# The limits of behavioral nudges to increase youth turnout: Experimental evidence from two French elections[1]

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**Abstract**

There is a significant gap in turnout between young people and older voters. The failure to instill a voting habit at an early age may have long term consequences in terms of future political participation as well as on other civic behaviors. Using a pre-registered online experiment with 3,790 subjects, we implemented behavioral interventions aiming to stimulate youth turnout in the 2022 French presidential election. We rely on an innovative incentive scheme to measure their consequences on (self-reported) actual voting behavior. We also provide evidence on the effect of one behavioral intervention on youth turnout in a less salient election, the French legislative election that took place two months after the Presidential one. The results from the two experiments show the absence of any differences in turnout between the baseline and the treatment conditions. We investigate several mechanisms that can explain our results.

## 1. Introduction

From a standard rational choice perspective, voting is considered an irrational decision because the payoff, which comes from the likelihood that one's vote will be decisive, is small compared to the cost (Downs, 1957; Agranov et al., 2018). However, national election data across the world show that a vast majority of voting-age population does vote. One-third of the OECD countries report participation levels higher than 70% (Pew research center, 2022). While those figures may seem relatively high, it has been shown that not all eligible voters turn out at the same rate. Although young people between 18 and 30 years old comprise one of the largest blocks of voting eligible citizens, they vote at significantly lower rates than older people. For example, young Americans are almost twice less likely to vote than those 60 years and older (Holbein and Hillygus, 2020). The same applies to Western European countries where turnout rates for young voters in national elections range between 60% and 70%, while participation among people between 60 and 69 years old often exceeds 90% (Pintor et al., 2004). A similar pattern can be observed across the globe. A recent survey covering 59 countries representing all the regions in the world found a 20-percentage point difference in participation between people aged 25 or under and those aged 26 or over (Haerpfer et al., 2022). It is important to understand what policy tools can increase youth turnout to ensure that

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young people's interests are politically represented. Furthermore, individuals who participate when they are young are more likely to continue voting throughout their lives (Coppock and Green, 2016), while those who don't are often locked-in as perpetual nonvoters. Finally, there may be positive spillovers from increasing youth turnout as voters are more likely to engage in other civic behaviors, like volunteering and donating (Lijphart, 1997).

We implemented a large-scale online experiment to test the effect of three behavioral interventions aiming at increasing university students' turnout in the first round of the 2022 French presidential election. Prior to the election, survey data indicated that young people (ages 18-29) had a lower intention to vote in the 2022 election compared to previous years (less than 60% intended to vote, while youth turnout in past presidential elections tended to be higher than 70%; see IFOP, 2022). Policy briefs based on survey data pointed to several factors explaining lower expected youth turnout, including a lack of interest in politics (Blais and Daoust, 2020), a growing involvement in alternative modes of political expression, such as protests or online activism (Muxel and Zulfikarpasic, 2022), and a lack of information on whether and where one is registered to vote (Assemblée Nationale, 2021). Some of these factors can be addressed using behavioral interventions. For example, given that young voters frequently relocate for study or work (Juelich and Coll, 2020), they may lack information about the polling station where they are registered to vote. A reminder about the polling station may help them form a voting-plan.[39]

Our experiment tests three behavioral interventions that were co-designed during a workshop that gathered researchers in behavioral economics, a group of social designers, and a group of students from different universities. Power analysis leads us to restrict to three interventions among the ones identified by workshop participants. The specific interventions included in the study were determined through a vote asking all contributors to rank them according to their expected effectiveness. Our first behavioral intervention, *Implementation-intention*, consists in informing university students about their polling station, and asking them to provide a plan stating when they will vote, how they plan to go to the polling station, and what do they plan to do after voting. These are similar questions to the ones used in the existing literature that found significant behavior change using an implementation-intention technique in the context of a US election (Nickerson and Rogers, 2010) as well as in a health-related intervention (Milkman et al., 2011). The novelty of our intervention is to complement plan formation with an information about one's polling station, an information that young people may lack. The second behavioral intervention, *Between-group comparison*, combines descriptive social information with a message that pits one's group against another group that has a higher turnout. Following previous research showing that one way to motivate cooperation in low-cooperative groups is to show them cooperation rates in high-cooperative groups (e.g., Cardenas and Mantilla, 2015), our second intervention implements social comparison with a form of inter-group competition that may increase intra-group cooperation. The third

[39] Every French citizen is automatically registered to vote at the age of 18 but needs to re-register when moving out and if they wish to vote in their new place of residency. In 2022, a non-governmental association, called *A Voté*, has run a campaign in France to inform young people on where they are registered to vote.

behavioral intervention, *Advice-giving*, tests whether writing a short motivational letter about the importance of voting can raise the turnout among advice givers. Previous literature has emphasized several reasons why advice-giving may motivate behavior change, including an effort to reduce cognitive dissonance (Aronson, 1999), prompting plan formation (Gollwitzer, 1999), and increasing one's self-confidence (Eskreis-Winkler et al., 2018).

We designed a multi-lab experiment that was conducted in partnership with eight laboratories in France, specialized in experimental economics. Although they do not involve comparison of different tests, as do, for example, meta-analyses or many-design experiments, multi-lab studies yield more reliable results than single studies due to larger sample sizes and greater geographical diversity. Subjects from the eight labs were randomized into three treatment conditions and one baseline. In all conditions, a few days before the election day, which took place on April 10, 2022, subjects were invited to complete a questionnaire, including questions related to their past participation in national elections as well as their intention to vote in the upcoming presidential election. 4,117 subjects completed this first phase of the experiment. The day following the election day, on April 11, subjects who completed Phase 1 were invited to complete Phase 2 of the study in which they were asked to report whether they had voted or not. Overall, 3,790 subjects completed the two phases of the experiment. To address concerns with self-reported measures, we implemented an incentive-compatible method to elicit subjects' actual voting behavior. In France, voting sheets signed by voters who cast a ballot on the election day are available for consultation until ten days after each poll. Before answering the voting question, subjects were informed that a subset of participants would be randomly selected to receive payment and that for those participants our team would visit their polling stations. A subject would receive 120€ if their self-reported voting decision corresponds to actual voting behavior (as confirmed by the administrative data), and 20€ otherwise. As we show in the design section, our procedure ensured truthful reports about subjects' voting behavior.

We find no statistically significant differences between the three behavioral interventions and the baseline. In the Baseline, 87% of the subjects reported having voted, a turnout rate similar to what we observe in the three conditions with a behavioral intervention. We investigate three possible explanations for the lack of impact from our behavioral interventions. We present new data based on a pre-registered follow-up experiment, and from a survey, both conducted after the presidential election. First, given the high baseline motivation to vote (87%), there may be no room for our behavioral interventions to increase turnout. We conducted a pre-registered follow-up experiment during the legislative election to partially address this concern. We studied whether subjects who were exposed to one of our interventions in the context of the presidential election were more likely to vote in the legislative election that followed two months later and where turnout was expected to be significantly lower. We should note, however, that the evidence from the follow-up experiment is only suggestive because we cannot isolate the possibility that the intervention's effect dissipates over time. We find no differences in turnout between our behavioral intervention and the Baseline condition in the context of the legislative election. We also conducted a survey to address what

one may consider an abnormally high turnout rate among young people in the Baseline. We find that the turnout rate in the Baseline is not the consequence of the invitation email that subjects received a few days prior to the election, that could have acted as a reminder about the upcoming election. Instead, the turnout in the Baseline is representative of the participation of highly educated individuals who compose our sample (i.e., university students). The third possible explanation for the null effect that we discuss relates to the rising literature finding limited (if any) impact from "light touch" interventions in several contexts.

Our study contributes to the understanding of whether behavioral interventions can work as an effective tool to increase voter turnout. Research leveraging behavioral insights to increase turnout has been mostly carried out in the context of US elections, that are characterized by a relatively low baseline voter participation (Gerber and Green, 2017). Behavioral interventions such as implementation-intention (Nickerson and Rogers, 2010), social information about high or low turnout (Gerber et al., 2008), reminders (Dale and Strauss, 2007; Malhotra et al., 2011), and pledges to vote (Costa et al., 2018) have been shown to positively impact voter turnout in some of the recent US elections. Outside of the US, the experimental evidence on the effect of behavioral interventions on voter participation is rare. Braconnier et al. (2017) tested the effect of door-to-door canvassing on voter registration and turnout in the 2012 French presidential and legislative elections. They found a positive effect from their intervention on turnout in the presidential election (for which the level of turnout is generally high, more than 70%), but a limited impact on turnout in the legislative election (with a significantly lower turnout than the presidential election, around 55%). Another behavioral intervention implemented outside of the US is by Bergh et al. (2018) who experimentally tested the effect of text reminders in the context of municipal elections in Norway where turnout is generally moderate to high (60% in 2015). They found a positive effect on turnout. We add to the existing literature by investigating the effect of behavioral interventions on youth turnout in the context of a presidential election, with a high turnout, especially among university students. Furthermore, we study whether one of our interventions affects turnout two months after its implementation, in the context of the French legislative election, with a moderate to low baseline participation. The existing research studies the effect of nudges in only one election context, characterized by either high or low turnout, while our study covers two elections with very different turnout rates.[40] In that sense, our work contributes to the recent literature investigating how a population's baseline motivation can affect the potential of nudges to change people's behavior (Campos-Mercade et al., 2021; Saccardo et al., 2024). Our results are, for example, similar to Campos-Mercade et al.'s (2021) findings regarding the effect of nudges on COVID-19 vaccination behavior in Sweden, where baseline vaccination rates were already high. Based on the results from the follow-up experiment on the legislative election, we show this result extends to a more moderate baseline turnout.

[40] Bracconier et al. (2017) studied the effects of their intervention in two different elections (Presidential and legislative). While they investigate the impact of a standard intervention in political mobilization, i.e., canvassing, we study three interventions that were elaborated based on behavioral insights.

Our second contribution to the literature is methodological. Most of the existing experimental research on voter turnout has been carried out in countries with a centralized access to administrative records of individual voting decisions (e.g., US and Norway), which is only available in a very limited set of countries. However, many countries around the world do not provide centralized access to administrative records of individual voting decisions (most of the European countries do not provide such access). Such a constraint poses serious challenges for researchers who seek to measure actual individual voting behavior. Braconnier et al. (2017) took pictures of attendance sheets at the 2012 French presidential and parliamentary elections and digitalized them. However, implementing Braconnier et al.'s procedure in a nationwide experiment would be extremely costly as it would require visiting many polling stations to digitalize attendance sheets. Our procedure rather relies on a probabilistic verification, and allows to address some of the concerns regarding self-reported measures at lower cost. The only constraint is to have access to attendance sheets, as is the case in France.

Our contribution is also relevant in terms of policy. The topic of youth participation in elections has received increased attention in policy discussions. Based on the results from the presidential and the legislative elections, our study suggests that behavioral interventions, at least the three that were tested in this paper, may not be the right policy tool to motivate university students to vote. Such failure of "light touch" interventions may encourage policy makers to invest in other types of policy tools, such as educational programs, that are more costly to implement but seem to have the potential to change young people's civic behavior (Briole et al., 2022).

The rest of the paper is organized as follows. Section 2 describes the design and implementation of our online experiment. In Section 3, we present the main results from the presidential election and in Section 4 we discuss three possible explanations for our results. Section 5 concludes.

## 2. Experimental design

We partnered with eight academic laboratories in France, specialized in experimental economics and possessing a subject pool managed through an online platform, such as hroot (Bock et al., 2014), ORSEE (Greiner, 2015) or SONA (www.sona-systems.com).[41] Subjects registered in one of our partner laboratory's databases received an invitation email to participate in an online experiment consisting of two phases: 1) the first phase took place from April 6$^{th}$ to April 8$^{th}$, 2022, and 2) the second phase from April 11$^{th}$ to April 13$^{th}$, 2022. From the study's research question, there were two main inclusion criteria: age and nationality. Young voters are generally defined as being between 18 (the minimum legal age to vote in France) and 29

[41] We restricted the collaboration to laboratories with a subject pool managed through an online platform because this allowed us to make sure that the same subjects could not participate multiple times in the experiment. Specifically, the online platforms mentioned above provide each subject with a unique ID that was used to restrict access to the experimental platform. The list of laboratories that were involved in the experiment: LEM in Lille, Grenoble Applied Economics Lab in Grenoble, Laboratory for Experimental Economics in Montpellier, Laboratory for Experimental Economics in Nice, Laboratory for Experimental Economics in Paris, Laboratory for Experimental Economics in Strasbourg, Laboratory for Experiments in Economics and Management in Rennes and Caen, Laboratory for Experimental Social Sciences and Behavioral Analysis in Dijon.

years old (e.g., Pintor et al., 2004; Assemblée Nationale, 2021). The other criterion is nationality, as voting in the presidential election is restricted to French citizens.

Being registered to vote is not a criterion in our study because everyone turning 18 and who holds the French citizenship is automatically registered to vote. The two participation criteria, age and nationality, were stressed out in the invitation email that every partner institution sent to their subject pool. The invitation email specified that the payment of earnings collected in this study is conditioned on the subject fulfilling the two criteria.

*2.1 First phase and the experimental conditions*

Subjects were randomized into three treatment conditions and one baseline. The different treatments were co-designed during a workshop that took place on February 8th, 2022. The workshop gathered researchers from several academic institutions in France, a group of social designers from a private company, and a group of students from various French universities. During the workshop, participants were divided into small groups and worked on identifying barriers and motivating factors behind youth civic engagement. The principal investigator (first author) also prepared a review of previously tested nudges aimed at changing young people's behavior in various domains. Based on these insights, all groups then developed solutions to increase youth turnout. Since not all workshop participants were familiar with nudging concepts or experimental economics, some proposed solutions did not qualify as nudges, while others were unsuitable for testing in an online experiment. During the post-workshop selection phase, the principal investigator eliminated ideas that were not feasible – such as implementing an online voting system or including the option of a blank vote. Next, we excluded interventions involving symbolic or monetary rewards, as we aimed to keep the material benefits of voting unchanged. These excluded ideas were giving voters a bracelet, a sticker ("Voted"), a lottery ticket, or university credits. This left us with five potential interventions: implementation-intention, between-group comparison, advice-giving, advice-receiving, and pledging. Following a power analysis (see Appendix B) and an estimation of the number of participants we could recruit from the eight laboratories, we decided to test only three interventions. To make the final selection, we conducted an online vote, where researchers involved in the project ranked the five behavioral interventions. We selected the three interventions that were expected to have the highest potential to increase turnout based on this ranking.

*2.1.1 Baseline condition*

In all conditions, subjects first consented to participate in the two phases of the experimental study, and were then asked to state how likely they were to vote in the first round of the upcoming presidential election, on April 10, 2022, by choosing a number between 0 (very unlikely to vote) and 10 (very likely to vote). This pre-intervention measure of the *intention-to-vote* allows to check the quality of the randomization between conditions.[42] This measure is also useful to investigate heterogenous effects of our interventions, since we expect our

[42] We find no difference in participants' intention-to-vote across our experimental conditions ($X^2$ *test*, p=0.81).

interventions to have a stronger effect on subjects with moderate preexisting motivations to vote (Saccardo et al., 2024).[43]

Subjects were then asked to complete a demographic questionnaire and to answer questions regarding their previous voting experience, political preferences, beliefs regarding the participation rate of the 18–29-year-old on the election day, risk preferences, and altruism (see complete instructions in Appendix C). The baseline condition did not contain any encouragement message to vote. The following three treatments resemble the baseline, with the exception that each includes a behaviorally informed intervention.

*2.1.2 Treatment 1: implementation-intention*

Implementation intention has been widely proven to be an effective strategy to promote desirable behaviors in the public health domain (Gollwitzer & Oettingen, 1998; Milne et al., 2000; Gollwitzer & Sheeran, 2006; Milkman et al., 2011). It mainly refers to a plan stating when, where and how to attain a goal (Gollwitzer, 1999). Developing such a plan requires the subject to activate the mental representation of the desirable behavior and to anticipate the situations associated with it, which thus facilitates the initiation and/or the maintenance of desirable behaviors (Gollwitzer, 1999). Even simple plans, containing only a few information, seem to produce an effect. For example, Milkman et al. (2011) simply prompted participants in their study to write down the date and time they planned to be vaccinated, which led to a significant increase in vaccination rates compared to the condition without the date and time of vaccination prompt. In the context of voting, Nickerson and Rogers (2010) asked American voters to write down when they would vote, where they would be coming from and what they would do before voting. In their case, the implementation intention increased turnout by 4.1 percentage points compared to a baseline without an intention implementation stage.

Our implementation intention treatment consisted of two steps. In the first step, participants were asked to verify the location of the polling station where they are registered to vote by clicking a link directing to the website "Service Public"[44], created by the French government and independent from our experimental platform. The verification procedure is quick and requires easy to recall information such as one's name, surname, gender, and date of birth. This first step addresses one of the key factors of the failure to vote among university students, i.e., the registration-location obstacle due to the frequent residential relocation. Evidence shows that young people often lack knowledge about the polling place where they are registered to vote (Assemblée Nationale, 2021). We facilitate plan-making by providing subjects with the information about the polling station where they are registered to vote. During this step, 98% of our subjects in this condition downloaded the information regarding the location of their polling station.

[43] Denni and Berton (2014) show that the individual self-reported intention to vote on a 0 to 10 scale is a good predictor of actual voting behavior.
[44] The link to the website: https://www.service-public.fr/particuliers/vosdroits/services-en-ligne-et-formulaires/ISE

In a second step, we prompted subjects to make a plan by asking them the three following questions: 1) When will you vote? 2) Will you go alone or with someone else? 3) What do you plan to do after casting your vote? Such questions are analogous to the ones typically used in the literature using an implementation-intention technique (Nickerson and Rogers, 2010; Milkman et al., 2011). In our case, only 9% of participants refused to make an entire plan.

*2.1.3 Treatment 2: between-group comparison*

Our second experimental treatment relies on the literature showing that the behavior of others influences many individual choices (Bicchieri, 2006; Bicchieri and Xiao, 2009; Bursztyn and Jensen, 2017). In the context of voter turnout in a US election, Gerber and Rogers (2009) found that showing participants that voter turnout in the upcoming election is expected to be high resulted in higher voter intentions than in the low turnout condition. However, other studies measuring actual turnout against a baseline with no social information found zero effects from a simple message emphasizing low or high turnout in one's community (Panagopoulos et al., 2013; Bergan et al., 2022). Furthermore, when it comes to the use of descriptive social information to change behavior, recent large-scale experiments found that this type of intervention has a limited impact by itself but can change behavior when complemented with some additional information (Milkman et al., 2022). For example, Milkman et al. (2022) complemented their descriptive information intervention with a message that the desired behavior is frequent and growing, which significantly increased gym attendance.

We designed an intervention combining descriptive social information with a message that pits one group against another with a higher turnout. Specifically, subjects in this treatment were exposed to the following message: "*In the first round of the last presidential election, 7 people out of 10 aged 18-29 years old voted. At the same time, 9 people out of 10 aged 60-74 years old voted in the same election. Who decides for your future?*"

We chose to compare the voting rates of young people with the age category on the other side of the age spectrum for two reasons. First, evidence shows that political preferences evolve over time and that younger people tend to vote with left-wing political parties while older people tend to vote for right-wing political leaders (Harris Interative, 2022). Thus, a political preference gap exists between the two age categories, which may create a stronger feeling of opposing interests and may motivate young people to vote. Second, the 18-29 age category had the lowest turnout rate in the preceding French presidential election (in 2017), whereas the 60-74 age category had the highest turnout rate.[45] Cardenas and Mantilla (2015) have shown that one way to motivate cooperation in low-cooperative groups is to show them cooperation rates in high-cooperative groups. This intervention therefore implements social comparison in the form of inter-group competition that can increase intra-group cooperation (voting within the 18-29 age category with the lowest turnout rate in the previous presidential election).

*2.1.4 Treatment 3: advice-giving*

[45] For voter turnout information in France, see https://www.insee.fr/fr/information/3142242

The advice-giving intervention was inspired by Eskreis-Winkler et al. (2019), who showed that asking students to advise their peers raised academic achievement of the advice-givers. Several reasons why advice-giving benefits the advisor have been proposed. First, while advocating for a specific opinion, people may be led to believe their advice as a way to reduce cognitive dissonance (Aronson, 1999). Second, advice-giving may motivate achievement by prompting plan formation (Gollwitzer, 1999). Third, giving advice may increase self-confidence (Eskreis-Winkler et al., 2018). Our advice-giving treatment tests whether writing a short motivational letter about the importance of voting can raise the turnout among advice-givers.

In Eskreis-Winkler et al. (2019), students received specific guidance before they were asked to give advice to others. That is, before giving their advice, they were asked a few questions that were meant to provide them with insights they could later use when giving their advice. In our advice-giving condition, subjects were first asked to answer five fact-based multiple-choice questions about voting in French presidential elections. These questions were designed to prompt participants to think about the importance and meaning of voting. They offered subjects some information that could be used as inputs when writing the motivational text.

To avoid selection bias (e.g., subjects with high intention to vote choose to write a motivational letter, but not subjects with a low intention to vote), we incentivized all subjects to write a short motivational letter (between 70 and 130 words). The advice-givers were informed that their advice would be shown to a peer and that the peer would have to indicate to what extent the written message is convincing from the following options: “not convincing at all”, “somewhat convincing”, “convincing”, “very convincing”. Subjects were informed that authors of “convincing” or “very convincing” messages would have a chance to win 80€. Specifically, 25 messages would be randomly chosen and authors of “convincing” or “very convincing” messages, among those messages, would receive 80€ (in addition to a fixed payment for participation in the experiment). Subjects were also given the possibility not to give any advice, which would exclude them from the possibility of winning 80€. Only 8% of subjects in this condition refused to give advice to another young individual on the importance of voting.

Another reason we chose to implement incentives for writing convincing messages is to reduce the number of subjects who would not take this task seriously. The mechanisms behind our advice-giving intervention require the advice-giver to use convincing enough arguments. In our experiment, out of the 836 messages, only one was not related to voting. Of the randomly chosen messages that were evaluated for payment, 80% were considered convincing or highly convincing by a panel of raters.[46]

*2.2 Second phase and the incentive structure to reveal voting behavior*

The second phase of the experiment started on the day after the election took place, on April 11th, and it ended on April 13th, 2022. Subjects from all four conditions were recontacted by

[46] Every message was randomly assigned to a rater. Raters were recruited from students who did not take part in one of the experimental conditions presented above. Raters were all students in the same age category (18-29) as our subjects who acted as advice-givers. Each of the 25 messages was rated by two independent raters to make sure there was agreement on the extent to which the written message was convincing. In case of disagreement, a third rater was asked to make the final decision based on the feedback from the initial two ratings.

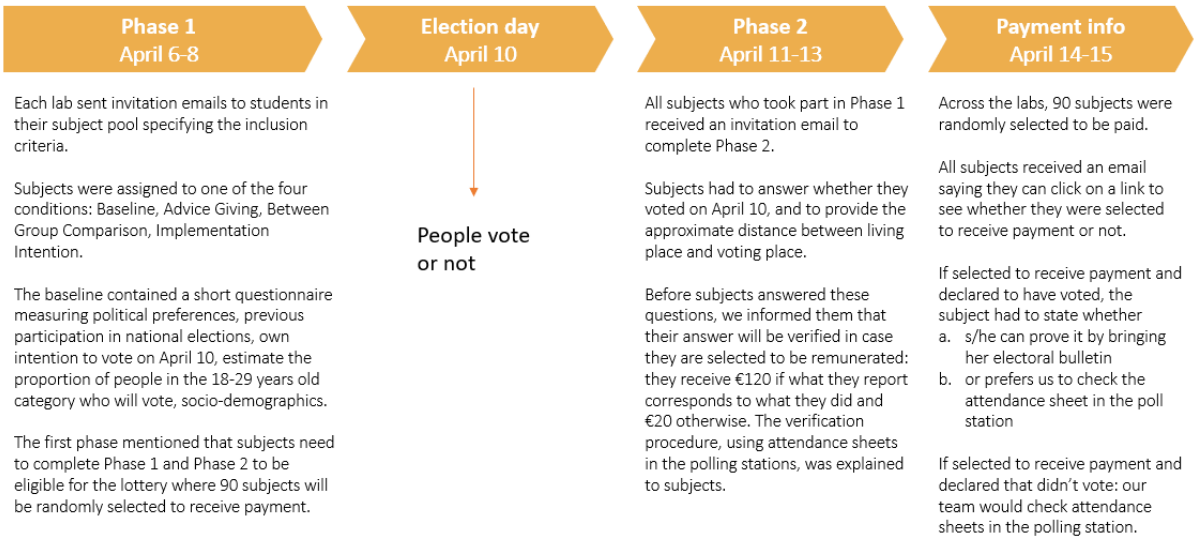
the same lab who had initially invited them to participate in the experiment. Note that, prior to the second phase of the experiment, subjects were not informed of the nature of the second phase; in particular, to reduce experimenter demand effects, they were unaware that their actual voting behavior would be elicited. They were simply informed that they would be invited to a second phase, and that if they completed both phases, they would be eligible for a draw to win up to €120. In the second phase of the experiment, subjects were asked to self-report whether they had voted or not on the election day, on April 10th. An obvious concern with self-reported measures is the problem of misreporting. Subjects may engage in misreporting for various reasons, including desirability bias or self-image concerns. There is evidence that questions on political behavior are particularly prone to misreporting (e.g., Wright, 1993).

We implemented an original, incentive-compatible, method to elicit subjects' actual voting behavior. Specifically, in the first phase of the experiment, in the invitation email, subjects were informed that 90 participants in this study would be randomly selected to receive payment for their participation. In the second phase, before self-reporting whether they had voted or not, subjects were informed that for the 90 participants who would receive payment, our team would visit their polling station to verify whether they actually voted or not.[47] In France, voting sheets signed by voters who cast a ballot on election day are available for consultation until ten days after each poll. We informed our subjects about the verification procedure and that the amount they would earn in this experiment would depend on their decision when self-reporting whether they voted or not: they receive 120€ if what they self-report corresponds to what they effectively did, as confirmed by the administrative data (e.g., if someone either reported to have voted and this is confirmed by the administrative data or that someone reported not to have voted and that this is confirmed by the administrative data), and 20€ otherwise. Out of the 90 subjects randomly selected to receive payment (whose self-reported voting behavior was thus verified) only one misreported.[48] Figure 1 summarizes our experimental design.

**Figure 1. Summary of the experimental design**

[47] Subjects were also given the option to show a proof that they had voted using their electoral card. Note that using the electoral card for everyone in this experiment would have been problematic. First, because not everyone has an electoral card. In France, it is not compulsory to have one. Second, given that the stamp on one's electoral card is not compulsory, it may happen that some people who do have an electoral card and who voted, would still not be able to show a stamp on their electoral card. We therefore used the electoral card as an option for subjects who do have one and who used it on the election day (without knowing that they could use their electoral card in the experiment given that all the information regarding the voting decision and verification procedure was provided to subjects after the election day).

[48] The subject self-reported not having voted, while the administrative data showed that s/he did cast a ballot. It is possible that the subject did not take the study instructions seriously or that there was a mistake in entering the response.




Some aspects of our design are inspired by Braconnier et al. (2017) who took pictures of attendance sheets at the 2012 French presidential and parliamentary elections and digitalized them. Their analysis was based on approximately 135,000 individual turnout observations. Implementing Braconnier et al.'s procedure in a nationwide experiment would however be extremely costly, as it would require visiting thousands of polling stations to verify attendance sheets. Our procedure using a probabilistic verification allows researchers to address some of the concerns regarding self-reported measures at lower cost.

## 3. Data and results

The experiment was implemented using the oTree web-based platform (Chen et al., 2016). Recruitment of subjects took place online, with all participating laboratories sending standardized invitation emails to their respective subject pools (for more information about the online recruitment, see Appendix A). In total, about 10,000 subjects received an invitation to participate in the study. 4,117 subjects signed up to participate in Phase 1 of the experiment, and 92% of the subjects who completed Phase 1 also completed Phase 2. Overall, 3,790 subjects completed the two phases of the experiment (see Appendix B for a power analysis). There were no differences in dropout rates across treatment conditions (see Table 1, last row, 4-sample test for equality of proportions, $\chi^2 = 4.8$, *p=0.18*). The final sample remained balanced across treatment conditions: 975 completed the Baseline, 910 completed the Advice-Giving condition, 969 completed the Between-Group Comparison condition, and 936 completed the Implementation-Intention condition.

**Table 1. Sample characteristics**

| | Baseline (N=975) | Advice-giving (N=910) | Intention-implementation (N=936) | Between-group comparison (N=969) | Total (N=3790) | SMD |
|---|---|---|---|---|---|---|
| **Age** | | | | | | |
| **Mean (SD)** | 22.0 (2.75) | 22.1 (2.79) | 22.0 (2.80) | 22.0 (2.80) | 22.0 (2.79) | 0.03 |
| **Median [Min, Max]** | 22.0 [18.0, 29.0] | 22.0 [18.0, 29.0] | 22.0 [18.0, 29.0] | 21.0 [18.0, 29.0] | 22.0 [18.0, 29.0] | |

| | | | | | | |
|---|---|---|---|---|---|---|
| **Gender** | | | | | | |
| **Female** | 647 (66.4%) | 585 (64.3%) | 612 (65.4%) | 624 (64.4%) | 2468 (65.1%) | 0.02 |
| **Male** | 328 (33.6%) | 325 (35.7%) | 324 (34.6%) | 345 (35.6%) | 1322 (34.9%) | |
| **Intention to vote** | | | | | | |
| **Mean (SD)** | 8.91 (2.53) | 8.85 (2.63) | 8.97 (2.41) | 8.96 (2.41) | 8.92 (2.49) | 0.03 |
| **Median [Min, Max]** | 10.0 [0, 10,0] | 10.0 [0, 10,0] | 10.0 [0, 10,0] | 10.0 [0, 10,0] | 10.0 [0, 10,0] | |
| **Past voting experience in national or municipal elections** | | | | | | |
| **Yes** | 675 (69.2%) | 677 (74.4%) | 677 (72.3%) | 697 (71.9%) | 2726 (71.9%) | 0.06 |
| **Refused to answer** | 7 (0.7%) | 5 (0.5%) | 2 (0.2%) | 4 (0.4%) | 18 (0.5%) | |
| **Professional status** | | | | | | |
| **Non-student** | 185 (19.0%) | 175 (19.2%) | 188 (20.1%) | 187 (19.3%) | 735 (19.4%) | 0.02 |
| **Student** | 790 (81.0%) | 735 (80.8%) | 748 (79.9%) | 782 (80.7%) | 3055 (80.6%) | |
| **Education level** | | | | | | |
| **None** | 1 (0.1%) | 0 (0%) | 3 (0.3%) | 3 (0.3%) | 7 (0.2%) | 0.03 |
| **Brevet des collèges/CAP** | 0 (0%) | 1 (0.1%) | 1 (0.1%) | 2 (0.2%) | 4 (0.1%) | |
| **High school diploma** | 236 (24.2%) | 212 (23.3%) | 235 (25.1%) | 230 (23.7%) | 913 (24.1%) | |
| **Bachelor** | 402 (41.2%) | 394 (43.3%) | 384 (41.0%) | 421 (43.4%) | 1601 (42.2%) | |
| **Master** | 331 (33.9%) | 296 (32.5%) | 301 (32.2%) | 305 (31.5%) | 1233 (32.5%) | |
| **PhD** | 5 (0.5%) | 7 (0.8%) | 12 (1.3%) | 8 (0.8%) | 32 (0.8%) | |
| **Political preferences** | | | | | | |
| **0 – 3 (left)** | 354 (36.3%) | 303 (33.3%) | 346 (37.0%) | 338 (34.9%) | 1341 (35.4%) | 0.04 |
| **4 – 6 (center)** | 370 (37.9%) | 364 (40.0%) | 355 (37.9%) | 367 (37.9%) | 1456 (38.4%) | |
| **7 – 10 (right)** | 200 (20.5%) | 188 (20.7%) | 192 (20.5%) | 192 (19.8%) | 772 (20.4%) | |
| **Refused to answer** | 51 (5.2%) | 55 (6.0%) | 43 (4.6%) | 72 (7.4%) | 221 (5.8%) | |
| **Distance from polling station** | | | | | | |
| **Less than 10km** | 698 (71.6%) | 649 (71.3%) | 681 (72.8%) | 686 (70.8%) | 2714 (71.6%) | 0.02 |
| **Between 10 and 100km** | 82 (8.4%) | 73 (8.0%) | 76 (8.1%) | 95 (9.8%) | 326 (8.6%) | |
| **Between 100 and 500km** | 94 (9.6%) | 105 (11.5%) | 101 (10.8%) | 99 (10.2%) | 399 (10.5%) | |
| **More than 500km** | 80 (8.2%) | 70 (7.7%) | 62 (6.6%) | 71 (7.3%) | 283 (7.5%) | |
| **Refused to answer or don't know** | 21 (2.2%) | 13 (1.4%) | 16 (1.7%) | 18 (1.9%) | 68 (1.8%) | |
| **Campus** | | | | | | |
| **Dijon** | 76 | 66 | 72 | 73 | 287 | 0.05 |
| **Grenoble** | 197 | 184 | 188 | 192 | 761 | |
| **Lille** | 66 | 62 | 56 | 65 | 249 | |
| **Montpellier** | 90 | 98 | 97 | 94 | 379 | |
| **Nice** | 89 | 78 | 81 | 92 | 340 | |
| **Paris** | 101 | 91 | 96 | 97 | 385 | |
| **Rennes** | 152 | 142 | 147 | 156 | 597 | |
| **Strasbourg** | 204 | 189 | 199 | 200 | 792 | |
| **Dropouts from Phase 1 to Phase 2** | | | | | | |
| | 70 | 89 | 78 | 90 | 327 | |

*Note: Column SMD reports Standardized Mean Differences.*

Table 1 provides a descriptive summary of the sample. A majority of our subjects were female (65%). Overall, the average age of our subjects was 22, 80% were students and the other 20% were employed (more than 70% had a university degree and about 20% were enrolled in a bachelor program). In terms of political orientation, our sample leaned left, but not more left than the representative young French population (e.g., Lardeux and Tiberj, 2022). 72% had already voted in a national or a municipal election, and the average intention to vote in the upcoming presidential election was high. On a scale from 0 to 10 where 10 meant "certain to vote", 74% reported a 10, and the average intention to vote was 8.9. The data, therefore, show that our sample had a high pre-existing motivation. This is consistent with survey results showing that young people with a university degree have a higher turnout rate than those without a university degree.[49] Finally, Table 1 also shows that the randomization across university campuses and all relevant variables (i.e., age, gender, education, political orientation, past electoral participation, distance from the polling station, and intention-to-vote in the upcoming election) were all balanced across treatment conditions.[50]

Below, we present our results in two steps. First, we focus on the average turnout rates across the four treatment conditions. In order to account for individual-level factors that may influence voting behavior, we also analyze the effect of our three behavioral interventions on individual turnout while controlling for the full set of our variables. In the second step, we present a series of robustness checks. Robustness checks consider the exclusion from the main analysis of subjects who refused to report whether they voted or did not fully comply with some treatments.

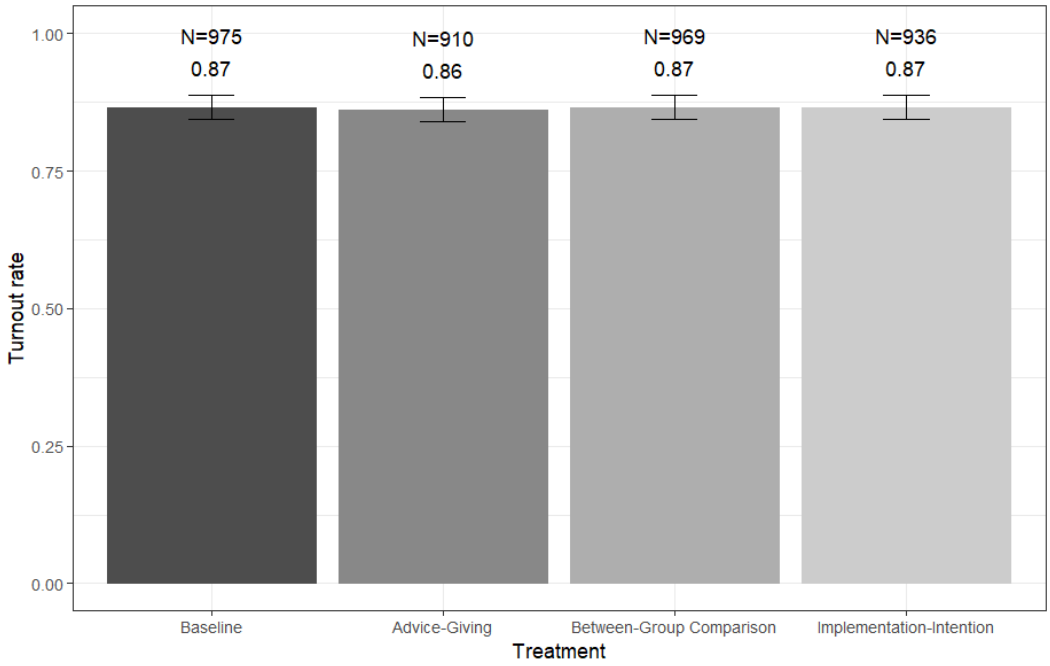
*3.1 Turnout rates across treatment conditions*

The average turnout rate in our sample is high. Overall, 87% reported having voted on the election day. Figure 2 shows the turnout rates in each of our four conditions. In the Baseline, 87% reported having voted (n=975), which is identical to the turnout rate in the Between-Group Comparison (n=910) and in the Implementation-Intention conditions (n=969). The Advice-Giving condition has the lowest turnout rate, 86% (n=936), but is not statistically different from the Baseline. Our first result is thus the absence of significant differences between the baseline turnout and the turnout rates in the other three conditions (4-sample test for equality of proportions, $\chi^2 = 1.41$, *p = 0.703*).

[49] Comparing young people with and without a bachelor's degree, Lardeux and Tiberj (2022) found a 20-percentage point difference in turnout between the two.

[50] Because balancing tests would be inappropriate and misleading (Austin 2009), in Table 1, we report Standardized Mean Differences (column SMD) as indicators of imbalances in all relevant variables we measure across treatments. All differences are below 0.1 points, indicating no substantial differences.

**Figure 2. Average turnout rates across conditions**




*Note: Error bars indicate 95% confidence intervals.*

We now look at the effect of our three behavioral interventions on individual voting behavior, controlling for several factors. We ran a mixed-effects logistic regression (MLR) to predict a given subject's (denoted *i*) voting behavior in a given city (*c*). We also include random intercepts at the location level.[51]

$$Voted_{ci} = \beta_{0c} + \beta_1 \cdot Treatment_i + \boldsymbol{\delta} * \boldsymbol{Z_i} + v_{ci} \qquad (1)$$

where, *Voted* is an indicator variable (1 when reported having voted and 0 otherwise); *Treatment* is the primary predictor variable in our regression, and $\delta$ is a vector of indicators for assignment to each of the study's three experimental conditions (an indicator for the control condition is omitted). In order to account for individual-level factors that may influence voting behavior, we also analyze the effect of our three behavioral interventions on individual turnout while controlling for the full set of our variables indicated with *z* such as demographics, the subjects' intention to vote, whether subjects voted before in any national or municipal election, and the distance to the polling station where subjects are registered to vote (for the full list, see the pre-registration document). Lastly, *v* is an idiosyncratic error.

Table 2 shows the absence of any statistically significant differences between our behavioral interventions and the Baseline, excluding (column 1) or including controls (column 2). In line with previous research on voter turnout, we find that the preexisting intention is a good predictor of actual voting (Deni and Berton, 2012), as is past participation in national or municipal

[51] Our model specification assumes a common treatment effect across cities (same slopes) but allows for different baseline levels (varying intercepts). Our choice is motivated by the fact that it allows us to account for city-level heterogeneity that is not captured via our covariates, and the correlation among observations within the same city.

elections (Coppock and Green, 2016); that a significant barrier to youth voting is the distance to the polling station (Dyck and Gimpel, 2005; Assemblée Nationale, 2021); that younger individuals are more likely to vote than slightly older individuals – which is consistent with national statistics showing that individuals in the 18-24 category are more likely to vote than those in the 25-29 category[52]; and that individuals with higher education levels are more likely to vote (Lardeux and Tiberj, 2022). Furthermore, we find that political preferences are significantly associated with poll participation. Individuals that reported to be more left-oriented are more likely to vote than those who are on the opposite side of the political spectrum.

*3.2 Robustness checks*

To ensure the validity of our results, we ran a series of robustness checks. Column 3 of Table 2 shows that the results remain very similar when we exclude all subjects who refused to report whether they voted or not on the election day. In total, 22 subjects (0.5%) refused to answer this question. Although subjects could refuse to answer the voting question, they were informed that by refusing, they would be excluded from the lottery giving rise to bonus payments. In the previous analyses (columns 1 and 2), we assumed that those who refused to answer the voting question did not vote. Since refusing to answer is costly in expected terms (the subject being excluded from the lottery), it seems reasonable to assume that people will refuse only if answering is also costly in some way. That cost appears likely only in the case of non-voters, who might feel some stigma, or at least shame, for their choice.

We also ran a robustness check to account for the take-up rates in the two conditions in which subjects could move forward without completing all tasks. This was the case, for example, in the Advice-Giving condition where subjects were offered the possibility to refuse writing motivational advice. Similarly, in the Implementation-Intention condition, subjects were free to check or not the information regarding where they were registered to vote. They could refuse to make a plan by not answering one of the plan-making questions. Columns 4 and 5 from Table 2 show that excluding subjects who did not go through the whole procedure in the two treatments does not alter the results.

1

[52] Based on official data from INSEE: https://www.insee.fr/fr/information/3142242

**Table 2. Mixed-effects logistic regression models of voting behavior with all controls (presidential election)**

| Sample: | *Dependent variable: Stated having voted* | | | | | |
|---|---|---|---|---|---|---|
| | *All* | | *Only valid vote response* | *Only with plan* | *Only with advice* | *All* |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Advice-Giving | -0.031 | 0.011 | 0.110 | 0.009 | 0.064 | -0.128 |
| | (0.135) | (0.182) | (0.185) | (0.180) | (0.186) | (0.483) |
| Between-Group Comparison | 0.002 | -0.061 | -0.035 | -0.060 | -0.059 | -0.068 |
| | (0.133) | (0.176) | (0.177) | (0.174) | (0.176) | (0.485) |
| Implementation-Intention | 0.003 | -0.141 | -0.095 | -0.075 | -0.140 | -0.754 |
| | (0.135) | (0.176) | (0.178) | (0.193) | (0.176) | (0.544) |
| Intention to vote | | 0.482*** | 0.484*** | 0.466*** | 0.479*** | 0.462*** |
| | | (0.022) | (0.022) | (0.023) | (0.022) | (0.040) |
| Past participation | | 0.880*** | 0.863*** | 0.892*** | 0.873*** | 0.888*** |
| | | (0.141) | (0.144) | (0.147) | (0.143) | (0.142) |
| Altruism | | -0.035 | -0.034 | -0.041 | -0.034 | -0.035 |
| | | (0.026) | (0.026) | (0.026) | (0.026) | (0.026) |
| Distance to poll | | -0.233*** | -0.226*** | -0.241*** | -0.229*** | -0.233*** |
| | | (0.022) | (0.022) | (0.023) | (0.022) | (0.022) |
| Predicted % of youth turnout | | 0.006 | 0.006 | 0.004 | 0.006 | 0.006 |
| | | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) |
| Left/Right | | -0.070** | -0.068** | -0.075** | -0.067** | -0.069** |
| | | (0.026) | (0.026) | (0.026) | (0.026) | (0.026) |
| Male | | 0.201 | 0.205 | 0.217 | 0.199 | 0.205 |
| | | (0.137) | (0.139) | (0.141) | (0.138) | (0.137) |
| Age | | -0.144*** | -0.137*** | -0.154*** | -0.142*** | -0.143*** |
| | | (0.039) | (0.040) | (0.040) | (0.039) | (0.039) |
| Student | | -0.093 | -0.085 | -0.101 | -0.074 | -0.093 |
| | | (0.218) | (0.221) | (0.226) | (0.219) | (0.218) |
| In a relationship | | 0.116 | 0.064 | 0.129 | 0.136 | 0.118 |
| | | (0.138) | (0.140) | (0.143) | (0.140) | (0.139) |
| Education level | | 0.137** | 0.128** | 0.141** | 0.144** | 0.134** |
| | | (0.049) | (0.050) | (0.050) | (0.049) | (0.049) |
| Monthly Income | | 0.032 | 0.027 | 0.036 | 0.020 | 0.031 |
| | | (0.055) | (0.056) | (0.057) | (0.055) | (0.055) |
| Advice-Giving*Intention-to-vote | | | | | | 0.017 |
| | | | | | | (0.057) |
| Between-Group Comparison*Intention-to-vote | | | | | | 0.001 |
| | | | | | | (0.057) |
| Implementation-Intention*Intention-to-vote | | | | | | 0.075 |
| | | | | | | (0.063) |
| Constant | 1.834*** | 0.513 | 0.393 | 0.974 | 0.421 | 0.664 |
| | (0.137) | (0.888) | (0.904) | (0.919) | (0.896) | (0.928) |
| Observations | 3,790 | 3,790 | 3,768 | 3,594 | 3,727 | 3,790 |

| | | | | | | |
|---|---|---|---|---|---|---|
| Log Likelihood | -1,492.420 | -926.482 | -900.454 | -872.879 | -912.885 | -925.581 |
| Akaike Inf. Crit. | 2,994.839 | 1,886.964 | 1,834.909 | 1,779.757 | 1,859.770 | 1,891.163 |
| Bayesian Inf. Crit. | 3,026.040 | 1,993.046 | 1,940.892 | 1,884.937 | 1,965.567 | 2,015.965 |

*Note: models 1-2 and 6 use our full sample, considering all participants who voluntarily did not provide an answer to the vote participation question as no voters, while model 3 excludes subjects who did not provide an answer. Models 3-4 exclude those participants who did not, respectively, responded to all questions about making a voting plan (in the treatment Implementation-Intention) and refused to write a motivational letter (in the treatment Advice-Giving).* $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$.

## 4. Discussion

In this section, we discuss three possible explanations for our results. In addition to data collected during the presidential election experiment, we present new data based on a follow-up experiment, and from a survey, both conducted after the presidential election. The follow-up experiment explores whether our results are explained by the high ex-ante intentions to vote, thus, leaving no room for behavior change, while the survey addresses what one may consider an abnormally high turnout rate in our Baseline. The survey was not pre-registered and, as such, is part of an exploratory discussion. The third possible explanation for our results relates to the rising literature finding limited (if any) impact from "light touch" interventions.

### *4.1 Does the effect of nudges depend on baseline motivation?*

Our results may be explained by the fact that there was no room for our nudges to increase turnout above the baseline level. In a study of vaccination behavior against COVID-19, Campos-Mercade et al. (2021) implemented three nudges on a population with high intentions to get vaccinated and found no effect. Using data from 125 RCTs, Saccardo et al. (2024) studied the heterogeneity of responses to nudges by looking at the individuals' ex-ante intentions to take up the promoted activity. They found that as baseline motivation moves from moderate (around 40%) to high levels (around 80%), nudges' effect sizes decline. In our data, we have three proxies of subjects' pre-existing motivation to vote that allow for an investigation of the link between baseline motivation and treatment effects: 1) intention to vote, which is a direct measure of initial motivation to vote, 2) distance from the polling station, which measures the cost of voting and therefore could function as an instrument for the motivation to vote, and 3) age, which in our sample is negatively correlated with turnout, thus suggesting that very young people may be more excited to vote because this is something new for them.[53]

To investigate whether our treatments' effects depend on the level of motivation to vote, we estimate the model in Eq. 1, with the addition of interaction terms between each of the three proxies taken individually and the treatment indicator. Furthermore, with respect to the intention to vote, we split our subjects into two groups: those self-reporting to be certain to vote (i.e., a self-reported value of 10) which represents 74% of the sample, and all the others

[53] We do not provide results using two other potential indicators of motivation to vote, education level and past participation, because these two are related to a subject's age (i.e., older subjects had the possibility to accumulate more education and to vote in past elections compared to very young subjects). However, we do not find any evidence of heterogenous effects. Results can be found in our online repository (https://researchbox.org/4116&PEER_REVIEW_passcode=QTDAFD).

with lower intentions (i.e., a value lower than 10). We follow a similar approach when analyzing heterogeneous treatment effects along distance to polling station. We split our subjects into those residing within 5 kilometers from the polling station (representing 67% of all participants), and those residing farther away from the voting place (33%). We also conduct the same heterogeneity analysis using the full scale of values obtaining similar results. Table 3 shows the regression results from our heterogeneous treatment effect analysis (Figure 3 in Appendix D presents a visual illustration of the results). We find no evidence of heterogeneous effects of our treatments with respect to the three dimensions of subjects' initial motivation to vote.

**Table 3. Heterogeneity in motivation to vote and treatment effects**

| | *Dependent variable: stated having voted* | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Advice-Giving | -0.216 | -0.022 | 0.510 |
| | (0.184) | (0.146) | (1.065) |
| Between-Group Comparison | -0.086 | -0.019 | 0.135 |
| | (0.183) | (0.144) | (1.055) |
| Implementation-Intention | 0.035 | -0.041 | 0.367 |
| | (0.185) | (0.145) | (1.067) |
| Intention to vote (High) | 2.546*** | | |
| | (0.221) | | |
| Intention to vote (High) * Advice-Giving | 0.617 | | |
| | (0.339) | | |
| Intention to vote (High) * Between-Group Comparison | 0.223 | | |
| | (0.318) | | |
| Intention to vote (High) * Implementation-Intention | -0.076 | | |
| | (0.313) | | |
| Distance to the polling station | | -0.656 | |
| | | (0.355) | |
| Distance poll (within 5km) * Advice-Giving | | 0.157 | |
| | | (0.522) | |
| Distance poll (within 5km) * Between-Group Comparison | | 0.333 | |
| | | (0.536) | |
| Distance poll (within 5km) * Implementation-Intention | | 0.266 | |
| | | (0.498) | |
| Age | | | -0.023 |
| | | | (0.034) |
| Advice-Giving*Age | | | -0.024 |
| | | | (0.047) |
| Between-Group Comparison*Age | | | -0.006 |
| | | | (0.047) |
| Implementation-Intention*Age | | | -0.016 |
| | | | (0.048) |
| Constant | 0.451** | 1.953*** | 2.349** |
| | (0.153) | (0.138) | (0.765) |
| Observations | 3,790 | 3,722 | 3,790 |
| Log Likelihood | -1,145.887 | -1,404.798 | -1,490.349 |
| Akaike Inf. Crit. | 2,309.774 | 2,827.596 | 2,998.697 |
| Bayesian Inf. Crit. | 2,365.935 | 2,883.594 | 3,054.858 |

*Note: all reported models use our full sample, considering all participants who voluntarily did not provide an answer to the vote participation question as no voters. We report in our replication material regression results when considering only valid responses about voting (i.e., excluding those who preferred not to reply, 22 observations). We only report a significant effect of the interaction term "Intention to vote (high) * Advice-Giving" (p=0.03), yet these results do not hold in the other robustness checks. All additional results can be found in the replication material available in our public repository. *p<0.05; **p<0.01; ***p<0.001.*

However, one limit to the results presented in Table 3 is that there is low heterogeneity in our sample along the three dimensions that we considered as proxies for subjects' initial motivation to vote. To further investigate the possibility that our behavioral interventions may prove effective in a different context, when applied to a population with a lower pre-existing motivation to vote, we conducted a pre-registered follow-up experiment. The presidential election in France is followed, two months later (mid-June), by the legislative election, for which turnout is generally significantly lower. For instance, in 2017, only 44% of the young people voted for the legislative election, while they were 78% to have voted for the presidential election (with university student participating at significantly higher rates).[54] We leveraged this opportunity to investigate whether our behavioral interventions may influence voter turnout in an election with moderate turnout rates.

We decided to focus on only one behavioral intervention, the Advice-Giving one. The other two were too specific to the presidential election, while writing a motivational message on the importance of voting may have created a sentiment that voting is essential not only in the context of the presidential election.[55] We studied whether subjects who participated in the Advice-Giving condition, in April 2022, were more likely to vote in the legislative election, in June 2022, where turnout was expected to be significantly lower. We compared the turnout rates in the first round of the legislative election, which took place on June 12th (two months after the presidential one), in the Baseline and in the Advice-Giving conditions. We should note, however, that the evidence presented below is only suggestive because we cannot isolate the possibility that the Advice-Giving treatment has an effect on a population with lower pre-existing motivation to vote but that the effect dissipates over time.

All subjects who had completed the Baseline and the Advice-Giving conditions, in April 2022, were invited to participate in a new experiment. The invitation was sent one day after the legislative election ended. The invitation stated that this was a follow-up study linked to the experiment conducted in April 2022, and that payment will be like in the first experiment: 30 subjects randomly selected to receive up to 120€, with the exact amount depending on whether the subject's self-reported voting decision is confirmed by administrative data. As in the first experiment, the instructions stated that our team would use administrative data to verify misreporting. Subjects were then asked whether they voted or not on June 12th, for the first round of the legislative election.

Of the 1,885 eligible subjects, 1,012 participated in the new experiment: 523 in the Baseline and 489 in the Advice-Giving. In the Baseline, 63% of subjects reported having voted. The

[54] Based on official data from INSEE: https://www.insee.fr/fr/information/3142242

[55] In the between-group comparison, we highlighted the gap between young and older voters' participation rates in the presidential election. However, it is unlikely that participants generalized this information to all elections, including the legislative one. In the intention-implementation nudge, participants received information about their voting bureau for the presidential election and made a specific voting plan for that election. This intervention was too context-specific to reasonably expect spillover effects on legislative election turnout. In contrast, advice-giving incentivized participants to generate convincing arguments about the importance of voting. This intervention may have led to a more general shift in beliefs about voting, which could, in turn, influence behavior in a subsequent election.

turn-out is very similar, equal to 62%, in the Advice-Giving condition. Table 4 shows the results from a mixed-effects logistic regression. There is no significant difference between the Baseline and the Advice-Giving condition, with and without controls.

**Table 4. Mixed-effect logistic regression models of voting behavior with all controls (legislative election)**

| | *Dependent variable:* | |
|---|---|---|
| | Stated having voted | |
| | (1) | (2) |
| Advice-Giving | -0.018 | 0.018 |
| | (0.131) | (0.135) |
| Past participation | | 0.602*** |
| | | (0.164) |
| Altruism | | 0.060* |
| | | (0.027) |
| Distance to polling station | | -0.077** |
| | | (0.026) |
| Predicted % of youth turnout | | 0.005 |
| | | (0.004) |
| Left/Right | | -0.098*** |
| | | (0.027) |
| Male | | 0.079 |
| | | (0.143) |
| Age | | -0.088* |
| | | (0.041) |
| Student | | 0.038 |
| | | (0.237) |
| In a relationship | | -0.062 |
| | | (0.143) |
| Education level | | 0.083 |
| | | (0.052) |
| Monthly Income | | 0.092 |
| | | (0.057) |
| Constant | 0.530*** | 1.263 |
| | (0.105) | (0.920) |
| Observations | 1,010 | 1,010 |
| Log Likelihood | -666.014 | -641.099 |
| Akaike Inf. Crit. | 1,338.028 | 1,310.198 |
| Bayesian Inf. Crit. | 1,352.781 | 1,379.046 |

*Note: all models use the data on participants from the Advice-Giving condition who participated to the follow-up experiment ran during the legislative election. *p<0.05; **p<0.01; ***p<0.001.*

One way to interpret the results from the presidential election and the legislative election experiments is that the Advice-Giving treatment cannot improve youth turnout, be it in a population with high or moderate levels of preexisting motivation to vote. However, the results from the legislative election (with moderate baseline turnout) are not as robust as the ones from the presidential election for several reasons, including lower sample size, and the two months that separated the implementation of the intervention and the legislative election. Notwithstanding these limitations, the results from the follow-up experiment tend to reinforce the insights from the presidential election experiment about the lack of interplay between our treatments and subjects' baseline motivation.

*4.2 Excluding the possibility that the Baseline acted as a reminder*

To further reinforce the message that we are unable to detect significant effects from the tested interventions in two different elections, we provide new data from a survey that addresses what one may consider as an abnormally high turnout rate in the Baseline (87%). In the Baseline, subjects received an invitation email prior to the election day asking them several questions about the presidential election. This may have acted as a reminder about the election day (Gravert, 2022), spurring turnout in the Baseline. Existing survey data show that a very high proportion of young people surveyed a few days prior to the election day were well-informed about the upcoming election day (80% knew the exact date and another 15% knew that it would take place soon; see IFOP, 2022). However, our invitation email may have put the election day on top of some of our participants' mind.

We conducted an additional survey eight months after the presidential election. We recruited 274 university students with similar characteristics to the sample of subjects who participated in the presidential election experiment (students, 22 years old, on average, and 63% female, as in our presidential election experiment). To avoid selection bias, the purpose of the survey was not revealed in the invitation email. Students received a fixed payment for their participation, which consisted in answering a socio-demographic questionnaire and a question about their participation in the first round of the French presidential election, which took place on April 10, 2022. Even if our survey took place eight months after the election day, given the saliency of the presidential election, chances are low that someone who had voted would forget about it. In the survey, 85% of respondents reported having voted in that election, which is very close to the turnout rate in our Baseline condition.

One drawback of the survey is that it relies on a self-reported measure, while the main experiment used an incentivized method to reveal voting behavior. The official data show that 66% of the 18-29-year-old voted in the first round of the 2022 presidential election.[56] Lardeux and Tiberj (2022) reported a 20-percentage point difference in turnout between students with a bachelor's degree and young people with only a high school degree. Given that our sample consists of highly educated individuals (all subjects have a university degree, 20% have a

[56] Based on official data from INSEE: https://urlz.fr/pJIy

bachelor's degree and 22% have a master's degree), the high turnout in the Baseline seems congruent with turnout data of highly educated young people in France.

*4.3 The limited impact of light touch interventions*

After showing that our results are not likely influenced by the design of our Baseline and the insignificant results are likely not explained by a high pre-existing motivation to vote, we discuss the literature on the limited power of nudges to change people's behavior. There is increasing evidence that nudges have a limited impact (if any), especially when brought to scale (Cantor et al., 2015; Carrera et al., 2018; Goldzahl et al., 2018; Oreopoulos and Petronijevic, 2019; Kristal and Whillans, 2020; Löschel et al., 2020; Gravert and Collentine, 2021; Andor et al., 2022; Holzmeister et al., 2022; Neckermann et al., 2022; Arroyos-Calvera et al., 2023). For example, Oreopoulos and Petronijevic (2019) designed six nudges to improve student grades and persistence that they tested on 25,000 students across three different campuses. They found no significant effects on the primary variables of interest. Similarly, Kristal and Whillans (2020) tested five standard nudges to reduce single-occupancy vehicle commutes and found that their interventions failed to increase carpool sign-up or usage. DellaVigna and Linos (2022) reviewed evidence from all published and unpublished large-scale nudge trials conducted by two major nudge units in the US. Comparing the nudge effects found in these large-scale trials to the effects of the nudges documented in the academic literature, the authors find that the average effect sizes in the large-scale field trials are much smaller than those reported in the literature and that publication bias explains a large share of the gap.

There is also evidence regarding the limited impact of some behavioral interventions to increase voter turnout. Norm-based interventions, one of the most popular nudging techniques, has produced mixed effects when used to increase voter turnout. For example, Gerber and Rogers (2009) found a significant effect on the intention to vote, while Panagopoulos et al. (2013) found no effect on actual turnout rates. The other behavioral intervention that we tested was inspired by the implementation-intention intervention tested in the context of a US election. Nickerson and Rogers (2010) hired research assistants to help their 287,228 subjects make a voting plan via phone. They found that forming a plan increased turnout by 4.1 percentage points. One of the main differences between their intervention and ours is that ours was implemented online. Differences in the implementation method may explain why their intervention was effective. Indeed, asking someone to make a plan on the phone may reduce the psychological distance between the one asking for a plan and the plan-maker compared to an online procedure. However, there are other important differences between our study and theirs (population characteristics, election type, geographical location), that could explain differences in results. Finally, although the existing evidence suggests that the Advice-Giving intervention works to change various behaviors (Eskreis-Winkler et al., 2018), ranging from school performance to weight loss, it has never been tested as a technique to increase voter turnout.

## 5. Conclusion

Governments and international organizations around the world still struggle to close the turnout gap between young people under 29 and older eligible voters. Encouraging young people to vote is important because the failure to instill a voting habit at an early age may have long term consequences in terms of political participation as well as on other civic behaviors (Lijphart, 1997; Coppock and Green, 2016).

In this study, we provide experimental evidence regarding the effect of three behavioral interventions on the turnout rate of young university students in the 2022 French presidential election. We find no significant differences between the baseline turnout and the turnout rates in the three treatments with a behavioral intervention. We discuss three possible explanations. First, we ran a follow-up experiment during the legislative election to explore whether there would be an effect from one of our behavioral interventions on turnout in a less salient election where participation is lower than in the presidential one. We found no significant differences in turnout between our baseline and the behavioral intervention in the context of the legislative election. Results from the legislative election thus reinforce the findings from the presidential election experiment suggesting that the absence of any significant effect from our behavioral interventions may not be the result of high baseline motivation. Second, given the high turnout rate in our baseline from the presidential election experiment, we ran a new survey to confirm that such a high baseline participation rate has more to do with the characteristics of our sample, consisting of highly educated young people, than any flaw in the design. Our final explanation relates to the growing body of literature that finds limited (if any) impact from behavioral interventions in various contexts. As explained by DellaVigna and Linos (2022), publication bias may account for why most of the published evidence from the early years of the nudging literature reports large positive effects on behavior change.

Our study adds to this literature by investigating the effect of behavioral interventions on the turnout of young university students in two contexts: 1) the French presidential election in which the turnout is generally high, and 2) the French legislative election which typically has moderate baseline participation. Most previous studies were conducted in the context of US elections characterized by relatively low levels of voter participation. The only other study that studied how an intervention affects turnout in two types of elections (one with high and the other with moderate levels of participation) is Braconnier et al. (2017). They studied a more traditional intervention in political science (canvassing), while we investigate the effect of interventions based on behavioral insights. Our set-up allows us to investigate whether a population's baseline motivation can affect the potential of behavioral interventions to change people's behavior (Saccardo et al., 2024). For example, our results are similar to Campos-Mercade et al.'s (2021) findings regarding the effect of nudges on COVID-19 vaccination behavior in Sweden, where baseline vaccination rates were already high. However, we also find no statistically significant difference between one of our behavioral interventions and the baseline in the context of the legislative election, with a moderate baseline turnout.

A second important contribution of his study is methodological. Most of the experimental studies that measured voter turnout used centralized administrative data of individual voting behavior. Such data do not exist in many countries, thus making it difficult for researchers to measure actual voter turnout. Our probabilistic verification procedure allowed us to encourage truthful reporting of voting behavior at a significantly lower cost than what has been implemented elsewhere (e.g., Braconnier et al., 2017). Such a method would be useful for researchers seeking to measure actual voting behavior in countries that do not provide access to administrative data about individual voting behavior.

The main limitation of our study is that we focused on a highly educated young population for which there is less room for behavior change. Future research on voter turnout could use our multi-labs design to study political participation of less educated young people who are also less likely to vote. Furthermore, it would be worth investigating the effect of other behavioral interventions in other elections where turnout is generally lower than in the presidential or legislative elections, such as the European election that mobilizes fewer voters. A limit to our methodological contribution is that our verification procedure to elicit voting behavior can only be implemented in countries that provide access to attendance sheets or any other information that can be used to verify whether someone voted or not.

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## Appendix A – Online implementation

The experiment was implemented using the oTree web-based platform (Chen et al., 2016). We used oTree because it allows to create "rooms", each room corresponding to a virtual laboratory where we can set conditions on who can participate in the experiment by using subjects' unique IDs. For the institutions involved in the experiment, a necessary condition was to have a subject pool managed via the hroot, ORSEE or SONA platforms because it allowed us to use the unique IDs generated by the platform as the subject's ID in the experiment. We created a separate room for every partner institution involved in the experiment. Each lab manager was able to do the recruitment independently by sending an invitation email to the subject pool from their university via their hroot, ORSEE or SONA platforms. Invitation emails were scheduled to be sent on April 6$^{th}$. However, given the large number of subjects in each hroot, ORSEE and SONA database, not everyone received the invitation email at the same time. In fact, some subjects received it within a few minutes from the moment when invitations were sent while other subjects received it about 24 hours later.

For the final payment of subjects, after the study's second phase, each partner institution received a list with the ORSEE, hroot, or SONA IDs of subjects who completed the study. We developed an algorithm for the random selection of paid participants that ensures that at least ten participants from each experimental platform are selected. The last ten participants were randomly assigned. Subjects were then contacted individually to be paid according to the local laboratory's compensation policy (in cash, via an online transfer, or using any other payment method) and depending on whether their self-reported voting decision matched what they actually did (information obtained from the administrative data or the subject's electoral card).

**Appendix B – Power analysis**

Our main variable of interest is the proportion of subjects who declared to have voted on April 10, 2022. To estimate the required sample size, we took the baseline proportion of poll participation equal to 0.7 (based on the turnout of young people in the 2017 French presidential elections) and assumed an expected increase associated with any of our behavioral interventions of 4 percentage points, i.e., passing from a baseline participation rate of 0.7 to 0.74 (an equivalent effect size Cohen's h=0.09). Furthermore, we assumed city-level homogeneity in the effect associated with any of our behavioral interventions, and the same baseline rate of participation to poll among cities. Power and significance levels are set to 0.80 and 0.05.

As laid out in the pre-registration document, we evaluate the effect of the three behavioral interventions using a mixed-effects logistic regression to predict a given's subject voting decision (a binary indicator). Using both proportion tests and logistic regressions, results from our analyses show that the required minimum sample size to capture a 4 percentage-points increase is 2,000 individuals in each condition. However, the baseline turnout rate in our sample may be significantly higher than the 0.7 turnout rate observed in 2017 in the age category 18-29 because our sample is composed of university students. There is evidence that young people with a university degree have higher turnout rates than those without a university degree. Survey results show a 20-percentage point difference between the two categories (Lardeux and Tiberj, 2022). Our sample is therefore not representative of the general youth population in France. To understand whether our desired sample size changes substantially by varying the baseline rate of poll participation, we run a power analysis with a 0.8 base rate participation. The expected increase in the poll participation rate is set to 4 percentage points. Power and significance levels are set to 0.80 and 0.05. Results show that the desired sample size is 1,444 individuals in each condition (base rate = 0.8).

**Appendix C – Experimental instructions**

Thank you for agreeing to participate in this study. This study is conducted by researchers from different laboratories in France, including the experimental economics laboratory that sent you the invitation. The study includes two phases. During the first phase, which starts today, you will have to answer an online questionnaire related to your intention to vote or not to vote in the first round of the presidential election of 2022.

In the second phase, which will take place between April 11$^{th}$ and 12$^{th}$, you will again be asked to answer a question about the same election online. Your participation in both phases will take less than 5 minutes and will allow you to be part of a draw with a significant financial gain.

If you participate in both phases of the study, you could win 120€ for your participation. Your winnings will depend on a random draw at the end of the study and **will be independent of your decision to vote in the presidential election**.

**To participate in the study, you must:**

- be between 18 and 29 years old at the time of your participation,

- have the legal right to vote in the 2022 French presidential election.

**Please note:** Participants who do not meet one or more of the above criteria will not be eligible to receive their earnings.

Your participation is completely voluntary. You may stop or withdraw from the study at any time without being held responsible. Your decision to participate or not will have no effect on your current or future relationship with anyone at the inviting laboratory or any other institution. However, if you do not participate in both phases of the study, you will not be eligible for the draw to win 120€.

**Baseline Questionnaire**

1. How likely are you to vote in the first round of the presidential election where 0 stands for “certain not to vote” and 10 for “certain to vote”?

○ 0 ○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7 ○ 8 ○ 9 ○ 10

2. Have you ever voted in at least one of the following elections: presidential elections, legislative elections, municipal elections?

○ Yes ○ No ○ Don’t know/don’t want to answer

3. When it comes to politics, people are often categorized as being on the left or the right. On a scale of 0 (very left-wing) to 10 (very right-wing), where would you place yourself?

○ 0 - very left ○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7 ○ 8 ○ 9 ○ 10 – very right

4. What percentage of 18–29 years old do you think will vote in the first round of the presidential election?

5. How do you see yourself? Are you generally a risk-taker or do you try to avoid risks? Please select the value that corresponds to you most from the proposed scale, knowing that 0 means 'Fear of risk' and 10 means 'Willing to take risks'.

○ 0 - fear of risk ○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7 ○ 8 ○ 9 ○ 10 – willing to take risks

6. Gender

○ Female ○ Male

7. Professional category

○ Employee ○ Student ○ Other or unemployed

8. Highest degree obtained

9. Do your parents own their home?

○ Yes ○ No

Thank you for your participation in the first phase of the study. You will receive an invitation between April 11 and April 13 to participate in the second phase of the study. As a reminder, your participation in both phases of the study is required if you wish to be eligible for compensation. You may now close this page.

**Advice-giving condition**

1. Since which year have women had the right to vote in France?

○ 1815 ○ 1880 ○ 1944

2. In which year was the election of the President of the Republic by direct universal suffrage introduced?

○ 1938 ○ 1962 ○ 1975

3. Is the German Chancellor elected by direct universal suffrage?

○ Yes ○ No

4. Is the President of the United States elected by direct universal suffrage?

○ Yes ○ No

5. According to survey evidence, for the French population, voting is mainly a duty or a right. Please indicate on the below scale the % of French citizens who said it is mainly a right.
6. Is eligible to vote in the French presidential election, any person of legal age who enjoys his or her civil and political rights, who is registered on the electoral list, and (several answers possible):

○ Is of French nationality established in France

○ Is of French nationality established outside France

○ Is of foreign nationality residing in France for at least 10 years

**Here you can check your answers and see what are the correct answers.**

We ask you to write a few lines explaining the reasons why you think it is important to vote in the 1st round of the presidential election. The objective is to motivate another young person to vote in the first round of the upcoming presidential election.

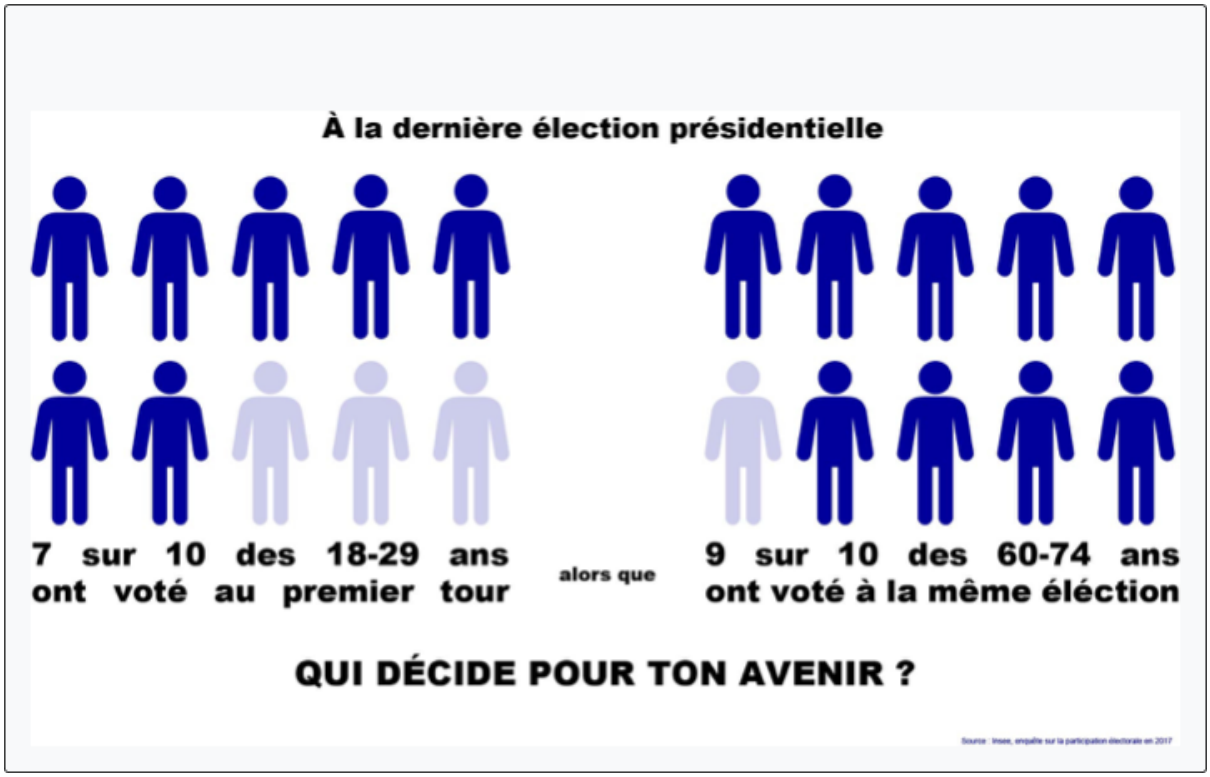
*After reading your message, the person will be asked if they found your message: Not at all convincing, not very convincing, convincing, or very convincing. 25 messages will be randomly selected at the end of the experiment. If your message is selected and the person who read your message indicated that your message was convincing or very convincing, then you will earn an additional 80€ (on top of the 120€ that you may earn for your participation, in case you are among the 90 subjects selected to receive a payment for their participation).*

Message to an 18-29-year-old to motivate them to vote (between 70 and 130 words)

☐ I don’t want to write the message.

**Between-group comparison condition**

In the first round of the last presidential election, 7 people out of 10 aged 18-29 years old voted. At the same time, 9 people out of 10 aged 60-74 years old voted in the same election. Who decides for your future?



**Implementation-Intention condition**

The French administration has developed an online service to check your electoral registration and your polling station.

*With your last name, first name and date of birth, it will take you two clicks to obtain the information regarding the voting bureau where you are registered to vote.*

The website is at the following address: https://www.service-public.fr/particuliers/vosdroits/services-en-ligne-et-formulaires/ISE.

**Please go to the website and check your voter registration and your registered polling place.**

☐ I confirm that I have checked my voter registration and my polling station.

For this last step, we ask you to answer the three questions below, assuming that you intend to go to vote in the 1st round of the presidential election on Sunday, April 10th. You may also select "Don't know / Don't want to answer".

1. At what time do you plan to vote?

○ In the morning ○ Between noon and 2 pm ○ In the afternoon

○ Don't know / Don't want to answer

2. Will you go alone or with someone else?

○ Alone ○ With someone else ○ Don't know / Don't want to answer

3. What do you plan to do right after you vote?

**End phase 1**

Thank you for your participation in this first phase of the study.

You will be contacted for the second part the study between April 11 and April 13, 2022.

**Welcome to the second phase of the study!**

We will ask you to report whether you voted in the first round of the presidential election held on Sunday, April 10.

Your answer to this question will have no effect on your potential earnings as long as your answer is truthful. At the end of the study, if you are drawn to be paid, you will have to send by email your certificate of registration on the electoral list. This document, which you can download from the service-public.fr website (we will send you the link), specifies the polling station in which you are registered to vote. Our team will then check the list of voters at your polling station (this list is communicated by the prefecture to any voter who requests it within ten days following the election) in order to verify whether your answer was truthful.

If you are randomly selected to be paid:

- if you have reported that you voted:
    - and our team finds your signature on the voters' list of your polling station, you will receive 120€.
    - but our team does not find your signature on the list of voters of your polling station, you will receive only 20€.

- if you have reported not to have voted:
    - and our team does not find your signature on the list of voters of your polling station, you will receive 120€.
    - but our team finds your signature on the list of voters of your polling station, you will receive only 20€.

If you choose not to answer the question by selecting "Do not wish to answer", and you are drawn to receive the payment, you will receive 20€.

**Question:**

Did you vote in the 1st round of the presidential election on Sunday, April 10th?

○ Yes ○ No ○ Don’t want to answer

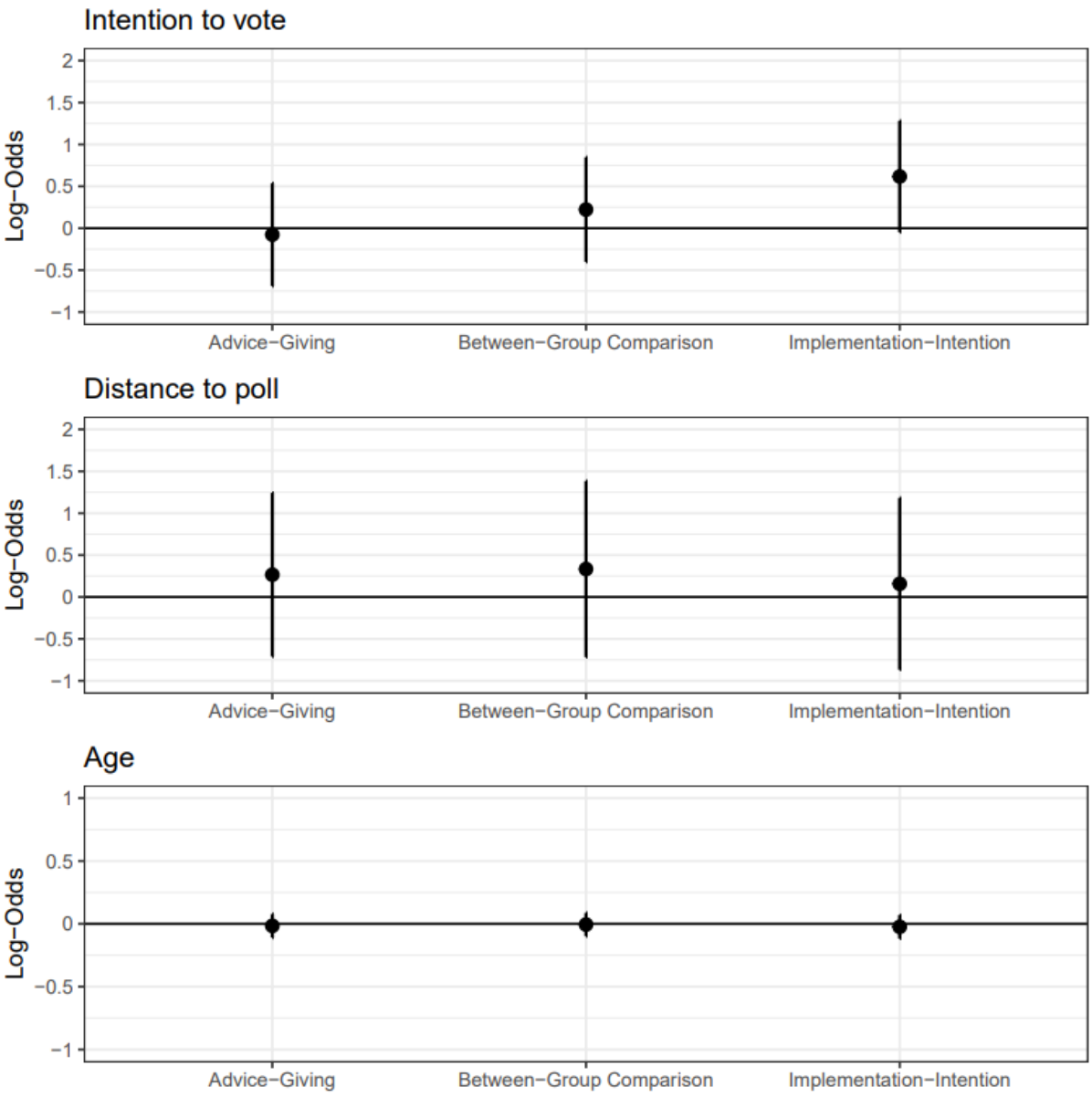
**End of the study**

Thank you for your participation in the two phases of this study, which is now finished. You will receive an email within 48 hours with a link to find out if you have been selected to be paid or not. As a reminder, 90 people who participated in both phases of the study will be randomly selected to be paid.

If you are one of the selected participants, you will find on the webpage the instructions to send your certificate of registration on the electoral list and the information regarding the procedure to collect your earnings.

**Appendix D – Additional analysis**

Figure 3. Heterogenous treatment effects




*Notes:* Bar reports the 95% confidence interval.