Targeting voter registration with incentives: A randomized controlled trial of a lottery in a London borough

Abstract

Does an incentive—in the form of a lottery—increase voter registration, particularly among poorer members of society? In the summer of 2012, two groups of 20,000 randomly selected households from a London Borough were informed that they would be placed into a prize draw if they registered to vote by 28 September 2012. One group was offered £5,000 and the other £1,000 while the remaining households in the borough received the standard letter with the same deadline but no lottery offer. The registration rates after the deadline were 46.2 per cent in the £1,000 group and 46.6 per cent in the £1,000 group compared to 44.7 in the control. Levels of registration in the two treatment groups are statistically different to the control, but not from each other. Households in poorer locations are more receptive of the lottery offer whereas there is no effect in places where richer households live. After the face-to-face canvass of all remaining non-registered households, which took place after the experiment, there were no statistically significant differences between the treatment groups and the control. The study builds on the work of Raja and Schaffner (2012) and Panagopoulos (2013) by finding that incentives work in different ways depending on their level and who is targeted.

Can lotteries motivate pro-social behaviors like voter registration and if so for whom? Public authorities for some time now have deployed financial incentives to encourage citizens to change their behaviours, for example to give up smoking tobacco or to take up exercise. The rationale is that society can benefit by more than the cost of providing the incentive. Untried so far have been incentives to foster civic behaviours, such as voter registration. Normally, public authorities make appeals to civic duty or in the case of voter registration and voter turnout (in some countries) invoke the sanction of law. But in certain circumstances it might be appropriate to consider incentives as a complement to existing approaches. Given the positive effects of Get Out The Vote (GOTV) interventions usually accrue to those who are already pre-disposed to participate (Arceneaux and Nickerson 2009), they tend to bypass a hard-core of non-participants who have a low propensity to vote. An incentive, like a lottery, might appeal to those who are not reached by other forms of mobilization. A differential effect of lotteries with small rewards has already been identified in a survey experiment on intentions to vote (La Raja and Schaffner 2012), but so far there has been no test of an intervention carried out by a public authorities in the field.

To test the claim that lotteries target poorer voters, two randomly selected groups of 20,000 households in a London Borough were entered into a lottery if they registered to vote by 28 September 2012. One group got an offer of £1,000 and the other £5,000 while the remaining households in the borough were placed into the control group. Both intervention groups were more likely to register to vote then those in the control but there is no statistical difference between the £1,000 and £5,000 offers suggesting the level of the incentive does

not matter in this range. As expected the treatment effect is stronger in locations with low-income groups while there was no impact in high-income locations. The effect, however, did not last beyond further reminder to register to vote: the differences between the treatment and control groups disappear after the remaining unregistered residents were canvassed. Overall, the lottery may have stimulated some residents to register on time.

The structure the paper is as follows. The first section is a review of the literature on incentives and pro-social behaviours. The second is an assessment of the literature on GOTV campaigns, which places incentives alongside other motivations to participate. Third, there is a short review of the literature on voter registration. Fourth is an account of voter registration in the UK. Fifth is a description of the study and its design. The penultimate section contains the report of the results. Finally, conclusions and implications are drawn.

Incentives and pro-social behaviours

An incentive is not the first tool of choice for policy-makers to encourage pro-social behaviours. But the menu of public actions is quite limited when compulsion or regulation is not an option. Education, persuasion, and mentoring are various interventions that can be deployed, but often information-based interventions do not influence groups that are habituated to certain kinds of non-desirable behaviours. With smoking the effects of providing incentives are quite modest and short-term (Cahill and Perera 2008), though there are strong effects from using incentives to ensure attendance at schools, which work for some students, such as for the PROGRESSA scheme in Mexico where the payments were quite large (Behrman et al 2005). Gneezy et al's (2011) review of

studies in education, contributions to public goods and changing lifestyles, such as stopping smoking and taking exercise, does not provide unqualified support for the use of incentives.

Incentives can also work in the opposite way. Most notably, Frey (1997) argues that they crowd out pro-social motivation. So the provision of an incentive may cause people to carry out a socially beneficial action less because it has been converted into monetary form, which may be compared to the costs of carrying out the action. This recalls Titmuss's (1970) who concluded that people donate blood because it is free rather than paid for. There may be psychological costs in responding to money (Bénabou and Tirole 2003, 2006) because people do not want to be paid for activities that they provide freely. For example, Gneezy and Rustichini (2000b) find that children collect less money for charity when they are paid. Nonetheless, studies also point in opposite direction, for example showing that paying for blood donations can increase the total level donated (Lacetera et al 2009; Lacetera and Macis 2010). Small incentives can highlight the pro-social nature of the act being promulgated, encouraging citizens to carry out what they would ideally like to do. For example, charging for plastic bags in supermarkets can help people realise their aim of becoming environmental citizens (Disney et al 2013).

Voter mobilization and incentives

There has been a large amount of research on voter mobilization in recent decades using experimental methods. Such research does go back to the 1920s (Gosnell 1926, also see also Eldersveld 1956). More recent studies use a variety of techniques from door-knocking, telephoning and leafleting, which obtain

significant and consistent treatment effects depending on the mode of intervention (see Green and Gerber 2008, Green et al 2012). These interventions often remind citizens of their civic duty thus attracting those who already have a predisposition to vote.

Complementing these psychological mechanisms, it is possible to argue there is a cost-benefit element to voter mobilisation, which has to do with the provision of information that can reduce the costs (Downs 1957). Similarly, it may be possible to increase participation by providing benefits (see Panagopoulos 2013: 267, 270-271). Indeed, in the past political parties and other organisations gave benefits, such as cash, food, alcohol, health care and poverty relief, to voters to encourage them to turn out (see Stokes et al 2012). There have been some recent interventions carried out by companies and charities. For example, Starbucks which wanted to give a free coffee to anyone who voted in the 2008 US elections (Davis 2008). However, this scheme was believed to be against the law so the company decided to offer anyone a free tall cup to anyone who asked for one. However, there are relatively few tests of the use of incentives in the voter mobilisation field. One field study that affected the costs and benefits of voting was a turnout festival (Addonizio et al 2007). In the election-day party, potential voters were encouraged to vote, receiving benefits such as free food. It is not possible to use this study as evidence for the effects of incentives because the festival probably activated a norm to encourage voting.

Closer to the intervention in this paper is the study by La Raja and Schaffner (2012) who test for the effect of a lottery using the 2011 Cooperative Congressional Election Study based on a survey of about 1,000 adults. They argue that incentives are similar to a nudge in behavoural economics, and that

even a small incentive can increase voter turnout (even as low as a dollar). Their survey experiment varied scenarios of different prizes in a lottery and sought to find out how likely they were to vote in the next election. Important for this current study, they suggest that low income voters are more attracted the stimulus and they find that those with incomes under $30,000 are more likely to respond to the treatment with no other significant variations.

The most important study examining incentives is by Panagopoulos (2013). He carried out a pilot in 2007 for the municipal elections in Gilroy, and a larger scale study in April 2010. Voters in each experiment were randomly assigned to receive a postcard mailing with either an encouragement to vote or, in addition, an offer to receive a financial reward of $2, $10, or $25 for voting, with a control group that did not receive a letter. He finds that nominal incentives have no effect on voting, but that higher amounts do, suggesting that incentives affect voter behaviour but need some threshold level to work. A further study of voter turnout is Shineman (2012) who included incentives alongside a mobilisation treatment as each subject was provided with a prepaid $25 Visa gift card. This produces a striking result: a 33.3 percentage point increase in turnout. But it is not possible to separate the effect of the incentive from other aspects of the mobilisation in this study.

There is good reason to believe that incentive-style interventions target particular groups. Incentives might appeal to low-income groups as the incentive would be less trivial representing a high proportion of their total resources if obtained making a greater contribution to marginal utility than for those with high incomes. Analyses of UK data show that lower income groups play the state lottery more than other groups (Farrell and Walker 1999). Yet it is hard to work

out whether incentives alone stimulate interest in playing lotteries. This might be particularly the case where there are low odds of winning. Lower educated groups might be inclined to overweigh the probability of winning because of the difficulty of calculating the odds (though such mistakes are not confined to the poorly educated—see Kahneman 2011). The benefit might come from participating in the draw rather the prize and its level. Lower income groups are more used to playing lotteries so are familiar with the request and meeting a deadline for entry. Peer groups among the poor reinforce this (Beckert and Luter 2012). There may be some intrinsic benefits to playing a lottery that meshes well with extrinsic benefits of the act. The reward is desired partly because of the collective experience of playing the lottery with peers and a wider group. Hence, in spite of the low odds, playing lotteries remains popular in most countries.

Finally, lotteries are used by survey companies to improve response rates, and also are a means by which charities raise funds. It is important to acknowledge that in survey experiments fixed payments do much better than lottery asks (e.g. Halpern et al 2011), though this might because filling a survey out is not seen as a civic duty.

Voter registration

Research on voter registration is less extensive than for turnout. There are many hundreds of field experiments on turnout (see reviews in Green and Gerber 2008, Green et al 2012). Nevertheless, voter registration was the object of one of the foundational experiments in the US by Gosnell (1926, 1927). The other main examples are Nickerson (2007) who e-mailed students in eight US universities in five states, some 232,716 subjects, to encourage them to register and vote;

Bennion and Nickerson (2010) who e-mailed 259,130 students drawn from twenty-six universities; and Braconnier et al (2013) who randomly allocated 20,500 households in France to receive visits during the 2012 presidential and parliamentary elections, offering information or provision to register at home, showing positive effects, particularly for the latter. In spite of the small number of studies of registration, it appears that responses to GOTV-style forms of mobilization mirror that of turnout in that the interventions work, and that more personalized forms of contact involving home visits are more efficacious than more impersonal interventions.

It is less clear who benefits from these interventions. In Arceneaux and Nickerson's study (2009), Get Out the Vote mobilisations were likely to have the strongest effects among those who had some prior likelihood of voting, but not at the lowest level. These low probability groups of voters are likely be of low social-economic status. On the other hand, Nickerson (2014) also shows that voter registration drives have some effect on households in poorer streets. He finds that GOTV efforts can be targeted on less affluent members of society, though in this case the effect was counterbalanced by an increase in turnout of newly registered voters on the affluent streets.

Observational studies of political participation also show that registration has similar features to voter turnout in that more affluent groups are more likely to be registered, with lower SES and more marginalised groups being less likely. This effect can be countered by the legal requirements to get registered. In practice, these laws are not enforced in some countries, such as the UK, where only one local authority enforces voter registration. Even compulsory voting systems like Australia, the decision to register is regarded permissively and is a

venue where citizens can exit from electoral participation without cost. Given the bias of participation and similar forms of mobilisation, it seems reasonable to expect that the techniques of a Get Out the Vote campaign would reflect the bias of participation by appealing to groups that are likely to register to vote in the first place.

The observational literature places a lot of emphasis on the costs of registration caused by voter registration requirements (more prevalent in the US), in particular on how those costs may be skewed and fall on lower socio-economic status groups. There is a debate about the extent to which costs apply, but not their salience (Highton 1997). The costs of voter registration are usually hard to alter given the legal regulation of registration; however it might be possible to alter the benefits to registration that might be weighed by the individual against those costs.

Voter registration in the United Kingdom

The administrative task of registration is assigned to the district local authority, which usually has an Electoral Services unit. Each year in February the council publishes a revised Register of Electors (or Voters Roll). UK households were required to fill out a form in the summer of each year to indicate whether its members are eligible to vote in any election (e.g. national, local, European Parliament, Scottish Government etc.). As well a registering themselves, respondents were also required to indicate who else in the household is eligible to vote and had to return the form by a due date at the end of September. Those who are already on the register the previous year may renew by telephone or on a website instead of returning the form by post. Once the date has passed for the

submission of the registration form, councils employ door-to-door canvassers who call on those who have not registered, which improve the numbers. About ninety per cent of the population are registered, but participation is lower for minority and low-income groups who tend to live in urban areas like London (the site for the present study). Enforcement is the responsibility of the local authority and households face fines if they do not register (these are highlighted on the form). In practice, local authorities do not often use these powers.

In 2014, after the research for this paper, the UK government moved to individual election registration (IER) following its introduction in Northern Ireland in 2002. Each individual elector now receives a communication that formerly went to the household.

The intervention

The experiment was carried out by a local council in London as part of its electoral registration cycle that started in the summer of 2012. The borough has an ethnically diverse and youthful population.

On 24 July 2012 the council drew a usable sample of 129,048 households, which it needed to do each year to update the electoral register. The council asked its data management company Xpress Software Solutions to select randomly two groups of 20,000 each from this population and left the rest of the households in the borough as the control group. In one treatment group households were invited to return their electoral registration by 28 September. They were told that if they returned it by this date or registered in another way

they would be entered into a lottery of £5,000.[1] Households in the other treatment group were offered the chance of a lottery of £1,000 in exactly the same way. The wording was 'You and your household could win [£5,000 or £1,000] if this form is returned before 28 September 2012', which was placed in a prominent position on the form, in capital letters, and above where the householder fills out the names of the eligible voters. In the letter there was a separate document that directly invited the residents to participate in the lottery. The outside of the envelope also contained a highlighted starburst that contained the words, 'Register to voter for a chance to win [£1,000 or £5,0000]' in capital letters. The other households received an invitation to register to vote by the same deadline, using the same design and form, but without the invitation to participate in a lottery (neither having the extra text on the form itself, nor the enclosed invitation, nor the starburst advert on the outside of the envelope). Letters were sent out by Royal Mail, using FDM Mail, over period of four days from 20 August. The final draw for the lottery was for £5,000 and was drawn from all residents in the borough who registered irrespective of assigned group and was drawn and won by one household on 17 December 2012.[2] The initial

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[1] Copies of the treatment letters are available from the lead author.

[2] Contagion between treated and non-treated subjects is a concern in a geographically concentrated trial like the current study. Although letters go separately to households and are opened in private, there remains the possibility that neighbours talked to each other about the lottery offer. In the hope of getting the lottery, they may have registered by the due date, earlier than they would have done normally, which would have increased early registration in the control group and understated the treatment effect. However, even if neighbours had become aware of the lottery, when they examined their own registration form they would have seen no such offer even though if they did return the form on time they would be entered. Of course, not getting a lottery offer when they knew that their neighbours were being offered one might have had the opposite effect: deterring registration because householders were frustrated at not being offered a benefit. The council was concerned about cross-contamination and set

registration period lasted until 28 September 2012. The council provided data on whether and when respondents registered themselves and other household members to vote up to the cut off period so as to find out whether those households who were part of a lottery were more inclined to register to vote.

For reasons of data protection there were not many household-level covariates available as they may have identified individual residents. The only one was the type of property of the household, which is broken down into forty-three categories. The largest categories are flats (96,623), terraced houses (22,193) and detached houses (1,196).

The dataset contains identifiers for the location of the households. This is for the smallest area for which census statistics are held, the Lower Layer Super Output Areas (LSOA).[3] There are 166 of these in the local authority area. The local authority attached these to the registration data for 128,848 cases. The missing cases occur because addresses continually change due to new buildings, changes of use and demolitions. Census level data were attached to explore the varying impact for the treatment, in particular the area deprivation score (index of multiple deprivation, *imdscore*, for 2010), which is derived from a basket of indicators and where a high number indicates a high level of deprivation.[4] These range from 9.93 to 52.01 (median is 31.35): see Figure 1.

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up a communications protocol to deal with concerns from the public. But no one called in or contacted the council, which is partial evidence that there was no effect.

[3]. For a description, see http://www.ons.gov.uk/ons/guide-method/geography/beginner-s-guide/census/super-output-areas--soas-/index.html

[4] Imdscore is made up of seven LSOA level domain indices. These relate to income deprivation, employment deprivation, health deprivation and disability, education skills and training deprivation, barriers to housing and services, living environment deprivation, and crime which reflect the broad

Table 1 reports the results of a test of whether the control and treatment groups are balanced with respect to these proportions. The deprivation score, type of property, and proportion of retired people are balanced, but there a greater proportion of white households in the treatment groups, which is statistically significant. Regression analysis with treatment allocation as the response term and *imdscore*, proportion white and proportion retired, clustered by LSOA, shows white to be significant only for the £1,000 offer. Also regression analysis using percentage white as an additional covariate produces similar results to those reported in Table 3.[5]

The outcome variable is whether registration was completed in the canvass period by the deadline indicated on the form. In addition, it was recorded how the household registered to vote according to the following categories: postal notification of change, postal notification of no change, internet no change, and telephone no change.

Results

The key results are the percentages of registrations received up to the end of October 2012, summarized in Table 2: 46.2 per cent of the £1,000 group returned their form compared to 44.7 in the control, a difference of 1.5 percentage points.[6] This compares to 46.6 per cent of the £5,000 group who

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range of deprivation. For further detail see *The English Indices of Deprivation 2010: Technical Report,* Ref: ISBN 9781409829225.
https://www.gov.uk/government/statistics/english-indices-of-deprivation-2010-technical-report
[5] Regression results are available from the lead author.
[6] If households did not open or see the letter, they were not treated which would make this an underestimate of the effect. However, the treatment message was also placed on the outside of the envelope (see Figure 3) so even if householders

registered, a difference of 1.9 percentage points. A two-sample test of proportions between the treatment and the control groups yields a z-score of -3.7 and -4.7 respectively, both statistically significant at p. > 0.001. The conclusion to draw is that the treatments must have raised household electoral registration after the postal canvass period for both sizes of offer.

The £5,000 lottery offer yields a higher treatment effect at 0.4 percentage points above that for the £1,000, but the two treatments are not statistically different from each other at this sample size. A two-sample test of difference in proportions, not including observations from the control group, yields a z-score of -0.6, which is not statistically significant. For these reasons, it makes sense to take the treatments together as one. When combined and compared to the control group, this yields the percentage of 46.4 per cent who had registered as a result of both lottery offers, a difference of 1.6 percentage points that yields a z score in the two-sample test of proportions of 5.5, significant at p. < 0.001.

There are statistically significant differences for the treatment in the methods of reporting no change in circumstances reported in the bottom panels of Table 2. The available modes are by post (for either a change or no change in circumstances), or by the internet or telephone (to inform there is no change in circumstances)—households can only inform the local council by post if there is change in circumstances, such as new residents living in the property as these changes have to be conveyed by filling out, signing and returning the form. Residents often registered several times using different modes (e.g. returning the form and using the internet) and the council let them do this, which means the

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did not open their registration letter, it is likely that nearly all of them saw the treatment message when they picked up their post.

totals by mode exceed the total registered as shown in the table. The main result is that the treatment effect is conveyed by mail as there respectively a 2.5 and 2.3 percentage-point difference in returning the form for the £1,000 and £5,000 groups. In contrast, there is a statistically significant negative effect for the internet respectively of .6 and .5 percentage points for the two treatments. There is also a negative effect of 1.0 per cent for renewing by telephone for the £1,000 treatment, though only 0.4 percentage points for the £5,000 treatment (not statistically significant). This effect may be consistent with crowding out as richer households have greater access to the internet. However, it is important to be cautious about interpreting the effects of the treatments on these subgroups as the mode of response may be affected by the treatments rather than different kinds of users responding differently.

The findings by mode may support the central expectation of this paper that there should be a differential impact of the treatment according to the income of the household. This effect can be tested at the LSOA level as it can be assessed whether the lottery offer is more attractive in places where low-income residents live. An tabular analysis of the relationship for the combined treatment gives a clear indication of the difference across LSOA areas: for the LSOA areas at less than the median *imdscore* of 31.35 (the wealthy areas) there is no effect with registrations at 49.0 per cent compared to control at 48.7, a difference of 0.3 percentage points which is not statistically significant (z=-0.8); but for the more deprived areas above the median there are 44.2 per cent of registrations compared to 41.6 per cent in the control, a 2.6 percentage point difference (significant at z=-6.5).

Probit regressions, with the combined treatment, the deprivation covariate and an interaction term of the treatment and the covariate as independent terms, can also appraise the differential impact of the treatment according to deprivation. The results, with clustered standard errors on LSOA, are reported in Table 3. Model 1 is the baseline showing the impact of the combined treatment. Model 2 shows the impact of deprivation, which is positive and significant. The interaction of the combined treatment and *imdscore* is also positive and significant, showing the greater impact of the treatment in the more deprived areas. The treatment variable has a negative sign, which is also significant. Model 3 tests whether the gradient for the interaction terms is the same for the £1,000 and £5,000 lottery offers. Instead of the combined treatment variable generating the interactions, this model includes interactions of *imdscore* and each lottery treatment. There is no difference in these coefficients suggesting the level of the lottery offer did not differentially affect low or high-income areas. This again guards against thinking that the level of the incentive is important at these levels of lottery offer, even for those living in poorer areas.

To explore the differential effect of the treatment further, we report marginal changes in the proportion of registered voters by level of *imdscore*, which can be observed in Figure 2. As expected, the slope is positive with greater treatment effects at higher levels of deprivation. Though it is a plausible conjecture that that there is a negative effect at lower deprivation levels indicating crowding out of pro-social motivation and is suggested by the values at less than zero at these levels, the confidence intervals show that it is not possible to rule out the hypothesis of no effect. In contrast, there is a statistically significant effect in high deprivation areas.

After the deadline of the submission of the forms, the council canvassed the remaining non-registered households, which took place before the production of the final register. It is an opportunity to find out whether the impact of the lottery is sustained over time. Because of changing properties in the borough the total sample size is 128,195 but essentially contains the same properties and households. In this period, registration reaches over 80 per cent. However there is no impact of the treatment, either for the £1,000 or the £5,000 offer, or for the combined effect. We find that 82.4 per cent of the households in the combined treatment groups were registered compared to 82.3 in the control, but this difference is not statistically significant (z=1.1). There appears to be a small effect for the £5,000 offer at 0.4 percentage points greater than the control, but this is a statistically insignificant difference. It appears that the canvassing had reduced the differences between control and treatment groups as households who were mobilized to register probably would have done so anyway later when canvassed door-to-door.

Conclusions

The research for this paper has sought to find out whether the offer of entry into a lottery encourages households to register to vote by a due date, which has not been evaluated in a field experiment before. The case study is a London Borough, which has the responsibility of compiling an annual register of electors, and a field experiment was conducted in 2012 to evaluate two lottery offers of £1,000 and £5,000 for the initial postal canvass of electors. It is of course important to acknowledge that the lottery in this study is relatively modest. Even though the sums are non-trivial, they are small by lottery standards and of course the

probability of winning of 1/128,000 is very low. Not that low probabilities stop people playing lotteries, but the combination of a low probability of winning and a small prize may have only offered a weak incentive in this case. Nevertheless 1.6 percentage difference (about a four per cent increase) in those households who registered before the September 28 deadline is still a non-trivial uplift. On the other hand, there is no apparent difference in impact between the £1,000 and £5,000 offers, which suggests the level of financial incentive did not operate in this case, or at least was not sharp enough to operate at these levels—it was enough just to have the lottery. It may have been the case that the residents of the borough found the lottery increased their interest in electoral registration rather than responding to the expectation of financial gain, so that the level of the offer did not make a difference or that the incentive is flat at these relatively low levels. In any case from the local authority's point of view, it is gratifying that the cheaper option had a very similar effect as the more expensive one (though the two offers cost the same in this case).

In support of a key expectation of this paper, the treatment had more impact in the poorer areas, which is consistent the operation of the incentive though it may be that those living in poorer areas may have had more experience of playing lotteries so were more predisposed to respond. There was no difference in the effect of deprivation for either the £1,000 or the £5,000 offer, suggesting the level of incentive did not affect the groups who were most inclined to respond to the lottery. In contrast, the lottery had no effect on—or may even have deterred—households in the richer areas indicating that incentives do not operate or there may have been crowding out among some of these households.

The effect was short term in that just over 80 per cent of all groups registered after the door-to-door canvas: there was no treatment effect for the final levels of electoral registration. This suggests that the lottery did not differentially affect the registration of the hardest-to-reach groups: that is those households who usually do not register—either in response to the postal canvass or door-to-door. It ensured that those who normally registered when canvassed face-to-face took the plunge a bit earlier when they were offered the lottery. The power of the face-to-face contact on all those who got canvassed, or the implied threat of sanctions if they did not register after being canvassed, eliminated the treatment effect.

Overall, lotteries have advantages as a means to get households to register to vote. They can raise registration overall, but appeal to the less well off who are less likely to register without an incentive. They are relatively easy to administer. But lotteries have no effect for some groups and their effects do not last when followed by more direct means of contact, the face-to-face canvas. Future experiments could vary the level of the lottery and make offers at different stages of registration. It may be the case the lottery has an effect if offered during canvassing and this would be sustained in actual registration figures and could target hard-to-reach households.

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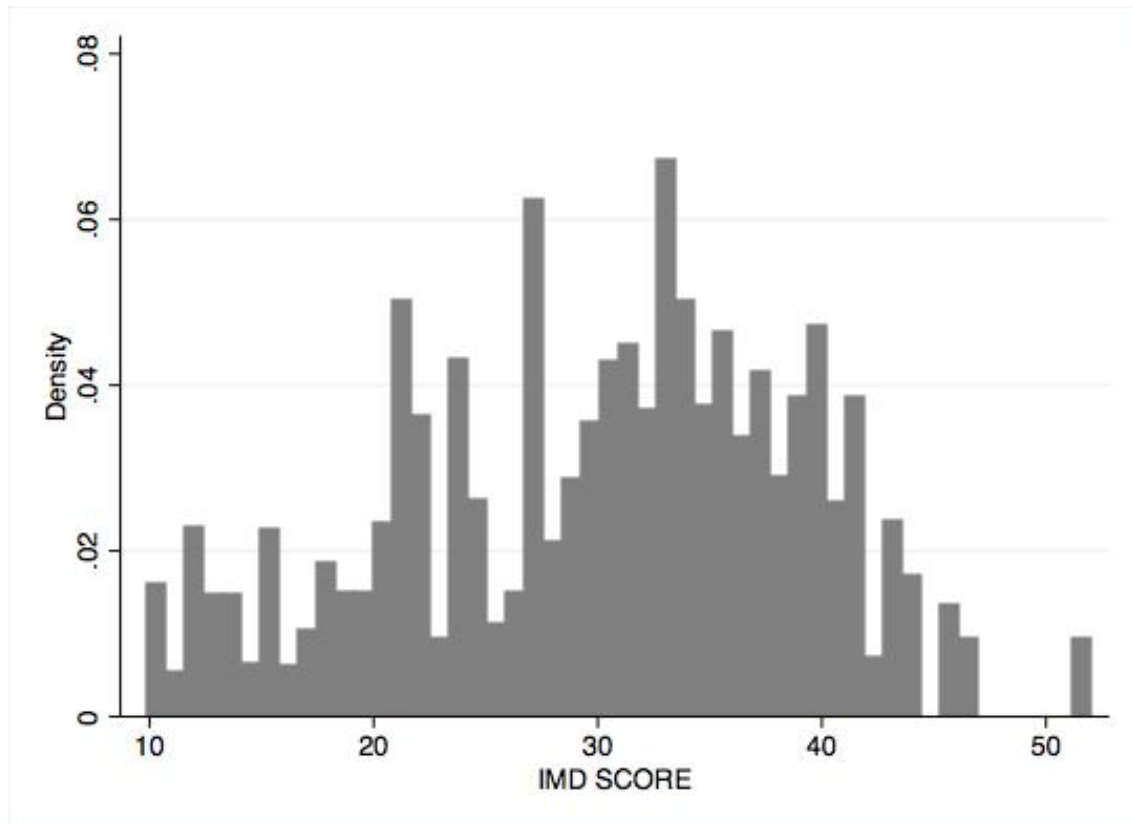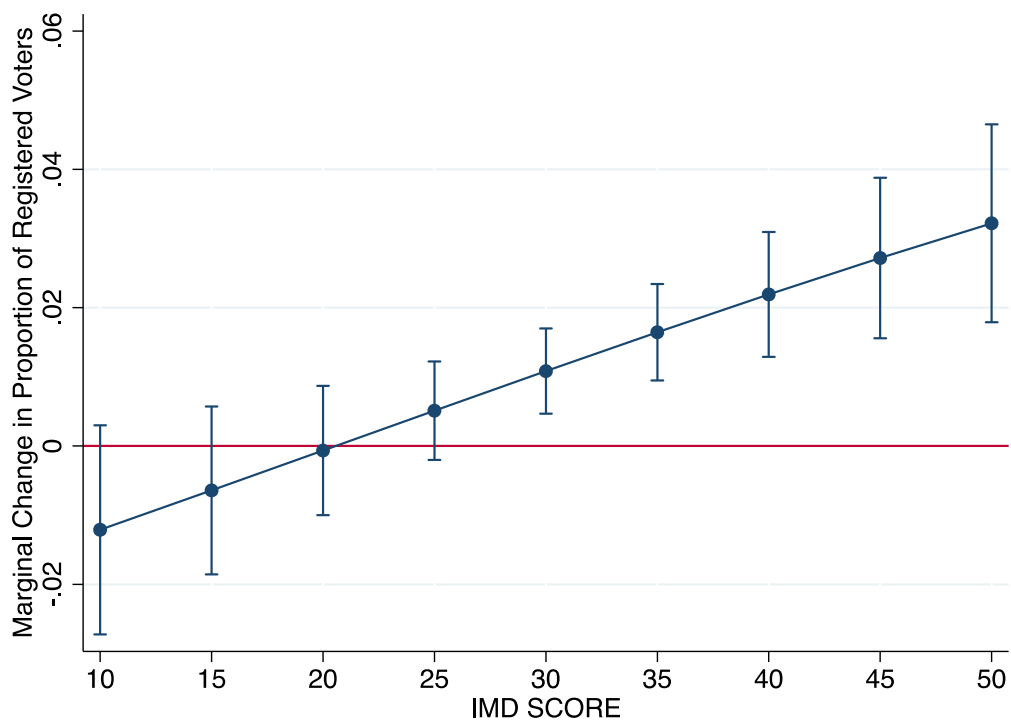
Figure 1: histogram of imdscore

Figure 2: Conditional marginal effects of combined treatment with imdscore (95 per cent confidence intervals)

Tables

Table 1: Main characteristics of the sample by treatment (balance tests)

	Mean	*£1,000*	*Difference to control*	*P-value*	*£5,000*	*Difference to control*	*P-value*
Flats	.750	.751	.000	.542	.748	. 002	.247
Terraces	.172	.174	.002	. 751	.173	. 002	.526
Houses	.009	.009	.000	.356	.009	.000	.907
Imdscore	30.17	30.16	.019	.396	30.12	.078	.143
Retired	.074	.074	.000	.895	.074	.000	.685
White	.406	.408**	.003	.001	.41*	.002	.034

P-values from t-tests or Z-scores, ** p.<0.01, * p.<0.05

Table 2: Proportions of households registering to vote before 31 October

	£1,000	*£5,000*	*Control*
Registered by due date (all)	.462*** (-3.6)	.466*** (-4.7)	.447
Registered by post	.335*** (-6.8)	.333*** (-6.2)	.310
Registered by internet	.068** (3.1)	.071* (2.3)	.075
Registered by telephone	.127*** (3.9)	.133 (1.7)	.137
Registered after canvass	.824 (0.3)	.827 (1.4)	.823
N	19,999	20,000	89,069

*** p.<0.001, ** p.<0.01, * p.<0.05
Z-Scores of differences to control in parentheses

Table 3: Impact of LSOA measures on treatment estimates (probit)

	(1)	(2)	(3)
Combined Treatment	0.039*** (0.011)	-0.059* (0.030)	
Imdscore		0.0029** (0.001)	-.012** (.001)
Treatment* Imdscore		-0.012*** (0.0016)	-0.012*** (0.0016)
£1,000 Treatment			-0.072 (0.039)
£5,000 Treatment			-0.046 (0.035)
£1,000*imdscore			0.0032** (0.0013)
£5,000*imdscore			0.0026* (0.0011)
Constant	-0.130*** (0.016)	0.255*** (0.054)	0.255*** (0.054)
Observations	128,848	117,468	117,468

Robust (clustered) standard errors in parentheses
*** $p<0.001$, ** $p<0.01$, * $p<0.05$