# Null Effects of Social Media Ads on Voter Registration: Three Digital Field Experiments

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**Abstract**

Civic organisations and progressive campaigns regard digital advertising as an essential method to register to vote low-participation groups, such as ethnic minorities, young voters, and frequent home movers like private-sector tenants. Digital strategies appear to be promising because the registration process can be completed online, usually in less than five minutes, using a web link in the advert. But do typical digital campaigns actually work in registering voters? To find out, we provide evidence from three randomised controlled trials: two conducted with advocacy organisations and the third run by the research team, carried out in two types of elections (general and local), and assigned either at the aggregate (studies 1 and 2) or individual (study 3) level. We find no evidence consistent with digital campaigning meaningfully affecting voter registrations. Despite wide reach and relatively high engagement rates, we find no effect of the campaigns on under-registered groups' voter registrations in the three trials. The findings raise questions about commonly-used digital advertising strategies to register marginalised groups. These findings are consistent with other studies that report either null or minimal effects of digital ads on other types of political behaviour.

**Keywords:** elections | social media | digital ads | voter registration | youth mobilization

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## Introduction

Voter registration and turnout among ethnic minority citizens, young people, and frequent residential movers—like private-sector renters—remain low (Fieldhouse et al. 2021b). These groups of citizens are therefore obvious targets for non-partisan campaigns that aim to increase electoral participation. They are also targeted by progressive campaigns who want to change the composition of the electorate in their favour (Foos and John 2018; Broockman and Kalla 2020). So, how can non-partisan and partisan campaigns effectively register people who are difficult to contact with conventional voter registration methods, such as door-to-door canvassing and direct mail? Social media ads appear to be promising because most people are active on social media. Moreover, in many jurisdictions, such as the United Kingdom and forty US-States, the registration process may be completed online, often taking less than five minutes. Since the act of registration does not require as much time and effort as voting (which usually happens offline), digital campaigns could be more effective at voter registration than directly mobilising registered voters to turn out at the polls.

However, the research record so far has not been promising. Some studies show that social media advertising campaigns produce relatively small positive effects on voter turnout (Bond et al. 2012), or they find that treatment effects are conditional (Haenschen and Jennings 2019) or, more frequently, null (Haenschen 2022; Aggarwal et al. 2023; Coppock forthcoming). There is also mixed evidence on whether digital ad campaigns can affect party vote shares (Hager 2019; Coppock et al. 2022; Aggarwal et al. 2023). Typical voter mobilisation campaigns often deploy a strategy of raising awareness about the efficacy of the vote, employing "cognitive mobilisation" messages such as "Your Vote Matters" or "Don't Miss Out". However, there is scepticism about whether such cognitive mobilisation messages actually work (Holbein and Hillygus 2020; Hersh 2020). Despite this concern, the lack of robust evidence means that many campaigns keep on relying on exactly these kind of messages using social media platforms. In contrast to voter turnout, there is also very limited experimental evidence on the impact of these campaigns on voter registration and the subsequent link to turnout.

We provide evidence from three digital trials that show that social media campaigns were ineffective at registering groups of under-registered voters, studied at two separate UK elections.

What distinguishes our approach is that we culminate findings from trials that have similarities in design and were applied within the same geographic context. The studies were also conducted by different organisations in different elections, which gives confidence that our results are not due to one particular messenger or a specific electoral contest: the first messenger was an advocacy organisation in a general election; the second is a researcher-led campaign conducted in a local election; and the third campaign was run by an advocacy organisation also at a local election. The studies also vary at the level of assignment, with the first and second study targeting postal sectors, and the third study assigned at the individual level. They also try out different types of mobilisation messages, either cognitive and non-cognitive, implemented on a variety of social media platforms. We cannot rule out that other combinations of these features in the UK context might work, but the variation and consistency of three null results from three trials in our experiments suggest that digital ads have no effect on voter registration.

## The scarcity of evidence on the effects of digital ads on electoral registration

Despite the increasing prominence of digital methods in election campaigns, and the heavy financial investments that modern campaigns make online (Jungherr et al. 2020; Fowler et al. 2020), randomized controlled trials of digital media adverts are still rare, and do not focus on voter registration (Fowler et al. 2020). Moreover, they are limited to a few social media platforms, such as Facebook and Twitter, and neglect others, like Instagram and Snapchat, some of the social media outlets used in our study, which are increasingly popular. The early randomized trial on Facebook using digital adverts in the USA increased turnout by around 0.5 percentage points (Bond et al. 2012), with a subsequent study targeted at millennial voters only effective in competitive districts (Haenschen and Jennings 2019). Another US study revealed a zero average treatment effect on turnout, though a positive impact where message, audience, and electoral context are congruent (Haenschen 2022). Null effects on turnout have also been reported based on experiments, where ads were mostly meant to persuade voters (Hager 2019; Aggarwal et al. 2023; Coppock et al. 2022). Coppock et al. (2022)'s US study finds minimal effects of digital ads on Democrat vote share. Beyond turnout and vote shares, studies show null effects of public or semi-public Facebook and

Twitter posts on online and offline political activism (Coppock et al. 2015; Foos et al. 2020). Even major changes to the Facebook online experience, such as changes to media feed algorithms, did not impact turnout (Guess et al. 2023), polarization, or political knowledge (Nyhan et al. 2023).

Experimental studies of voter registration have so far mostly relied on direct mail and door-to-door canvassing. Face-to-face canvassing has been shown to have larger positive effects in the region of 2.2 percentage points (Nickerson 2015; Braconnier et al. 2017), but was less feasible during the Covid-19 pandemic. Direct mail and postcards have also been employed as effective means of voter registration (Mann and Bryant 2020; John et al. 2015). Other registration experiments done by email and text message explicitly targeted at low registration groups have recorded divergent findings (Nickerson 2007; Bennion and Nickerson 2018; Kölle et al. 2019; Cheng-Matsuno et al. 2023). Some of the most promising interventions take place within the school context. Studies using classroom presentations in colleges and high schools show substantively large increases in youth voter registration (Bennion and Nickerson 2016; Addonizio 2011), but they are difficult to scale up.

## Study designs

We present three trials to test the impact of digital ads on voter registration.[1] Voter registration in the UK may be done online, requiring basic information, such as name, address, nationality, date of birth, National Insurance Number (NI number), and email address. The process takes approximately five minutes to complete in one session. This digital registration process closely resembles the procedure used in the 40 US states and DC that allow for digital voter registration. The three trials share a common context which is elections happening in the UK between 2019-2021, and delivered by two NGOs and the research team. As in many democracies, campaigning in the UK increasingly uses social media, whether done by political parties, advocacy groups, and even traditional governmental organisations concerned with voter registration and turnout (Dommett 2021). One of the trials was a pragmatic intervention, done with an advocacy group using its own campaign materials. The other two followed a common research design to the trials and

[1]The studies were pre-registered on OSF: see Appendix E for the PAP in Study 1; Study 2 and 3 are anonymised on OSF respectively here and here. Studies 1, 2 and 3 were approved by the LSE Research Ethics Committee under references 1032, 22182 and 21816. Study 2 was approved by the KCL Research Ethics Committee under HR-20/21-22567.

interventions, reflecting recent work about the most effective interventions. Campaigns targeting low-registration voters, such as young people, tend to assume that they need to be mobilised by making them aware of the general importance of politics and their role in it. There are good reasons to question the validity of the assumption that these voters lack the motivation to vote (Holbein and Hillygus 2020). Tasks like registration are perceived to be costly and many citizens, such as young people, experience difficulty navigating the process (Holbein and Hillygus 2020). According to this reasoning, common cognitive mobilisation campaigns, such as 'Rock the Vote', are run based on the assumption that a main cause of low turnout is disengagement from politics. But based on many metrics, interest and engagement with politics have increased in recent decades (Dalton 2007). Many people also have a civic orientation. If people are already cognitively mobilised, then a campaign based on cognitive mobilisation would be bound to fail, especially when citizens do not have the non-cognitive skills to complete a task and go through a bureaucratic process, such as voter registration. By non-cognitive skills we mean the ability to plan, to process information, and then to prepare to convert an intention into action, "For new voters, the registration requirement, in particular, is recognised as especially burdensome – it typically must be completed by a certain deadline, it must be updated with every change in address" (Holbein and Hillygus 2020, p.33). Messages that might help individuals complete the process may be more effective, but typically campaigns focus on cognitive mobilisation (Ahmed 2019).

## Research Design for Study 1: Social Media Ads from a Civic Organisation

We worked with a civic organisation, randomly assigning a part of its well-organised campaign to test if social media ads are effective at registering young people to vote in the UK 2019 General Election.[2][3] We assigned 879 postcode sectors[4] located within 40 UK parliamentary constituencies to two groups: one control, and one treatment group that received voter registration ads from the organ-

[2]It is important to emphasise that we did not evaluate the entire campaign. We did not randomly assign the campaign in its highest priority seats, and we did not run policy-based ads. The 40 constituencies in the experimental sample still contained large numbers of young voters and a mix of seats ranging from majorities smaller than 1% to majorities greater than 10%. We do not find that campaign effects vary conditional on marginality in the sample of seats that were included in the experimental sample.

[3]As pre-registered, we also intended to test if Get-Out-The-Vote reminders sent via social media 2-3 days before the election amplified the campaign's effect on turnout, but we were unable to obtain validated voter turnout data for 37 out of 40 constituencies. Since the GOTV messages were sent after the voter registration deadline, the voter registration outcomes reported in this paper could not have been influenced by GOTV ads. We address deviations from the Pre-Analysis Plan in Appendix B.3.

[4]Postcode sectors represent the lowest level of geography reachable on social media platforms in the UK.

isation via Instagram and Snapchat. Typical example ads used in the trial by the organisation are displayed in Figure B.1 in the Appendix. All ads contained a direct link (via swipe-up on Instagram) to the UK Government's voter registration website (https://www.gov.uk/register-to-vote).

Ads were targeted at young people aged between 18 and 35 years. The registration messages appeared in postcode sectors assigned to the treatment group in the week before the voter registration deadline on 26 November. To avoid spillovers, they were displayed in the mornings and evenings only. The organisation also provided data on the successful placement of ads in each postcode sector, as well as spending, impressions, and engagement metrics at the campaign level.

**Table 1: Campaign statistics**

| | |
|---|---|
| Postcode sectors successfully targeted | 394/437 |
| Spend Instagram | £4423.52 |
| Spend Snapchat | £3535.09 |
| Total impressions | 2,058,431 |
| Total clicks | 18,421 |

Table 1 displays the organization's expenditure on Instagram and Snapchat ads, totaling approximately £8,000 over seven days. The adverts generated more than two million impressions and over 18 thousand clicks. Despite having a budget of £10,000 for the entire campaign, not all funds were utilized, suggesting that the allocated amount was sufficient to saturate the platforms for the week. In the context of UK elections, strict spending limits are enforced, with campaign spending capped at a maximum of £30,000 per parliamentary constituency for each candidate. The campaign showcased extensive reach and garnered significant online engagement, particularly from young people. Post-election, we obtained de-identified voter registration data from public registers in the 40 constituencies included in the experimental sample. We matched this data with their experimental assignment through the postcode column (ensuring individual-level de-identification, with the smallest unit being the postcode). The crucial question remains whether the social media clicks translated into actual voter registration.

In Appendix Section B.2, we address how we handle non-reporting postcode sectors, which were missing due to being located outside the experimental sample. Table B.12 presents these results. As expected, there are no significant differences in whether control and treatment sectors report voter registration numbers, indicating that missingness is unlikely to be a result of the treatment.

To ensure the validity of our experimental design, we conducted balance checks in Appendix Table A.9. These checks demonstrate that census covariates are balanced across treatment and control sectors. For further insights, descriptive statistics of the covariates are provided in Appendix Table A.4, while descriptive statistics of the outcome variables can be found in Appendix Table A.3.

### Research Design for Study 2: Large-scale Trial on Social Media Adverts

This online field experiment was conducted in the context of the 2021 English local elections. The aim of the experiment was to test if digital advertising campaigns via Instagram and Facebook positively affect voter registrations. The experimental sample comprised 1,981 postcode sectors located in 69 local authorities. We followed three criteria to select these postcode sectors: sector size, mean age, and share of BAME (Black, Asian, and minority ethnic) residents.[5]

The 1981 postcode sectors are block-randomly assigned with equal probabilities to one treatment group or a pure control group. The assignment is stratified by region and postcode sector size. The treatment was a 10-days-long digital ad campaign on Instagram and Facebook that ran in postcode sectors assigned to treatment. The ad campaign included a bundle of three social media ads that built on the following behavioural themes, 1) follow-through 2) anti-sludge and 3) dynamic social norms. A follow-through type aims to nudge individuals by strengthening their sense of grit to tackle obstacles. An anti-sludge type aims to vary the perception about the costs of voting. A dynamic norm type of message appeals to social pressure when given information about what other individuals in the same community are doing. The ads can be found in Appendix Section C.1.

Campaign statistics can be found in Table 2. The lower click-through rate in comparison to Study 1 can be explained by local elections not generating as much attention as general elections. After the election, we collected voter registration data with help from the Electoral Commission, and matched them to our experimental assignment via the postcode sector column. Note that we were only able to obtain digitised data on registrations that also applied for a postal vote. While this is an important limitation, given the context of the Covid-19 pandemic, it is less severe than would have been the case in other periods. We discuss deviations from the PAP in detail in Appendix section C.3. We show non-reporting postcode sectors in Table C.18. As expected given

[5] We chose postcode sectors with a minimum of ten postcodes each. Our selection focused on sectors with a lower mean age and higher proportion of BAME residents compared to the overall average across sectors.

random assignment, we find no significant differences in whether control and treatment sectors report voter registration numbers, indicating that missingness is unlikely to have occurred as a function of the treatment. Balance checks are displayed in Table A.9 in the Appendix and show that census covariates are balanced across treatment and control sectors. Descriptive statistics of the covariates can be found in Appendix Table A.6. Descriptive statistics of the outcome variable can be found in Appendix Table A.5.

**Table 2: Digital Trial 2021 - Digital campaign statistics**

| | |
|---|---|
| Postcode sectors successfully targeted | 1978/1981 |
| Spend Instagram | £4000 |
| Spend Snapchat | £4000 |
| Total impressions | 2,983,790 |
| Total clicks | 13,804 |

## Research Design for Study 3: Social Media Ads from an Issue Advocacy Organisation

During the 2021 English local elections, we conducted a field experiment to examine the impact of digital ads and SMS text messages on voter registrations among members and sympathizers of an advocacy organization focused on affordable and decent housing for low-income individuals. The digital experiment was a collaborative effort between the community association and the researchers. The Facebook campaign specifically targeted individuals using the emails provided by the community association at the individual-level. Subjects had opted in to contact by the organisation. The sample comprised 9,290 individuals. Sample 1 encompassed 7,174 participants who shared both email addresses and phone numbers, while Sample 2 encompassed 2,116 participants with email addresses only. Random assignment for Sample 1 was based on block (by city) and cluster (by household) to two factors: Factor 1 determined the mode of contact (Facebook ads, SMS, or pure control group), and Factor 2 indicated the option of a contact number in the Facebook ads or SMS to aid with voter registration. Subjects in Sample 2 were randomly assigned to three conditions: pure control, Facebook ads, and Facebook adverts plus a callback option. In this paper, we focus on presenting and evaluating the effect of Facebook adverts only, as text messages are not within the scope of this study.[6]

[6]Those results are published elsewhere, and are null as well.

The Facebook campaign encompassed three distinct messages centered around the themes of 1) follow-through (days 1-3), 2) anti-sludge (days 4-6), and 3) social norms (days 7-9). These themes employed behavioral (non-cognitive) approaches to voter registration, recognizing that individuals might already be motivated to register, but may require support or encouragement to navigate the process effectively. The ads used in the campaign can be found in Section D.1. Similar to Study 2, the same ads were used, but for individual-level targeting, an additional option of 'callback' was included to offer assistance to subjects in the registration process, if requested. Two volunteers from the organization were assigned to distinct cities to provide assistance.

Registration and turnout data were collected from the local council premises, with the outcome (turnout) being a binary variable. Descriptive statistics can be found in Tables A.7 and A.8.

## Results

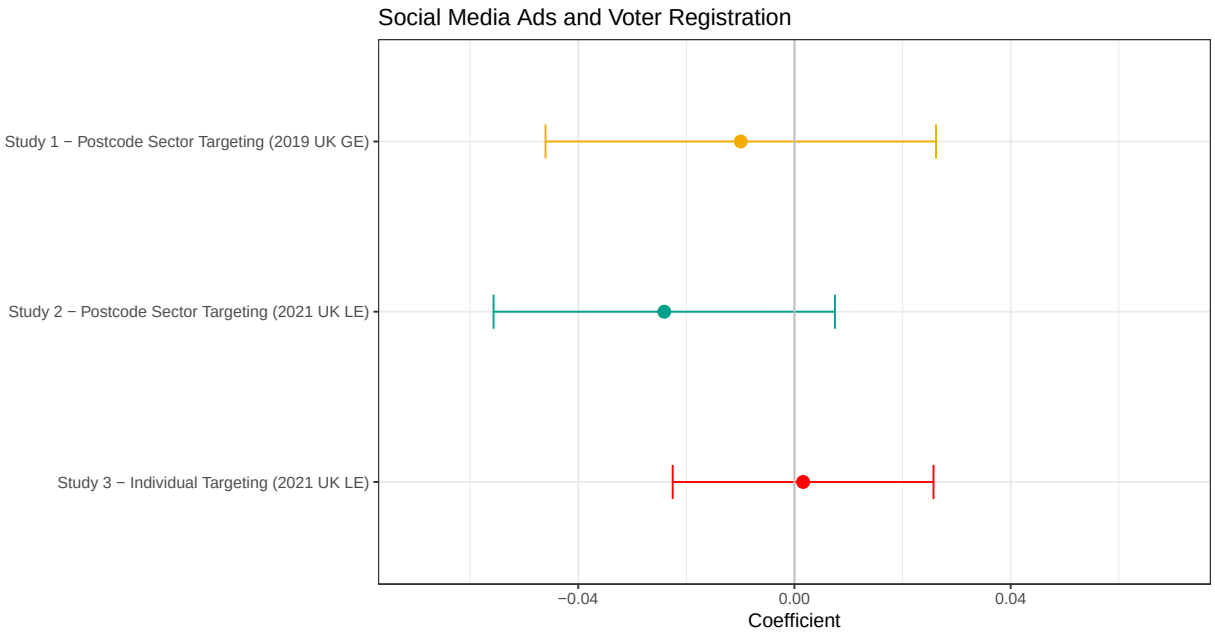
In all studies, we estimate the Intent-to-Treat (ITT) effect using linear regression with heteroskedasticity-robust (HC2) standard errors:

$$Y_s = \alpha + \beta_1 RegistrationAd_s + \gamma C_s + \epsilon_s \tag{1}$$

where $Y$ represents the population-scaled proportion of registered voters per postcode sector, ranging between 0 and 1, in Study 1 and Study 2, and a binary registration indicator in Study 3. *RegistrationAd* indicates whether a sector (Study 1 and 2) or an individual (Study 3) received voter registration ads (Snapchat, Instagram in Study 1; Instagram, Facebook in Study 2 and 3). $C$ stands for fixed effects for constituency (Study 1), region-postcode sector size (Study 2), and county (Study 3).

We present the estimated ITTs of all three trials in Figure 1. Recall that Study 1 was a campaign designed and conducted by a civic organization using cognitive messages, assigned at the postcode sector level. Extended results are displayed in Table B.10. Study 2 was conducted by the research team, assigned at the postcode sector level and used behavioral messages. The extended results can be found in Table C.17. Lastly, Study 3 is based on the advocacy organization's campaign, assigned at the individual level. We present the extended results of Study 3 in Table D.21. Across the three trials, we observe null effects of social media ads on voter registrations. The estimated

Intent-to-Treat effects across trials are -0.9 percentage-points (Study 1), -2 percentage points (Study 2), and 0.1 percentage points (Study 3). None of the estimated effects are substantively large and positive, or significantly different from zero. Moreover, across all trials, estimates larger than 2.3 percentage-points lie outside the estimated 95% CIs. Further details and robustness checks for each study can be found in the respective Appendix sections: B, C, and D.




Notes: Treatment effects can be interpreted as changes in percentage-point versus the control group. Study 1 and 2 coefficients are derived from covariate-adjusted full models, to reduce sampling variability. In Study 1 the dependent variable scales the absolute number of young people registered by population per postcode sector and in Study 2 it scales the absolute number of postal voters registered by population per postcode sector. In Study 3 the ITT coefficient is also derived from the covariate-adjusted model, reporting difference-in-proportions to allow for parallel interpretation.

**Figure 1: Comparison of the three trials**

Unsurprisingly, given the consistent null effects on registrations, in Study 3, where we were able to collect validated turnout data, we also find null effects on turnout (see Table D.23). Moreover, as pre-registered, we report heterogeneous effects of the digital ad campaign. Tables B.16 and C.20 show that we do not find any heterogeneous effects conditional on mean age or the mean share of BAME residents in a postcode sector.

## Conclusion

This study provides a sobering picture of consistent null effects obtained from three typical social media ad campaigns, evaluated via large randomised trials, aimed at under-registered groups. We have used Facebook but also other popular social media platforms, Instagram and Snapchat. The latter have received little attention from researchers conducting randomised campaign trials. Our results show that social media-based cognitive and non-cognitive mobilisation messages aimed at under-registered groups, such as ethnic minorities, private-sector tenants, and young people, were ineffective at increasing voter registrations. We can rule out medium to large effects on voter registration rates, an outcome that should be easier to affect via digital ads than turnout or vote choice, given that registration happens online in the UK. The causal evidence we provide raises questions about whether trying to increase the electoral participation of under-registered voters via social media campaigns is a promising electoral strategy. Of course, the busy campaign environment of the campaigns may have contributed to the null effects we observe because ads compete for attention with other campaign messages, and the environment might be saturated (Kalla and Broockman 2017). What would speak against such an interpretation is that results are consistently null, even in local elections, where the online space was less saturated with ads than in the 2019 General Election. Moreover, the digital ad campaigns received a relatively large number of online impressions and clicks, and would likely have been classified as "successful" campaigns, based on commonly measured digital soft outcomes.

Did the campaigns fail because of the medium, the message, the context, or a combination of all of the above? While this question is impossible to answer conclusively, the experimental designs of our studies provide variation along important dimensions. First, while all trials were conducted in Britain in a period of political upheaval (Fieldhouse et al. 2021a), there is important variation in context - one trial was conducted in the 2019 General Election, the other two in the 2021 local elections. While the 2019 trial was conducted before the Covid-19 pandemic, the 2021 trials were conducted during the Covid-19 pandemic, with social distancing rules still in place in the UK and a greater focus on postal voting. Moreover, while we cannot rule out that the underlying theoretical strategies aimed at cognitive and non-cognitive mobilisation could have been implemented more effectively via digital ads, we worked together with two different outside groups, and used focus

groups and A/B testing to trial the messages before fielding them in large-scale digital trials. What we find is that social media ads, no matter if they use cognitive, or non-cognitive mobilisation messages, or a combination of both, did not translate into higher registration rates. The results cannot rule out that the underlying theories are correct, given the variety of ways one can think of presenting and delivering such messages in the offline and online space. Studies that evaluate non-cognitive messages using methods other than digital ads have produced more positive results (Holbein and Hillygus 2020), which would point to the method of delivery as an important factor. While our trials focus on voter registration as the main outcome, the results are consistent with those obtained from trials that evaluated the effects of digital ads on turnout in a different country context, the United States (Aggarwal et al. 2023; Coppock forthcoming).

With all these caveats, we still believe that the findings based on the three trials reported here provide important evidence that digital ads did not result in detectable effects. Process-wise, registrations should be easier to increase than turnout, since individuals can complete the registration process online in one go. That leaves us with the question about the role of the target population, young people, ethnic minority voters and private sector renters. These populations are the natural target populations for digital registration campaigns in Britain because they contain the largest shares of non-registered individuals, and they are hard-to-reach offline. Moreover, we do not find any evidence of heterogeneous effects by age or share of ethnic-minority voters in any of the trials. While these results are likely underpowered to detect small differences in effect sizes conditional on demographic covariates, we doubt that heterogeneous effects could explain our findings. While the results of these trials are sobering, social media platforms will likely remain one medium of choice for many organisations that attempt to register voters. Given that even very small effects could easily scale on social media, the effects of digital ads remain an important topic to be studied via large-scale trials and meta-analyses. We hope that we have contributed some data points to that effort.

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Nickerson, D. W. (2007): "Does email boost turnout," *Quarterly Journal of Political Science*, 2, 369–379.

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# Contents

# Appendix

## A Descriptive statistics and balance tests

We provide descriptive summaries for Study 1 and Study 2 (postcode level targeting), displaying outcome variables in Table A.3 and Table A.5, respectively. Control variables are presented in Table A.4 and Table A.6, respectively. While we acknowledge the potential value of including additional control variables related to the socioeconomics of the postcode sectors, we were limited to using population-related census data from the year 2021 due to data availability during our analysis. Nonetheless, we have confidence in the validity of our results, as the treatment estimates obtained from the covariate-adjusted models align with those from the unadjusted models in both studies. Furthermore, in Study 1 and 2, we used 2011 census data to obtain the mean age in each sector, enabling us to test our pre-registered hypothesis effectively. For Study 3 (individual level targeting), we provide descriptive summaries in Table A.7 for the outcome variables and in Table A.8 for the covariate gender, which we obtained by predicting gender based on the names of the respondents. In Table A.9, we present balance tests for all three studies. These checks involve regressing treatment assignment on the covariates recorded in the 2021 census for Study 1 and Study 2, 2011 census data for mean age in Study 2, and gender information for Study 3. As anticipated, the results show no covariate imbalances between the treatment and control groups. Note that reduced sample sizes reflect missing data in covariates.

| | 0 (N=442) | 1 (N=437) | Total (N=879) | p value |
|---|---|---|---|---|
| **N of young reg.** | | | | 0.788 |
| N-Miss | 82 | 80 | 162 | |
| Mean (SD) | 291.117 (251.020) | 286.314 (226.108) | 288.725 (238.786) | |
| Range | 0.000 - 1336.000 | 0.000 - 1116.000 | 0.000 - 1336.000 | |
| **Pop. scaled-N of young reg.** | | | | 0.775 |
| N-Miss | 99 | 94 | 193 | |
| Mean (SD) | 0.345 (0.271) | 0.339 (0.253) | 0.342 (0.262) | |
| Range | 0.000 - 1.000 | 0.000 - 1.000 | 0.000 - 1.000 | |

**Table A.3: Descriptive statistics of Study 1 - Outcome variables**

| | 0 (N=442) | 1 (N=437) | Total (N=879) | p value |
|---|---|---|---|---|
| **All population** | | | | 0.280 |
| N-Miss | 71 | 65 | 136 | |
| Mean (SD) | 7468.550 (3558.634) | 7190.449 (3454.618) | 7329.312 (3507.339) | |
| Range | 252.000 - 20452.000 | 224.000 - 22366.000 | 224.000 - 22366.000 | |
| **Young population** | | | | 0.408 |
| N-Miss | 71 | 65 | 136 | |
| Mean (SD) | 1048.348 (670.696) | 1008.011 (656.561) | 1028.152 (663.516) | |
| Range | 29.000 - 4505.000 | 26.000 - 3744.000 | 26.000 - 4505.000 | |
| **BAME population** | | | | 0.396 |
| N-Miss | 71 | 65 | 136 | |
| Mean (SD) | 4151.178 (5068.743) | 3844.460 (4763.382) | 3997.612 (4917.307) | |
| Range | 4.000 - 25671.000 | 15.000 - 31588.000 | 4.000 - 31588.000 | |
| **Sector size** | | | | 0.611 |
| N-Miss | 1 | 0 | 1 | |
| Mean (SD) | 171.333 (79.751) | 168.609 (78.871) | 169.977 (79.281) | |
| Range | 1.000 - 380.000 | 1.000 - 388.000 | 1.000 - 388.000 | |
| **Number of overlap** | | | | 0.677 |
| Mean (SD) | 1.948 (0.802) | 1.970 (0.785) | 1.959 (0.793) | |
| Range | 1.000 - 5.000 | 1.000 - 5.000 | 1.000 - 5.000 | |
| **Mean age** | | | | 0.981 |
| N-Miss | 77 | 65 | 142 | |
| Mean (SD) | 38.877 (3.847) | 38.871 (3.604) | 38.874 (3.724) | |

**Table A.4: Descriptive statistics of Study 1 - Covariates**

| | 0 (N=990) | 1 (N=991) | Total (N=1981) | p value |
|---|---|---|---|---|
| **Pre-treatment N** | | | | 0.222 |
| N-Miss | 766 | 765 | 1531 | |
| Mean (SD) | 5297.446 (3702.545) | 4876.743 (3597.084) | 5086.160 (3651.970) | |
| Range | 1.000 - 14118.000 | 2.000 - 13868.000 | 1.000 - 14118.000 | |
| **Pop-scaled N of postal reg.** | | | | 0.029 |
| N-Miss | 636 | 614 | 1250 | |
| Mean (SD) | 0.133 (0.180) | 0.107 (0.141) | 0.119 (0.161) | |
| Range | 0.000 - 1.000 | 0.000 - 1.000 | 0.000 - 1.000 | |
| **N of postal reg.** | | | | 0.029 |
| N-Miss | 569 | 559 | 1128 | |
| Mean (SD) | 723.929 (819.855) | 609.306 (704.792) | 665.878 (765.452) | |
| Range | 1.000 - 3757.000 | 1.000 - 5117.000 | 1.000 - 5117.000 | |

**Table A.5: Descriptive statistics of Study 2 - Outcome variables**

| | 0 (N=990) | 1 (N=991) | Total (N=1981) | p value |
|---|---|---|---|---|
| **All population** | | | | 0.215 |
| N-Miss | 636 | 614 | 1250 | |
| Mean (SD) | 7827.308 (4362.671) | 7428.138 (4330.083) | 7621.443 (4347.501) | |
| Range | 131.000 - 21144.000 | 127.000 - 20555.000 | 127.000 - 21144.000 | |
| **Young population** | | | | 0.658 |
| N-Miss | 636 | 614 | 1250 | |
| Mean (SD) | 1377.958 (1060.254) | 1344.557 (976.107) | 1360.732 (1017.162) | |
| Range | 20.000 - 11069.000 | 19.000 - 5970.000 | 19.000 - 11069.000 | |
| **BAME population** | | | | 0.190 |
| N-Miss | 636 | 614 | 1250 | |
| Mean (SD) | 6254.633 (5213.730) | 5748.318 (5209.149) | 5993.510 (5213.949) | |
| Range | 39.000 - 25714.000 | 62.000 - 37581.000 | 39.000 - 37581.000 | |
| **Sector size** | | | | 0.793 |
| N-Miss | 2 | 1 | 3 | |
| Mean (SD) | 172.266 (68.641) | 173.068 (67.487) | 172.667 (68.050) | |
| Range | 3.000 - 346.000 | 6.000 - 380.000 | 3.000 - 380.000 | |
| **Mean age** | | | | 0.701 |
| Mean (SD) | 35.843 (3.702) | 35.906 (3.694) | 35.875 (3.697) | |
| Range | 23.900 - 44.409 | 24.564 - 44.428 | 23.900 - 44.428 | |

**Table A.6: Descriptive statistics of Study 2 - Covariates**

| | 0 (N=4044) | 1 (N=5246) | Total (N=9290) | p value |
|---|---|---|---|---|
| **Registered** | | | | 0.675 |
| N-Miss | 1083 | 1449 | 2532 | |
| Mean (SD) | 0.609 (0.488) | 0.604 (0.489) | 0.606 (0.489) | |
| Range | 0.000 - 1.000 | 0.000 - 1.000 | 0.000 - 1.000 | |

**Table A.7: Descriptive statistics of Study 3 - Outcome variables**

| | 0 (N=4044) | 1 (N=5246) | Total (N=9290) | p value |
|---|---|---|---|---|
| **Gender** | | | | 0.950 |
| N-Miss | 1031 | 1360 | 2391 | |
| Female | 1460 (48.5%) | 1876 (48.3%) | 3336 (48.4%) | |
| Male | 1302 (43.2%) | 1678 (43.2%) | 2980 (43.2%) | |
| Unknown | 251 (8.3%) | 332 (8.5%) | 583 (8.5%) | |

**Table A.8: Descriptive statistics of Study 3 - Covariates**

| | Study1 | Study2 | Study3 |
|---|---|---|---|
| Sector size | −0.0003 | −0.0012 | |
| | (0.0004) | (0.0009) | |
| Young population | 0.0000 | 0.0001 | |
| | (0.0001) | (0.0000) | |
| BAME population | −0.0000 | −0.0000 | |
| | (0.0000) | (0.0000) | |
| Pre-treatment registrations | | −0.0000 | |
| | | (0.0000) | |
| Mean age | | 0.0125 | |
| | | (0.0105) | |
| Sample | | | −0.1137*** |
| | | | (0.0138) |
| Male | | | 0.0032 |
| | | | (0.0125) |
| Other/Unknown | | | −0.0046 |
| | | | (0.0221) |
| FE | ✓ | ✓ | ✓ |
| $R^2$ | 0.0115 | 0.0559 | 0.0107 |
| Adj. $R^2$ | −0.0478 | 0.0003 | 0.0091 |
| Num. obs. | 743 | 415 | 6910 |
| RMSE | 0.5122 | 0.5005 | 0.4938 |

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

**Table A.9: Balance check based on regression of treatment assignment on the covariates**

# B Study 1

## B.1 Materials and Methods

In Figure B.1 we present typical example ads used in the Study 1 by the organisation. These messages were targeted towards young people aged between 18 and 35 years, and featured social media influencers. Note that the organisation's logo is blinded with black squares.

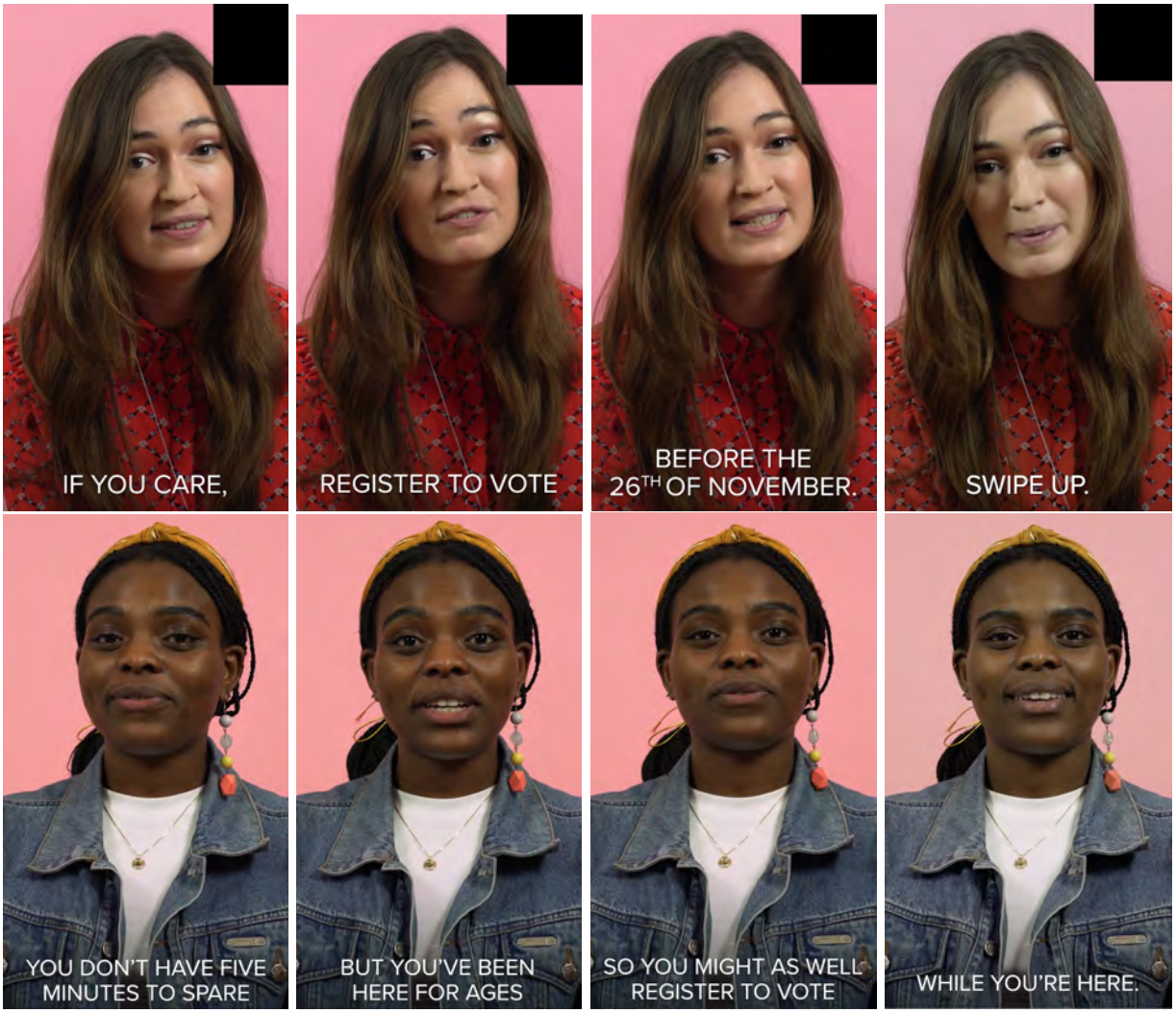


**Figure B.1: Voter Registration Ads**

## B.2 Results

In Table B.10, we present the Study 1 results, focusing on the young population-scaled proportion of registrations in each postcode sector (ages 18-35), as intended in our targeting. This variable is bounded between 0 and 1. The full model includes constituency fixed effects and controls for postcode sector size and the proportion of BAME residents in the area. In our sample, 162 out of 879 assigned postcode sectors did not record the age of the subjects, resulting in a reduced targeted (young) sample of 717 postcode sectors. Additionally, the sample was further reduced to 686 postcode sectors due to the absence of the young population variable, which we use to scale our dependent variable, in 31 sectors. Table B.11 displays the outcomes when standardizing the variable with the entire postcode sector population, but the results remain consistent. To handle missing registration data in postcode sectors due to being located within multiple parliamentary constituencies, we adopt two approaches. First, we conduct attrition checks (reported in Table B.12) by regressing non-reporting on treatment assignment. The results reveal no significant differences in reporting voter registration numbers between the treatment and control groups, suggesting that the treatment is unlikely to be the cause of missingness. Secondly, we introduce an overlap variable that quantifies the number of constituencies matching a postcode sector. In Table B.13, we control for this variable. In Table B.14 and Figure B.2, we narrow down the sample to complete vs. partial matches between postcode sectors and constituencies, with a complete match being the ideal scenario where a postcode sector corresponds to only one constituency. We then extend the sample to the full dataset, demonstrating the effect of the number of matches in each regression. These additional analyses affirm our earlier finding that missingness is unlikely to influence the results. In Table B.15, we present the results using the absolute outcome variable. In this analysis, we incorporate the proportion of young people in the sector as a control variable, instead of using it for scaling purposes. Notably, the coefficient associated with this control variable is negative and significant. Consequently, we opt to use the standardized version of the dependent variable as our primary results, as it offers a clearer and more meaningful interpretation. Finally, in Table B.16, we present pre-registered heterogeneous treatment effects by mean age. We do not find any significant heterogeneity conditional on mean age.

| | Basic model | Extended model | Full model |
|---|---|---|---|
| Treatment | −0.0087 | −0.0102 | −0.0099 |
| | (0.0185) | (0.0184) | (0.0184) |
| Sector Size | | −0.0003 | −0.0003 |
| | | (0.0002) | (0.0002) |
| Prop. of BAME | | | −0.0489 |
| | | | (0.0589) |
| Constituency FE | ✓ | ✓ | ✓ |
| PS size | - | ✓ | ✓ |
| Covariates | - | - | ✓ |
| $R^2$ | 0.2006 | 0.2045 | 0.2054 |
| Adj. $R^2$ | 0.1510 | 0.1538 | 0.1535 |
| Num. obs. | 686 | 686 | 686 |
| RMSE | 0.2416 | 0.2412 | 0.2412 |

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

**Table B.10: Registration of young people at postcode sector level scaled by young population**

| | Basic model | Extended model | Full model |
|---|---|---|---|
| Treatment | 0.0083 | 0.0080 | 0.0075 |
| | (0.0238) | (0.0238) | (0.0237) |
| Sector Size | | −0.0000 | −0.0001 |
| | | (0.0002) | (0.0002) |
| Prop. of BAME | | | 0.1194 |
| | | | (0.0742) |
| Constituency FE | ✓ | ✓ | ✓ |
| PS size | - | ✓ | ✓ |
| Covariates | - | - | ✓ |
| $R^2$ | 0.0997 | 0.0998 | 0.1037 |
| Adj. $R^2$ | 0.0439 | 0.0425 | 0.0452 |
| Num. obs. | 686 | 686 | 686 |
| RMSE | 0.3110 | 0.3112 | 0.3108 |

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

**Table B.11: Registration of all people at postcode sector level (Outcome variable: number of registrations scaled by all population)**

| | Basic model | Extended model | Full model |
|---|---|---|---|
| Treatment | −0.0032 | −0.0089 | 0.0006 |
| | (0.0253) | (0.0205) | (0.0177) |
| Sector Size | | −0.0029*** | −0.0004 |
| | | (0.0002) | (0.0003) |
| All population | | | −0.0000 |
| | | | (0.0000) |
| Prop. of BAME | | | −0.0727 |
| | | | (0.0670) |
| Constituency FE | ✓ | ✓ | ✓ |
| PS size | - | ✓ | ✓ |
| Covariates | - | - | ✓ |
| $R^2$ | 0.0995 | 0.4096 | 0.2211 |
| Adj. $R^2$ | 0.0565 | 0.3806 | 0.1732 |
| Num. obs. | 879 | 878 | 743 |
| RMSE | 0.3768 | 0.3047 | 0.2422 |

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

**Table B.12: Sectors with no young registrations (Outcome variable: variable indicating missing sectors)**

| | Basic model | Extended model | Full model |
|---|---|---|---|
| Treatment | −0.0017 | −0.0030 | −0.0030 |
| | (0.0172) | (0.0172) | (0.0172) |
| Sector Size | | −0.0003 | −0.0003 |
| | | (0.0002) | (0.0002) |
| Prop. of BAME | | | −0.0206 |
| | | | (0.0539) |
| Number of overlap | −0.1163*** | −0.1158*** | −0.1155*** |
| | (0.0108) | (0.0108) | (0.0107) |
| Constituency FE | ✓ | ✓ | ✓ |
| PS size | - | ✓ | ✓ |
| Covariates | - | - | ✓ |
| $R^2$ | 0.3066 | 0.3095 | 0.3097 |
| Adj. $R^2$ | 0.2624 | 0.2644 | 0.2634 |
| Num. obs. | 686 | 686 | 686 |
| RMSE | 0.2251 | 0.2248 | 0.2250 |

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

**Table B.13: Controlling for the number of constituencies a postcode sector matches to**

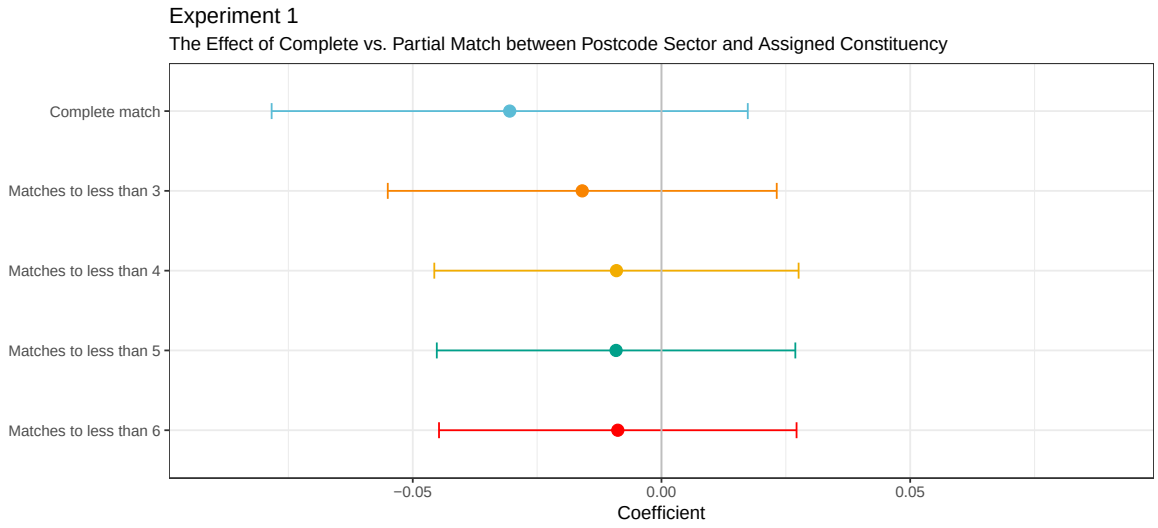


**Figure B.2: The effect of complete vs. partial match between postcode sector and its assigned constituency**

| | Complete match | Less than 3 | Less than 4 | Less than 5 | Less than 6 |
|---|---|---|---|---|---|
| Treatment | −0.0325 | −0.0163 | −0.0103 | −0.0103 | −0.0099 |
| | (0.0247) | (0.0200) | (0.0187) | (0.0184) | (0.0184) |
| Sector size | −0.0002 | −0.0003 | −0.0003 | −0.0003 | −0.0003 |
| | (0.0002) | (0.0002) | (0.0002) | (0.0002) | (0.0002) |
| Prop. of BAME | −0.3124* | −0.1120 | −0.0644 | −0.0464 | −0.0489 |
| | (0.1292) | (0.0712) | (0.0620) | (0.0597) | (0.0589) |
| $R^2$ | 0.5078 | 0.2418 | 0.2057 | 0.2027 | 0.2054 |
| Adj. $R^2$ | 0.4006 | 0.1760 | 0.1517 | 0.1504 | 0.1535 |
| Num. obs. | 219 | 527 | 661 | 684 | 686 |
| RMSE | 0.1673 | 0.2312 | 0.2408 | 0.2415 | 0.2412 |

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

**Table B.14: The Effect of Complete vs. Partial Match between Postcode Sector and Assigned Constituency (Outcome variable: number of registrations scaled by young population)**

| | Basic model | Extended model | Full model |
|---|---|---|---|
| Treatment | −6.6783 | −3.4317 | −4.2512 |
| | (16.9022) | (15.3561) | (15.6721) |
| Sector Size | | 1.6408*** | 1.3159*** |
| | | (0.1508) | (0.1926) |
| Young population | | | 0.0520* |
| | | | (0.0240) |
| Prop. of BAME | | | 130.1530* |
| | | | (51.5747) |
| Constituency FE | ✓ | ✓ | ✓ |
| PS size | - | ✓ | ✓ |
| Covariates | - | - | ✓ |
| $R^2$ | 0.1623 | 0.3018 | 0.3143 |
| Adj. $R^2$ | 0.1127 | 0.2594 | 0.2683 |
| Num. obs. | 717 | 717 | 686 |
| RMSE | 224.9308 | 205.4909 | 204.8955 |

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

**Table B.15: Registration of young people at postcode sector level (Outcome variable: absolute number of registrations)**

| | Preregistered HTE |
|---|---|
| Treatment | 0.0481 |
| | (0.1992) |
| Mean age | 0.0234*** |
| | (0.0049) |
| Sector size | −0.0003 |
| | (0.0002) |
| Prop. of BAME | 0.1148 |
| | (0.0737) |
| Treatment x Mean age | −0.0016 |
| | (0.0053) |
| Constituency FE | ✓ |
| PS size | ✓ |
| Covariates | ✓ |
| $R^2$ | 0.2465 |
| Adj. $R^2$ | 0.1929 |
| Num. obs. | 664 |
| RMSE | 0.2368 |

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

**Table B.16: Heterogeneous effects: Registration of young people at postcode sector level scaled by young population**

## B.3 Deviations from the Pre-Analysis Plan

The Pre-Analysis Plan can be found in Section E. We cannot provide a direct link to OSF due to privacy reasons, as the pre-registered PDF contains non-de-identified information. In what follows, we outline and explain any deviations from the Pre-Analysis Plan.

### B.3.1 Levels of outcome measurement

We pre-registered that we would define the outcome variable as the absolute number of registrations per household, and as the absolute number of registrations per polling district. As anticipated in the PAP, due to data protection concerns, we were unable to obtain outcome data at the household level. Moreover, while we were able to obtain outcome data at the polling district level, polling districts are not perfectly nested within postcode sectors, the unit of assignment. We failed to identify this issue in advance. Since single polling districts often cross multiple postcode sectors, the same polling district would be assigned to multiple experimental conditions at once, and therefore results would mechanically be biased towards zero. We therefore decided to record the outcome at the level of assignment, the 4-digit postcode sector.

### B.3.2 Secondary outcome variable: Turnout

As pre-registered, we also intended to test if Get-Out-The-Vote reminders assigned via a factorial design and sent via Instagram and Snapchat 2-3 days before the election, amplified the Voter Registration ads' effect on turnout, but we were unable to obtain validated voter turnout data for 37 out of 40 constituencies. Since the GOTV messages were all sent after the voter registration deadline, the voter registration outcomes reported in this paper could not have been influenced by GOTV ads. Since the effects of the voter registration ads on voter registrations are null, our best guess is that any downstream effects on turnout will also have been null.

### B.3.3 Inclusion of Census Covariates

As pre-registered, we intended to use voter registration in the 2017 UK General Election for covariate-adjustment, but we were unable to obtain the 2017 registers. We anticipated that this might happen in the PAP. To gain statistical power via covariate-adjustment, we instead use 2021 Census covariates as a second best option. We did not pre-register the Census covariates because we did not think about the possibility of matching the 2011 Census to treatment assignment via place identifiers. Covariate-adjustment improves precision, but as would be expected given random assignment, point estimates are similar.

# C Study 2

## C.1 Materials and Methods

Drawing from research by Holbein and Hillygus (2020) and considering our null results using cognitive approaches in Study 1, we devised three types of behavioral messages for Study 2. These approaches include the (i) Anti-Sludge, (ii) Follow-Through, and (iii) Dynamic Norms approach. The corresponding social media ads are displayed in Figures C.3, C.4, C.5, and C.6. We collaborated with a Graphic Designer to develop these ads and tested them in a focus group and survey experiment to determine the final ad graphics. Note that our university logo is blinded with black squares.


**Figure C.3: Digital ad using Anti-Sludge for Study 2, version 1**

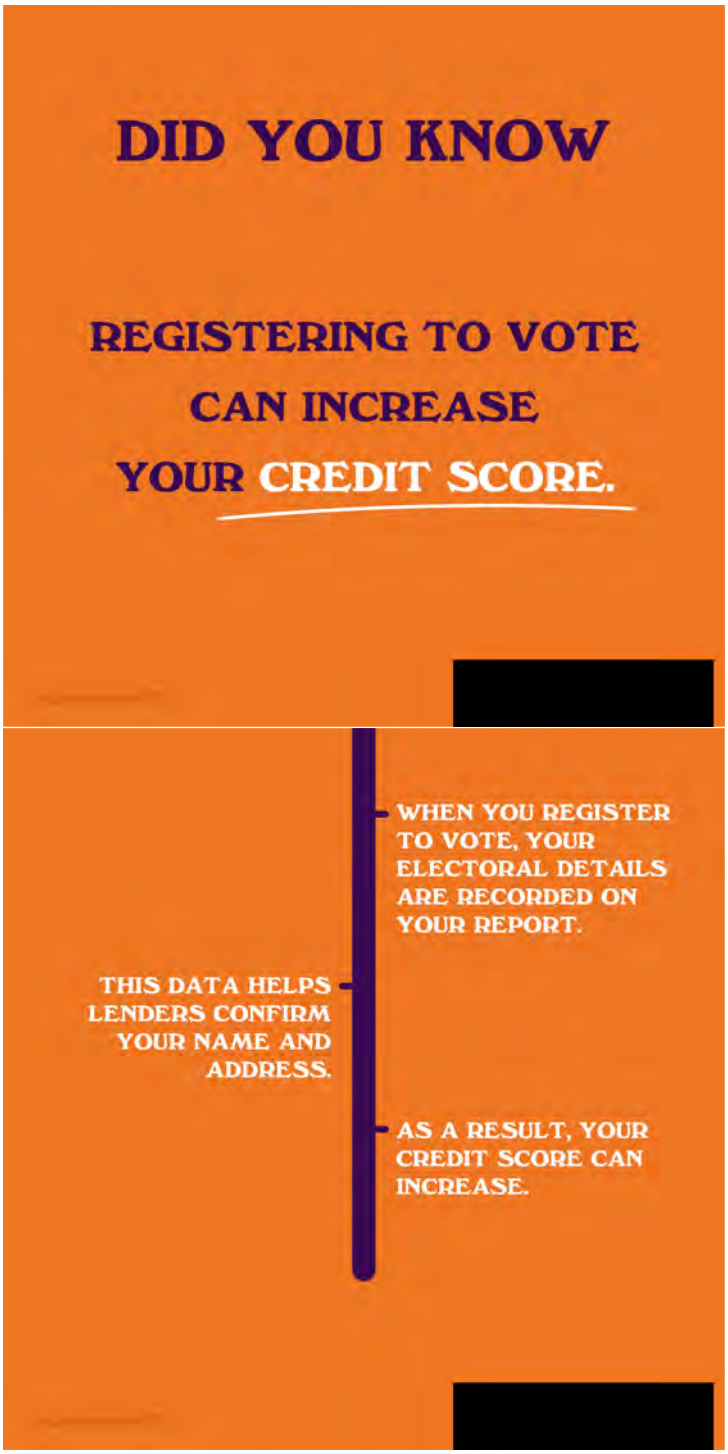


**Figure C.4: Digital ad using Anti-Sludge for Study 2, version 2**

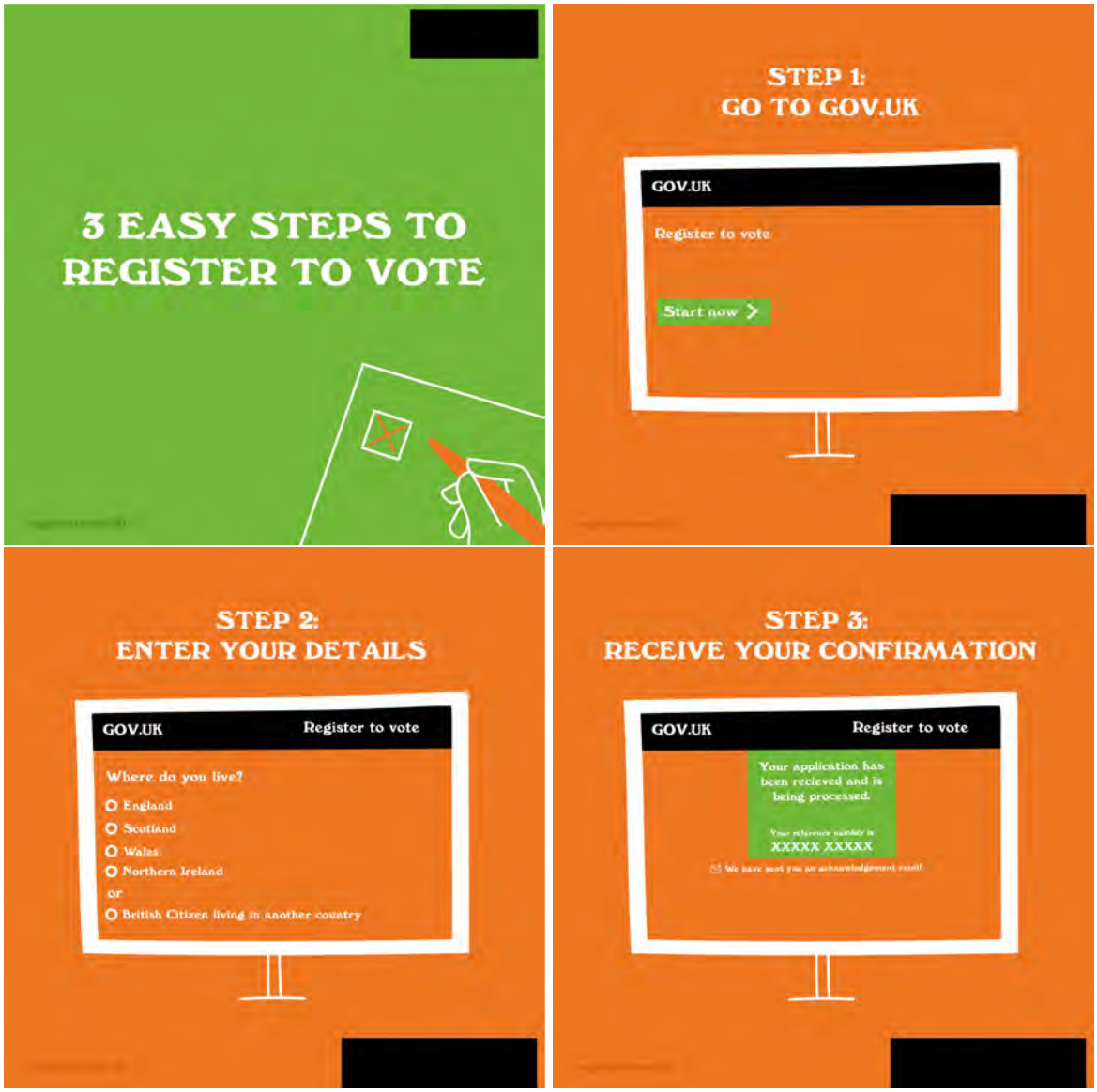


**Figure C.5: Digital ad using Follow-Through for Study 2**

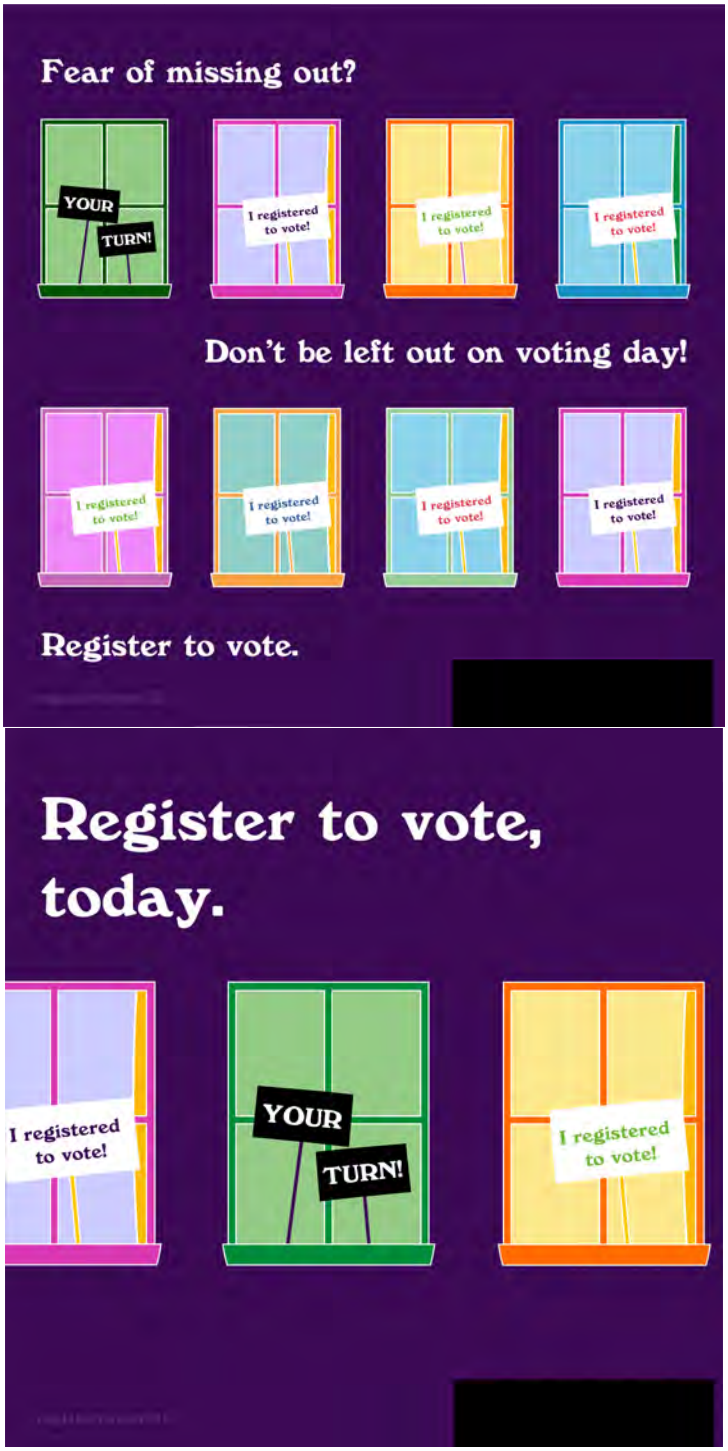


**Figure C.6: Digital ad using Dynamic Norms for Study 2**

## C.2 Results

Table C.17 presents the results of Study 2. The dependent variable is the population-scaled proportion of people who registered to vote and requested a postal ballot in each postcode sector, with the variable bound between 0 and 1. This dependent variable diverges from the PAP, where we registered that we would look at the overall number of registrations (not only those who also requested a postal ballot). The reason for this change is that we were only able to obtain the number of registered postal voters centrally from the Electoral Commission. Given the context of the first set of elections conducted in the Covid-19 pandemic, a much larger share of individuals registered to vote by post. Moreover, since the postal voting deadline was just slightly later than the registration deadline, the timing of the campaign is unlikely to have made a difference. Note that we were only partially able to obtain pre-treatment registrations from the Electoral Commission for this study, which lead to a reduced sample size for analysis. Although we initially assigned 1981 postcode sectors to treatment and control groups, we were only able to gather data for 1592 postcode sectors from the Electoral Commission. The reason for this limitation is that these specific postcode sectors did not have postal voters included in their digital databases. Out of these, only 450 postcode sectors had pre-treatment registration numbers available. Furthermore, as we included additional Census controls, the number of observations further decreased to 415 postcode sectors. We use this sample for our main analysis, but also report the findings on the sample of 1592 postcode sectors. The results are unchanged. The full model includes region fixed effects and controls for postcode sector size, the number of registrations pre-treatment, the proportion of BAME residents in the area, and the mean age in the sector. In Table C.18 we conduct attrition checks, reporting missingness between treatment and control groups. As expected, we do not find significant differences in reporting postal voter registration numbers between the two groups. In Table C.19, we present the results using the absolute outcome variable. For this analysis, we include the proportion of young people in the sector as a control variable, rather than using it for scaling the outcome variable. As pre-registered, we present heterogeneous treatment effects by mean age and the proportion of BAME population at the postcode sector in Table C.20. However, our analysis did not yield statistically significant heterogeneity by these covariates.

| | Basic model | Extended model | Full model |
|---|---|---|---|
| Treatment | −0.0250 | −0.0245 | −0.0241 |
| | (0.0160) | (0.0160) | (0.0161) |
| Pre-treatment registration | 0.0000*** | 0.0000*** | 0.0000** |
| | (0.0000) | (0.0000) | (0.0000) |
| Sector size | | 0.0002 | 0.0002 |
| | | (0.0002) | (0.0002) |
| Mean age | | | 0.0022 |
| | | | (0.0038) |
| Prop. of BAME | | | 0.0701 |
| | | | (0.0511) |
| Regional FE | ✓ | ✓ | ✓ |
| Pre-treatment N | ✓ | ✓ | ✓ |
| PS size | - | ✓ | ✓ |
| Covariates | - | - | ✓ |
| $R^2$ | 0.1459 | 0.1469 | 0.1548 |
| Adj. $R^2$ | 0.1025 | 0.1013 | 0.1050 |
| Num. obs. | 415 | 415 | 415 |
| RMSE | 0.1461 | 0.1462 | 0.1459 |

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

**Table C.17: Registration of postal voters at the postcode sector level scaled by population**

| | Basic model | Extended model | Full model |
|---|---|---|---|
| Treatment | −0.0107 | 0.0000 | 0.0000 |
| | (0.0196) | (0.0000) | (0.0000) |
| Pre-treatment registration | | 0.0000 | 0.0000 |
| | | (0.0000) | (0.0000) |
| Sector size | | | 0.0000 |
| | | | (0.0000) |
| Mean age | | | 0.0000 |
| | | | (0.0000) |
| Prop. of BAME | | | 0.0000 |
| | | | (0.0000) |
| Regional FE | ✓ | ✓ | ✓ |
| PS size | - | ✓ | ✓ |
| Covariates | - | - | ✓ |
| $R^2$ | 0.2301 | | |
| Adj. $R^2$ | 0.2207 | | |
| Num. obs. | 1981 | 450 | 415 |
| RMSE | 0.4372 | 0.0000 | 0.0000 |

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

**Table C.18: Sectors with no postal registrations (Outcome variable: variable indicating missing sectors)**

| | Basic model | Extended model | Full model |
|---|---|---|---|
| Treatment | −147.4840* | −135.4906 | −146.9823 |
| | (70.2062) | (70.5334) | (75.3603) |
| Pre-treatment registration | 0.1163*** | 0.1089*** | 0.1068*** |
| | (0.0126) | (0.0123) | (0.0126) |
| Sector size | | 2.4564* | 1.9735 |
| | | (1.0487) | (1.2977) |
| Mean age | | | 25.4002 |
| | | | (15.4346) |
| Prop. of BAME | | | 258.2705 |
| | | | (155.6409) |
| Prop. of young people | | | −382.2825 |
| | | | (568.9355) |
| Regional FE | ✓ | ✓ | ✓ |
| PS size | - | ✓ | ✓ |
| Covariates | - | - | ✓ |
| $R^2$ | 0.3520 | 0.3598 | 0.3522 |
| Adj. $R^2$ | 0.3218 | 0.3284 | 0.3124 |
| Num. obs. | 450 | 450 | 415 |
| RMSE | 715.8433 | 712.3688 | 731.2907 |

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

**Table C.19: Registration of postal voters at the postcode sector level**

| | Pre-registered HTE 1 | Pre-registered HTE 2 |
|---|---|---|
| Treatment | 0.1568 | −0.0639 |
| | (0.1685) | (0.0505) |
| Mean age | 0.0046 | 0.0024 |
| | (0.0049) | (0.0037) |
| Pre-treatment registration | 0.0000** | 0.0000** |
| | (0.0000) | (0.0000) |
| Sector size | 0.0002 | 0.0002 |
| | (0.0002) | (0.0002) |
| Prop. of BAME | 0.0686 | 0.0422 |
| | (0.0509) | (0.0638) |
| Treatment x Mean age | −0.0051 | |
| | (0.0047) | |
| Treatment x Prop. of BAME | | 0.0561 |
| | | (0.0608) |
| Regional FE | ✓ | ✓ |
| Pre-treatment N | ✓ | ✓ |
| PS size | ✓ | ✓ |
| Covariates | ✓ | ✓ |
| $R^2$ | 0.1571 | 0.1573 |
| Adj. $R^2$ | 0.1053 | 0.1055 |
| Num. obs. | 415 | 415 |
| RMSE | 0.1459 | 0.1459 |

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

**Table C.20: Heterogeneous Effects: Registration of postal voters at the postcode sector level scaled by population**

## C.3 Deviations from the Pre-Analysis Plan

The Pre-Analysis Plan for this study is available via https://osf.io/dt76p/. In what follows we outline and explain any deviations from the Pre-Analysis Plan.

### C.3.1 Levels of outcome measurement

In our collection of registration data, we have focused on postal voters (despite registering that we would collect outcomes for all voters) because digitalized records were only available for this segment of the population, as provided by the Electoral Commission. Although this limitation restricts our analysis, we want to clarify that the missing data for non-postal voters is not due to any deliberate treatment bias or intentional selection process. We have no reasons to believe that the lack of digitalized records for other voter segments is related to the treatment.

### C.3.2 Secondary outcome variable: Turnout

Despite our pre-registration, we were unable to assess the impact of the digital ads campaign on validated votes due to the unavailability of digitalized and validated voter turnout data.

# D Study 3

## D.1 Materials and Methods

Drawing from research by Holbein and Hillygus (2020) and considering our null results using cognitive approaches in Study 1, we devised three types of behavioral messages for Study 3. These approaches include the (i) Anti-Sludge, (ii) Follow-Through, and (iii) Dynamic Norms approach. The corresponding social media ads are displayed in Figures D.7, D.8, D.9, D.10 and D.11. The following ads from D.12 to D.16 correspond to the same three behavioural strategies but include a callback option. We collaborated with a Graphic Designer to develop these ads and tested them in a focus group and survey experiment to determine the final ad graphics. Note that the organisation's logo is obscured with black squares.

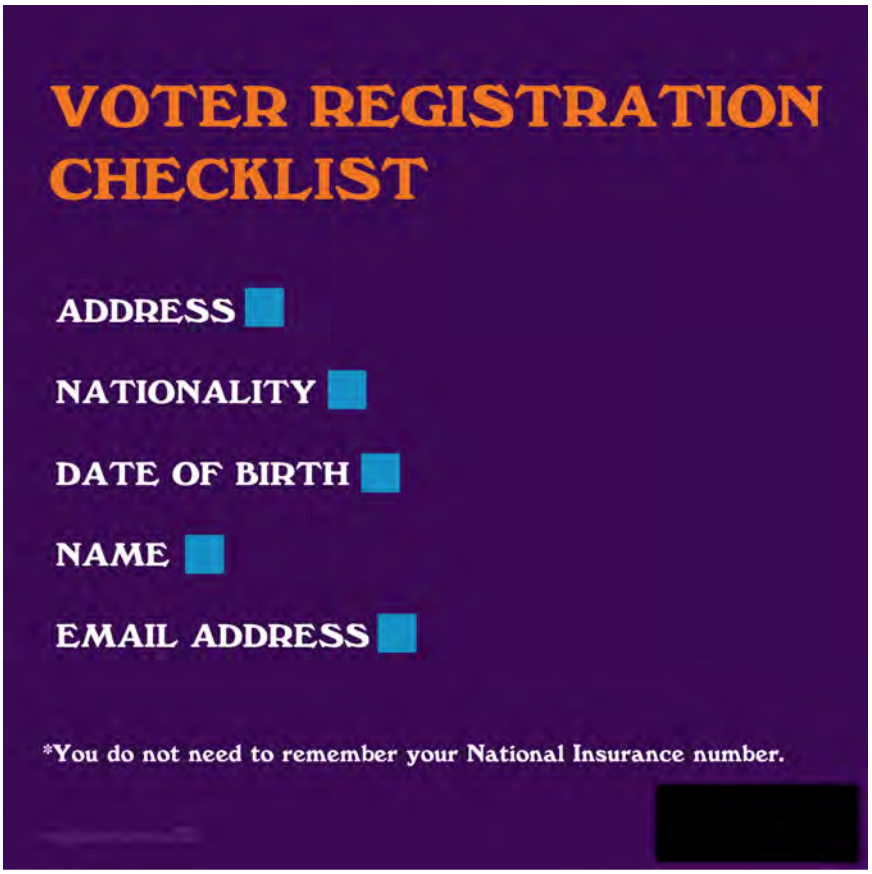


**Figure D.7: Digital ad using Anti-Sludge for Study 3, version 1**

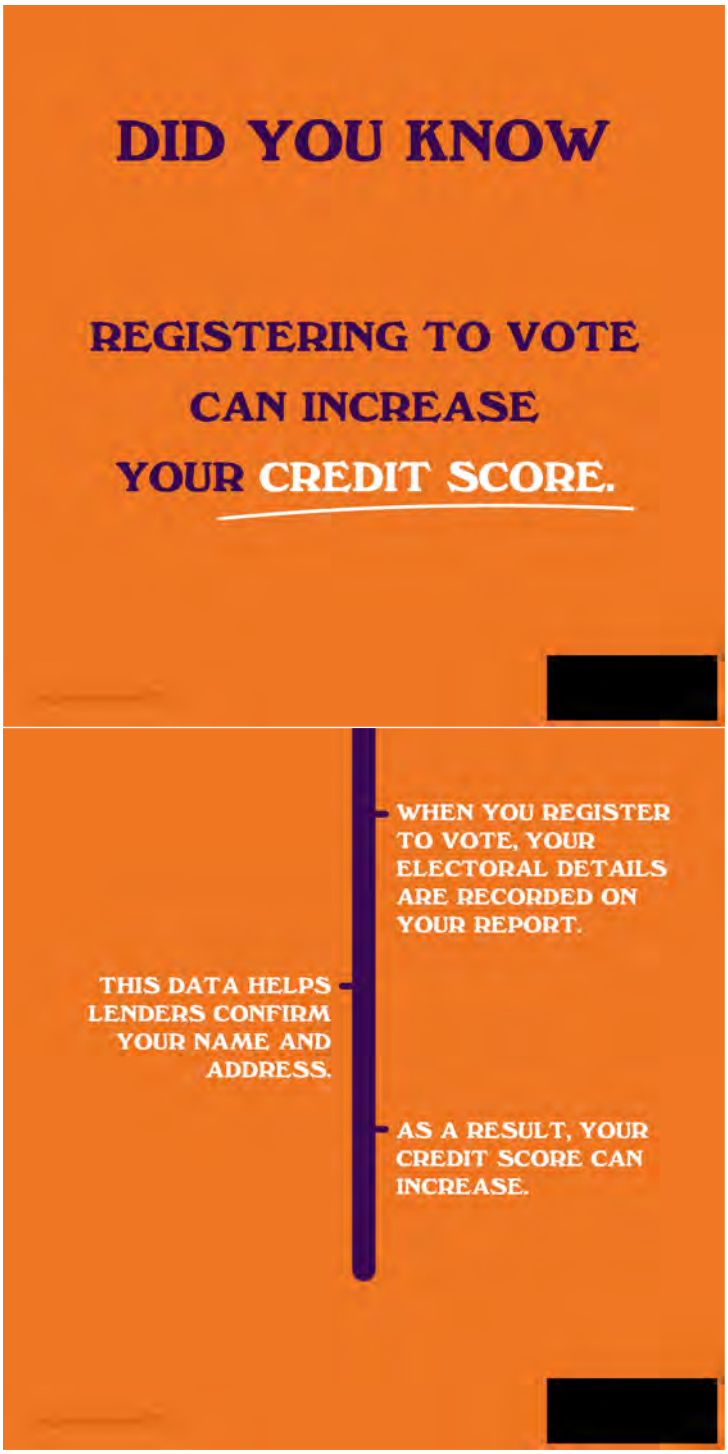


**Figure D.8: Digital ad using Anti-Sludge for Study 3, version 2**

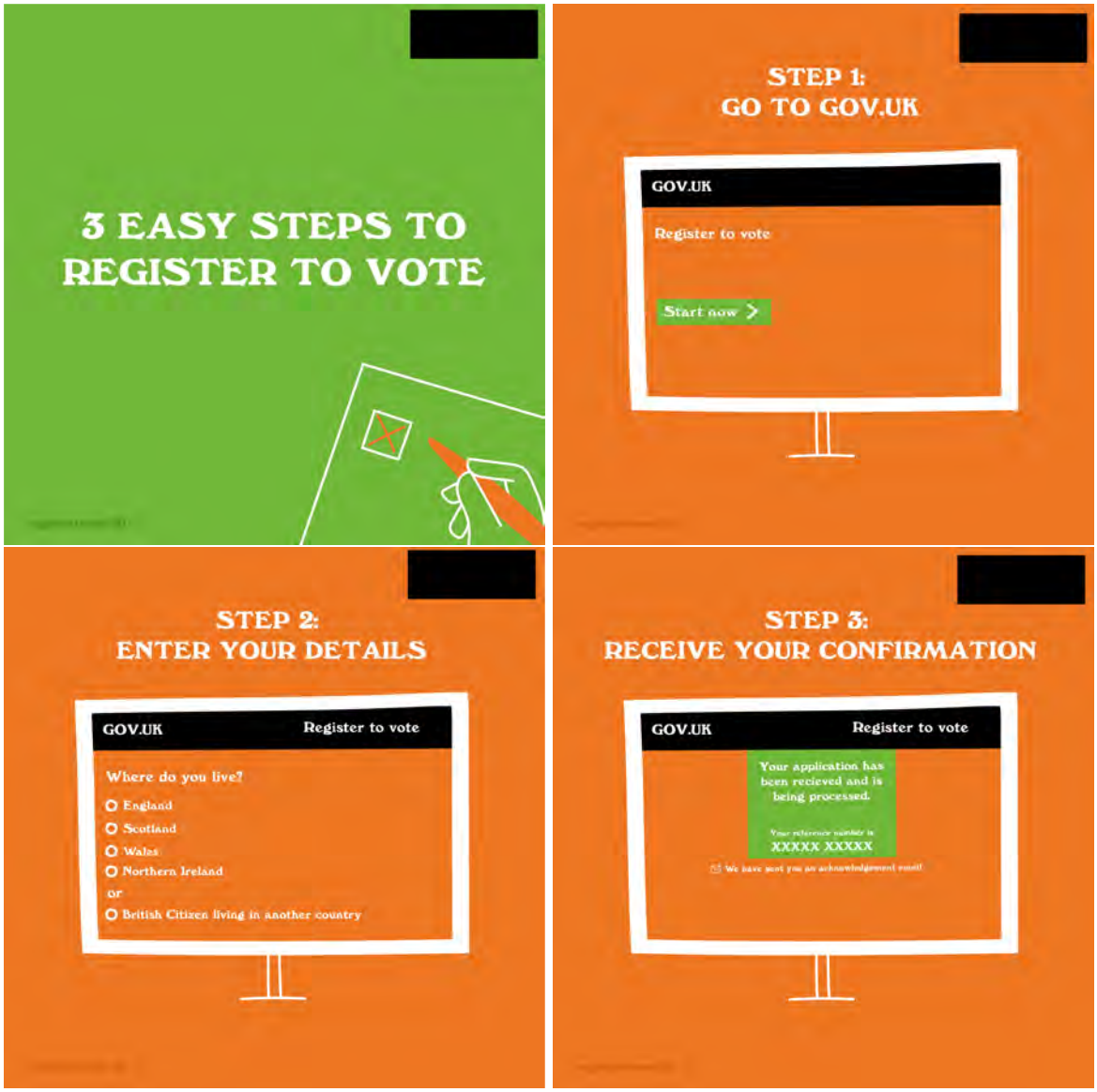


**Figure D.9: Digital ad using Follow-Through for Study 3, version 1**

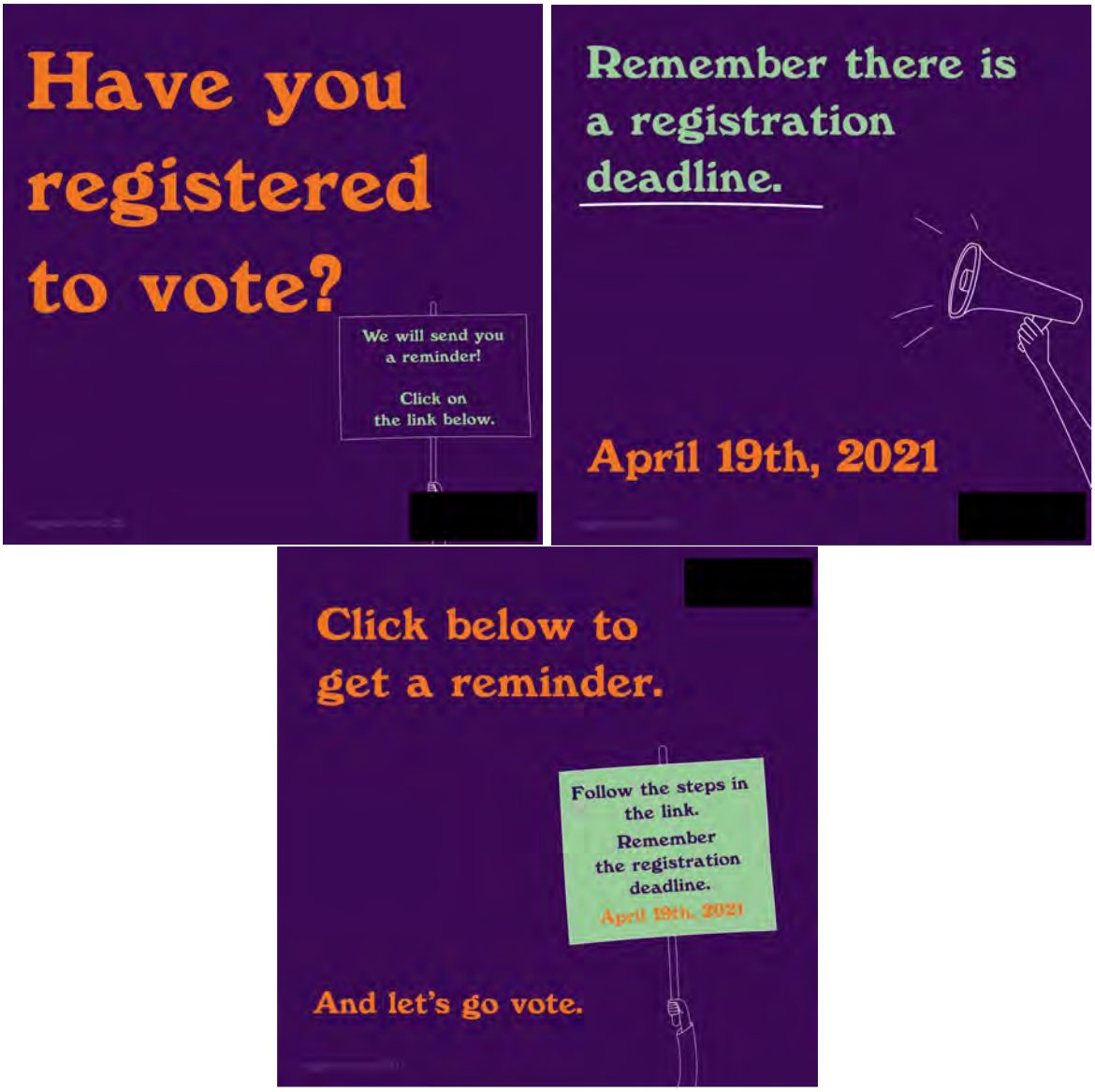


**Figure D.10: Digital ad using Follow-Through for Study 3, version 2**

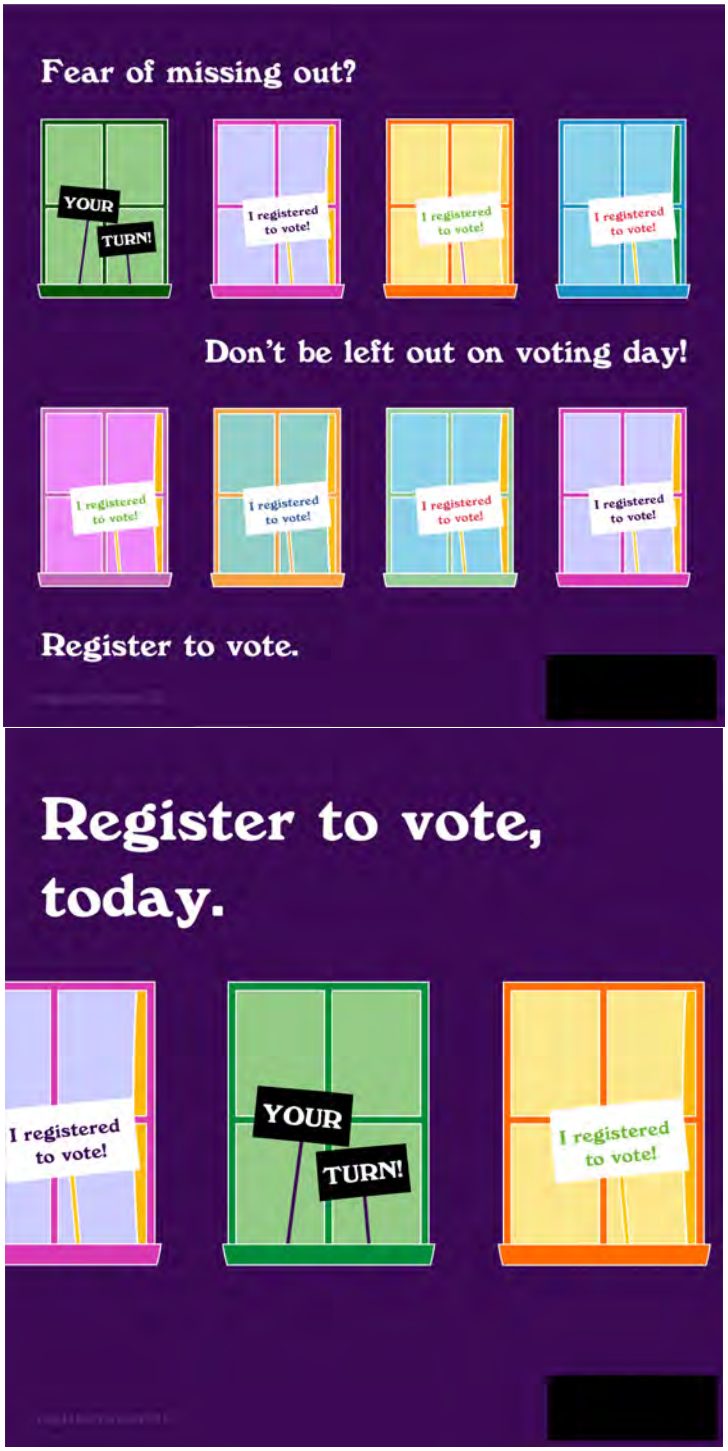


**Figure D.11: Digital ad using Dynamic Norms for Study 3**


**Figure D.12: Digital ad using Anti-Sludge with callback option, option 1**

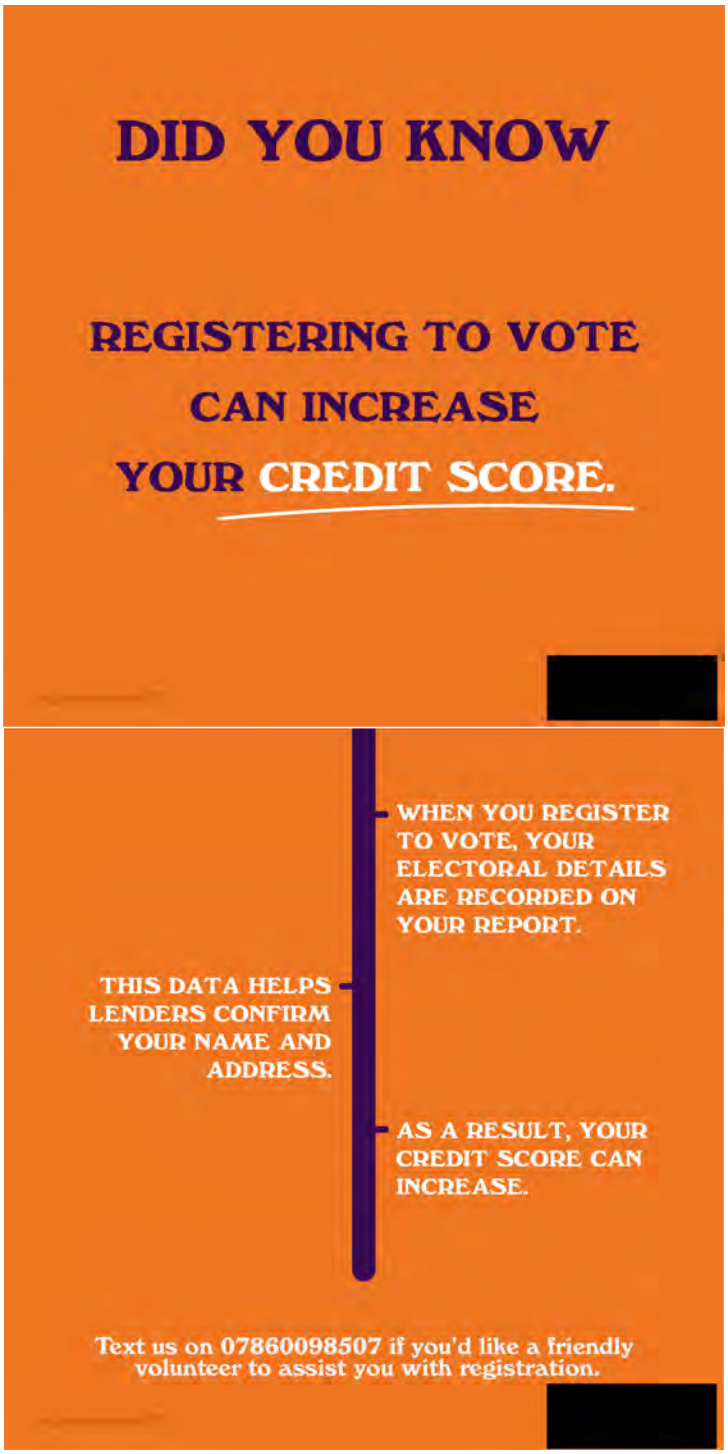


**Figure D.13: Digital ad using Anti-Sludge with callback option, option 2**

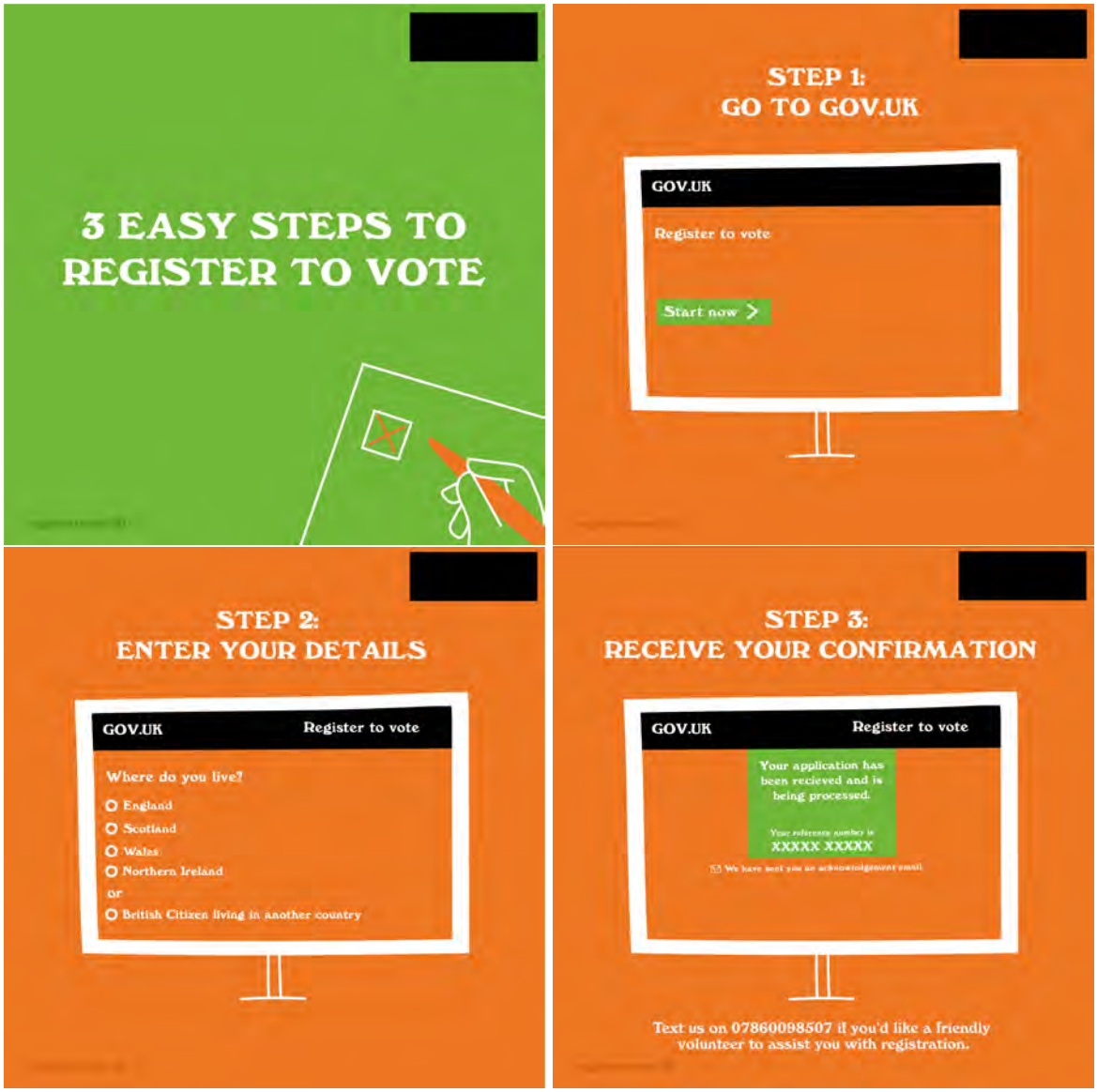


**Figure D.14: Digital ad using Follow-Through with callback option, option 1**

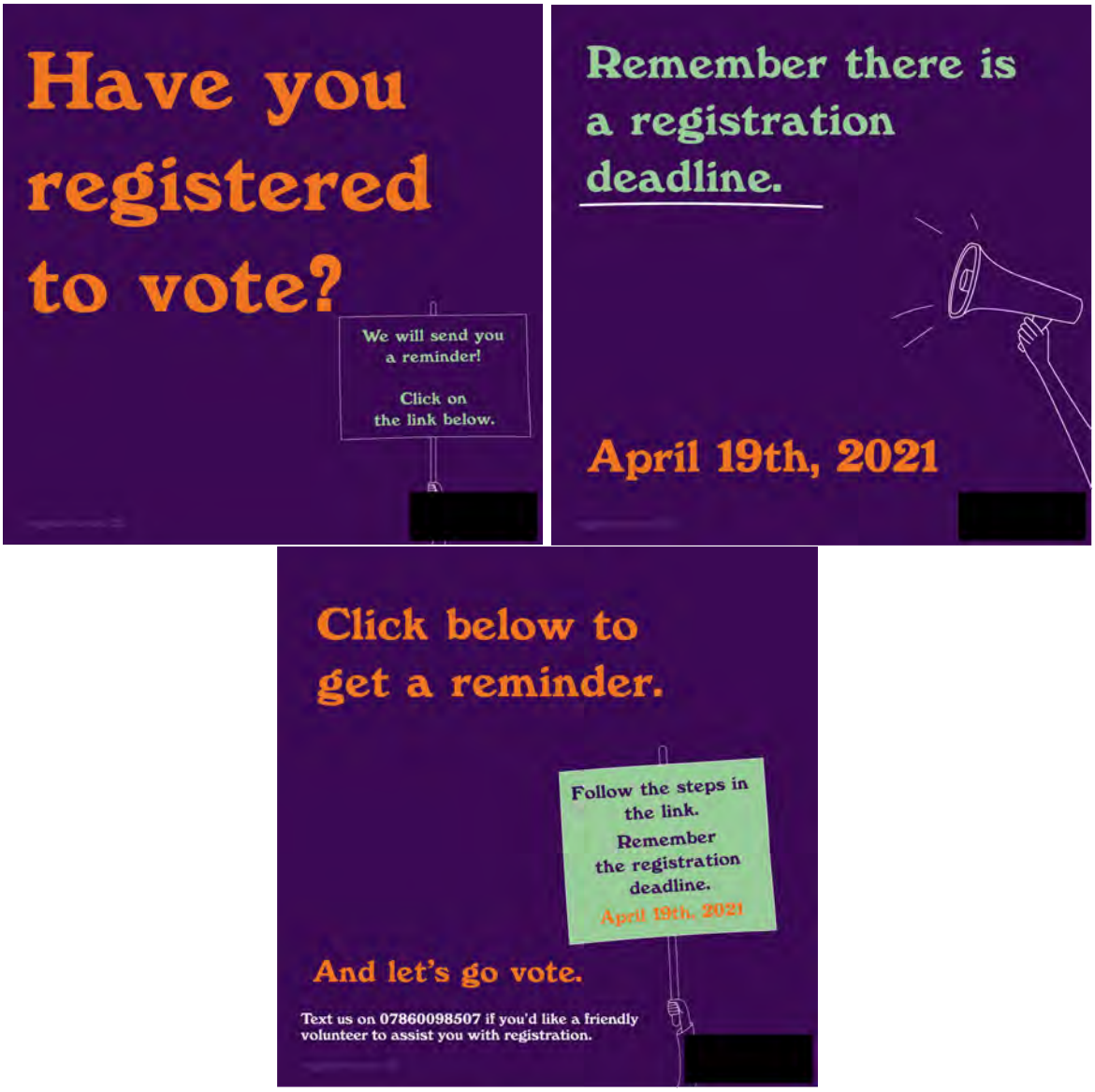


**Figure D.15: Digital ad using Follow-Through with callback option, option 2**

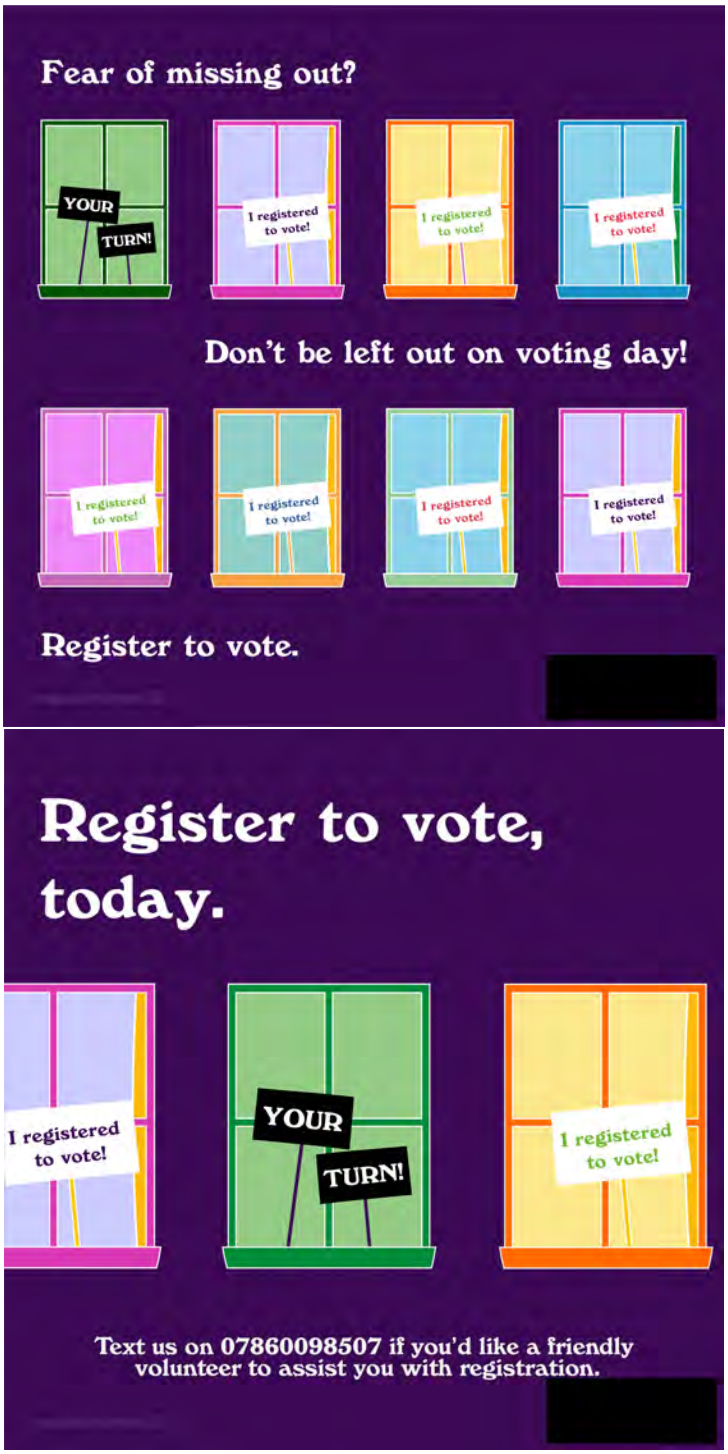


**Figure D.16: Digital ad using Dynamic Norms with a callback option**

## D.2 Results

Table D.21 presents the results of Study 3, focusing on individual-level digital targeting for voter registration. Furthermore, in Table D.22, we display the effect of the callback offer, and Table D.23 illustrates the treatment effect on the dependent variable turnout. Our analysis does not reveal any significant effect of the callback option on voter registration or digital targeting on turnout.

| | Basic model | Extended model | Full model |
|---|---|---|---|
| Treatment | −0.0047 | 0.0017 | 0.0016 |
| | (0.0123) | (0.0123) | (0.0123) |
| Sample | | 0.0795*** | 0.0795*** |
| | | (0.0150) | (0.0150) |
| Gender - Male | | | −0.0312* |
| | | | (0.0123) |
| Gender - Unknown/Other | | | −0.0455* |
| | | | (0.0220) |
| County FE | ✓ | ✓ | ✓ |
| Sample | - | ✓ | ✓ |
| Covariates | - | - | ✓ |
| $R^2$ | 0.0059 | 0.0105 | 0.0117 |
| Adj. $R^2$ | 0.0045 | 0.0090 | 0.0100 |
| Num. obs. | 6758 | 6758 | 6758 |
| RMSE | 0.4875 | 0.4864 | 0.4862 |
| N Clusters | 6298 | 6298 | 6298 |

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$; $^{\cdot}p < 0.1$

**Table D.21: Individual-level voter registration**

| | Basic model | Extended model | Full model |
|---|---|---|---|
| Ad with callback | −0.0133 | −0.0133 | −0.0137 |
| | (0.0205) | (0.0205) | (0.0205) |
| Sample | 0.0859*** | 0.0859*** | 0.0877*** |
| | (0.0202) | (0.0202) | (0.0202) |
| Gender - Male | | | −0.0574** |
| | | | (0.0183) |
| Gender - Unknown/Other | | | −0.0165 |
| | | | (0.0316) |
| County FE | ✓ | ✓ | ✓ |
| Sample | - | ✓ | ✓ |
| Covariates | - | - | ✓ |
| $R^2$ | 0.0153 | 0.0153 | 0.0184 |
| Adj. $R^2$ | 0.0121 | 0.0121 | 0.0145 |
| Num. obs. | 3070 | 3070 | 3070 |
| RMSE | 0.4883 | 0.4883 | 0.4877 |
| N Clusters | 2822 | 2822 | 2822 |

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$; $^{\cdot}p < 0.1$

**Table D.22: Individual-level voter registration - callback option**

| | Basic model | Extended model | Full model |
|---|---|---|---|
| Treatment | 0.0069 | 0.0069 | 0.0067 |
| | (0.0130) | (0.0130) | (0.0130) |
| Sample | 0.0548*** | 0.0548*** | 0.0546*** |
| | (0.0152) | (0.0152) | (0.0152) |
| Gender - Male | | | −0.0090 |
| | | | (0.0130) |
| Gender - Unknown/Other | | | −0.0201 |
| | | | (0.0236) |
| County FE | ✓ | ✓ | ✓ |
| Sample | - | ✓ | ✓ |
| Covariates | - | - | ✓ |
| $R^2$ | 0.0648 | 0.0648 | 0.0650 |
| Adj. $R^2$ | 0.0632 | 0.0632 | 0.0630 |
| Num. obs. | 5769 | 5769 | 5769 |
| RMSE | 0.4756 | 0.4756 | 0.4756 |
| N Clusters | 5353 | 5353 | 5353 |

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$; $^{\cdot}p < 0.1$

**Table D.23: Individual-level turnout**

# E Study 1 Pre-analysis plan

## Motivation

There is a not a great deal of research on voter registration using RCTs in contrast to voter turnout (GOTV), and hence there is more uncertainty about the best ways of studying this question. While there had been an effort in the literature to explore communication strategies such as email-based treatments, mailings and canvassing to encourage people to register to vote, the role of social media platforms in voter registration remains largely unexplored, despite the fact that many organisations use social media platforms to register voters, particularly those groups that are hard to reach by conventional means. Many non-partisan organisations view online campaigning efforts as efficient means of activating citizens to encourage to register to vote, but we have no information about whether these efforts are effective. The research is not decisive about the effects of online adverts, such as Bond's 2012 research on facebook and google in the US and research in Germany (Hager 2019), both showing weak effects, the latter about .5 per cent.

Moreover, the vast bulk of experiments in GOTV tend to conceive mobilisation as directed toward one behavioural act, usually with a single intervention, whether a door-knock, telephone call or leaflet, and with one persuasive message, done for either registration or GOTV but rarely both at the same time (Green and Gerber 2008). Citizens of course are on a journey in their political lives, whereby one behavioural act leads to another down the line. The hope for any mobiliser is that the outcome that has been stimulated continues in resonance over time in turn out a later electoral contacts, and that the habit of one act spills over to the next one in the future. However, mobilising organisations need to be aware that a successful result at one stage does not mean the citizen is mobilised at the next. The very factors that caused a citizen not to mobilise in the first place may cause the person to relapse to inactivity if not further prompted. Registration studies have found just this, that mobilised citizens to are registered to vote don't in fact turn out (Braconnier and Pons). Registration is regarded as a more formal process that is seen as mandatory and similar to registration for other government services and requirement, whereas GOTV is more political and where there is no obligation to vote in any case. A citizen could be encouraged to register to vote responding to the need for compliance, but the same citizen might not use their vote in a subsequent election because they are not politically motivated to do so. Mobilisation might be good for a short-term act such as registration but these may not be sustained into actual voter turnout. The alternative model is that people need mobilisation at both registration and voting stages to remind them of their civic duty at each stage.

- Facebook and Google ads have a small positive (0.5%p) effect on vote shares (Hager 2018, 2019)
- Facebook ads can inform voters (Enriquez, Larreguy and Marshall 2019)
- Direct messages (but not public tweets) have an effect on petition signatures (Coppock, Guess, and Ternovski 2016).
- We don't know the effects of large scale campaigns on social media other than Facebook (which are increasingly

popular with younger cohorts).

- Neither effects on electoral and non-electoral participation beyond turnout.
- Voter registration literature predominantly focused on door-to-door canvassing and direct mail (Nickerson 2015; Braconnier et al. 2017; John et al. 2015, Kolle et al. 2019)
- Problem: Young voters are harder to reach using conventional methods of voter registration.
- Effects of email are null among young voters (Nickerson 2007)
- Classroom-based interventions are effective at increasing registration among students (Addonizio 2011; Bennion and Nickerson, 2018), but might be difficult to scale.

# Experimental set-up

In this field experiment, we work with a non-partisan organization [blinded] to test if their social media registration drive is effective at registering citizens to vote in the General Election, and if a Get-Out-The-Vote reminder 2-3 days before the election amplifies the campaigns effect on turnout. In a 2x2 factorial design, we will assign 879 postcode sectors located within 40 constituencies to four groups: one control group, one group that receives a number of voter registration messages (see templates attached) via Instagram and Snapchat, one group that receives turnout encouragements via Instagram and Snapchat, and one group that receives both voter registration and GOTV ads. These messages are targeted towards young people aged between 18 and 35 years. The voter registration messages will appear in postcode sectors assigned to the treatment group in the week before the voter registration deadline on 26 November. The GOTV ads will appears on Tuesday and Wednesday 10 and 11 December.

After the election, we will collect voter registration and turnout data at the polling station level (polling stations can perfectly be matched to postcode sectors) to test whether the campaign increased voter registration and voter turnout. [Blinded] will also provide us with data on click-throughs and engagement metrics at the postcode sector level.

Finally, we plan to work with the Electoral Commission to obtain the de-identified land register matched to the marked voter register to measure voter registration and turnout at the household level. This allows for increased statistical power and more precise estimates compared to the analysis conducted at the polling station level.

# Hypotheses

1. Voter registration ads targeted at young people have a positive effect on voter registration.
2. GOTV ads targeted at young people have a positive effect on turnout.

3. Voter registration ads have a more positive effect on turnout in combination with GOTV ads.

# Analysis

We will run the following linear models, clustering standard errors at the level of assignment, the postcode sector level:

$$YREG_{pc} = \alpha + \beta_1 RegistrationAd_{pc} + \epsilon_{pc} \quad (1)$$
$$YVOTE_{pc} = \alpha + \beta_1 RegistrationAd_{pc} + \beta_2 TurnoutAd_{ic} + \epsilon_{pc} \quad (2)$$
$$YVOTE_{pc} = \alpha + \beta_1 RegistrationAd_{pc} + \beta_2 TurnoutAd_{pc} + \beta_3 RegistrationXTurnoutAds_{pc} + \epsilon_{pc} \quad (3)$$

$$YREG_{hc} = \alpha + \beta_1 RegistrationAd_{hc} + \epsilon_{hc} \quad (4)$$
$$YVOTE_{hc} = \alpha + \beta_1 RegistrationAd_{hc} + \beta_2 TurnoutAd_{hc} + \epsilon_{ic} \quad (5)$$
$$YVOTE_{hc} = \alpha + \beta_1 RegistrationAd_{hc} + \beta_2 TurnoutAd_{hc} + \beta_3 RegistrationXTurnoutAds_{hc} + \epsilon_{hc} \quad (5)$$

where $YREG$ is the absolute number of registered voters at the polling district level (at times t-1 and t+1), $YVOTE$ is the absokute number of voters at the polling district level (at times t-1 and t+1)

where $\beta_1 Registration$ Ad is whether the unit was assigned to receive registration ads (1) or not (0), and $\beta_2 TurnoutAd$ is whether the unit was assigned to receive turnout ads (1) or not (0). $\epsilon$ is the error term clustered at the postcode sector level.

We will re-run the same models adjusting for pre-treatment registration and turnout. We will conduct balance checks using randomization inference to estimate the f-statistic from regressing assignment to treatment or control streets on turnout in the 2013 local elections, as well as pre-treatment party support, and simulating assignment to treatment or control 10'000 times under the sharp null hypothesis. We will conduct differential attrition checks using randomization inference to estimate the f-statistic from regressing missingness on assignment to treatment or control streets, and simulating assignment to treatment or control 10'000 times under the sharp null hypothesis. If differential attrition occurs, we will bound ITT estimates using Lee bounds.

For any analysis choices that may arise that are not pre-specified in this document, we will follow the standard operating procedure (SOP) as outlined in Lin et al. (2016).

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