descendants say that they strongly feel French (Simon, 2012). Low integration of immigrants affects not only their own well-being, but also the overall social cohesion in the receiving societies (Givens, 2007; Bloemraad *et al.*, 2008). Over the last decade in several European countries, growing tensions between an estranged youth population of immigrant origin and the police and other institutions have manifested themselves in major suburban revolts (Duprez, 2009; Dancygier, 2010).

Broadly speaking, policies implemented to foster immigrants' integration fall into three groups. Laws regulating the access to citizenship, citizenship tests and related civic integration policies directly affect immigrants' efforts and aptitude to integrate (Bevelander and Spång (2015) for a recent review of the literature), while anti-discrimination laws address stigmatisation and discrimination by the mainstream population. Finally, a range of labour market and housing policies seek to improve immigrants' socioeconomic status (Joppke, 2007). Increasing immigrants' political

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We are very thankful to www.meilleursagents.com for providing housing price data at the address level for all addresses of our sample and to the George and Obie Shultz Fund and MIT France for their generous funding. We are grateful to Stephen Ansolabehere, Abhijit Banerjee, Rafaela Dancygier, Esther Dufló, Alan Gerber, Benjamin Marx, Benjamin Olken, Daniel Posner, Todd Roggers and James Snyder for their invaluable feedback about the study. All replication files are available online on Harvard Dataverse.

participation may be an important complementary tool. While a growing number of immigrants become citizens and obtain the right to vote, they often remain less likely to participate in elections than others.¹ Enhanced participation could affect their ability to advance political claims and make it more likely that their preferences are taken into account in policy choices, thus improving their well-being (Dahl, 1989; Bohman, 1996; Koopmans *et al.*, 2012; De Graauw and Vermeulen, 2016), similar to the policy shifts which ensued from the enfranchisement of American women (Miller, 2008), Black citizens in the US South (Cascio and Washington, 2014) and less educated citizens in modern Brazil (Fujiwara, 2015). The difficulty is that low integration and tensions with the rest of the society and its institutions may make it difficult to mobilise immigrants politically, perhaps requiring that voter outreach appeals be specifically tailored to their concerns to be successful. In the US, for instance, political parties, which historically have neglected immigrants and focused on courting the votes of groups with higher participation, are increasingly adopting this strategy (Verba *et al.*, 1995; Leighley, 2001). They target mobilisation efforts at groups of citizens of immigrant origin deemed close to their ideological platform, hoping to win votes by increasing their participation. Non-partisan local community-based organisations, too, increasingly engage in field campaigns targeting immigrants (Barreto, 2005; Garcia Bedolla and Michelson, 2012). In an attempt to boost the impact of these drives, the message is typically tailored to the targeted communities, conveyed in their native language and delivered by coethnic political activists.

This strategy may not be generalisable to all contexts, however. Where the immigrants are geographically dispersed and individual-level information on citizenship and country of birth is unavailable, targeting specific communities can be challenging and costly.² In addition, while supporters of the multi-culturalist model are certainly sympathetic to initiatives that allow immigrants to root their participation in society within their own community, champions of the assimilationist model believe that their full integration requires overcoming cultural, religious and ethnic divides (Kymlicka and Norman, 1994; Koopmans *et al.*, 2005; Howard, 2009; Koopmans, 2010). In the many countries where the latter model is dominant, voter appeals that explicitly target specific ethnic groups and contain ethnic cues may simply not be politically acceptable.

In this article, we examine whether a voter outreach effort can successfully increase the participation of immigrants even when it neither targets nor crafts its appeal to any specific immigrant community, and whether the effect on citizens of immigrant origin is as large as on native-born citizens.

In the four weeks leading to the French 2010 regional elections, activists from the *Parti Socialiste* (PS) canvassed eight cities of the Île-de-France region to encourage registered citizens to vote. Ethnic diversity in these cities is wide: immigrants contribute

¹ For evidence in the US, for instance, Bass and Casper (2001), Jiménez (2011) or United States Census Bureau (2012). In France, immigrants are much less likely to be registered than other citizens (Brouard and Tiberj, 2011; Niel and Lincot, 2012). Conditional on being registered, they are just as likely to vote in presidential elections (Niel and Lincot, 2012) but less likely in local elections, a difference that is at least partly driven by neighbourhood effects (Maxwell, 2010).

² See, for instance, the papers discussing the challenges involved in surveying immigrants (Font and Méndez, 2013; Reichel and Morales, 2017).

to a larger share of the population than in the country as a whole and they come from over 100 different countries. The campaign did not target any one group. Instead, the experimental sample of 1,347 addresses and 23,773 citizens includes immigrants of all origins as well as citizens born in France. From this sample, 678 addresses were randomly allocated to the treatment group, which received the visits of the canvassers, and the remaining 669 addresses to the control group, which did not receive any visit. In our analysis, we use 'immigrant' to refer to citizens of foreign birth (the 'first generation') and their French-born children living with them (who belong to the 'second generation').

Our results are surprising. Not only were immigrants successfully mobilised by the intervention, but their response was also significantly larger. Using administrative voter rolls and turnout data, we find that the canvassers' visits increased the turnout of immigrants by 3.4 percentage points in the first round and 2.8 percentage points in the second round without having any statistically significant effect on other citizens. These impacts correspond to persuasion rates of 10.6% and 9.3%, respectively. Immigrants' participation increased whether they were first or second generation, and whether they were originally from Maghreb, Sub-Saharan Africa or Asia, the three main regions of origin in the sample.

The heterogeneous impact of the visits is all the more striking, as contact rate was similar among immigrants and others, and turnout levels were comparable in both populations in the control group, once other characteristics are controlled for. We are able to verify that treatment effect heterogeneity is driven by immigrant origin, rather than observed factors correlated with it. To further shed light on underlying mechanisms, we administered a post-electoral survey to 900 respondents within two months after the regional elections. We first find that immigrants had significantly less political information than non-immigrants, which may have contributed to make the information conveyed by the political activists more impactful for them. In addition, immigrants may have been more receptive to the PS activists as their actual reported voting suggests they are more left-leaning than other citizens.

While the short-run impact of the visits on immigrants compares favourably with the impact of door-to-door canvassing measured in other settings (DellaVigna and Gentzkow, 2010), it diminishes over time: it is smaller and non-significant in the two rounds of the cantonal elections that were organised one year later. Overall, the results suggest that voter outreach efforts can successfully increase the political participation and integration of immigrants even if they are not specifically tailored to them, but that the effects of one-shot contacts will decay over time.

0.1. *Contribution to the Literature*

A large literature reports a negative correlation between ethnic diversity on one hand and public goods provision and social engagement on the other (Easterly and Levine, 1997; Alesina *et al.*, 1999; Alesina and La Ferrara, 2000; Miguel and Gugerty, 2005). In a recent study in France, Algan *et al.* (2016) find that ethnic diversity decreases the quality of public goods, such as local public spaces. Our study focuses on similar neighbourhoods, characterised by large ethnic diversity and prevalent social housing. Consistent with previous findings, political participation – a particular form of public

good contribution insofar as it benefits other citizens, for instance by enhancing elected officials' representativeness and accountability (Barr *et al.*, 2014) – is very low, on average, in our sample. Building on the existing literature, we test whether it can successfully be increased by a mobilisation campaign.

Methodologically, we draw on the large get-out-the-vote literature pioneered by Gosnell (1930) and Gerber and Green (2000). The most distinctive feature of our experiment is as follows. A few studies have estimated the impact of campaigns that target immigrants and have found large effects of phone banks and door-to-door canvassing (Michelson, 2005; Ramirez, 2005; Trivedi, 2005; Wong, 2005; Garcia Bedolla and Michelson, 2012). In existing studies, ethnic cues, explicitly present in the script or resulting from the very presence of coethnic canvassers and the use of voters' native languages, may have increased voters' receptiveness and contributed to the results. By contrast, the content of our intervention was not tailored to any specific group of voters: immigrants were actually underrepresented among the canvassers, who did not target coethnics and conveyed an ethnic-neutral message. The first main contribution of this article is thus to test whether a mobilisation campaign can successfully increase the participation of immigrants, even when it neither targets nor crafts its appeal to any specific community.

Second, our large sample size, together with the diversity of people visited by the canvassers, allows us to compare the impact of an identical voter outreach effort on immigrants and non-immigrants and across different groups of immigrants. While many studies compare the levels of registration, participation or the political preferences of different groups of citizens defined by ethnicity or place of birth (Wolfinger and Rosenstone, 1980; Verba *et al.*, 1995; Cho, 1999; Ramakrishnan and Espenshade, 2001; Jackson, 2003; Xu, 2005; Maxwell, 2010; Bevelander and Pendakur, 2011), few get-out-the-vote experiments compare treatment effects across different groups of the population (Arceneaux and Nickerson, 2009; Fieldhouse *et al.*, 2014; Braconnier *et al.*, 2017). This evidence is required, however, to assess the impact of voter mobilisation campaigns on the demographic composition and partisan balance of the electorate, both outcomes which are at least equally important as overall participation. The difficulty is that any sociodemographic characteristic, such as immigrant origin, correlates with many other variables. As a result, treatment effect heterogeneity measured along one dimension may capture the influence of correlated factors (Horiuchi *et al.*, 2007; Imai and Strauss, 2011; Green and Kern, 2012). We disentangle treatment effect heterogeneity along immigrant origin from heterogeneity along other dimensions by including other variables interacted with the treatment dummy in the same regression. To control for the influence of factors that we do not observe at the individual level in the entire sample, we either restrict the analysis to survey respondents or use aggregate administrative data on these factors at the finest geographical level available.

Third, the unusual setting of the experiment makes it an important data point in testing the external validity of the effect of get-out-the-vote campaigns. While most mobilisation experiments were conducted in the US, in partnership with non-partisan organisations, our study completes the smaller number of experiments evaluating a partisan effort (Gerber, Green and Green 2003*a*; Nickerson *et al.*, 2006; Arceneaux and Nickerson, 2010; Barton *et al.*, 2014; Pons, 2016), and it is the first organised in France.

Fourth, we not only measure the short-run impact of door-to-door canvassing, but also the persistence of the effect, akin to only a handful of other experiments. Similar to experimental (Gerber, Green and Shachar 2003*b*; Cutts *et al.*, 2009; Davenport *et al.*, 2010; Garcia Bedolla and Michelson, 2012) and non-experimental work (Meredith, 2009; Coppock and Green, 2016; Fujiwara *et al.*, 2016) that show how voting can be habit-forming, the impact of the visits on the participation of immigrants decreased but remained positive in the follow-up elections, even though it was no longer statistically significant.

Fifth, we contribute to the literature which seeks to identify mechanisms underlying the effect of voter outreach efforts. Results from experiments varying message content suggest that the type of arguments used to encourage voting (such as civic duty, the possibility of deciding a close election or neighbourhood solidarity) does not matter much (Gerber and Green, 2000; Michelson, 2003; Dale and Strauss, 2009), but that the effect increases with the amount of social pressure exerted on voters (Gerber *et al.*, 2008; Green *et al.*, 2013; Gerber and Green, 2016). Our survey data on respondents' informedness enables us to investigate the role of information provision as a complementary mechanism.

The remainder of the article is organised as follows. Section 1 describes the context of the experiment and its design. Section 2 presents the results and Section 3 investigates underlying mechanisms. Section 4 concludes.

1. Experimental Setting and Data

1.1. *The 2010 French Regional Elections*

The intervention took place during the campaign for the 2010 regional elections in Île-de-France, the region that includes Paris and surrounding areas. French regions were created in 1982 to decentralise some of the power concentrated in Paris, and were allocated authority over social housing, high schools, transportation, environment, town and country planning, business support, adult skills training and research. The legislative and executive branches of the regional government are the regional assembly and its president, both elected for six years. Regional elections have two rounds. In each round, voters cast a ballot for a list. Seats at the regional assembly are attributed in the first round if a list obtains more than 50% of the expressed votes, and in the second round otherwise. Lists that obtain more than 10% of the expressed votes in the first round qualify for the second round. Lists that obtain more than 5% of the expressed votes can merge with a qualified list. The list that obtains the largest vote share receives 25% of the seats at the regional assembly, while the remaining 75% are divided proportionally between all lists receiving more than 5% of expressed votes. The members of the regional assembly then elect one of their own, typically the head of the list with the largest vote share, as region's president.

Turnout at the 2010 elections was relatively low: only 46% and 51% of the registered citizens participated in the first and second rounds, on 14 and 21 March. The PS won 21 of the 22 metropolitan regions. In Île-de-France, the participation was also low. The PS gathered 25% of the votes in the first round and merged its list with two other left-

wing lists in the second round. The merged list received 57% of the votes, enabling the incumbent region's president, Jean-Paul Huchon, to be reelected.³

Cantonal elections took place one year later, on 20 and 27 March 2011, in half of the cities of the sample, accounting for 68% of registered citizens. Cantonal elections determine the members of the departmental councils. *Départements* (the administrative units below the region) have authority over middle schools, social assistance, transportation, housing, culture, local development and tourism. They are divided into small constituencies, the cantons. Each canton elects one departmental council member under a two-round plurality voting rule. Until 2015, council members were elected for six years; every three years, half the cantons of each department came up for election. In all cantons represented in the sample, no candidate won in the first round, and the election was decided by simple plurality in the second round. Similar to the 2010 regional elections, the 2011 cantonal elections were characterised by a low turnout (45% in both rounds) and by the overall domination by left-wing parties. We use these elections to estimate the medium-run impact of the intervention.

1.2. *The Intervention*

A total of 1,347 buildings with 23,773 citizens were included in the experiment: 678 addresses were randomly allocated to the treatment group, which received the visits of the canvassers, and the remaining 669 addresses to the control group, which did not receive any visit. All citizens living in the same building thus belonged to the same group by design.⁴ Before randomly allocating the buildings between the treatment and control groups, we stratified them by street and size to ensure balance of the two groups.⁵

The door-to-door visits took place on evenings and Saturdays during the four weeks before the first round of the 2010 election. Between the first and second rounds, in one city, Montrouge, canvassers continued to cover treatment group buildings that they had not covered before the first round.⁶ Canvassers knocked on doors in groups of two. They were active members of the PS and were not compensated for their participation in the experiment. Only a few of them had

³ Despite this clear-cut victory, the outcome of the election in Île-de-France had been uncertain during most of the campaign: on the left of the political spectrum, 'the Greens' emerged before the first round as a powerful third force and threatened to receive a larger vote share than the PS. On the right, Valérie Pécresse, who at the time was Minister for Higher Education and Research, was a serious contender. She was heading the list of the 'UMP', the party of President Nicolas Sarkozy, which held more than half the seats at the National Assembly.

⁴ Conducting the randomisation at the apartment level would have increased our statistical power, but proved infeasible. Indeed, registered citizens usually do not indicate their apartment number on the voter rolls.

⁵ The size of a building, proxied by the number of registered citizens living in it, is a good indicator of socioeconomic status since larger buildings often contain social housing, in the areas included in the sample.

⁶ Some buildings were cross-randomised to receive a second visit between the two rounds. Unfortunately, only 84 buildings housing 2,144 registered citizens could be integrated in this second randomisation, since the two rounds were separated by one week only. This very small sample limits the precision of the comparisons we can draw between the impact of one *versus* two visits, and one visit before the first round *versus* one visit between the two rounds; thus, we do not report these estimates.

previous experience of door-to-door canvassing, and all received a training course that included role-playing.

To ensure that the intervention would be administered uniformly across canvassers, the training course was identical in all cities, and all canvassers received a toolkit with detailed instructions and advice on how to start and lead the conversations. The full toolkit is available in online Appendix H in both the original and English versions (Figures H3 and H4). Canvassers were instructed to provide basic information systematically about the date of the election and the location and opening times of the poll office. They urged people to vote, using general arguments about the importance of voting, discussing the forthcoming elections and personal examples and stories. They further encouraged people to vote for the PS, as the campaign sought to increase both overall turnout and voter support. At the end of the discussion, the canvassers gave their interlocutor a leaflet summarising the platform of this list. When no one opened, this leaflet was left at the door.

1.3. *Sampling Frame*

The experimental sample spans eight cities in Île-de-France, mapped in Figure H1 in online Appendix H. The cities were chosen based on two criteria: low political turnout in previous elections and interest in the experiment shown by the PS' local unit.⁷ In each city, the sample includes polling stations characterised by low historical electoral participation.

The sample population primarily lives in the '*banlieues*', suburban neighbourhoods that face an important set of interrelated economic and social challenges, including poverty, housing decay, low employment rates, high criminality and poor educational achievement. These neighbourhoods are marked by increased internal tensions, notably between the youth and the police, and a widening gap with the rest of the country.

France has Europe's second-largest foreign-born population, and immigrants contribute to a larger share of the population in the sample cities than in the country as a whole. They come from over 100 different countries, mostly former French colonies in Maghreb, Sub-Saharan Africa and Asia, resulting in large ethnic diversity.⁸ Importantly, the door-to-door campaign targeted neither any one group nor immigrants as a whole, and it did not tailor its message to speak to their particular concerns. All the interactions were in French, and ethnic minorities were actually underrepresented among the canvassers, as they are in general among members of the PS (Dargent and Rey, 2014).

⁷ Cities in the experiment are Sevran, Villetaneuse, Pierrefitte (in the department Seine-Saint-Denis), Montrouge, Bagneux, Malakoff (in the department Hauts-de-Seine), Domont (in the department Val d'Oise) as well as the 11th arrondissement of Paris.

⁸ Before World War II, immigrants to France came primarily from other European countries. Afterwards, the majority came from outside Europe, primarily former French colonies. Most of them belong to ethnic minorities (Weil, 2005).

1.4. *The Data*

1.4.1. *Voter rolls*

To define our sample, we use the lists of registered citizens that are maintained at the municipality level.⁹ The voter rolls include the addresses of all registered citizens, as well as their gender and date and place of birth. This last variable enables us to identify immigrants, following the United Nations' definition of people born abroad. Since we know neither the citizenship at birth nor the ethnicity of registered citizens, we cannot disentangle citizens born abroad with a foreign citizenship (and who mostly belong to visible ethnic minorities) from those who were born French (and are mostly white), but the latter constitute a small minority in the sample neighbourhoods.

The voter rolls further enable us to reconstruct households and identify descendants of immigrants born in France. We assume that citizens sharing the same last name (either as their birth name or married name) and living at the same address belong to the same household,¹⁰ and that two individuals of the same household who were born more than 15 years apart are from different generations. This method allows us to identify the subset of immigrants' descendants registered to vote who live with their parents and whose parents are registered. It misses those who do not live with their parents or whose parents are not French citizens or are not registered.

1.4.2. *Monitoring spreadsheets*

Canvassers were asked to report the date and number of doors on which they knocked, and which were opened, in each building. Overall, they knocked on 9,070 doors and 4,432 (48.9%) were opened. Although it is difficult to precisely evaluate the relative importance of the different reasons why doors do not open, the major reason by far is that no one was at home during the canvassers' visit. Other reasons include a resident's distrust of strangers and children at home alone. The percentage of doors opened was slightly higher in buildings with a larger fraction of immigrants: from a bivariate regression, we find that the door-opening rate increases by 0.1 percentage points when the fraction of immigrants increases by one percentage point. This suggests that immigrants were slightly more likely than other citizens to open their door to the canvassers, but that the difference was small.

1.4.3. *Individual turnout*

In France, each voter who participates in an election signs an attendance sheet (see an example in Figure H2 in online Appendix H). These sheets are available for public review up to 10 days after the election. We took pictures of attendance sheets for the two rounds of the 2010 regional and the two rounds of the 2011 cantonal elections and digitised them. This administrative data enables us to measure the actual voting behaviour of all registered citizens in our sample without bias, unlike

⁹ Since the door-to-door visits took place after the registration deadline, they did not effect registration, and the administrative voter rolls that each municipality collates every year are an accurate description of the sample population.

¹⁰ Although imperfect, this method is relatively satisfying: we obtain an average number of 1.9 of registered citizens per household, when respondents to the post-electoral survey report an average of 2.1.

self-reports of voter turnout, which are often unreliable (Ansolabehere and Hersh, 2012). Altogether, our analysis is based on approximately 78,000 individual turnout observations.

1.4.4. *Post-electoral survey*

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

Of the people we called, 892 (24%) responded, and of those, 839 completed the entire survey. The pool of people who were called, and the pool of respondents, was not randomly drawn from the entire pool of registered voters: their participation is higher and they live disproportionately in Sevran (46%, compared to 31% for the entire sample).¹¹

1.4.5. *Additional sources of data at the building level*

Using Google Maps, we measure and control for the distance between a person's home and polling station. In addition, we obtained housing price data at the building level from the real estate company www.MeilleursAgents.com, which we use as a proxy for income. Finally, we identified all sample buildings included in a ZUS (*zone urbaine sensible*, the government's designation for underprivileged areas), using the atlas available at <http://sig.ville.gouv.fr/Atlas/ZUS/>.

1.4.6. *Aggregate data at a fine geographical level*

We geolocalised all 1,347 addresses in the sample, and matched them with 54 census blocks and 153 squares of 200 by 200 metres. We collected all publicly available data at these two levels for the year 2010: five databases from the population census at the census block level on education, economic activity, household size and structure, housing, and socioprofessional category; a fiscal database on income components and distribution at the census block level; and a database with information on income, age distribution, household size and housing based on tax returns geolocalised at the level of 200 by 200 square metres. We include URLs for the databases: <https://www.insee.fr/fr/statistiques/2028259>, <https://www.insee.fr/fr/statistiques/2028670>, <https://www.insee.fr/fr/statistiques/2028571>, <https://www.insee.fr/fr/statistiques/2028271> and <https://www.insee.fr/fr/statistiques/2028604>, for the census level data; <https://www.insee.fr/fr/statistiques/1893299> for the fiscal database and <https://www.insee.fr/fr/statistiques/2520034> for the 200 metres gridded data.

¹¹ Conversely, Montrouge and Villeteuse are underrepresented among the respondents to the survey due to the order in which the surveyors received the lists of phone numbers to call in the different cities. They were asked to conduct 900 surveys and stopped when they achieved this goal.

2. Results

2.1. Verifying Randomisation

Randomisation ensures that all observable and unobservable characteristics should be symmetrically distributed between the treatment and control groups. Table 1 verifies this for a series of observed characteristics. It presents summary statistics for registered citizens in the sample, separately for the control and treatment groups. We also show the difference between the means of the two groups and report the p-value of a test of the null hypothesis that they cannot be distinguished from each other. Overall, registered citizens in the two groups are very similar. Out of 41 differences shown in Table 1, three are significant at the 5% level and five at the 10% level, which is in line with what would be expected.¹²

Table 1
Summary Statistics

	Control group		Treatment group		P-value treatment = control	Number of observations
	Mean	SD	Mean	SD		
Panel (a): Building characteristics						
City where building is located						
Paris, 11th arrondissement	0.056	0.230	0.046	0.210	0.668	23,773
Bagneux	0.098	0.297	0.104	0.306	0.788	23,773
Domont	0.049	0.216	0.049	0.216	0.978	23,773
Malakoff	0.116	0.320	0.118	0.322	0.960	23,773
Montrouge	0.169	0.375	0.162	0.369	0.865	23,773
Pierrefitte-sur-Seine	0.065	0.247	0.061	0.240	0.810	23,773
Sevran	0.300	0.458	0.315	0.465	0.699	23,773
Villetaneuse	0.146	0.353	0.144	0.351	0.933	23,773
Based in a ZUS	0.344	0.475	0.354	0.478	0.812	23,773
Housing price	3,447	1,423	3,394	1,401	0.706	23,773
Distance to the polling station	0.272	0.243	0.268	0.248	0.867	23,773
Panel (b): Individual characteristics (voter rolls, whole sample)						
Male	0.449	0.497	0.461	0.498	0.069	23,773
Age	44.2	17.9	44.2	17.8	0.935	23,773
Immigrant	0.291	0.454	0.301	0.459	0.476	23,760
Maghreb origin	0.112	0.316	0.116	0.320	0.683	23,760
Sub-Saharan African origin	0.085	0.279	0.087	0.282	0.858	23,760
Asian origin	0.056	0.231	0.063	0.243	0.387	23,760
Other origin	0.039	0.193	0.037	0.190	0.746	23,760
Born in Île-de-France	0.520	0.500	0.504	0.500	0.126	23,760

¹² Education indicates the highest diploma obtained by the respondent and takes seven possible values, starting with no diploma. Employment status takes five values: employed worker, unemployed worker, student, retired worker and other inactivity. Socioprofessional category indicates the current occupation or most recent occupation, disentangled into eight possible responses, following the French nomenclature of occupation types (Insee, 2003). These three variables are thus included in Table 1 as a series of (respectively, seven, five and eight) dummies, similarly as the city where the building is located and immigrants' broad origin. Balance tests between the control and treatment groups are not independent for these sets of dummies. Completing Table 1, Table B1 in online Appendix B also shows summary statistics and balance between the treatment and control groups for all variables measured at the level of census blocks or squares of 200 by 200 metres. Registered citizens in the treatment and control groups are very similar on these variables too.

Table 1
(Continued)

	Control group		Treatment group		P-value treatment = control	Number of observations
	Mean	SD	Mean	SD		
Panel (c): Individual characteristics (post-electoral survey)						
Called for a survey	0.154	0.361	0.163	0.369	0.467	23,773
Survey conducted	0.242	0.428	0.232	0.422	0.508	3,766
Education						
No diploma	0.144	0.352	0.137	0.344	0.784	817
Primary school certificate	0.045	0.207	0.046	0.209	0.958	817
Middle school certificate	0.050	0.217	0.071	0.257	0.175	817
Professional qualification	0.227	0.419	0.170	0.376	0.042	817
End-of-high-school diploma	0.227	0.419	0.254	0.436	0.403	817
Short higher education diploma	0.154	0.361	0.147	0.355	0.800	817
Long higher education diploma	0.154	0.361	0.175	0.381	0.437	817
Employment status						
Employed worker	0.588	0.493	0.583	0.494	0.885	804
Unemployed worker	0.109	0.312	0.077	0.266	0.126	804
Student	0.070	0.256	0.105	0.307	0.076	804
Retired worker	0.179	0.384	0.169	0.375	0.715	804
Other inactivity	0.053	0.225	0.066	0.249	0.452	804
Socioprofessional category						
Category 1 (farmers)	0.002	0.050	0.003	0.051	0.970	783
Category 2 (craftsmen, retail traders)	0.032	0.177	0.018	0.134	0.200	783
Category 3 (executives)	0.070	0.255	0.113	0.317	0.028	783
Category 4 (middle-tier professions)	0.251	0.434	0.207	0.406	0.147	783
Category 5 (office workers)	0.291	0.455	0.262	0.441	0.383	783
Category 6 (labourers)	0.065	0.246	0.063	0.243	0.926	783
Category 7 (retired workers)	0.184	0.388	0.173	0.379	0.709	783
Category 8 (other inactivity)	0.104	0.306	0.160	0.367	0.026	783

Notes. For each variable, we report the means and standard deviations in both the control and treatment group and indicate the p-value of the difference. Standard errors are adjusted for clustering at the building level. 'Immigrant' refers to citizens of foreign birth and their French-born children living with them.

Slightly more than half of the registered citizens in our sample live in three cities of the department Seine-Saint-Denis (Sevran, Villetaneuse and Pierrefitte-sur-Seine) that are known for their underprivileged neighbourhoods and high crime rates. Overall, more than a third live in a ZUS. The average housing price, 3,400 euro per square metre, is nonetheless relatively high, due to the close proximity of Paris.

The average registered citizen lives only 270 metres away from his polling station and is 44 years old. Of the registered citizens, 45% are males and 30% are immigrants. Among immigrants, 87% come from one of three broad regions: Maghreb (39%), Sub-Saharan Africa (29%) or Asia (20%).

The reply and survey completion rates of the post-electoral survey in the control and treatment groups are very similar. Among the respondents, 45% do not have a

high school diploma, 59% are employed workers and 9% report being unemployed. Middle-tier professions and office workers largely dominate other types of activities.

Immigrants in the control and treatment groups are also extremely similar. Table B2 in online Appendix B shows the difference between the means of immigrants in the two groups for the same variables as those shown in Table 1. No difference is statistically significant.

2.2. Overall Impact of the Visits

We first estimate the average impact of the visits on all citizens using the following IV regression:

$$Y_{i,b} = \alpha_1 + \beta_1 \text{Visited}_b + \mathbf{X}'_b \gamma_1 + \mathbf{Z}'_{i,b} \delta_1 + \sum_s \lambda_b^s + \epsilon_{i,b}, \quad (1)$$

where $Y_{i,b}$ is turnout of individual i living in building b , Visited_b is a dummy variable equal to 1 if building b received the visit of canvassers, \mathbf{X}_b is a vector of building characteristics (its housing price, distance to the polling station and whether it is located in a ZUS), $\mathbf{Z}_{i,b}$ is a vector of individual characteristics (age, age², gender and whether the individual was born in the region Île-de-France) and λ_b^s are strata fixed effects. Visited_b is instrumented with T_b , a dummy equal to 1 if the building was allocated to the treatment group. The first stage was 0.92, due primarily to time constraints, which prevented the canvassers from covering some buildings in the treatment group. In addition, a few buildings in the control group were covered by mistake. These two sources of difference between treatment group and actual treatment received are not particularly interesting, so that the ‘intention to treat’ effect does not have any interest *per se* in this case, and we only report the ‘treatment-on-the-treated’ effect (Angrist *et al.*, 1996). In this and all remaining regressions, we adjust the standard errors for clustering at the level of the building, since randomisation was conducted at this level. Failure to do this would result in an underestimate of the standard errors.¹³

The results are shown in Table 2, panel (a). Average participation was low in the control group in the first round: 34.2%. This turnout rate is 10 percentage points lower than the regional average (43.8%), consistent with the choice of polling stations with a low turnout history. Participation in the second round was slightly higher than the first (37.8%), but still nine percentage points lower than the regional average (47.1%).

The impact of the visits on overall participation was positive but small and non-significant both in the first round (column (1)) and in the second round of the 2010

¹³ Online Appendix G also shows the first stage and reduced form of the main regressions when using the same adjustment of standard errors: regular cluster robust standard errors at the level of the address (Tables G1 and G2). The first stage is significant at the 1% level, and the significance levels of the reduced form exactly correspond to the significance levels in the IV. The results are robust to using regular cluster robust standard errors at the level of the street (Tables G3 and G4) or allowing for correlation of the error terms at the level of the street or the city with the wild cluster bootstrap procedure proposed by Cameron *et al.* (2008) (Tables G5–G8) and pairs cluster bootstrap procedure proposed by Esarey and Menger (2018) (Tables G9–G12).

Table 2
Impact of the Visits on Participation in the 2010 Regional Elections

	(1) First round	(2)	(3) Second round	(4)	(5) Average of first and second rounds	(6)
Panel (a): Overall impact						
Visited	0.004 (0.008)	0.003 (0.008)	0.006 (0.008)	0.003 (0.008)	0.005 (0.007)	0.003 (0.007)
Strata fixed effects	x	x	x	x	x	x
Building and individual controls		x		x		x
Observations	23,773	23,760	23,773	23,760	23,773	23,760
R ²	0.06	0.10	0.05	0.10	0.06	0.11
Mean in control group	0.342	0.342	0.378	0.378	0.360	0.360
Panel (b): Impact on immigrants and non-immigrants						
Visited	-0.008 (0.009)	-0.011 (0.009)	-0.005 (0.010)	-0.008 (0.009)	-0.006 (0.009)	-0.009 (0.008)
Immigrant × Visited	0.041** (0.017)	0.044*** (0.017)	0.032* (0.019)	0.036** (0.018)	0.037** (0.017)	0.040** (0.016)
Immigrant	0.003 (0.011)	-0.010 (0.012)	0.017 (0.012)	0.002 (0.012)	0.010 (0.011)	-0.004 (0.011)
Strata fixed effects	x	x	x	x	x	x
Building and individual controls		x		x		x
Observations	23,760	23,760	23,760	23,760	23,760	23,760
R ²	0.06	0.10	0.06	0.10	0.07	0.11
Mean in control group, non-immigrants	0.354	0.354	0.385	0.385	0.369	0.369

Notes. The unit of observation is the individual. Standard errors are adjusted for clustering at the building level and reported in parentheses. In panel (a), we estimate the overall impact of the visits. In panel (b), we estimate their impact for immigrants and non-immigrants separately. Visited is instrumented with Treatment and Immigrant × Visited with Immigrant × Treatment. Building controls include: housing price, distance to the polling station, and whether it is located in a ZUS. Individual controls include: age, age², gender and whether the individual was born in Île-de-France. ***, **, * indicate significance at 1%, 5% and 10%.

elections (column (3)). This finding is robust to the inclusion of individual and building controls (columns (2) and (4)). Considering the upper bound of the 95% confidence interval, we can reject any effect higher than 1.8 percentage points in the first round at the 5% level and any effect higher than 1.9 percentage points in the second round, in the specification including all controls.

This result differs from much of the pre-existing get-out-the-vote literature, which has found large effects of door-to-door canvassing across a large variety of settings (Gerber and Green, 2015). The location of our experiment may contribute to explaining this difference: in a recent meta-analysis, Bhatti *et al.* (2016) find that the effects of door-to-door canvassing in Europe tend to be substantially smaller than in the US. In addition, most of the existing evaluations of mobilisation campaigns examine non-partisan interventions. But partisan mailing, calling and canvassing campaigns, such as in the present study, have been found to generate weaker and more varying effects, including in the US (Green *et al.*, 2013; Gerber and Green, 2016).

2.3. Impact on Immigrants and Non-immigrants

We then use specifications of the form in (2) to estimate the treatment effects separately for immigrants and non-immigrants:

$$Y_{i,b} = \alpha_2 + \beta_2 \text{Visited}_b + \theta_2 I_{i,b} + \lambda_2 \text{Visited}_b \times I_{i,b} + X'_b \gamma_2 + Z'_{i,b} \delta_2 + \sum_s \lambda_b^s + \epsilon_{i,b}, \quad (2)$$

where $I_{i,b}$ is a dummy equal to 1 if the person is an immigrant. Visited_b and $\text{Visited}_b \times I_{i,b}$ are instrumented by T_b and $T_b \times I_{i,b}$. In this equation, θ_2 estimates the differential participation of immigrants in the control group. β_2 and $\beta_2 + \lambda_2$ estimate the impact of receiving the visit of canvassers for non-immigrants and immigrants, respectively, and λ_2 estimates the differential impact of the visits for immigrants. The results are shown in Table 2, panel (b).

The participation of immigrants at the first round of the 2010 regional elections did not significantly differ from non-immigrants (column (1)), a finding robust to including individual and building controls (column (2)). Similarly, the participation of immigrants in the second round of the elections did not significantly differ from non-immigrants (columns (3) and (4)). While immigrant origin is not associated with a different turnout level, it accounts for an important share of treatment effect heterogeneity.

As shown in columns (1) and (2), the visits had a significantly larger impact on immigrants than non-immigrants in the first round. The difference, 4.4 percentage points, is significant at the 1% level in the specification with individual and building controls. The participation of non-immigrants was not affected significantly: the coefficient on Visited is actually negative, although small and not significant at the standard levels. However, adding the coefficients on Visited and Immigrant \times Visited, we find that the visits increased the participation of immigrants by 3.4 (= 4.4 – 1.1) percentage points, which is significant at the 5% level. Scaling this estimate by the inverse of the fraction of doors opened (48.9%), we obtain that the visits increased the first round participation of immigrants who live in an apartment which opened its door by approximately 7.0 percentage points ($3.4 \times 1/0.489$).¹⁴ Our estimate can be compared to the existing literature using the method proposed by DellaVigna and Kaplan (2007). We divide the treatment effects by the proportion of immigrants that could potentially be mobilised to vote by the treatment (i.e. 1 minus the fraction of voters in the control group among immigrants). We obtain a persuasion rate of $0.034 / \{0.489 \times [1 - (0.354 - 0.010)]\} = 10.6\%$. This is of the same order of magnitude as persuasion rates measured by previous studies examining the impact of door-to-door canvassing on participation. For instance, Gerber and Green (2000) and Green *et al.*

¹⁴ This result is derived under two assumptions. First, we assume that the impact of the visits on households that did not open their door is negligible. This assumption is supported by Sinclair *et al.* (2012) finding that voter mobilisation does not yield spillovers across households. Second, we assume that the number of citizens living in households that opened their door is equal, on average, to the number of citizens living in households that did not, so that we can proxy the fraction of citizens living in households that opened their door by the fraction of doors opened. Note that in the post-electoral survey, members of larger households were more likely to say that their household did not receive the canvassers' visit during the campaign. Thus, if anything, the fraction of doors opened provides an upper bound to the fraction of citizens living in households that opened their door, and taking the inverse provides a lower bound to the true impact of the visits on citizens living in an apartment that opened its door.

(2003) find persuasion rates of door-to-door canvassing of 15.6% and 11.5% respectively.

In the second round, the impact of the visits on immigrants was larger again than on non-immigrants (columns (3) and (4)). The difference, 3.6 percentage points, is significant at the 5% level. While the participation of non-immigrants was not significantly affected, the participation of immigrants increased by 2.8 (= 3.6 – 0.8) percentage points, an effect significant at the 10% level. This corresponds to a persuasion rate of $0.028 / \{0.489 \times [1 - (0.385 + 0.002)]\} = 9.3\%$. Averaging over both rounds, the impact of the intervention on immigrants was 3.1 percentage points and significant at the 5% level.

Finally, we distinguish between different groups of citizens of immigrant origin. First, we separate citizens born abroad from their children, identified based on the voter rolls as individuals living in the same household and 15 years younger or more. The results are presented in Table 3, panel (a). The differential impact of the visits was positive on both subgroups in both rounds, but the interaction with the treatment is significant only for citizens born abroad, which constitute a larger group. Second, in panel (b), we separate immigrants by broad origin, Maghreb, Sub-Saharan Africa and Asia, and compare the impact of the visits across them. The impact on turnout at the first and second rounds was large for citizens born in the three regions. Even though the small sample size limits the statistical precision of this comparison, we do find that Maghreb origin and, averaged over both rounds, Sub-Saharan African origin are significant when interacted with the treatment dummy. Overall, interactions between the treatment indicator and dummies for immigrants' subgroups are uniformly positive in both rounds and across all groups examined in panels (a) and (b), reinforcing our conclusion of a positive differential treatment effect on immigrants.

2.4. *Effect Persistence*

Previous studies investigating whether mobilisation effects persist in subsequent elections have found large and significant – albeit decaying – effects up to eight years after the initial intervention (Gerber, Green and Shachar, 2003b; Cutts *et al.*, 2009; Davenport *et al.*, 2010; Garcia Bedolla and Michelson, 2012). In their review of experimental and quasi-experimental evidence on habit formation in voting, Coppock and Green (2016) note that effects of shocks to turnout are more likely to persist in a downstream election if the upstream election has low salience and if both elections are of the same type. Both conditions apply to our setting: the two elections we consider were both arguably of low salience with only about half of registered citizens participating nationwide in the 2010 regional elections, and even fewer in the 2011 cantonal elections. They were both local, and they took place almost exactly one year apart, perhaps further facilitating persistence.

To assess the extent to which the effect of the canvassers' visits on the participation of immigrants persisted in the 2011 cantonal elections, we restrict the sample to the four cities in which cantonal elections were held in 2011: Montrouge, Pierrefitte-sur-Seine, Sevran and Villetaneuse. These four cities account for 68% of the entire sample.

Table 3

Impact of the Visits on Participation in the 2010 Regional Elections for Different Groups of Immigrants

	(1) First round	(2) Second round	(3) Average of first and second rounds
Panel (a): Immigrants born abroad <i>versus</i> their children			
Visited	-0.011 (0.009)	-0.008 (0.009)	-0.009 (0.008)
Immigrants born abroad × Visited	0.057*** (0.018)	0.036* (0.019)	0.046*** (0.017)
Immigrants' children × Visited	0.007 (0.031)	0.037 (0.033)	0.022 (0.030)
Immigrants born abroad	-0.028** (0.014)	-0.008 (0.013)	-0.018 (0.012)
Immigrants' children	0.034* (0.020)	0.023 (0.021)	0.028 (0.019)
Strata fixed effects	x	x	x
Building and individual controls	x	x	x
Observations	23,760	23,760	23,760
R ²	0.10	0.10	0.11
Mean in control group, non-immigrants	0.354	0.385	0.369
Panel (b): Immigrants of different origins			
Visited	-0.012 (0.009)	-0.009 (0.009)	-0.010 (0.008)
Maghreb origin × Visited	0.064** (0.025)	0.056** (0.027)	0.060** (0.024)
Sub-Saharan African origin × Visited	0.045 (0.029)	0.045 (0.029)	0.045* (0.027)
Asian origin × Visited	0.036 (0.036)	0.017 (0.035)	0.027 (0.033)
Other origin × Visited	0.020 (0.044)	0.008 (0.047)	0.014 (0.042)
Maghreb origin	-0.039** (0.016)	-0.019 (0.017)	-0.029* (0.015)
Sub-Saharan African origin	0.046** (0.020)	0.039** (0.019)	0.043** (0.018)
Asian origin	-0.030 (0.026)	0.000 (0.025)	-0.015 (0.024)
Other origin	-0.023 (0.026)	-0.021 (0.029)	-0.022 (0.026)
Strata fixed effects	x	x	x
Building and individual controls	x	x	x
Observations	23,760	23,760	23,760
R ²	0.10	0.10	0.12
Mean in control group, non-immigrants	0.354	0.385	0.369

Notes. The unit of observation is the individual. Standard errors are adjusted for clustering at the building level and reported in parentheses. In panel (a), we estimate the impact of the visits for non-immigrants, immigrants born abroad and their French-born children living with them, separately. Visited is instrumented with Treatment, Immigrant born abroad × Visited with Immigrant born abroad × Treatment and Immigrants' children × Visited with Immigrants' Children × Treatment. In panel (b), we estimate the impact of the visits for non-immigrants and immigrants of different origins separately. Visited is instrumented with Treatment, Maghreb origin × Visited with Maghreb origin × Treatment, Sub-Saharan African origin × Visited with Sub-Saharan African origin × Treatment, Asian origin × Visited with Asian origin × Treatment and Other origin × Visited with Other origin × Treatment. Building controls include: housing price, distance to the polling station and whether it is located in a ZUS. Individual controls include: age, age², gender and whether the individual was born in Île-de-France. ***, **, * indicate significance at 1%, 5% and 10%.

The results are shown in Table 4. Similarly as with the national average, voters in our sample were less likely to vote at the cantonal elections than at the regional elections. As for the regional elections, the participation of immigrants did not significantly differ from non-immigrants, and the impact of the visits on overall participation (panel (a)) and on the participation of non-immigrants (panel (b)) was small and non-significant both in the first and second round. The impact on immigrants was positive but non-significant and smaller than at the regional elections: 1.6 (= 2.0 – 0.4) and 1.8 (= 2.9 – 1.1) percentage points at the first and second rounds, respectively, against 3.4 and 2.8 percentage points. Table A1 in online Appendix A disentangles between different groups of citizens of immigrant origin. Unlike the regional elections, the impact, while positive in general, was statistically significant neither for citizens born abroad nor for their children (panel (a)), and it was only significant (at the 5% level) for immigrants of Maghreb origin in the second round (panel (b)). These results suggest that the effect of the short interaction that mobilised some immigrants for the elections immediately following the intervention decayed over time. However,

Table 4
Impact of the Visits on Participation in the 2011 Cantonal Elections

	(1) First round	(2)	(3) Second round	(4)	(5) Average of first and second rounds	(6)
Panel (a): Overall impact						
Visited	0.004 (0.009)	0.003 (0.009)	0.001 (0.009)	-0.001 (0.009)	0.003 (0.009)	0.001 (0.008)
Strata fixed effects	x	x	x	x	x	x
Building and individual controls		x		x		x
Observations	15,416	15,405	15,410	15,399	15,410	15,399
R ²	0.06	0.11	0.05	0.10	0.06	0.12
Mean in control group	0.262	0.262	0.291	0.291	0.277	0.277
Panel (b): Impact on immigrants and non-immigrants						
Visited	-0.003 (0.012)	-0.004 (0.012)	-0.010 (0.012)	-0.011 (0.011)	-0.006 (0.011)	-0.007 (0.010)
Immigrant × Visited	0.019 (0.021)	0.020 (0.020)	0.028 (0.021)	0.029 (0.021)	0.024 (0.019)	0.025 (0.019)
Immigrant	-0.012 (0.014)	-0.016 (0.014)	0.020 (0.014)	0.015 (0.015)	0.004 (0.013)	0.000 (0.013)
Strata fixed effects	x	x	x	x	x	x
Building and individual controls		x		x		x
Observations	15,405	15,405	15,399	15,399	15,399	15,399
R ²	0.06	0.11	0.05	0.10	0.06	0.12
Mean in control group, non-immigrants	0.274	0.274	0.293	0.293	0.283	0.283

Notes. The unit of observation is the individual. Standard errors are adjusted for clustering at the building level and reported in parentheses. In panel (a), we estimate the overall impact of the visits. In panel (b), we estimate their impact for immigrants and non-immigrants separately. Visited is instrumented with Treatment and Immigrant × Visited with Immigrant × Treatment. Building controls include: housing price, distance to the polling station and whether it is located in a ZUS. Individual controls include: age, age², gender and whether the individual was born in Île-de-France. ***, **, * indicate significance at 1%, 5% and 10%.

comparing the point estimates from Tables 2 and 4 in a seemingly unrelated regressions (SUR) framework (Table C1 in online Appendix C), we cannot formally reject the hypothesis that the effects were equal in both years.¹⁵ Taken at face value, the 2011 estimates account for 49% and 64% of the first and second round 2010 effects, which is close in magnitude to the one year persistence of about half of initial mobilisation effects reported by Gerber, Green and Shachar (2003*b*) and Cutts *et al.* (2009). While limited statistical power precludes any firm conclusion, our estimates of decaying but non-zero persistence on citizens who were successfully mobilised in the election immediately following the visits are in line with the existing literature.

3. Mechanisms Explaining the Heterogeneous Treatment Effects

The fact that the visits successfully increased the participation of immigrants in the 2010 regional elections answers our main question: voter outreach efforts that are not tailored to specific immigrant communities can nonetheless improve their participation. The significantly larger impact among immigrants is, however, unexpected and puzzling. We now investigate underlying mechanisms, which may explain this treatment effect heterogeneity. Purely mechanical explanations are unlikely to account for it: the door-opening rate was not much larger in buildings with a larger fraction of immigrants, and their participation is not lower than other citizens in the control group, so that the effect cannot be interpreted as a mere catch-up effect. Instead, we first examine whether treatment effect heterogeneity along immigrant origin captures the influence of correlated sociodemographic factors. We then turn to differences in political attitudes, namely political knowledgeability and ideological closeness to the PS, as potential drivers of the large impact of the visits among immigrants.

3.1. *Isolating the Influence of Immigrant Origin on Treatment Effect Heterogeneity*

We now address the possibility, inherent to subgroup analysis, that treatment effect heterogeneity measured along one dimension – here, immigrant origin – may capture the influence of other correlated factors. Immigrant origin is indeed correlated with multiple sociodemographic variables, as shown in Table 5. Panels (a) and (b) focus on sociodemographic variables observed on the full sample and present summary statistics separately for immigrants and other citizens. We show the difference between the means of the two groups and report the p-value of a test of the null hypothesis that they cannot be distinguished from each other. Immigrants are significantly more likely than others to live in a ZUS, they are slightly younger on average, and they live in buildings where housing prices are lower. Among immigrants, a larger share are males, and they are, of course, more likely to be born outside of the Île-de-France region. Heterogeneous treatment effects along immigrant origin may actually come from differences in these correlated characteristics. For instance, the treatment may have affected all

¹⁵ Table C1 compares the 2010 effects estimated on the full sample to the 2011 effects estimated on the sample of cities in which cantonal elections were held in 2011. In Table C2, we estimate both the 2010 and 2011 effects in the latter subsample to compare effect size across time while controlling for geographical differences. Once again, we cannot reject the hypothesis that the effects were equal in both 2010 and 2011.

Table 5

Differences between the Characteristics of Immigrants and Non-immigrants

	Non-immigrants		Immigrants		P-value native-borns = immigrants	Number of observations
	Mean	SD	Mean	SD		
Panel (a): Building characteristics						
Based in a ZUS	0.296	0.456	0.476	0.499	0.000	23,760
Housing price	3,594	1,456	3,010	1,208	0.000	23,760
Distance to the polling station	0.273	0.243	0.263	0.250	0.188	23,760
Panel (b): Individual characteristics (voter rolls, whole sample)						
Male	0.437	0.496	0.497	0.500	0.000	23,760
Age	44.4	18.3	43.6	16.6	0.001	23,760
Born in Île-de-France	0.634	0.482	0.222	0.416	0.000	23,760
Panel (c): Individual characteristics (post-electoral survey)						
Education						
No diploma	0.116	0.320	0.194	0.396	0.003	816
Primary school certificate	0.052	0.223	0.027	0.161	0.093	816
Middle school certificate	0.058	0.234	0.065	0.246	0.703	816
Professional qualification	0.224	0.417	0.148	0.356	0.011	816
End-of-high-school diploma	0.228	0.420	0.266	0.443	0.231	816
Short higher education diploma	0.154	0.361	0.144	0.352	0.731	816
Long higher education diploma	0.168	0.374	0.156	0.363	0.658	816
Employment status						
Employed worker	0.575	0.495	0.612	0.488	0.322	803
Unemployed worker	0.086	0.280	0.110	0.313	0.275	803
Student	0.075	0.263	0.114	0.318	0.068	803
Retired worker	0.212	0.409	0.090	0.287	0.000	803
Other inactivity	0.053	0.224	0.075	0.263	0.229	803
Socioprofessional category						
Category 1 (farmers)	0.004	0.061	0.000	0.000	0.339	782
Category 2 (craftsmen, retail traders)	0.020	0.142	0.037	0.188	0.181	782
Category 3 (executives)	0.099	0.299	0.073	0.261	0.254	782
Category 4 (middle-tier professions)	0.238	0.426	0.212	0.410	0.421	782
Category 5 (office workers)	0.263	0.440	0.310	0.464	0.167	782
Category 6 (labourers)	0.050	0.219	0.094	0.292	0.020	782
Category 7 (retired workers)	0.216	0.412	0.094	0.292	0.000	782
Category 8 (other inactivity)	0.110	0.313	0.180	0.385	0.007	782
Panel (d): Knowledgeability and partisanship (post-electoral survey)						
Political information						
Knows that he/she is registered	0.845	0.363	0.796	0.404	0.196	456
Knows who was elected pres. of the region	0.517	0.501	0.443	0.499	0.148	440
Able to cite responsibilities of the region	0.332	0.472	0.281	0.451	0.278	440
Overall information index	0.000	0.708	-0.132	0.710	0.062	458
Political preferences						
Voted for PS at first round	0.459	0.501	0.500	0.506	0.662	127
Voted for PS at second round	0.727	0.448	0.900	0.304	0.024	139

Notes. For each variable, we report the means and standard deviations both for non-immigrants and for immigrants and indicate the p-value of the difference. Standard errors are adjusted for clustering at the building level in panel (a). In panel (d), the sample is restricted to individuals in the control group.

Table 6
Impact of the Visits on Participation in the 2010 Regional Elections, Allowing for Heterogeneous Treatment Effects along Other Individual Dimensions than Immigrant

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Heterogeneous effects allowed along	Gender	Age	Île-de-France	Based in a ZUS	Housing price	Distance to the polling station	All	((2)-(7))
Panel (a): First round								
Immigrant × Visited	0.044*** (0.017)	0.043** (0.017)	0.044*** (0.017)	0.042** (0.019)	0.041** (0.017)	0.045*** (0.017)	0.044** (0.017)	0.040*** (0.019)
Strata fixed effects	x	x	x	x	x	x	x	x
Building and individual controls	x	x	x	x	x	x	x	x
Observations	23,760	23,760	23,760	23,760	23,760	23,760	23,760	23,760
R ²	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10
Panel (b): Second round								
Immigrant × Visited	0.036*** (0.018)	0.034* (0.018)	0.036*** (0.018)	0.035* (0.020)	0.035* (0.018)	0.038*** (0.018)	0.036*** (0.018)	0.034* (0.020)
Strata fixed effects	x	x	x	x	x	x	x	x
Building and individual controls	x	x	x	x	x	x	x	x
Observations	23,760	23,760	23,760	23,760	23,760	23,760	23,760	23,760
R ²	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10

Notes: The unit of observation is the individual. Standard errors are adjusted for clustering at the building level and reported in parentheses. We estimate the impact of the visits for immigrants and non-immigrants separately and allow for heterogeneous treatment effects along other dimensions: gender, age, whether the individual was born in Île-de-France, housing price, distance to the polling station, whether the building is located in a ZUS and, in column (8), all these variables. Each of these variables is interacted with Visited and the interaction is instrumented by the interaction of the same variable with Treatment. Building controls include: housing price, distance to the polling station and whether it is located in a ZUS. Individual controls include: age, age², gender and whether the individual was born in Île-de-France. ***, **, * indicate significance at 1%, 5% and 10%.

citizens born outside of the region, not just immigrants, by helping them to bridge a knowledge gap. Alternatively, what mattered was perhaps not being of immigrant origin, but standard of living, proxied by housing price.

To disentangle the influence of immigrant origin on treatment impact from the influence of correlated variables, we allow for heterogeneity in the treatment effects by other dimensions than place of birth. Specifically, we include interaction terms between the treatment indicator and each of these variables in a unique regression:

$$Y_{i,b} = \alpha_4 + \beta_4 \text{Visited}_b + \theta_4 I_{i,b} + \lambda_4 \text{Visited}_b \times I_{i,b} + \mathbf{W}'_{i,b} \rho_4 + \text{Visited}_b \times \mathbf{W}'_{i,b} \tau_4 + \mathbf{X}'_b \gamma_4 + \mathbf{Z}'_{i,b} \delta_4 + \epsilon_{i,b}, \quad (3)$$

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

In columns (2)–(7) of Table 6, panel (a), we allow the treatment effect in the first round of the 2010 regional elections to be heterogeneous along the following dimensions successively: gender, age, born in the region Île-de-France, ZUS, housing price and distance to the polling station. The differential effect obtained on immigrants is remarkably consistent across all specifications, both in magnitude (between 4.1 and 4.5 percentage points) and in statistical significance (at the 5% or 1% level). In column (8), we allow for heterogeneity by all these dimensions simultaneously, and measure a consistent differential effect of 4.0 percentage points, significant at the 5% level. The estimate of the differential effect of canvassing on the participation of immigrants in the second round is also consistent with allowing for heterogeneity of the treatment effect along these dimensions, as shown in Table 6, panel (b). In all specifications, the estimate of this differential effect lies between 3.4 and 3.8 percentage points, and it is significant at the 5% or 10% level.

While these results suggest that immigrant origin is the key factor responsible for treatment effect heterogeneity, we cannot exclude that other sociodemographic factors unobserved on the full sample, but correlated with immigrant status, contributed to it. In particular, education, employment status and occupation are commonly considered important determinants of voter turnout levels (Campbell *et al.*, 1960; Rosenstone and Hansen, 1993). We collected these variables on the subsample of respondents to the post-electoral survey. Panel (c) of Table 5 shows differences between immigrants and other citizens for values taken by the three variables. We also run chi-square tests of homogeneity to determine whether immigrants and non-immigrants have the same distribution of each of these variables. Table D1 in online Appendix D reports the test statistics and p-values of Pearson's chi-squared test and the likelihood-ratio chi-square test. We reject the hypothesis that immigrants and non-immigrants have the same distribution of socioprofessional categories, education levels and employment status at the 1% level: in particular, as shown in Table 5, immigrants are more likely to have no diploma and to be labourers, and they are less likely to be retired workers.

To check whether the effect of the visits on immigrants is driven by these correlated characteristics, we first focus on respondents to the post-electoral survey to control for

treatment effect heterogeneity along these factors measured at the individual level. The results are shown in online Appendix E (Table E2). The visits had a significantly larger impact on immigrants in the subsample of survey respondents as well (column (1)), and this effect is once again consistent in magnitude across specifications, and statistically significant in all (at the 1%, 5% or 10% level). Allowing for heterogeneity of the treatment effect along all sociodemographic factors measured in the post-electoral survey, as well as those that we observe on the full sample and that we consider in Table 6, we measure a consistent differential effect on the participation of immigrants. This is significant at the 1% level in the first round and at the 5% level in the second round (column (6)).¹⁶

Second, while education, employment status and occupation are only measured at the individual level for respondents to the post-electoral survey, we were able to collect census and fiscal data on these and other factors for our full sample at the fine geographical levels of census blocks and squares of 200 by 200 metres, as described in subsection 1.4. Differences between immigrants and non-immigrants found using these aggregated data are broadly consistent with those present in the post-electoral survey: as shown in Table D2 (in online Appendix D), on average, immigrants live in blocks containing higher fractions of people with no diploma, students, unemployed workers, labourers and office workers; and lower fractions of middle-tier professions, executives and retired workers, consistent with individual differences between immigrants and non-immigrants shown in Table 5, panel (b). Tables F1–F8, in online Appendix F, control successively for the interaction of treatment with variables from census data on education, economic activity, household size and structure, housing and socioprofessional category, variables on income components and distribution from fiscal data, and variables from the 200 metres gridded data, using specifications of the form in (2). The effect on immigrants is consistent in magnitude and significance across all specifications. When allowing for heterogeneity of the treatment effect along all the 80 census block and 200 metres gridded variables as well as the six individual and building level variables considered in Table 6, we find differential effects of 5.0 and 3.7 percentage points at the first and second rounds, significant at the 5% and 10% level (Table F8, column (9)).

These results reinforce our interpretation that the estimated influence of being an immigrant on the effect of the treatment does not merely capture the influence of correlated sociodemographic factors. To investigate possible mechanisms underlying this differential treatment effect, we now discuss whether differences in political knowledgeability and partisanship may have contributed to it.

¹⁶ Similar to our results in the full sample, the effect of the visits among survey respondents was close to zero overall, negative among non-immigrants and positive among immigrants (Table E1 in online Appendix E). The magnitude of the effects on non-immigrants and immigrants is substantially larger than on the full sample, however. In the specification with individual and building controls, the effect on non-immigrants is not significant in the first round and averaged over both rounds, but it is significant at the 10% level in the second round and the effect on immigrants is significant at the 5% level in the first round and averaged over both rounds but not in the second round. Given the small sample size and the related low statistical power, these results and their difference with the main sample should be interpreted with caution.

3.2. *Knowledgeability and Partisanship*

3.2.1. *Provision of information*

An important part of the canvassers' visits was to provide information about the date of the election, the location and opening times of the polling station, and the political agenda of their candidate. Information on the whereabouts of the election can significantly reduce the logistical costs of voting (Wolfinger *et al.*, 2005), while information on political platforms can help formulate political opinions, reducing the cognitive costs of becoming engaged with and informed about the political world, which may be an even greater barrier to voter turnout (Berinsky, 2005). In the present study, the larger impact of the visits on immigrants may simply come from the fact that the information provided by the canvassers was more impactful for them, as they were originally less informed about French politics and about the regional elections as a result of being born in a foreign country, for the most part, and having fewer contacts with other citizens on average than native-borns do.

Panel (d) of Table 5 compares levels of political information among immigrants and other citizens along several dimensions, measured in the post-electoral survey: whether the respondent knows that she is registered,¹⁷ who was elected as the president of the region and what the responsibilities of the regional council are (in terms of programmes overseen). We restrict the sample to citizens in the control group to avoid contaminating the estimation with the effect of the treatment. Immigrants are less informed on all three dimensions, although these differences, while sizable, are not statistically significant. To summarise all differences in a unique measure, we then group all four outcomes into a global index, defined to be the equally weighted average of the z-scores of its components, following Kling *et al.* (2007).¹⁸ The difference between immigrants and non-immigrants is 0.13 standard deviation on average and it is significant at the 10% level. This gives some empirical support to the interpretation that the larger impact of the visits on immigrants came from the fact that these had less information.

We then investigate whether the visits actually increased information by using specifications of the form in (1) and (2), where we replace turnout by information as the outcome $Y_{i,b}$. The results are shown in online Appendix A (Table A2). Both the overall impact (panel (a)) and the impact on non-immigrants (panel (b)) are small and non-significant. The impact on immigrants' informedness is larger, suggesting that simple information on voter registration status and eligibility to participate as well as on the candidates and the election helped increase immigrants' voter turnout. However, due to the small sample size and related low statistical power, the effects are non-statistically significant, precluding any firm conclusion on the impact of the visits on political informedness.

¹⁷ We asked each respondent whether he or she was registered and compare their answers to actual status, which is registered for all, since we only surveyed citizens registered on the voter rolls.

¹⁸ The z-scores are calculated by subtracting the mean among non-immigrants in the control group and dividing by the standard deviation among them. Some outcomes are missing for some citizens, who used their right to refuse to answer. Following Kling *et al.* (2007), if an individual's response is known for at least one of the four outcomes, then any missing values for the other outcomes 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.

3.2.2. *Political closeness to the PS*

A complementary explanation for the heterogeneous impact of the visits is that the discussions with the canvassers resonated differently for those with political views closest to the ideological platform of the PS. Like other European progressive parties, the PS is perceived as more sensitive to immigrants' interests than right-wing parties (Givens and Luedtke, 2005). It promotes immigrant naturalisation, anti-discrimination policies and the right to vote in local elections for non-naturalised immigrants. Unsurprisingly, then, immigrants in France generally lean more to the left, ideologically (Brouard and Tiberj, 2011). Self-reported voter choice measured in the post-electoral survey suggests that this is true in the sample, as well. While a large majority of citizens in the sample are on the left, immigrants who voted and disclosed whom they voted for were even more likely to vote for the PS than non-immigrants (Table 5, panel *(d)*). At the second round, the difference was particularly large (17 percentage points) and significant at the 5% level. Immigrants' stronger ideological closeness to the PS may have contributed to the differential impact of the visits.

4. Conclusion

This article examines whether voter outreach efforts can successfully increase the participation of immigrants, and whether the effect on immigrants is larger or smaller than on other citizens. In the four weeks leading up to the French 2010 regional elections, members of the PS canvassed eight cities of the region surrounding Paris to encourage citizens to vote. The 23,773 citizens in the sample included immigrants from over 100 different countries in Maghreb, Sub-Saharan Africa and Asia, as well as native-born citizens. The door-to-door campaign targeted neither any one group nor immigrants as a whole. Initially, immigrants' turnout level did not greatly differ from native-born citizens. However, the door-to-door visits increased immigrant turnout by 3.4 percentage points in the first round and 2.8 percentage points in the second, without having any statistically significant effect on non-immigrants. The effect in the cantonal elections that were organised one year later, while still positive, was smaller and non-significant. These results are robust to controlling for treatment effect heterogeneity along other sociodemographic variables.

What makes our results particularly striking is that the economic scarcity and lack of immigrant electoral power that characterise the French suburbs have typically generated conflicts between immigrant communities and the state (Lagrange and Oberti, 2006; Duprez, 2009; Dancygier, 2010). The large impact of the visits suggests that current tensions, rather than dampening the mobilisation of immigrants, actually may have made the intervention particularly noticeable and impactful. Many citizens of immigrant origin share a feeling of being stigmatised and rejected by the mainstream population: in France, 59% of immigrants believe that French society does not give people of different origin the means to integrate (Brouard and Tiberj, 2011) and 45% feel that natives do not regard them as French (Simon, 2012). Immigrants may have seen the canvassers' visit as a break with everyday experience – a signal that they were perceived and treated as full citizens whose votes mattered. This effect – together with immigrants' lower baseline level of information about the elections and their political views closer to the PS, measured by our survey – could explain why the

impact of the visits was significantly larger among immigrant citizens than the mainstream population.

This study shows that voter outreach efforts do not need to be tailored to any community or target any one group to successfully increase the participation of citizens of immigrant origin. This result is particularly relevant for countries with an assimilationist model of integration, where citizens are expected to overcome cultural and ethnic divides. In contrast to the idea that immigrants are unwilling to advance their integration in the receiving societies, our findings suggest that proactively extending a hand to them could complement existing policies and advance the integration of new arrivals. Future research will hopefully identify additional ways in which political parties, non-partisan organisations and public institutions can use direct and personal contacts to improve immigrants' integration and participation. A one-time visit in the context of an election, with effects decaying over time, will obviously not suffice.

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Accepted: 19 November 2017

Additional Supporting Information may be found in the online version of this article:

Appendix A. Additional Impact Tables.

Appendix B. Additional Balance Checks.

Appendix C. Seemingly Unrelated Regressions.

Appendix D. Differences Between the Characteristics of Immigrants and Non-immigrants on Additional Dimensions.

Appendix E. Impact and Heterogeneity Analysis on the Subsample of Respondents to the Postelectoral Survey.

Appendix F. Allowing for Heterogeneous Treatment Effects along Aggregate Variables.

Appendix G. Clustered Standard Errors.

Appendix H. Figures.

Data S1.

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