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

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

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 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

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38　nonvoters. Finally, there may be positive spillovers from increasing youth turnout as voters
39　are more likely to engage in other civic behaviors, like volunteering and donating (Lijphart,
40　1997).

41　We implemented a large-scale online experiment to test the effect of three behavioral
42　interventions aiming at increasing youth turnout in the first round of the 2022 French
43　presidential election. Prior to the election, survey data indicated that young people (ages 18-
44　29) had a lower intention to vote in the 2022 election compared to previous years (less than
45　60% intended to vote, while youth turnout in past presidential elections tended to be higher
46　than 70%; see IFOP, 2022). Policy briefs based on survey data pointed to several factors
47　explaining lower expected youth turnout, including a lack of interest in politics (Blais and
48　Daoust, 2020), a growing involvement in alternative modes of political expression, such as
49　protests or online activism (Muxel and Zulfikarpasic, 2022), and a lack of information on
50　whether and where one is registered to vote (Assemblée Nationale, 2021). Some of these
51　factors can be addressed using behavioral interventions. For example, given that young voters
52　frequently relocate for study or work (Juelich and Coll, 2020), they may lack information about
53　the polling station where they are registered to vote. A reminder about the polling station
54　may help them form a voting-plan.[1]

55　Our experiment tests three behavioral interventions that were co-designed during a
56　workshop that gathered researchers in behavioral economics, a group of social designers, and
57　a group of students from different universities. Our first behavioral intervention,
58　*Implementation-intention*, consists in informing participants about their polling station, and
59　asking them to provide a plan stating when they will vote, how they plan to go to the polling
60　station, and what do they plan to do after voting. These are similar questions to the ones used
61　in the existing literature that found significant behavior change using an implementation-
62　intention technique in the context of a US election (Nickerson and Rogers, 2010) as well as in
63　a health-related intervention (Milkman et al., 2011). The novelty of our intervention is to
64　complement plan formation with an information about one's polling station, an information
65　that young people may lack. The second behavioral intervention, *Between-group comparison*,
66　combines descriptive social information with a message that pits one's group against another
67　group that has a higher turnout. Following previous research showing that one way to
68　motivate cooperation in low-cooperative groups is to show them cooperation rates in high-
69　cooperative groups (e.g., Cardenas and Mantilla, 2015), our second intervention implements
70　social comparison with a form of inter-group competition that may increase intra-group
71　cooperation. The third behavioral intervention, *Advice-giving*, tests whether writing a short
72　motivational letter about the importance of voting can raise the turnout among advice givers.
73　Previous literature has emphasized several reasons why advice-giving may motivate behavior
74　change, including an effort to reduce cognitive dissonance (Aronson, 1999), prompting plan

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[1] 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.

75 formation (Gollwitzer, 1999), and increasing one's self-confidence (Eskreis-Winkler et al.,
76 2018).

77 We designed a multi-labs experiment that was conducted in partnership with eight
78 laboratories in France, specialized in experimental economics. Subjects from the eight labs
79 were randomized into three treatment conditions and one baseline. In all conditions, a few
80 days before the election day, which took place on April 10, 2022, subjects were invited to
81 complete a questionnaire, including questions related to their past participation in national
82 elections as well as their intention to vote in the upcoming presidential election. 4,117
83 subjects completed this first phase of the experiment. The day following the election day, on
84 April 11, subjects who completed Phase 1 were invited to complete Phase 2 of the study in
85 which they were asked to report whether they had voted or not. Overall, 3,790 subjects
86 completed the two phases of the experiment. To address concerns with self-reported
87 measures, we implemented an incentive-compatible method to elicit subjects' actual voting
88 behavior. In France, voting sheets signed by voters who cast a ballot on the election day are
89 available for consultation until ten days after each poll. Before answering the voting question,
90 subjects were informed that a subset of participants would be randomly selected to receive
91 payment and that for those participants our team would visit their polling stations. A subject
92 would receive 120€ if their self-reported voting decision corresponds to actual voting
93 behavior (as confirmed by the administrative data), and 20€ otherwise. As we show in the
94 design section, our procedure ensured truthful reports about subjects' voting behavior.

95 We find null effects from the three behavioral interventions. In the Baseline, 87% of the
96 subjects reported having voted, a turnout rate similar to what we observe in the three
97 conditions with a behavioral intervention. We investigate three possible explanations for the
98 lack of impact from our behavioral interventions. We present new data based on a follow-up
99 experiment, and from a survey, both conducted after the presidential election. First, given
100 the high baseline motivation to vote (87%), there may be no room for our behavioral
101 interventions to increase turnout. We conducted a follow-up experiment during the
102 legislative elections to address this concern. The turnout rate for the legislative election being
103 significantly lower than for the presidential election, our follow-up experiment allows us to
104 explore the effect of one of our behavioral interventions in two contexts, one with a high
105 baseline motivation to vote, and one with a moderate baseline motivation. We find no
106 differences in turnout between our behavioral intervention and the Baseline condition in the
107 context of legislative elections. We also conducted a survey to address what one may consider
108 an abnormally high turnout rate among young people in the Baseline. We find that the
109 turnout rate in the Baseline is not the consequence of the invitation email that subjects
110 received a few days prior to the election, that could have acted as a reminder about the
111 upcoming election. Instead, the turnout in the Baseline is representative of the participation
112 of highly educated individuals who compose our sample (i.e., university students). The third
113 possible explanation for the null effect that we discuss relates to the rising literature finding
114 limited (if any) impact from "light touch" interventions in several contexts.

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115 Our study contributes to the understanding of whether behavioral interventions can work as
116 an effective tool to increase voter turnout. Research leveraging behavioral insights to increase
117 turnout has been mostly carried out in the context of US elections, that are characterized by
118 a relatively low baseline voter participation (Gerber and Green, 2017). Behavioral
119 interventions such as implementation-intention (Nickerson and Rogers, 2010), social
120 information about high or low turnout (Gerber et al., 2008), reminders (Dale and Strauss,
121 2007; Malhotra et al., 2011), and pledges to vote (Costa et al., 2018) have been shown to
122 positively impact voter turnout in some of the recent US elections. Outside of the US, the
123 experimental evidence on the effect of behavioral interventions on voter participation is rare.
124 Braconnier et al. (2017) tested the effect of door-to-door canvassing on voter registration and
125 turnout in the 2012 French presidential and legislative elections. They found a positive effect
126 from their intervention on turnout in the presidential election (for which the level of turnout
127 is generally high, more than 70%), but a limited impact on turnout in the legislative election
128 (with a significantly lower turnout than the presidential election, around 55%). Another
129 behavioral intervention implemented outside of the US is by Bergh et al.'s (2018) who
130 experimentally tested the effect of text reminders in the context of municipal elections in
131 Norway where turnout is generally moderate to high (60% in 2015). They found a positive
132 effect on turnout. We add to the existing literature by investigating the effect of behavioral
133 interventions on youth turnout in two contexts: 1) the French presidential election, with a
134 relatively high turnout, even among young people, and 2) the French legislative election with
135 a moderate to low baseline participation. The existing research studies the effect of nudges
136 in only one election context, characterized by either high or low turnout, while our study
137 covers two elections with very different turnout rates.[2] In that sense, our work contributes to
138 the recent literature investigating how a population's baseline motivation can affect the
139 potential of nudges to change people's behavior (Saccardo et al., 2024).

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

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[2] 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.

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152 measures at lower cost. The only constraint is to have access to attendance sheets, as is the
153 case in France.

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

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

166 **2. Experimental design**

167 We partnered with eight academic laboratories in France, specialized in experimental
168 economics and possessing a subject pool managed through an online platform, such as hroot
169 (Bock et al., 2014), ORSEE (Greiner, 2015) or SONA (www.sona-systems.com).[3] Subjects
170 registered in one of our partner laboratory's databases received an invitation email to
171 participate in an online experiment consisting of two phases: 1) the first phase took place
172 from April 6th to April 8th, 2022, and 2) the second phase from April 11th to April 13th, 2022.
173 From the study's research question, there were two main inclusion criteria: age and
174 nationality. Young voters are generally defined as being between 18 (the minimum legal age
175 to vote in France) and 29 years old (e.g., Pintor et al., 2004; Assemblée Nationale, 2021). The
176 other criterion is nationality, as voting in the presidential election is restricted to French
177 citizens.

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

183 *2.1 First phase and the experimental conditions*

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[3] 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.

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184   Subjects were randomized into three treatment conditions and one baseline. The different
185   treatments were co-designed during a workshop that took place on February 8th, 2022. The
186   workshop gathered researchers from several academic institutions in France, a group of social
187   designers from a private company, and a group of students from various French universities.
188   During the workshop, participants were divided into small groups and worked on developing
189   several behavioral solutions to increase youth turnout in the 2022 French presidential
190   election. At the end of the workshop, five behavioral solutions emerged as possible
191   candidates to be tested experimentally. The five behavioral solutions were subsequently
192   submitted to an online vote. The researchers involved in this project were invited to rank the
193   five solutions. We selected the three solutions which were expected to have the highest
194   potential to increase turnout according to this ranking.[4]

2.1.1 Baseline condition

196   In all conditions, subjects first consented to participate in the two phases of the experimental
197   study, and were then asked to state how likely they were to vote in the first round of the
198   upcoming presidential election, on April 10, 2022, by choosing a number between 0 (very
199   unlikely to vote) and 10 (very likely to vote). This pre-intervention measure of the *intention-*
200   *to-vote* allows to check the quality of the randomization between conditions.[5] This measure
201   is also useful to investigate heterogenous effects of our interventions, since we expect our
202   interventions to have a stronger effect on subjects with moderate preexisting motivations to
203   vote (Saccardo et al., 2024).[6]

204   Subjects were then asked to complete a demographic questionnaire and to answer questions
205   regarding their previous voting experience, political preferences, beliefs regarding the
206   participation rate of the 18–29-year-old on the election day, risk preferences, and altruism
207   (see complete instructions in Appendix C). The baseline condition did not contain any
208   encouragement message to vote. The following three treatments correspond to our three
209   behavioral interventions.

2.1.2 Treatment 1: implementation-intention

211   Implementation intention has been widely proven to be an effective strategy to promote
212   desirable behaviors in the public health domain (Gollwitzer & Oettingen, 1998; Milne et al.,
213   2000; Gollwitzer & Sheeran, 2006; Milkman et al., 2011). It mainly refers to a plan stating
214   when, where and how to attain a goal (Gollwitzer, 1999). Developing such a plan requires the
215   subject to activate the mental representation of the desirable behavior and to anticipate the
216   situations associated with it, which thus facilitates the initiation and/or the maintenance of
217   desirable behaviors (Gollwitzer, 1999). Even simple plans, containing only a few information,

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[4] Following a power analysis and our expectation of the number of subjects that could be recruited from each location, we decided to test three interventions rather than five.

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

[6] 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.

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218 seem to produce an effect. For example, Milkman et al. (2011) simply prompted participants
219 in their study to write down the date and time they planned to be vaccinated, which led to a
220 significant increase in vaccination rates compared to the condition without the date and time
221 of vaccination prompt. In the context of voting, Nickerson and Rogers (2010) asked American
222 voters to write down when they would vote, where they would be coming from and what
223 they would do before voting. In their case, the implementation intention increased turnout
224 by 4.1 percentage points compared to a baseline without an intention implementation stage.

225 Our implementation intention treatment consisted of two steps. In the first step, participants
226 were asked to verify the location of the polling station where they are registered to vote by
227 clicking a link directing to the website "Service Public"[7], created by the French government
228 and independent from our experimental platform. The verification procedure is quick and
229 requires easy to recall information such as one's name, surname, gender, and date of birth.
230 This first step addresses one of the key factors of low youth turnout, i.e., the registration-
231 location obstacle due to the frequent residential relocation of young people. Evidence shows
232 that young people often lack knowledge about the polling place where they are registered to
233 vote (Assemblée Nationale, 2021). We facilitate plan-making by providing participants with
234 the information about the polling station where they are registered to vote. During this step,
235 98% of our subjects in this condition downloaded the information regarding the location of
236 their polling station.

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

242 *2.1.3 Treatment 2: between-group comparison*

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

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[7] The link to the website: https://www.service-public.fr/particuliers/vosdroits/services-en-ligne-et-formulaires/ISE

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255 message that the desired behavior is frequent and growing, which significantly increased gym
256 attendance.

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

262 We chose to compare the voting rates of young people with the age category on the other
263 side of the age spectrum for two reasons. First, evidence shows that political preferences
264 evolve over time and that younger people tend to vote with left-wing political parties while
265 older people tend to vote for right-wing political leaders (Harris Interative, 2022). Thus, a
266 political preference gap exists between the two age categories, which may create a stronger
267 feeling of opposing interests and may motivate young people to vote. Second, the 18-29 age
268 category had the lowest turnout rate in the preceding French presidential election (in 2017),
269 whereas the 60-74 age category had the highest turnout rate.[8] Cardenas and Mantilla (2015)
270 have shown that one way to motivate cooperation in low-cooperative groups is to show them
271 cooperation rates in high-cooperative groups. This intervention therefore implements social
272 comparison in the form of inter-group competition that can increase intra-group cooperation
273 (voting within the 18-29 age category with the lowest turnout rate in the previous presidential
274 election).

2.1.4 Treatment 3: advice-giving

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

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

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

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291 offered subjects some information that could be used as inputs when writing the motivational
292 text.

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

306 Another reason we chose to implement incentives for writing convincing messages is to
307 reduce the number of subjects who would not take this task seriously. The mechanisms
308 behind our advice-giving intervention require the advice-giver to use convincing enough
309 arguments. In our experiment, out of the 836 messages, only one was not related to voting.
310 Of the randomly chosen messages that were evaluated for payment, 80% were considered
311 convincing or highly convincing by a panel of raters.[9]

312 *2.2 Second phase and the incentive structure to reveal voting behavior*

313 The second phase of the experiment started on the day after the election took place, on April
314 11th, and it ended on April 13th, 2022. Subjects from all four conditions were recontacted by
315 the same lab who had initially invited them to participate in the experiment. In the second
316 phase of the experiment, subjects were asked to self-report whether they had voted or not
317 on the election day, on April 10th. An obvious concern with self-reported measures is the
318 problem of misreporting. Subjects may engage in misreporting for various reasons, including
319 desirability bias or self-image concerns. There is evidence that questions on political behavior
320 are particularly prone to misreporting (e.g., Wright, 1993).

321 We implemented an original, incentive-compatible, method to elicit subjects' actual voting
322 behavior. Specifically, in the first phase of the experiment, in the invitation email, subjects
323 were informed that 90 participants in this study would be randomly selected to receive
324 payment for their participation. In the second phase, before self-reporting whether they had
325 voted or not, subjects were informed that for the 90 participants who would receive payment,

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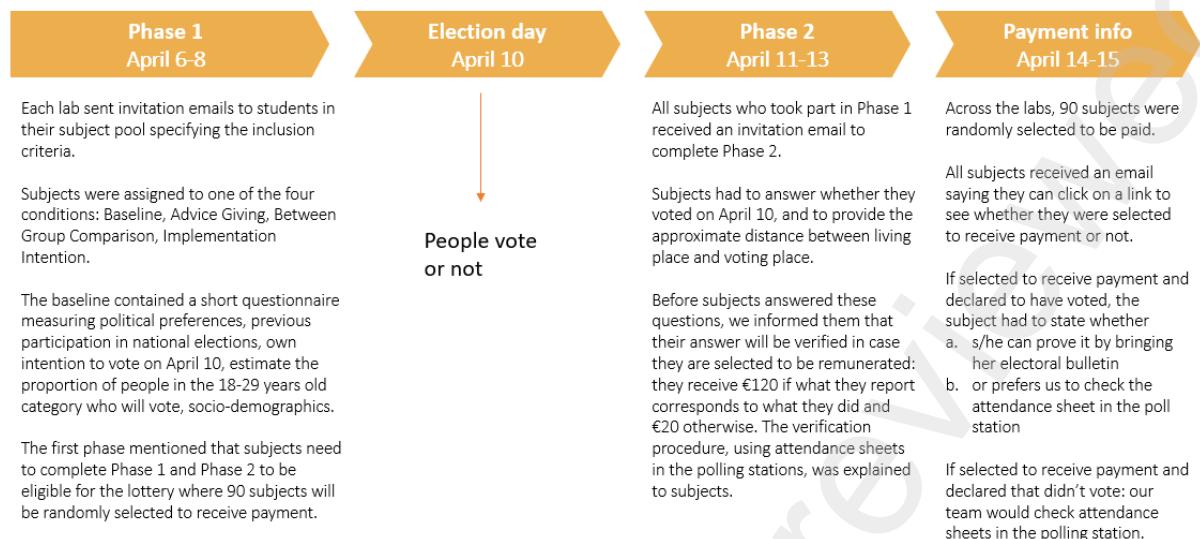
[9] Every message was randomly assigned to a rater. Raters were recruited from students who did not take part in one of the
experimental conditions presented above. Raters were all students in the same age category (18-29) as our subjects who
acted as advice-givers. Each of the 25 messages was rated 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.

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326　our team would visit their polling station to verify whether they actually voted or not.[10] In
327　France, voting sheets signed by voters who cast a ballot on election day are available for
328　consultation until ten days after each poll. We informed our subjects about the verification
329　procedure and that the amount they would earn in this experiment would depend on their
330　decision when self-reporting whether they voted or not: they receive 120€ if what they self-
331　report corresponds to what they effectively did, as confirmed by the administrative data (e.g.,
332　if someone either reported to have voted and this is confirmed by the administrative data or
333　that someone reported not to have voted and that this is confirmed by the administrative
334　data), and 20€ otherwise. Out of the 90 subjects randomly selected to receive payment
335　(whose self-reported voting behavior was thus verified) only one misreported.[11] Figure 1
336　summarizes our experimental design.

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[10] 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).

[11] 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.

Figure 1. Summary of the experimental design

| Phase 1 April 6-8 | Election day April 10 | Phase 2 April 11-13 | Payment info April 14-15 |

Each lab sent invitation emails to students in their subject pool specifying the inclusion criteria.

Subjects were assigned to one of the four conditions: Baseline, Advice Giving, Between Group Comparison, Implementation Intention.

The baseline contained a short questionnaire measuring political preferences, previous participation in national elections, own intention to vote on April 10, estimate the proportion of people in the 18-29 years old category who will vote, socio-demographics.

The first phase mentioned that subjects need to complete Phase 1 and Phase 2 to be eligible for the lottery where 90 subjects will be randomly selected to receive payment.

People vote or not

All subjects who took part in Phase 1 received an invitation email to complete Phase 2.

Subjects had to answer whether they voted on April 10, and to provide the approximate distance between living place and voting place.

Before subjects answered these questions, we informed them that their answer will be verified in case they are selected to be remunerated: they receive €120 if what they report corresponds to what they did and €20 otherwise. The verification procedure, using attendance sheets in the polling stations, was explained to subjects.

Across the labs, 90 subjects were randomly selected to be paid.

All subjects received an email saying they can click on a link to see whether they were selected to receive payment or not.

If selected to receive payment and declared to have voted, the subject had to state whether
a. s/he can prove it by bringing her electoral bulletin
b. or prefers us to check the attendance sheet in the poll station

If selected to receive payment and declared that didn't vote: our team would check attendance sheets in the polling station.

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 ($X^2 \, test$, $p=0.18$). 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)	p-value
Age						
Mean (SD)	22.0 (2.75)	22.1 (2.79)	22.0 (2.80)	22.0 (2.80)	22.0 (2.79)	0.72

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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%)	0.873
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)	0.846
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%)	0.203
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%)	0.979
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%)	0.929
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%)	0.952
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%)	0.918
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%)	

Note: p-values are based on Chi-squared tests that were performed to evaluate the equality across conditions.

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

12
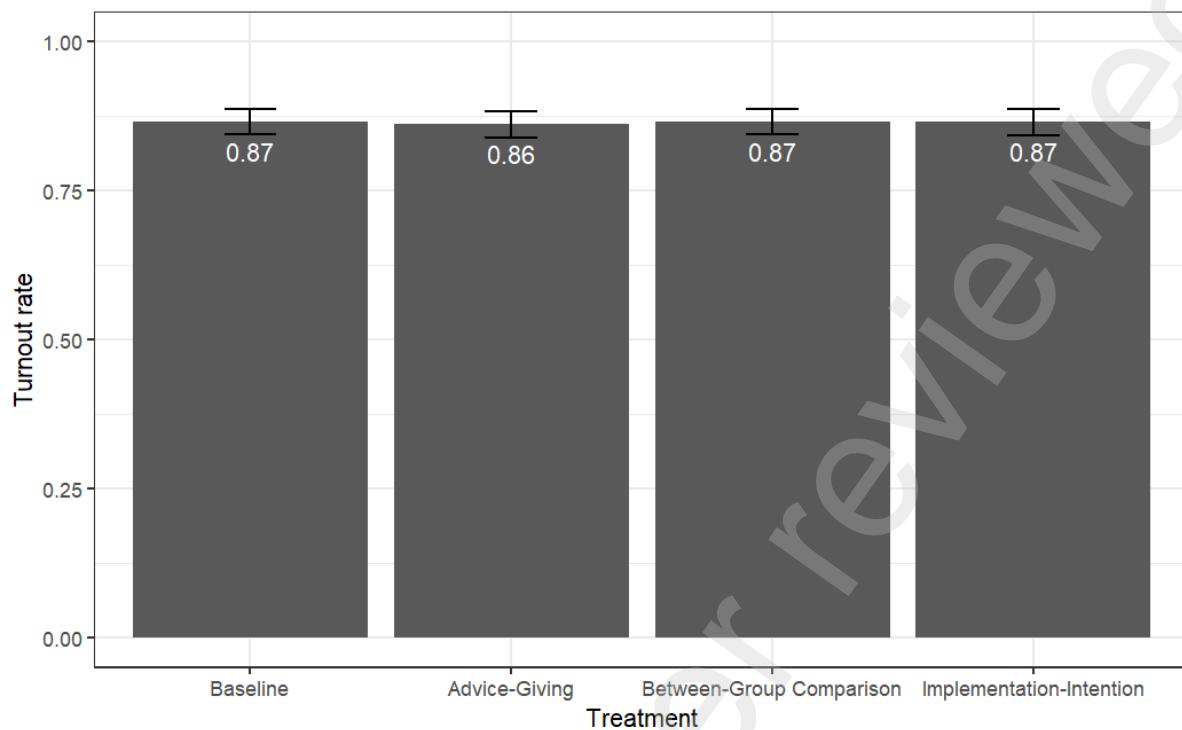
367 vote", 74% reported a 10, and the average intention to vote was 8.9. The data, therefore,
368 show that our sample had a high pre-existing motivation. This is consistent with survey results
369 showing that young people with a university degree have a higher turnout rate than those
370 without a university degree.[12] Finally, Table 1 also shows that age, gender, education, political
371 orientation, past electoral participation, distance from the polling station, and intention-to-
372 vote in the upcoming election were all balanced across treatment conditions.

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

380 *3.1 Turnout rates across treatment conditions*

381 The average turnout rate in our sample is high. Overall, 87% reported having voted on the
382 election day. Figure 2 shows the turnout rates in each of our four conditions. In the Baseline,
383 87% reported having voted, which is identical to the turnout rate in the Between-Group
384 Comparison and in the Implementation-Intention conditions. The Advice-Giving condition has
385 the lowest turnout rate, 86%, but is not statistically different from the Baseline. Our first result
386 is thus the absence of significant differences between the baseline turnout and the turnout
387 rates in the other three conditions (proportion test, *p = 0.703*).

---

[12] 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.

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.

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

where, *Voted* is an indicator variable (1 when reported having voted and 0 otherwise); *Treatment* is the primary predictor variable in our regression, and δ is a vector of indicators for assignment to each of the study's three experimental conditions (an indicator for the control condition is omitted). z is a vector of controls in our model, including 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 that our behavioral interventions had no significant effect on voter turnout compared to 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 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

14

412 those in the 25-29 category[13]; and that individuals with higher education levels are more likely
413 to vote (Lardeux and Tiberj, 2022). Furthermore, we find that political preferences are
414 significantly associated with poll participation. Individuals that reported to be more left-
415 oriented are more likely to vote than those who are on the opposite side of the political
416 spectrum.

3.2 Robustness checks

418 To ensure the validity of our results, we ran a series of robustness checks. Column 3 of Table
419 2 shows that the results remain very similar when we exclude all subjects who refused to
420 report whether they voted or not on the election day. In total, 22 subjects (0.5%) refused to
421 answer this question. Although subjects could refuse to answer the voting question, they
422 were informed that by refusing, they would be excluded from the lottery giving rise to bonus
423 payments. In the previous analyses (columns 1 and 2), we assumed that those who refused
424 to answer the voting question did not vote. In practice, revealing non-voting may come with
425 a psychological cost that, for some subjects, may be higher than the expected monetary
426 earnings from the experiment (and we see no reasons why someone who had voted would
427 refuse to answer the voting question).

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

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

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Table 2. Mixed-effects logistic regression models of voting behavior with all controls

438 **(presidential election)**

Sample:			Only valid vote response	Only with plan	Only with advice	All
	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

439 *Note: models 1-2 and 6 use our full sample, considering all participants who voluntarily did not provide an*
440 *answer to the vote participation question as no voters, while model 3 excludes subjects who did not provide an*
441 *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 the lack of impact from our behavioral interventions. 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 the null effect is 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 among young people in our Baseline. The survey and the follow up experiment were not pre-registered and, as such, are part of an exploratory discussion. The third possible explanation for the null effect that we discuss below 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?

The null 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.[14]

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 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

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[14] 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 are available upon request.

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478 subjects into those residing within 5 kilometers from the polling station (representing 67% of
479 all participants), and those residing farther away from the voting place (33%). We also
480 conduct the same heterogeneity analysis using the full scale of values obtaining similar
481 results. Table 3 shows the regression results from our heterogeneous treatment effect
482 analysis (Figure 3 in Appendix D presents a visual illustration of the results). We find no
483 evidence of heterogeneous effects of our treatments with respect to the three dimensions of
484 subjects' initial motivation to vote.

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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 models use our full sample, considering all participants who voluntarily did not provide an answer to the vote participation question as no voters. *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 an exploratory (not 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

495 2017, only 44% of the young people voted for the legislative election, while they were 78% to
496 have voted for the presidential election.[15] We leveraged this opportunity to investigate
497 whether our behavioral interventions may influence voter turnout in an election with
498 moderate turnout rates.

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

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

519 Of the 1,885 eligible subjects, 1,012 participated in the new experiment: 523 in the Baseline
520 and 489 in the Advice-Giving. In the Baseline, 63% of subjects reported having voted. The
521 turn-out is very similar, equal to 62%, in the Advice-Giving condition. Table 4 shows the results
522 from a mixed-effects logistic regression. There is no significant difference between the
523 Baseline and the Advice-Giving condition, with and without controls.

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

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 detect no 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.[16] 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,

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

23

566 20% have a bachelor's degree and 22% have a master's degree), the high turnout in the
567 Baseline seems congruent with turnout data of highly educated young people in France.

568 *4.3 The limited impact of light touch interventions*

569 After showing that our results are not influenced by the design of our Baseline and showing
570 that the null effects are not explained by a high pre-existing motivation to vote, we discuss
571 the literature on the limited power of nudges to change people's behavior. There is increasing
572 evidence that nudges have a limited impact (if any), especially when brought to scale (Cantor
573 et al., 2015; Carrera et al., 2018; Goldzahl et al., 2018; Oreopoulos and Petronijevic, 2019;
574 Kristal and Whillans, 2020; Löschel et al., 2020; Gravert and Collentine, 2021; Andor et al.,
575 2022; Holzmeister et al., 2022; Neckermann et al., 2022). For example, Oreopoulos and
576 Petronijevic (2019) designed six nudges to improve student grades and persistence that they
577 tested on 25,000 students across three different campuses. They found no significant effects
578 on the primary variables of interest. Similarly, Kristal and Whillans (2020) tested five standard
579 nudges to reduce single-occupancy vehicle commutes and found that their interventions
580 failed to increase carpool sign-up or usage. DellaVigna and Linos (2022) reviewed evidence
581 from all published and unpublished large-scale nudge trials conducted by two major nudge
582 units in the US. Comparing the nudge effects found in these large-scale trials to the effects of
583 the nudges documented in the academic literature, the authors find that the average effect
584 sizes in the large-scale field trials are much smaller than those reported in the literature and
585 that publication bias explains a large share of the gap.

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

604

24

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 youth turnout 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 for the null effect relates to the rising literature finding limited (if any) impact from soft behavioral interventions in several contexts.

Our study adds to this literature by investigating the effect of behavioral interventions on youth turnout 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).

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

644 for researchers seeking to measure actual voting behavior in countries that do not provide
645 access to administrative data about individual voting behavior.

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

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Appendix A – Online implementation

799   The experiment was implemented using the oTree web-based platform (Chen et al., 2016).

800   We used oTree because it allows to create "rooms", each room corresponding to a virtual

801   laboratory where we can set conditions on who can participate in the experiment by using

802   subjects' unique IDs. For the institutions involved in the experiment, a necessary condition

803   was to have a subject pool managed via the hroot, ORSEE or SONA platforms because it

804   allowed us to use the unique IDs generated by the platform as the subject's ID in the

805   experiment. We created a separate room for every partner institution involved in the

806   experiment. Each lab manager was able to do the recruitment independently by sending an

807   invitation email to the subject pool from their university via their hroot, ORSEE or SONA

808   platforms. Invitation emails were scheduled to be sent on April 6th. However, given the large

809   number of subjects in each hroot, ORSEE and SONA database, not everyone received the

810   invitation email at the same time. In fact, some subjects received it within a few minutes from

811   the moment when invitations were sent while other subjects received it about 24 hours later.

812   For the final payment of subjects, after the study's second phase, each partner institution

813   received a list with the ORSEE, hroot, or SONA IDs of subjects who completed the study. We

814   developed an algorithm for the random selection of paid participants that ensures that at

815   least ten participants from each experimental platform are selected. The last ten participants

816   were randomly assigned. Subjects were then contacted individually to be paid according to

817   the local laboratory's compensation policy (in cash, via an online transfer, or using any other

818   payment method) and depending on whether their self-reported voting decision matched

819   what they actually did (information obtained from the administrative data or the subject's

820   electoral card).

Appendix B – Power analysis

823 Our main variable of interest is the proportion of subjects who declared to have voted on
824 April 10, 2022. To estimate the required sample size, we took the baseline proportion of poll
825 participation equal to 0.7 (based on the turnout of young people in the 2017 French
826 presidential elections) and assumed an expected increase associated with any of our
827 behavioral interventions of 4% (Cohen's h=9%). Furthermore, we assumed city-level
828 homogeneity in the effect associated with any of our behavioral interventions, and the same
829 baseline rate of participation to poll among cities. Power and significance levels are set to
830 0.80 and 0.05.

831 As laid out in the pre-registration document, we evaluate the effect of the three behavioral
832 interventions using a mixed-effects logistic regression to predict a given's subject voting
833 decision (a binary indicator). Using both proportion tests and logistic regressions, results from
834 our analyses show that the required minimum sample size to capture a 4% increase is 2,000
835 individuals in each condition. However, the baseline turnout rate in our sample may be
836 significantly higher than the 70% turnout rate observed in 2017 in the age category 18-29
837 because our sample is composed of university students. There is evidence that young people
838 with a university degree have higher turnout rates than those without a university degree.
839 Survey results show a 20-percentage point difference between the two categories (Lardeux
840 and Tiberj, 2022). Our sample is therefore not representative of the general youth population
841 in France. To understand whether our desired sample size changes substantially by varying
842 the baseline rate of poll participation, we run a power analysis with a 0.8 base rate
843 participation. The expected increase in the poll participation rate is set to 4%. Power and
844 significance levels are set to 0.80 and 0.05. Results show that the desired sample size is 1,444
845 individuals in each condition (base rate = 0.8).

846 **Appendix C – Experimental instructions**

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

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

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

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

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

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

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

864 Your participation is completely voluntary. You may stop or withdraw from the study at any
865 time without being held responsible. Your decision to participate or not will have no effect on
866 your current or future relationship with anyone at the inviting laboratory or any other
867 institution. However, if you do not participate in both phases of the study, you will not be
868 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.
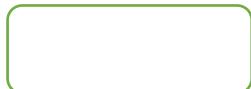
After reading your message, the person will be asked if they found your message: Not at all convincing, not very convincing, convincing, or very convincing. 25 messages will be randomly selected at the end of the experiment. If your message is selected and the person who read your message indicated that 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)

929

930
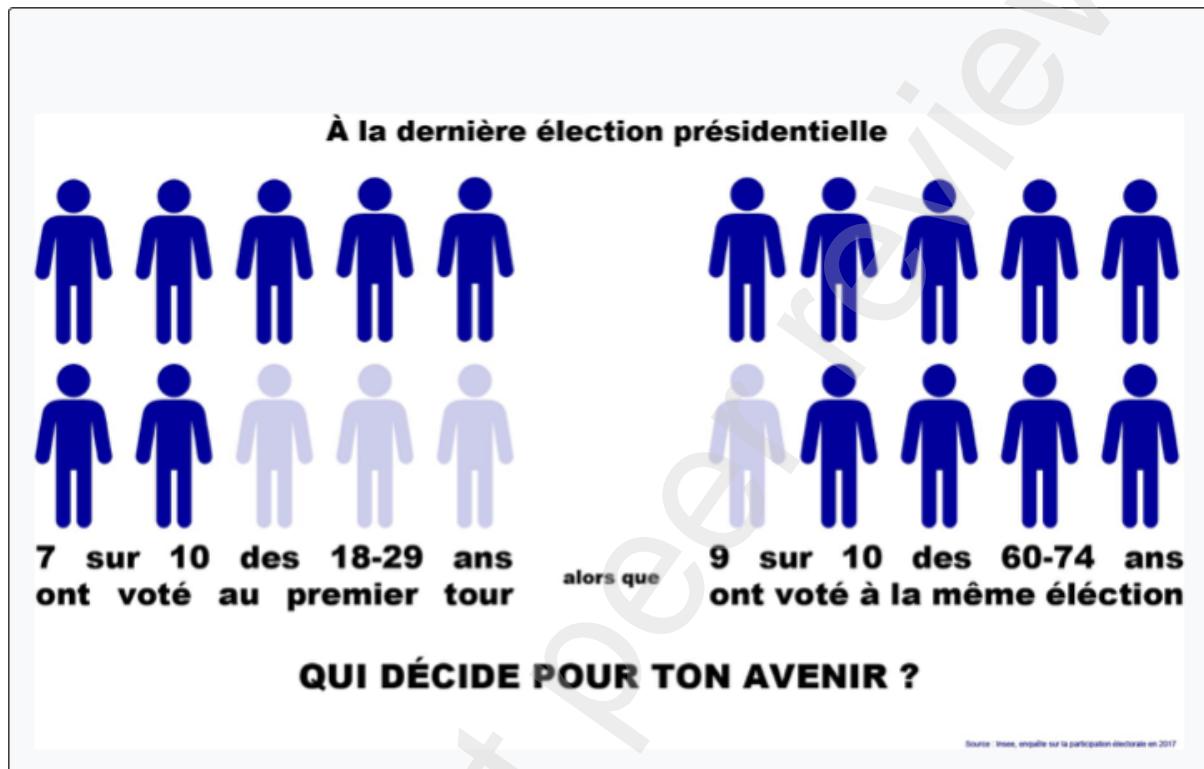
931 ☐ I don't want to write the message.

933 **Between-group comparison condition**

934 In the first round of the last presidential election, 7 people out of 10 aged 18-29 years old

935 voted. At the same time, 9 people out of 10 aged 60-74 years old voted in the same election.

936 Who decides for your future?

937

938

939

940

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?
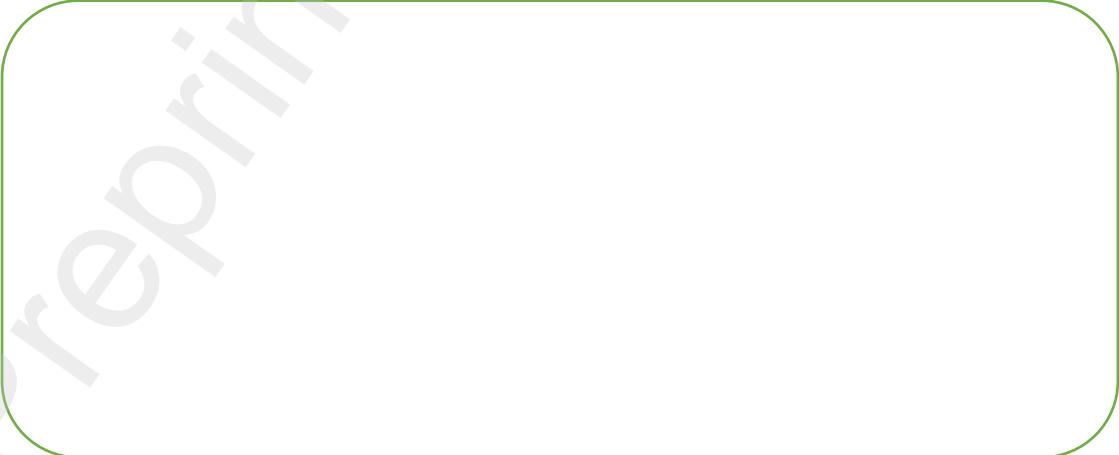
968   **End phase 1**

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

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

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

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

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

981 If you are randomly selected to be paid:

982 • if you have reported that you voted:
983 ○ and our team finds your signature on the voters' list of your polling station, you
984 will receive 120€.
985 ○ but our team does not find your signature on the list of voters of your polling
986 station, you will receive only 20€.
987

988 • if you have reported not to have voted:
989 ○ and our team does not find your signature on the list of voters of your polling
990 station, you will receive 120€.
991 ○ but our team finds your signature on the list of voters of your polling station,
992 you will receive only 20€.
993

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

996 **Question:**

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

998 ○ Yes   ○ No   ○   Don't want to answer

999

1000   **End of the study**
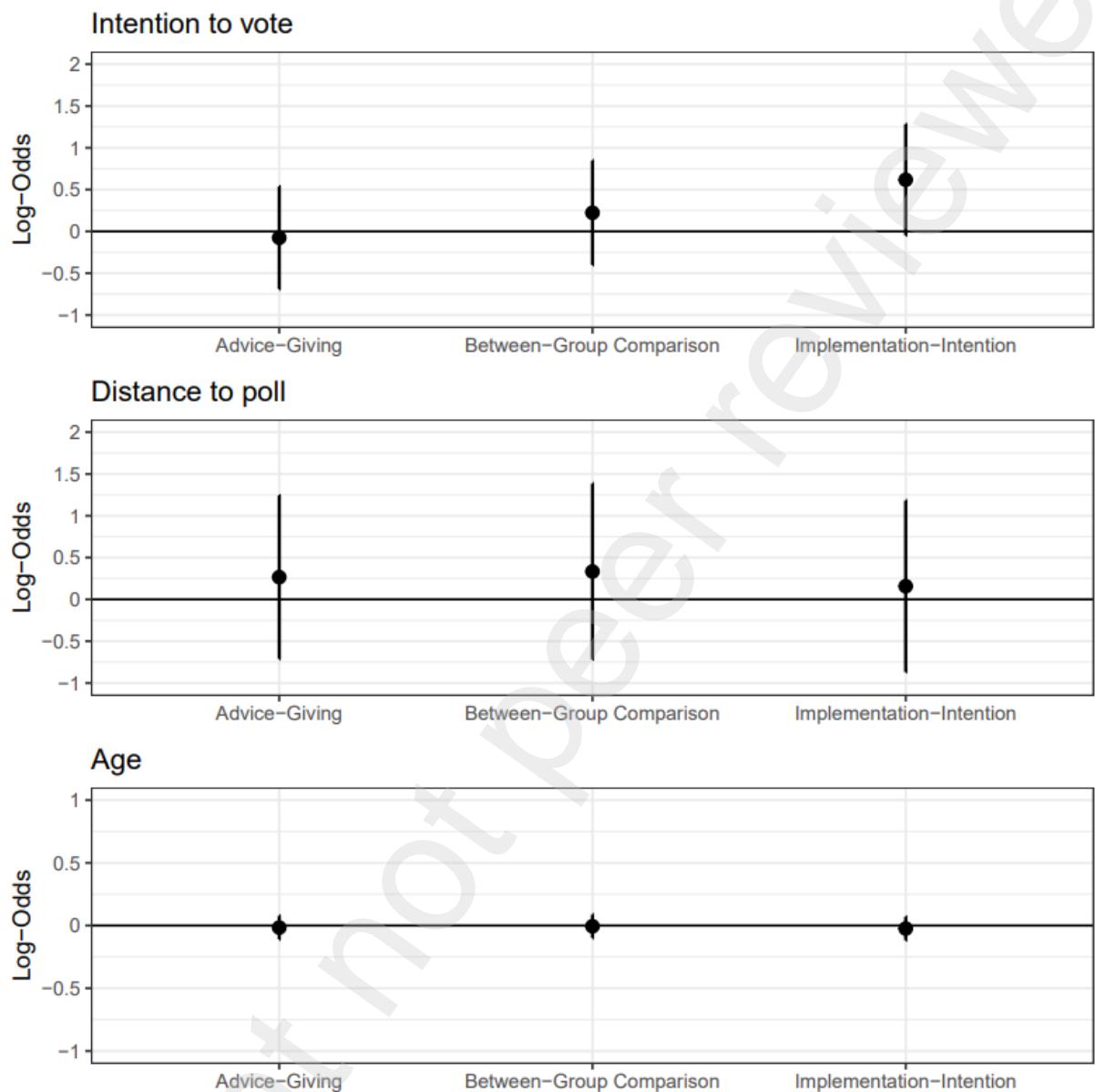
1001   Thank you for your participation in the two phases of this study, which is now finished. You
1002   will receive an email within 48 hours with a link to find out if you have been selected to be
1003   paid or not. As a reminder, 90 people who participated in both phases of the study will be
1004   randomly selected to be paid.

1005   If you are one of the selected participants, you will find on the webpage the instructions to
1006   send your certificate of registration on the electoral list and the information regarding the
1007   procedure to collect your earnings.

1008

Appendix D – Additional analysis

1010   Figure 3. Heterogenous treatment effects

Intention to vote

Distance to poll

Age

1011

1012   *Notes:* Bar reports the 95% confidence interval.

1013