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Low-cost interventions to promote voter registration: What works and why?

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Low-cost interventions to promote voter registration: what works and why?

Felix Kölle[a], Tom Lane[b], Daniele Nosenzo[c], Chris Starmer[d]

20 December 2017

Abstract

We report two studies investigating whether, and if so how, different interventions affect voter registration rates. In a natural field experiment conducted before the 2015 UK General Election, we varied messages on a postcard sent by Oxford City Council to unregistered student voters encouraging them to register to vote. Relative to a baseline, emphasising negative monetary incentives (the possibility of being fined) significantly increased registration rates, while positive monetary incentives (chances of winning a lottery) had no significant effects. In the second study, we show that the success of the negative monetary incentive intervention and failure of the positive monetary incentive intervention can be partly explained by social norms.

JEL classifications: C93: Field Experiments; D03: Behavioral Microeconomics: Underlying Principles; D72: Political Processes: Rent-Seeking, Lobbying, Elections, Legislatures, and Voting Behavior;

Keywords: Voter Registration; Voting; Field Experiment; Nudging; Social Norms; Fines; Rewards.

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1. Introduction

Behavioural science is increasingly informing the use of low-cost interventions across a growing spectrum of public policy areas. The associated body of research is being built by – and is of interest to – both academics (e.g. Chetty, 2015; Hallsworth et al., 2017) and applied policy units (e.g. Behavioural Insights Team, 2010, 2011, 2012, 2016). Interventions often use subtle forms of influence or persuasion intended to systematically change people's behaviour, towards some desried target, at low cost for the policymaker.[1] Our work is motivated by two stylised facts about the existing literature. First, while there is now considerable evidence that low cost interventions can sometimes have a significant impact on particular target behaviours, it seems that some types of intervention work well in some contexts and not others. Second, as yet there is very limited understanding of the underlying mechanisms which determine when and where a low cost intervention will or will not work. In this paper, we examine the impact of different low cost interventions in a field experiment and, via a follow-up online experiment, we test a potential mechanism to explain key patterns in the field data. As such we see ourselves as contributing to an agenda for examining not only what works, but also what are the mecanisms that determine what works.

We do this in the context of a particular policy area: voter registration. In many countries – including the United Kingdom, where our study is conducted – any citizen wishing to vote must first register on the electoral roll. Registration in the UK is technically mandatory, with non-registration punishable with an £80 fine, although in the past two decades a substantial gap has emerged between the numbers of eligible and registered voters (Bite the Ballot, 2016), one which was further increased by the implementation of a legislative change in 2014: previously all members of a household could be registered collectively, but the law now requires each person to register individually (Electoral Registration and Administration Act 2013). Besides any intrinsic benefits of wide democratic participation, high registration rates serve the government's interest insofar as the electoral roll has secondary uses such as fraud-detection and jury recruitment. Employing low-cost behavioural interventions – if this can be shown to be effective – would be an attractive strategy for such organisations to

[1] While the interventions we employ might be called 'nudges' by some in the behavioural science literature, we do not use this terminology because some of our interventions may work, in part, via the impact of financial incetives and would not, therefore, qualify as 'nudges' under some accepted definitions, including that of Thaler and Sunstein (2008). However, in the spirit of nudging, our interventions are low cost, easy to implement, and may work, at least in part, through psychological mechanisms.

employ in pursuit of this goal. Our study therefore explores interventions that can be applied, at minimal financial expense, to encourage citizens to register to vote in elections.

In the first part of this paper, we report the results of a natural field experiment run ahead of the 2015 UK General Election in partnership with one such interested party, Oxford City Council, who sent postcards to students living in university accommodation, encouraging them to register. Councils have a particular interest in discovering successful ways of targeting such students, as they represent a segment of society whose registration rates have been particularly affected by the recent legal change – previously universities could register en masse all accommodated students, but students are now required to register themselves individually. While all the postcards urged recipients not to miss their chance to vote, we systematically varied the precise content of their messages in order to test the effects of different persuasion strategies on registration rates.

We report the effects of two types of intervention involving monetary incentives in the form of either a small *gain* or a small *loss*. There is a large and diverse literature in economics showing that interventions based on negative incentives, such as the threat of monetary loss, may produce relatively strong responses, while those relying on positive incentives, such as the promise of monetary gains, may produce relatively weak ones (for reviews of these literatures, see, e.g., Balliet et al., 2011; van Lange et al., 2014; Nosenzo, 2016). In the context of policy interventions aimed at reinforcing civic duties, as in the case of voter registration, positive and negative incentives may produce different behaviours because of the way they interact with the very notion of 'civic duty': while negative incentives may reinforce the perception that registering to vote is what one ought to do, positive incentives may have the opposite effect. In the second part of our paper, we investigate this hypothesis by examining the effects that positive and negative incentives have on the perception that registering to vote is a normative obligation.

In our field experiment, we implement these interventions across four treatments. In a baseline treatment, the postcard that was sent to unregistered student voters simply encouraged them to register, without any additional message. We investigate the effectiveness of monetary losses in a second treatment by adding a message highlighting the existence of the potential £80 fine for those who fail to register. Emphasising the possibility of facing costly legal action has previously been found to exert a substantial positive effect in other domains of policy intervention, such as the enforcement of TV license registration

(Fellner et al., 2013), debt repayment (Bursztyn et al., 2015), or traffic violations (Lu et al., 2016).[2] Kleven et al. (2011) similarly found that the threat of audits raised tax returns, although mixed evidence was found in this domain by Blumenthal et al. (2001) and Slemrod et al. (2001).[3]

To test the effectiveness of monetary gains, we designed two further treatments where students were offered entry into a lottery to win small cash prizes (of £80) for those who registered early. Financial inducements have previously been found to raise voter registration (John et al., 2015) and voter turnout (Panagopoulos, 2013), although in the latter case only when the inducements were sufficiently large. Moreover, in John et al. (2015) the lottery involved large financial incentives (between £1000 and £5000) and produced only a small positive effect (an increase of two percentage points in registration rates).

Our results show that emphasising the possibility of being fined yielded a large positive effect, with subjects exposed to this intervention being around 1.6 times more likely to register than those in the baseline condition. In contrast, the prospect of financial gain had a negative, but insignificant effect on registration.

In a follow up, online experiment, we investigated a possible mechanism underlying the effectiveness of the negative monetary incentives and the ineffectiveness of positive incentives: that is, their contrasting effects on the perception of what constitutes socially appropriate behaviour in the context of voter registration. A growing body of recent economic research (e.g., Burks and Krupka, 2012; Gächter et al., 2013; Krupka and Weber, 2013; Barr et al., 2015; Banerjee, 2016; Gächter et al., 2017; Krupka et al., 2017) suggests that compliance with social norms drives behaviour in a wide range of contexts. We hypothesised that the fine and lottery treatments may have divergent effects on perceptions of the normative appropriateness of registering to vote: emphasising that failing to register is punishable by law may strengthen the perception that one ought to register, while offering monetary inducements for registering may weaken the perception that doing so is an action already expected within society.

Our results support these hypotheses. Using the incentivised norm-elicitation method of Krupka and Weber (2013), we found that exposing individuals to the fine intervention

[2] However, Lu et al. (2016) found that messages were effective only when they contained personalised information about own past traffic violations, and not when they communicated the mere existence of fines.

[3] See Hallsworth (2017) for a recent review of field experiments on tax compliance interventions.

strengthened their perception that failing to register was socially inappropriate behaviour, while exposing them to the lottery intervention weakened the perception that registering was socially appropriate behaviour. Consequently, we propose that strengthening/weakening the social norm relating to registration had a direct effect on respondents' likelihood of registering, and that this partly explains why the fine intervention was successful while the lottery intervention was not. Indeed, a possible interpretation of our results is that– just as in some previous research (e.g. Frey and Oberholzer-Gee, 1997; Ariely et al., 2009; Gneezy et al., 2011; Bowles and Polania-Reyes, 2012) – monetary incentives crowded out individuals' intrinsic motivation to engage in socially constructive behaviour, and the adverse effect of the lottery intervention on the perceived social norm of registering may be partly behind this effect.

Our study contributes to the literature regarding behavioural insights and low-cost interventions in public policy. One distinctive feature of our study is that our assessment of the effectiveness of positive and negative monetary incentives is done within a unified experimental design and in the context of a natural field experiment. We identify a low-cost strategy – emphasising the possibility of a fine for not registering – that governments can use to substantially increase registrations. However, we also show that offering monetary rewards for registering can fail to work.

Another distinctive feature of our paper is that it probes the underlying behavioural and psychological mechanisms that make specific interventions more (or less) successful. In particular, we believe we are the first to show that policy interventions using positive incentives may weaken, whereas those relying on negative incentives may strengthen, perceptions of the normative appropriateness of target behaviours. These differences in normative perceptions may explain the relative success of the positive and negative monetary interventions in our field experiment. Hence, our paper also adds to recent economic literature on the importance of social norms for understanding human social behaviour.

2. Study I: Field experiment on voter registration

2.1 Experimental design

Our field experiment was designed to test the effectiveness of a set of low cost interventions for raising voter registration rates ahead of the 2015 UK General Election. The full design involved a set of six interventions including a baseline, three treatments involving small monetary incentives, and two treatments involving non-monetary incentives. The interventions were implemented via adjustments to a message sent in a bulk, randomised, mail out (details below) to more than 7000 students living in the UK city of Oxford. The non-monetary treatments had no observable effect and will not be discussed further in the main text.[4]

We implemented one negative monetary intervention invoking the threat of a *monetary loss* for failing to register. We did this by highlighting to subjects the truthful fact that they could be fined £80 if they did not register. This penalty is specified in UK law, and it is referred to in standard materials that Oxford City Council (OCC) use to promote voter registration. Despite the power existing in law, however, it is rarely used by councils and it is unclear how aware a typical unregistered voter would be of the possibility of being fined.

We implemented two interventions involving the prospect of *monetary gain* for registering. We did this with two treatments offering entry into a lottery to win cash prizes of the value of £80 for those who registered by a specific deadline.[5] The two treatments differed only in that one attempted also to harness regret aversion (Loomes and Sugden, 1982), by telling recipients that those who did not register would still be entered into the lottery and informed if they won, but would be unable to claim their prize. Regret aversion has previously been

[4] The two additional treatments did not involve any financial incentives but relied on purely psychological mechanisms to affect behavior. Specifically, we attempted to harness the so called 'foot-in-the-door' mechanism (Freedman and Fraser, 1966; Burger, 1999), wherein people are more likely to comply with a large request, oriented towards a particular goal, after they have first complied with a small request. In our case, the small requests took the form of asking students to provide their phone number (so that they could be sent a reminder to register); or simply to report by text whether they intended to register. However, only a handful of students complied with the small requests and these interventions had no measurable impact relative to the baseline. We therefore do not discuss these two treatments in the main text. See Appendix A for more details about these treatments and their results.

[5] Note that, although the Monetary Loss and Monetary Gain treatments both employ the value of £80 as the possible loss or gain, the subjective probabilities subjects perceived of these eventualities occurring may have differed between the loss and gain treatments. This is not, therefore, an attempt to conduct a comparison between the effects of monetarily equivalent positive and negative incentives; rather, we are interested in comparing both against a baseline treatment featuring neither.

shown to affect entry decisions into lotteries (Zeelenberg and Pieters, 2004; Gneezy, 2014; Imas et al., 2016).

Our interventions were transmitted via postcards, which OCC mailed to all unregistered voters living in student accommodation belonging to the University of Oxford and Oxford Brookes University on March 9-10 2015, ahead of the April 20 deadline for voters to register in the General Election. We collaborated with the Council to engineer the content of these postcards. While all postcards encouraged their recipients to register, the content of the messages they contained varied, allowing us to test the effects of the different persuasion strategies on registration rates.

All postcards were double sided (see Figure 1 for a copy of the postcard used in our baseline condition and Appendix B for copies of the other postcards). The back simply contained the message, '*IMPORTANT INFORMATION ABOUT YOUR RIGHT TO VOTE, OVERLEAF.*' The front featured the heading, '*DON'T MISS YOUR CHANCE TO VOTE! According to our records you have not yet registered to vote. It's easy to register online. To go to the registration page simply use one of the links below.*' The bottom of this side contained the address of the government web page for registering to vote, and a QR code which would take recipients to the same page. These features were held constant across treatments. The postcards differed by treatment only according to the text included in a box below the heading on the front side.

Figure 1: Postcard used in the Baseline treatment

2.2 Treatments

In the **Baseline** treatment, the box was left blank (Figure 1). This treatment therefore serves as a basis for comparison against the other treatments.

In the **Monetary Loss** treatment, the box contained the message: '*If you don't register you could be fined £80.*'.

In treatment **Monetary Gain** the box contained the message: '*If you register by 27 March 2015 you will be entered into a lottery to receive one of ten £80 prizes. Winning students will be notified in June 2015.*'. In treatment **Monetary Gain Regret**, the box contained the message: '*You have been entered into a lottery to receive one of ten £80 prizes. Winners will be notified in June 2015 but you will only be able to claim your prize if you were already registered by 27 March 2015. If not your prize will go to another student.*'.

2.3 Assignment to treatment

These four postcards were sent out on March 9-10 2015 to 5,117 voters who were still unregistered at the time and who lived in student accommodation buildings belonging to the University of Oxford and Oxford Brookes University. In order to minimise the likelihood of subjects seeing postcards belonging to treatments other than the one they were assigned to, we randomised assignment to treatment at the building level: all students living in a single building were assigned to the same treatment. For the University of Oxford, all students living in a single college were assigned to the same treatment. For Oxford Brookes, all students living in a single hall of residence were assigned to the same treatment, with the exception that two very large halls were split into several geographically distinct units of assignment. This was to ensure balance between treatments in the proportion of subjects attending each university – we considered this important, given large demographic (in particular, socioeconomic) differences between the student populations of each university. We further balanced treatment assignment by residence size (small and large) and to the age of college (ancient and modern) to account for other potential unobserved characteristics. The resulting sample sizes were as follows: Baseline (n = 1193); Monetary Loss (n = 1357); Monetary Gain (n = 1250); and Monetary Gain Regret (n = 1317). See Appendix C for further details on the assignment procedure and for a full breakdown of the colleges and halls assigned to each treatment.

2.4 The dataset

OCC provided us with anonymised data on registration rates amongst students residing in each college and hall at various points in time between January and April 2015. In particular, for each individual, our dataset specifies whether or not they were registered on: January 2, March 8 (the day before the postcards of our experiment were sent out), and any subsequent day between March 9 and April 20 (the formal deadline to register to vote for the General Election). The data also identify the treatment each individual was assigned to, their university affiliation (University of Oxford or Oxford Brookes University), and their hall or college. Other demographic data such as gender, age, etc. were not available to the Council.

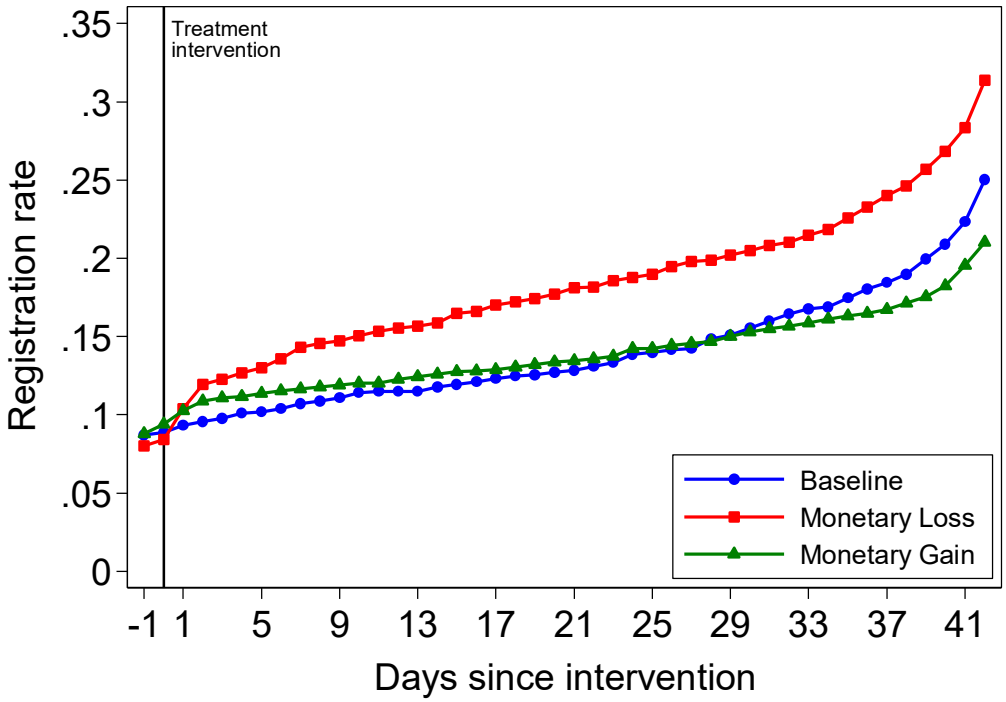
3. Results

In the following analysis we pool data from the two Monetary Gain treatments. This is because we found there were no significant differences between the effects of Monetary Gain and Monetary Gain Regret (see Table A1 in the Appendix). Thus, in the remainder of the section, our analysis is based on the following three conditions: Baseline (n = 1193); Monetary Loss (n = 1357); and Monetary Gain (n = 2567).

Figure 2 shows how registration rates differ between treatments over the entire period between the intervention and the formal deadline for registering. On a daily basis between March 8 and April 20, it displays the cumulative fraction, by treatment, of registered subjects amongst those who were unregistered on January 2.

Pre-intervention registration rates (i.e. in the period January 2 – March 8) are very similar across all treatments; the fraction of registered subjects ranges between 0.080 and 0.088, showing no significant differences across treatments (see Table 1 below). This suggests that later treatment differences are unlikely to be driven by pre-existing differences between the subjects assigned to each intervention.

Figure 2: Cumulative registration rates by treatment



Notes: Figure 2 shows, on a daily basis between March 8 and April 20, the amount of registered students in the treated buildings as a fraction of all students in these buildings who had been unregistered on January 2. The vertical line at Day 0 represents the day of our treatment intervention (March 9).

After our intervention (i.e. in the period March 9 – April 20), substantial differences emerge in the registration rates across treatments. On April 20 (the day of the registration deadline), the fraction of registered students amounts to 0.25 in Baseline, 0.31 in Monetary Loss, and 0.21 in the Monetary Gain treatments. Hence, compared to the case of a simple postcard, only the emphasis of (potential) negative monetary consequences had a positive effect on registration; registration rates in Monetary Loss are 24% higher than in Baseline. The emphasis of (potential) positive monetary consequences, in contrast, had no positive effect on registration rates. While registration rates are initially similar to the ones in Baseline, they become lower towards the end of our observation period. Overall, the registration rate in our Monetary Gain treatments is 16% lower than in Baseline.

To further explore the observed treatment differences in registration rates, we run logistic regressions to model the individual level registration decision. Our dependent variable is 1 or 0 depending on whether or not an individual has registered in a given period. As independent variables, we use dummies to represent the treatment an individual was assigned to. Given the very different nature of the two universities that were included in our study, we also

include a dummy variable for whether a student was affiliated with either Oxford Brookes University or University of Oxford. As further controls we include: which of the two Oxford voting areas (General Election constituencies) a given student resides in; the size of the residence unit they live in; and a 'modern' dummy, which takes value 0 if the college or hall in which they live is older than 100 years, and value 1 otherwise.[6] To correct for heteroscedasticity and potential dependency of observations within halls, we cluster standard errors by residence unit.[7] The results of these regressions are reported in Table 1.

Model (1) reports, for each treatment relative to Baseline (the omitted category), the factor changes in the odds of registering in the period before our intervention (i.e. between January 2 and March 8). The sample includes all students in treated buildings who were unregistered on January 2. In the Baseline treatment, the odds of registering in the pre-intervention period are 0.117, i.e. there are expected to be approximately 8 unregistered students for each registered student in our benchmark condition. The odds of registering are very similar in the other treatments: in all cases the factor changes in the odds are close to 1 and none of the treatment variables are significant (all p-values > 0.275). This confirms that registration rates are indistinguishable across treatments in the pre-intervention period.

In model (2), we look at the effect of our different interventions after their implementation. The dependent variable now is whether an individual registered or not during the period between March 9 and April 20, the day of the registration deadline. The sample includes all students in treated buildings who were still unregistered on March 8 (i.e., we drop those who registered before the intervention, since they did not receive the postcards that were sent out on March 9). The treatment dummies therefore represent treatment differences in registration rates after the intervention. The odds of registering in Baseline are now 0.240. The higher baseline odds of registering in the post-intervention period relative to the pre-intervention period may reflect an impact of sending the postcard per se, or a natural increasing trend in registrations as the deadline for the General Election draws nearer.[8]

[6] We included this dummy because we conjectured that there could be some difference in ethos or culture which could be relevant to the registration decision, comparing older and more newly established colleges.

[7] That is, each college, hall, residence block, and/or cross-college accommodation is treated as providing a cluster of observations, leading to a total of 53 clusters (see Appendix B for further details).

[8] We cannot distinguish between these two explanations because, in designing our treatments in collaboration with OCC, we agreed not to have a treatment where no postcard was sent.

Table 1: The effects of treatments on registration rates

Dependent variable	Registered (1 if yes, 0 otherwise)	
	(1)	(2)
	Before Intervention (Jan 2 – Mar 8)	After Intervention (Mar 9 – Apr 20)
Monetary Loss	0.748	1.579**
	(0.199)	(0.348)
Monetary Gain	1.103	0.763
	(0.212)	(0.176)
Odds of registering in Baseline	*0.117****	*0.240****
	(0.020)	*(0.050)*
Controls	Yes	Yes
N	5596	5117

Notes: The table reports odds ratios from logistic regressions. Note that a ratio greater than 1 implies a positive effect, whereas a ratio smaller than 1 implies a negative effect. The dependent variable indicates whether an individual registered within a given period. Model (1) includes all subjects that were unregistered on January 2. Model (2) includes all subjects that participated in our experiment, i.e., that were not registered on March 9, the day we send out our postcards. Robust standard errors with n = 53 clusters at the residence unit are reported in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

The Monetary Loss treatment increases the Baseline odds by a factor of 1.6 and the effect is significant at the 5% level. This implies an expected ratio of more than 3:1 between unregistered and registered students in the Monetary Loss treatment (the odds of registering are 0.240 x 1.579 = 0.379). In contrast, the Monetary Gain treatments reduce the odds of registering relative to Baseline by a factor of 0.763, although the effect is not statistically significant (p = 0.240). In Appendix D, we show that duration analysis, using a Cox proportional hazard model, yields qualitatively similar results for these post-intervention treatment differences.

4. Study II: The effects of the interventions on social norms

Our field experiment finds that the threat of a monetary fine is effective in encouraging registrations, while the chance of a monetary gain is not. A number of possible mechanisms might explain this, for example loss aversion or a bias in beliefs whereby individuals assess the probability of being fined as higher than the probability of winning the lottery. While we do not rule out such mechanisms being at work in our data, we explore a possible explanation

that would potentially give rise to *opposing* impacts of money gains and money losses in our design.[9] Specifically, we investigate the potentially different effect of these interventions on *social norms*, i.e. collectively recognised rules of behaviour that define which actions are viewed as socially appropriate (Elster, 1989; Ostrom, 2000).

We conjecture that the Monetary Loss treatment may *strengthen* a pre-existing social norm that registering to vote is what one ought to do: emphasising that failing to register is against the law may solidify one's perception that such behaviour is socially inappropriate.[10] In contrast, the Monetary Gain treatments may *weaken* that same social norm – the offer of money for registering may suggest to recipients that registering is not something already unconditionally demanded of them by society. If social norms influence registration behaviour, such alterations of subjects' perceptions of them could directly affect their decisions over registration. Indeed, the failure of the Monetary Gain treatments is reminiscent of previous research showing that the introduction of economic incentives can crowd out people's intrinsic motivation to behave pro-socially. A plausible mechanism behind the ineffectiveness of financial rewards could be that the lottery weakens the social norm of registering, offsetting any positive effects of the monetary incentive.

4.1 Experimental design and procedures

To investigate this, we ran an online study, employing the social norm elicitation task pioneered by Krupka and Weber (2013). In this study, we first described to subjects the setting of our field experiment. We then exposed each subject to the postcard used in one of three treatments – Baseline, Monetary Loss and Monetary Gain[11] – and in each case measured the social norms they perceived pertaining to registration behaviour.

This study was run in June 2016, with subjects who were students at the University of Nottingham, recruited through ORSEE (Greiner, 2015), an online database of experimental participants. Thus, the subjects would have been demographically similar to those in the field experiment, but would not have been previously exposed to the postcards. In total $n = 189$

[9] Recall that while monetary losses have a significant positive impact on registration rates (relative to baseline), there is some indication in our data that the impact of monetary gains may in fact be negative, as registration is lower under the Monetary Gain treatments than under Baseline (though this is not significant).

[10] There is some debate over whether social norms and laws are substitutes, or whether laws directly shape norms. See, for instance, Posner (2009) and Benabou and Tirole (2011).

[11] We only focus on one version of the Monetary Gain treatments because in the field experiment the Monetary Gain Regret treatment was statistically indistinguishable from the Monetary Gain treatment and we thought that the Monetary Gain treatment was easier to describe to subjects.

subjects participated in the study: 65 were shown the Baseline postcard, 61 the Monetary Loss postcard, and 63 the Monetary Gain postcard. The study was conducted using Qualtrics (Qualtrics, 2016), an online survey platform.

At the beginning of the experiment, subjects were told to '*Imagine that the date is March 8, 2015. There is an upcoming General Election on May 7, and a local council wants to encourage people to register to vote before the deadline on April 20.*'. They were further informed that the council is considering strategies to raise registration amongst students in university accommodation, where rates have been particularly low. They were then told that the council decides to send a card to every unregistered student living in university accommodation, and were presented with a picture of one of three cards. These were replicas of the postcards sent out to students in the Baseline, Monetary Loss and Monetary Gain treatments (the only difference was that the cards were cropped to cut off the OCC logo).

Subjects were then asked to evaluate '*how socially appropriate most people would think it would be for a student, having received this card, to register to vote or not to register to vote.*'. Earlier in the instructions, we had defined social appropriateness as '*behaviour that you think most people would agree is the "correct" thing to do. Another way to think about what we mean is that if someone were to behave in a socially inappropriate way, then other people might be angry at them.*'.[12]

We then asked subjects to evaluate the social appropriateness of each action (register to vote, not register to vote) on a four-point scale, encompassing 'very socially appropriate', 'somewhat socially appropriate', 'somewhat socially inappropriate', and 'very socially inappropriate'.[13] These evaluations were incentivised such that subjects were encouraged to coordinate on the social norm: we told subjects we would randomly select one of the two actions, and for this action, they would be eligible to receive a cash prize if their evaluation of its social appropriateness was the same as that chosen by the most other subjects.[14]

[12] This follows the experimental instructions introduced by Krupka and Weber (2013). See Appendix E for the experimental instructions and screenshots of the online survey.

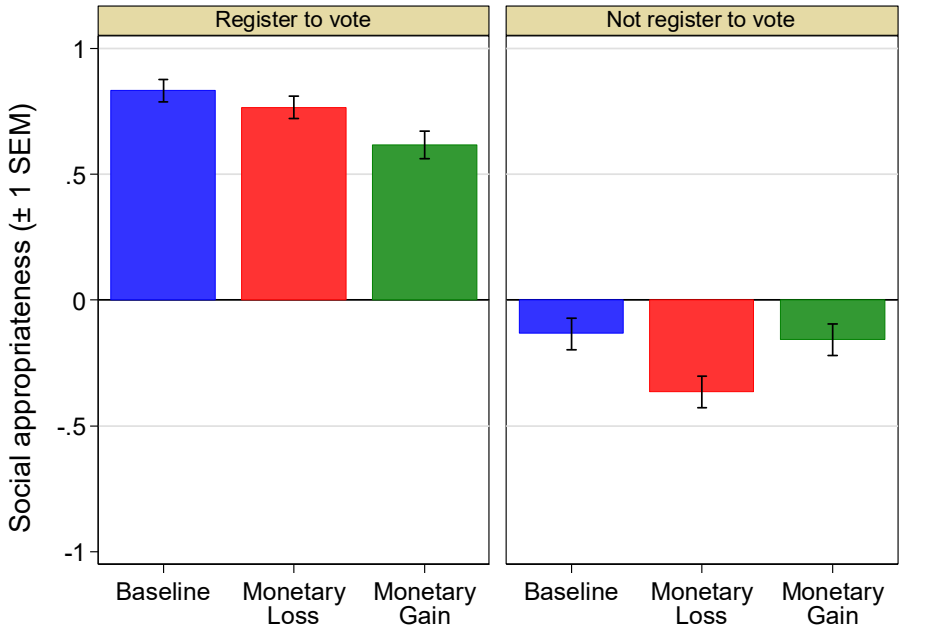
[13] Asking subjects to evaluate the appropriateness of *all* possible actions in the given scenario is standard procedure in studies following the methodology of Krupka and Weber (2013). In cases like ours, where there are only two possible actions (register to vote, not register to vote), the appropriateness rating given to one action may not be the inverse of the rating given to the alternative action. For instance, it is conceivable that participants could believe that both registering to vote and not registering to vote were very socially appropriate. Thus it is necessary to measure treatment effects on the appropriateness of both actions.

[14] As the study was very short and conducted online, we paid only one out of every eight subjects, determined retrospectively at random (subjects knew about this at the beginning of the experiment). Those chosen for

4.2 Results

To analyse the data, we follow Krupka and Weber (2013) in transforming the evaluations into numerical values. We assign evenly-spaced values of -1 for the rating 'very socially inappropriate', -0.33 for the rating 'somewhat socially inappropriate', 0.33 for the rating 'somewhat socially appropriate' and 1 for the rating 'very socially appropriate.' We then calculate the mean ratings for each action by subjects exposed to each treatment. The results are displayed in Figure 3.

Figure 3: Social appropriateness of registration behaviour by treatment



Notes: Figure 3 shows the mean appropriateness ratings assigned to each action (registering to vote, not registering to vote) by subjects exposed to the Monetary Loss, Baseline and Monetary Gain postcards. Mean ratings are taken by assigning values of -1, -0.33, 0.33 and 1 for the ratings 'very inappropriate', 'somewhat inappropriate', 'somewhat appropriate' and 'very appropriate' respectively, and averaging the values for each action for all participants exposed to a given treatment. Error bars indicate standard errors of the mean.

We find that regardless of the treatment subjects are exposed to, registering to vote tends to be seen as highly appropriate behaviour, while failing to register is generally seen as inappropriate. However, there are also subtle but significant treatment differences in people's appropriateness judgments. In particular, subjects exposed to the Monetary Gain treatment perceived registering to vote to be less appropriate than did those exposed to the Baseline

payment received an automatic £10, plus a further £30 if their evaluation matched that of the most other subjects in their treatment. Although most subjects would not be paid, the study was still incentivised to a conventional level: all subjects had a 1/8 chance of receiving between £10 and £40 for an approximately five-minute task.

treatment (two tailed Fisher Randomisation Test, $p = 0.012$).[15] Moreover, failing to register to vote was perceived to be more inappropriate by subjects exposed to the Monetary Loss treatment than it was by those exposed to the Baseline treatment ($p = 0.016$). In contrast, we find no significant differences in the perception of appropriateness of registering between Monetary Loss and Baseline ($p = 0.543$), or the inappropriateness of non-registering between Monetary Gain and Baseline ($p = 0.718$).

Table 2 sheds light on how these treatment differences arise. It presents, for each treatment, the distribution of subjects' evaluations of the social appropriateness of each action. It shows that the lower perceived social appropriateness of registering under Monetary Gain is driven by fewer subjects regarding registering as 'very socially appropriate' relative to Baseline (50.8% versus 78.1%). We also see that the higher perceived social inappropriateness of failing to register in the Monetary Loss treatment is driven by more subjects regarding non-registering as 'very socially inappropriate' relative to Baseline (25.0% versus 10.9%), and by fewer regarding it as 'somewhat socially appropriate' (13.3% versus 28.1%).

Table 2: Distribution of social appropriateness evaluations

Appropriateness of registering to vote

	Very socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Very socially appropriate
Baseline	1.6	0	20.3	78.1
Monetary Loss	0	1.7	31.7	66.7
Monetary Gain	0	8.2	41.0	50.8

Appropriateness of not registering to vote

	Very socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Very socially appropriate
Baseline	10.9	54.7	28.1	6.3
Monetary Loss	25.0	58.3	13.3	3.3
Monetary Gain	9.8	60.7	23.0	6.6

Notes: Table 2 displays, by treatment, the percentage of subjects who evaluated registering to vote (top) or not registering to vote (bottom) as very socially inappropriate, somewhat socially inappropriate, somewhat socially appropriate, or very socially appropriate.

[15] See Moir (1998) for discussion of the randomisation test, and Kaiser and Lacy (2009) for information on the Stata command to apply it. In Study II, we correct p-values for testing multiple hypotheses for two interrelated dependent variables (the appropriateness of registering to vote and not registering to vote). For each dependent variable, we test Monetary Gain vs. Baseline and Monetary Loss vs. Baseline, a total of four tests. The correction method is that of Benjamini and Hochberg (1995) and corrected p-values are displayed in the text.

To summarise the results of Study II, we find that subjects exposed to the Monetary Loss treatment perceived a relatively strong social norm against failing to register, while subjects exposed to the Monetary Gain treatment perceived a relatively weak social norm in favour of registering. This suggests that emphasising the fine for failing to register strengthens the social norm against such behaviour, while offering monetary incentives for successfully registering weakens the social norm demanding such behaviour. Given the strong evidence from previous studies (see introduction) that social norms influence economic behaviour, we propose that these normative effects explain at least part of the success of the Monetary Loss treatment and ineffectiveness of the Monetary Gain treatments.

5. Conclusion

We investigated the effectiveness of different low-cost interventions aimed at raising voter registration rates. A unique feature of our study is that it combines two types of experiment: a natural field experiment to measure which persuasion strategy is most effective in raising registrations, and an online experiment to investigate possible reasons why different strategies may trigger different behavioural responses.

Our field experiment shows that highlighting to citizens the possibility of being fined for failing to register is an effective strategy for public bodies to use. The effect we identified was not only statistically significant, but also of a substantial magnitude: having the fine emphasised made subjects around 1.6 times more likely to register. In contrast, we find no evidence that offering financial inducements for registration is an effective strategy. We speculate that the lack of success of our Monetary Gain treatments may represent another case of economic incentives crowding out people's intrinsic motivation to behave in socially constructive ways (e.g. Frey and Oberholzer-Gee, 1997; Ariely et al., 2009; Gneezy et al., 2011; Bowles and Polania-Reyes, 2012). We note that, in this respect, our study's results differ from those of John et al. (2015), who also offered entry into a cash lottery as a reward for registering to vote in the UK. They found a small (approximately 2%) but significant positive effect of monetary rewards on registration rates. A possible explanation for the contrast in results is that the maximum winnings offered by John et al. (2015) were much larger than ours (between £1000 and £5000). Their large material incentives may well have been enough to produce a positive effect, even if they had to overcome a crowding out effect (Bowles and Polania-Reyes, 2012). A tentative conclusion could then be that, when offering cash for behaviours where there is danger of crowding out intrinsic motivations, one must

'pay enough or don't pay at all' (Gneezy and Rustichini, 2000). This would also be consistent with Panagopoulos (2013), who found that financial inducements raised voter turnout but only once they were sufficiently large.

Finally, our online experiment suggests a possible explanation for the contrasting effects of positive and negative monetary incentives on voter registration rates: that is, because of their diffrerential effects on relevant social norms. Our online experiment shows that emphasising the fine strengthens the perception that failing to register is socially inappropriate, while offering money for registering weakens the perception that registering is socially appropriate. We interpret this as evidence that social norms are a significant factor determining voter registration, a finding which is consistent with other recent experimental literature pointing to the importance of social norms as drivers of a wide range of behaviours (e.g. Burks and Krupka, 2012; Gachter et al., 2013; Krupka and Weber, 2013; Barr et al., 2015; Banerjee, 2016; Kimbrough and Vostroknutov, 2016; Krupka et al., 2017). A key novel contribution of our study in relation to this literature is in identifying the potentially important role of social norms in determining the effectiveness of different types of low-cost intervention.

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Online Appendices

Appendix A: Additional Treatments

In addition to the treatments reported in the main text, we ran two further treatments which attempted to employ purely non-monetary persuasion strategies. There are a number of non-monetary interventions that have been shown to positively affect voter turnout, e.g. personalised get-out-the-vote contact (e.g. Gerber and Green, 2000; Ramirez, 2005), priming one's identity as a voter (Bryan et al., 2011), or applying social pressure on people to vote (e.g. Gerber et al., 2008, 2010; Davenport, 2010; Panagopoulos, 2010). See Rogers et al. (2013) for a recent review of this literature. While one might expect that what works for voter turnout should also work for voter registration, ultimately this is an empirical question. Nevertheless, the fact that voter turnout appears quite susceptible to psychological processes provides a strong motive for exploring non-monetary interventions as a policy tool regarding registration.

The interventions we chose were motivated by the so called foot-in-the-door effect (Freedman and Fraser, 1966). More specifically, we tested whether message recipients were more likely to register (i.e., comply with a large request) if they had first complied with a small request related to voter registration: in both variants, subjects were asked to send a one-word text message to OCC; in one case they were asked to text the word 'myvote' to indicate that they planned to register; in the other, they could text the word 'reminder' to receive a free reminder text to prompt them to register nearer the deadline for voters to register in the 2015 UK General Election. Foot-in-the-door mechanisms have previously been tested in the context of voter turnout, with mixed results: asking subjects if they intended to vote was found to have a positive impact on turnout by Greenwald et al. (1987, 1988), but not by Smith et al. (2003).

In treatment Non-Monetary 1, the box below the standard text in the postcard contained the message: 'We'd like to know if you are intending to register. If you are, please text 'myvote' to 60886.'. In treatment Non-Monetary 2, the box contained the message: 'Would you like us to send you a text reminder? If you do, please text 'reminder' to 60886.'. In both cases, texts were free of charge and this was clearly stated in the postcard.

Our primary interest in relation to the treatment Non-Monetary 2 was in assessing whether the act of registering for an automated text reminder (which was designed to be very quick and easy) would act as a foot in the door, enhancing the propensity to subsequently register to vote. We were not especially interested in the later effect that a subsequent automated reminder might have (we do not have controlled comparisons for benchmarking any such effect). Of course all of our treatments can be considered as sending slightly different forms of reminder and, more broadly, reminders have been shown to be effective interventions in relation to voter registration (Bennion and Nickerson, 2011), as well as in other contexts (e.g. Altmann and Traxler, 2014).

The results of the two Non-Monetary treatments in relation to the Baseline treatment can be viewed in Table A1. The logistic regressions reported in Table 1 are repeated, with the additional inclusion of data from the Non-Monetary treatments (and also with the two Monetary Gain treatments separated). In the post-intervention period, both Non-Monetary treatments have odds ratios close to 1 and are not significant. We therefore have no evidence that either intervention was effective in raising registration rates. This is not surprising, given the response rates to our requests for text messages; of the 3562 people who were asked to comply with our small requests, only 13 did so. The foot-in-the-door effect was therefore unlikely to take place, as we did not get our foot through the door in the first place.

Table A1: The effects of treatments on registration rates (uncombined)

Dependent variable: = 1 if registered, 0 if not	Logistic Regression	
	Before Intervention (Jan 8 – March 8)	After Intervention (March 9 – April 20)
Monetary Loss	0.955 (0.244)	1.738*** (0.353)
Monetary Gain	1.086 (0.192)	0.707 (0.224)
Monetary Gain Regret	1.170 (0.265)	0.795 (0.204)
Non-Monetary 1	0.842 (0.266)	1.002 (0.208)
Non-Monetary 2	1.019 (0.195)	1.013 (0.196)
Odds of registering in Baseline	0.118*** (0.019)	0.237*** (0.045)
Wald-test (p-values): Monetary Gain = Monetary Gain Regret	0.640	0.742
Controls	Yes	Yes
N	8397	7679

Notes: Reported are odds ratios. Model (1) includes all subjects that were unregistered on January 2. Model (2) includes all subjects that participated in our experiment, i.e., that were not registered on March 9, the day we send out our postcards. Robust standard errors with n = 53 clusters at the residence unit are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The table further includes the p-value from a Wald test of the hypothesis that the odds ratios differ between the two types of Monetary Gain treatments.

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Appendix B: Postcard Intervention in Study I

Baseline Treatment

Monetary Loss treatment

Monetary Gain Treatment

Monetary Gain Regret Treatment

Appendix C: Details of assignment to treatment

Table C1 presents a breakdown, by university and place of residence, of the subjects assigned to each treatment. As stated in Section 2.3, we randomised assignment to treatment at the building level. For the University of Oxford, all students living in a single college were assigned to the same treatment.

For Oxford Brookes University, all students living in a single hall of residence were assigned to the same treatment, with the exception of two very large halls, Cheney and Clive Booth, which were split into several units of assignment. Cheney and Clive Booth are naturally split into discrete residence blocks, so we subdivided them on this basis, ensuring maximum geographical distance between the subjects in these halls assigned to different treatments. For the purposes of clustering in our regression analysis, each subdivision that we split Cheney and Clive Booth into is treated as one unit of residence.

Beyond balancing assignment at the university level, the randomisation was also subject to the following constraints. Each treatment had to feature a mixture of large and small residence units; we ensured this by splitting the residence units into pools based on size, and assigned one unit from each pool to each treatment. Each treatment also had to feature a mixture of ancient and modern University of Oxford colleges, to the extent that the average college age in all treatments had to be within 200 years. Finally, the total number of subjects assigned to the largest treatment had to be no more than 15% greater than the number assigned to the smallest. We repeated the randomisation until it produced an assignment which met all the above criteria.

We ran regressions to test whether individual characteristics were correlated with treatment assignment. These are reported in Table C2. Model (1) is a logistic regression where the dependent variable is a dummy variable indicating whether an individual attends Oxford Brookes University or Oxford University, while Model (2) is a logistic regression with the dependent variable a dummy variable indicating whether an individual lives in a modern hall/college (i.e. less than 100 years old), and Model (3) is an OLS regression with the dependent variable the size of the residence unit the individual lives in. These variables were regressed against treatment assignment dummies, with Baseline the omitted category. In each regression some of the treatment dummies are significant, indicating that although we applied a randomisation technique particular types of people are significantly more likely to be assigned to some treatments than others. For this reason, it is important that our analysis of treatment effects includes control variables capturing university, hall/college age and residence unit size.

Mailing errors resulted in some subjects not being assigned to the intended treatments (in italics in Table C1). 16 students in Lincoln College were assigned to the Monetary Loss treatment, while the other 132 were assigned to the Baseline treatment. As a robustness check, we re-ran our analysis excluding Lincoln from our dataset. All of our results are robust to this exclusion.

Table C1: Breakdown of treatment assignment by university and college/hall

College/hall	Number assigned	Percentage of total assigned to treatment	College/hall	Number assigned	Percentage of total assigned to treatment
Baseline			**Monetary Gain**		
University of Oxford	**889**	**74.52**	**University of Oxford**	**940**	**75.20**
Keble	206	17.27	Worcester	311	24.88
Magdalen	201	16.85	Hertford	236	18.88
St Hugh's	200	16.76	Trinity	154	12.32
Lincoln	132	11.06	Oriel	140	11.20
Corpus Christi	120	10.06	Mansfield	74	5.92
Harris Manchester	26	2.18	Linacre	25	2.00
All Souls	4	0.34			
Oxford Brookes	**304**	**25.48**	**Oxford Brookes**	**310**	**24.80**
Clive Booth	153	12.82	Clive Booth	162	14.56
Paul Kent	151	12.66	Cheney	128	10.24
Total	**1193**	**100.00**	**Total**	**1250**	**100.00**
Monetary Loss			**Monetary Gain Regret**		
University of Oxford	**1026**	**75.61**	**University of Oxford**	**1044**	**79.27**
St. Catherine's	254	18.72	St. Edmund	398	30.22
Jesus	188	13.85	Merton	253	19.21
Pembroke	188	13.85	The Queen's	209	15.87
University College	147	10.83	St. Hilda's	156	11.85
St. Peter's	146	10.76	Nuffield	14	1.06
Green Templeton	71	5.23	Wycliffe	14	1.06
Lincoln	*16*	*1.18*			
Kellogg	11	0.81			
140 Walton Street	5	0.37			
Oxford Brookes	**331**	**24.39**	**Oxford Brookes**	**273**	**20.73**
Clive Booth	187	13.78	Clive Booth	168	12.76
Warneford	144	10.61	Cheney	105	7.97
Total	**1357**	**100.00**	**Total**	**1317**	**100.00**

Table C2: Treatment Balance Regressions

	(1) Brookes (Logit)	(2) Modern (Logit)	(3) Hall Size (OLS)
Monetary Loss	0.931 (0.083)	4.492*** (0.374)	-31.431*** (10.745)
Monetary Gain	0.935 (0.085)	1.081 (0.096)	0.892 (10.927)
Monetary Gain Regret	0.752*** (0.069)	0.806** (0.074)	169.424*** (10.808)
N	5596	5596	5596

Notes: Models (1) and (2) report odds ratios from logistic regressions; Model (3) reports coefficients from an OLS regression. Note that an odds ratio greater than 1 implies a positive effect, whereas a ratio smaller than 1 implies a negative effect. In Model (1) the dependent variable is a dummy variable taking the value of 1 if an individual is a student at Oxford Brookes University, and 0 otherwise; in Model (2) it is a dummy variable taking the value of 1 if an individual is resident in a hall/college which is less than 100 years old, and 0 otherwise; in Model (3) it is the size of the residence unit in which an individual lives. The regressions include all students who were assigned to the Baseline, Monetary Loss, Monetary Gain and Monetary Gain Regret treatments. Standard errors are reported in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Appendix D: Duration analysis

To test the robustness of the results in Table 1 of the main text, for the post-intervention period we use the Cox proportional hazard approach to model the duration until an individual registers. Estimated hazard ratios are reported in Table D1. In line with the results from the logistic regression, we find a positive and significant effect of the Monetary Loss dummy: the hazard ratio of 1.524 indicates that the probability of registering on a given date (conditional on not having registered before) is 52.4% larger than in Baseline. The corresponding hazard ratio for the Monetary Gain treatments is negative but not significant.

Table D1: Duration analysis

Dependent variable	Duration to registration after Intervention (Mar 9 – Apr 20)
Monetary Loss	1.524**
	(0.304)
Monetary Gain	0.795
	(0.170)
Controls	Yes
N	5117

Notes: The table reports hazard ratios from a Cox proportional hazard model. A ratio greater (smaller) than 1 implies a positive (negative) effect. Robust standard errors, clustered on residence, in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Appendix E: Screenshots of Study II

Screen 1:

Thank you for participating in this survey. It should take a few minutes to complete. If you need to stop, you can save your responses and return to the survey later. The anonymity of your responses in this survey is guaranteed.

First, please enter your university email address. Make sure you enter this correctly, as we will use it to contact you regarding payment.

Screen 2:

Regarding payment:

After all participants have completed the survey, we will randomly pick one out of every eight to receive payment. We will email all participants by June 10 to notify them whether they have been selected for payment or not. Participants selected for payment will then be able to collect their money from the Clive Granger Building on University Park Campus. If you have any questions regarding payment for this survey, please email lextl9@nottingham.ac.uk.

If you are selected for payment, you will receive a participation fee of £10. Based on your response to the survey, you may also receive an additional £30. Further details will be provided at the relevant point in the survey.

Screen 3:

Information about this survey

This survey will ask how socially appropriate certain behaviour is. By socially appropriate, we mean behaviour that you think most people would agree is the "correct" thing to do. Another way to think about what we mean is that if someone were to behave in a socially inappropriate way, then other people might be angry at them.

Screen 4:

Imagine that the date is March 8, 2015. There is an upcoming General Election on May 7, and a local council wants to encourage people to register to vote before the deadline on April 20. Registration rates are particularly low amongst students living in university accommodation, and the council is considering various strategies it can use to attempt to raise registration rates amongst these students.

The council decides to send every unregistered student living in university accommodation this card which includes a message urging students not to miss their chance to vote. The card also warns students that those who do not register may be fined £80.

Screen 5:

Before the deadline, students must decide either to register to vote or not to register to vote. Below, you will be asked to evaluate how socially appropriate most people would think it would be for a student, having received this card, to register to vote or not to register to vote.

After you have completed the survey, we will look at your evaluation of one of the two actions (registering to vote, or not registering to vote). To reward you, if your evaluation of the social appropriateness of this action is the same as that provided by the highest number of participants in this survey, and if you are one of the participants selected for payment, we will give you £30 in addition to your participation fee.

How socially appropriate would most people think it would be for a student, having received this card, to either register or not register to vote?

Note on how to fill out the table below: for each of the two actions (registering to vote, or not registering to vote), please indicate whether most people would think that action is very socially appropriate, somewhat socially appropriate, somewhat socially inappropriate, or very socially inappropriate. To do so, tick exactly one box for each of the actions in the table.

	Very socially appropriate	Somewhat socially appropriate	Somewhat socially inappropriate	Very socially inappropriate
Register to vote	○	○	○	○
Not register to vote	○	○	○	○