

1 **Thinking about default enrollment lowers**
2 **vaccination intentions and public support in G7**
3 **countries**

4 Sanchayan Banerjee^{1,2}, Peter John³, Brendan Nyhan⁴, Andrew
5 Hunter[‡], Richard Koenig[‡], Blake Lee-Whiting⁵, Peter John Loewen[¶],
6 John McAndrews⁶, and Manu Savani⁷

7
8 **Abstract**

9 Policymakers often face a conundrum between being transparent about
10 policies and ensuring that those policies are effective. This challenge is
11 particularly relevant for behavioral nudges, which are not usually disclosed.
12 Rather than avoiding transparency, we suggest that policy-makers encourage
13 citizens to reflect on nudges to help them understand their own views and
14 align those views with their behaviors. Using data from an online survey
15 experiment with 24,303 respondents in the G7, we examine the impact of
16 reflection on a hypothetical default nudge policy for COVID-19 booster
17 appointments. Contrary to expectations, participants say they would be less
18 likely to get the booster when automatically enrolled compared to a control
19 condition. Similarly, encouraging citizens to think about the status quo
20 (baseline) policy also reduces intentions for boosters. These interventions
21 had no effect on approval of the policy. Further, encouraging people to think
22 about automatic enrollment decreased approval of the policy and further
23 decreased their intentions to get vaccinated. These findings suggest that
24 reflection on a nudge can increase backlash from a nudge and also elicit
25 policy disapproval, thereby aligning policy support with behavioral
26 intentions.

27 **JEL:** C90, D91, I12, I18, J18

28 **Keywords:** Nudge, Nudge+, Reflection, Policy effectiveness, Policy support.

¹ Corresponding author: S.Banerjee@vu.nl. Vrije Universiteit Amsterdam, Netherlands.
Contact: +31(0)647778058

² Vrije Universiteit Amsterdam, Netherlands

³ King's College London, United Kingdom

⁴ Dartmouth College, United States

⁵ University of Toronto, Canada

⁶ McMaster University, Canada

⁷ Brunel University London, United Kingdom

1 **Significance Statement:** Behavioral nudges can effectively encourage citizens to
2 engage in prosocial behavior; but often operate covertly. To enhance their
3 legitimacy, we propose encouraging the public to reflect on nudges. In a survey
4 experiment conducted among 24,303 participants in G-7 countries, we evaluated
5 the effect of reflection in the context of a hypothetical COVID-19 booster
6 appointment default policy. Contrary to expectations, the default reduced
7 vaccination intentions and did not measurably change policy approval. Reflecting
8 on the default exacerbated this negative effect on intentions and also diminished
9 policy support. In this sense, reflection on nudges may help citizens form policy
10 evaluations that align with the behavioral effects of the interventions in question.
11

12 **Introduction**

13 Providing explicit explanations and justifications of public policies can
14 sometimes decrease the positive effects of these policies, generating a trade-off
15 between transparency and citizen welfare [43]. This dilemma is particularly
16 acute for behavioural policies like nudges, through which governments attempt
17 to improve the choices made by citizens by altering their “choice architecture”
18 without directly dictating individual actions [50]. This style of “libertarian
19 paternalism” [49, 47] has sparked debate about the ethics of policy interventions
20 that shape people’s choices without disclosure [38, 16, 41, 36, 29, 37].
21 Behavioural nudges are said to differ from traditional “command and control”
22 policies like taxes in their public visibility — often referred to as the publicity
23 principle [21]. While most traditional public policies are overt, some nudges, like
24 defaults, seek to alter the choice architecture that individuals face rather than,
25 say, communicating information. The covertness of the nudge facilitates its
26 effectiveness, as exemplified by the phrase that nudges often “work in the dark”
27 [6]. Prior research has considered the effects of disclosure either prior to or after
28 a nudge intervention [20, 40, 7], but not simultaneously. There is mixed evidence
29 on the impact of disclosure on the effectiveness of the nudge, varying in the type
30 of disclosure used [31, 32].

31 To address concerns about the potential undue influence of nudges, we
32 evaluate a new type of behavioural public policy intervention called “nudge+”
33 [4]. This intervention seeks to make nudges more legitimate by encouraging
34 people to think about the policy or choice in question and thereby facilitating
35 citizen reflection on nudges. Such an alternate approach can empower citizen
36 autonomy and agency by making individuals watchful of government policies
37 and intentional in their choices and actions. Nudge+ builds on prior research
38 suggesting that offering such transparency and reflection may improve the

1 effectiveness of nudges when citizens' goals are aligned with the nudge [28, 2,
2 19].

3 Building on studies testing the effects of nudges on vaccination uptake [19,
4 39, 48, 24, 27, 26, 12, 30], we extend research on "nudge+" to evaluate its effects
5 at promoting booster vaccine uptake intentions during the COVID-19 pandemic.
6 Participants in an online survey experiment conducted among 24,303
7 participants in the G7 group of advanced industrialised countries (Canada,
8 France, Germany, Italy, Japan, the United Kingdom, and the United States of
9 America) were randomly assigned to one of four conditions in a 2×2 factorial
10 design. Individuals were randomized along two dimensions: default enrolment,
11 in which they would either be automatically enrolled into vaccine booster
12 appointments with local clinics calling to schedule appointments at their
13 convenience or one in which they would make their own appointments; and
14 reflection in which they were either encouraged to reflect on the government's
15 actions separately or not. This design yields four conditions: a condition in which
16 participants were presented with a policy in which individuals initiated their
17 own appointments for a booster vaccine (control); a condition in which
18 participants were presented with a policy in which they would be automatically
19 enrolled, by default, to receive a vaccine and the local clinic would contact them
20 to schedule appointments at their convenience (nudge); a condition in which
21 participants were presented with the control condition and then asked to reflect
22 on it (think); and a condition in which participants were presented with the
23 nudge condition and then asked to reflect on it (nudge+). We consider the effects
24 of these interventions on two outcomes: vaccination intention for the booster
25 and approval of the government's actions.

26 Contrary to prior research [39, 48, 11, 10], we find that a hypothetical policy
27 of default enrollment into scheduled vaccine appointments produces a backlash,
28 reducing people's behavioural intentions to get the vaccine for themselves.
29 Approval of this policy did not measurably differ from the status quo in the
30 control condition, however. Further, when participants assigned to automatic
31 enrollment were prompted to think about the policy, they were even less likely to
32 say they would get a vaccine and their approval of the policy correspondingly
33 decreased. Based on these findings, we conclude that a hypothetical default opt-
34 out nudge does not increase reported willingness to get a COVID-19 vaccine
35 booster. Our findings contradict common assumptions about the power of a
36 (hypothetical) default nudge to increase vaccinations [34] and adds a growing
37 (mixed) evidence base of using defaults to influence vaccination outcomes[35].

38 We make two important contributions to the growing literature in
39 behavioural science and public policy. First, our experiment evaluates the effects
40 of a range of interventions considered in behavioural public policy, such as

1 nudges, thinks, and nudge+, in the context of a timely public policy issue. Second,
2 our findings suggest that nudge+ can reconcile the trade-off between
3 effectiveness and support — encouraging citizens to reflect on the default
4 enrollment policy diminished public support for what turned out to be an
5 ineffective nudge, suggesting that reflection may help people better align policy
6 approval of nudges with their behavioural consequences and thereby provide a
7 valuable signal to policymakers.

8 **Experimental design**

9 **Survey design**

10 We administered a preregistered online survey experiment to 24,303
11 respondents in Canada, France, Germany, Italy, Japan, United Kingdom, and the
12 United States of America. The sample size was selected based on the power
13 analysis reported in the Online Appendix. The survey was administered on
14 Qualtrics to national samples that were representative by age, gender, education,
15 and subnational region (for summary statistics, see Tables S2–S11 in Online
16 Appendix) by Dynata from January 27–February 26, 2022. Table S12 in Online
17 Appendix provides country-specific date ranges for the periods in which surveys
18 were fielded within this interval. Respondents were paid at standard rates
19 recommended by Dynata. The original survey was written in English and
20 localised into French, German, Italian, and Japanese languages by translators at
21 Dynata, which were then cross-validated by first-language speakers. The study
22 preregistration is available [online](#) from OSF. The English (UK) version of the
23 survey is provided in the Online Appendix and all versions of the surveys are also
24 available [online](#).

25 **Experimental vignettes**

26 We used a between-subjects experimental design with four different treatment
27 conditions, including the control. In each condition, respondents were presented
28 with a hypothetical scenario taking place in October 2022 in which “COVID-19
29 cases are rising in your area” and “[t]he government is making another vaccine
30 booster shot freely available to you as winter is approaching.”

31 Following this information, respondents were randomized into four different
32 experimental vignettes that are described in Table 1 below. The experimental
33 conditions can be expressed as a 2×2 factorial design in which individuals are
34 either automatically enrolled into receiving a booster vaccine, with local clinics
35 calling them to schedule appointments at their convenience, or make their own
36 appointments (default enrolment) and are either encouraged to reflect on the
37 government’s actions or not (reflection). Table 2 shows this 2 × 2 factorial

1 design. The treatment conditions can be expressed as combinations of these
 2 dimensions: control (default enrolment=no, reflection=no), nudge (default
 3 enrolment=yes, reflection=no), think (default enrolment=no, reflection=yes), and
 4 nudge+ (default enrolment=yes, reflection=yes). For example, respondents in the
 5 control condition were told that people who want a booster would have to
 6 schedule an appointment. Respondents in the nudge condition were told that
 7 they would be automatically enrolled, by default, to receive a vaccine and the
 8 local clinic would contact them to schedule appointments at their convenience.
 9 We designed this nudge to be as flexible as possible to minimise opt-outs of the
 10 default enrolment due to scheduling conflicts. Respondents in the think
 11 condition were provided with an open-text question asking them to reflect on
 12 whether the government’s policy is appropriate and would work for them. These
 13 questions were chosen to first de-bias participants of any undue influence of the
 14 vaccine policy — debiasing individuals, in this way, has been shown to help
 15 citizens in articulating their true preferences (see, e.g., Fischhoff 15, Arkes 1,
 16 Milkman et al. 25), and then build their agency by empowering them to evaluate
 17 the goals of the nudge. Respondents who wrote fewer than 75 English characters
 18 (or equivalent in other languages as preregistered; see Table S20 in the Online
 19 Appendix for details) in the think condition were asked to write more. Finally,
 20 those in the nudge+ condition were told of the default enrolment policy and then
 21 asked to reflect about it in an identical manner to the think condition. Since, the
 22 interpretation of the term “enrolment” can vary in different contexts such as the
 23 different G7 countries, we provide country-level versions of the results presented
 24 below in Tables S17–S18.

25 [INSERT TABLE 1 HERE]

26 Respondents were then asked the following outcome measures: • Intentions
 27 to get the booster dose on a six-point scale from very unlikely
 28 (1) to very likely (6)

29 • Approval of the actions of the government on an 11-point scale from “I
 30 disapprove of the government’s action” (0) to “I approve of the
 31 government’s action” (10)

32 The first outcome measures participants’ stated intention to accept the
 33 booster vaccine and the second measures support for the policy. Due to the self-
 34 reported nature of our survey experiment, we are unable to measure real
 35 vaccination behaviours. To measure respondents’ compliance with the
 36 experimental vignettes, we used a preregistered manipulation check to assess
 37 their recall of the vaccine policy shown in their condition. Respondents were

1 asked “what did the government do to manage rising COVID-19 cases in your
2 area?” in the scenario and asked to choose among four choices including default
3 appointment scheduling and self-directed scheduling (for exact wording, see the
4 questionnaire provided in Online Appendix. The third option in our
5 manipulation check question had a typo. It stated that “The government
6 announces that every *living adult* in your country...” instead of “The government
7 announces that every *adult living* in your country...” This error was consistent
8 across all treatment conditions.)

9 We preregistered the hypotheses that being assigned to the nudge or nudge+
10 conditions would improve people’s intentions to get vaccinated (H1a and H2a,
11 respectively) and approval of the government’s policies (H1b and H2b,
12 respectively) versus the control condition. Further, we also preregistered that the
13 nudge+ intervention would increase the effects of the nudge (H3a) and public
14 approval of the policy (H3b) versus the nudge condition. These hypotheses
15 follow Banerjee & John [4], which theorises that spurring people to think about a
16 nudge enables them to assess its merits and evaluate it with respect to their own
17 goals. If those goals are aligned with the nudge (on average), then uptake of the
18 nudge should increase. Support for the policy may also increase as well due to
19 the transparency of this approach. For further details on our theoretical
20 reasoning, see the Online Appendix.

21 [INSERT TABLE 2 HERE]

22 **Methods**

23 Our experimental design, protocol, and methods were approved by the research
24 ethics board of King’s College London and the London School of Economics and
25 Political Science. Informed consent was obtained from all participants prior to
26 their participation. All methods were performed in accordance with the relevant
27 guidelines and regulations.

28 We test our preregistered hypotheses using OLS models with robust standard
29 errors in which we regress behavioural intentions to get the booster vaccine
30 dose and approval of the government’s actions on indicators for the
31 experimental treatments (nudge, think, and nudge+). As preregistered, each
32 model includes country fixed effects as well as covariates selected using the lasso
33 to increase the precision of our treatment effect estimates [5]. These models
34 estimate intent to treat effects (ITT). All results are unweighted. An exploratory
35 analysis of the ITT effects of the experimental conditions, expressed as a 2 × 2
36 factorial design, in which we measure the effect of interaction between default
37 enrolment and reflection is provided in Table S19 of the Online Appendix.

1 However, we find that receipt of treatment is often low as described below.
 2 We therefore follow our preregistration in also estimating complier average
 3 causal effects (CACE) using two-stage least-squares models in which we use
 4 random assignment as an instrument for the following measures of treatment
 5 compliance:

- 6 • Nudge condition compliance: 1 if respondent assigned to the nudge
 7 condition and answers the manipulation check question about it correctly,
 8 0 otherwise
- 9 • Think and nudge+ compliance: number of sentences written if assigned to
 10 the condition in question and answers the manipulation check question
 11 about it correctly, 0 otherwise) For the think and nudge+ compliance, we
 12 also use two alternative compliance specifications: we standardise the
 13 number of sentences by country (exploratory) or take the square root of
 14 the number of characters written by respondents (preregistered). Results
 15 of these alternative specification are available in Table S16 in Online
 16 Appendix.

17 We also control for selected lasso covariates and country fixed effects and use
 18 robust standard errors as in the models described above. For all models
 19 estimated below, our inference is based on randomization-*t* *p*-values [51]. We did
 20 not preregister any ex-post multiple hypotheses correction method. Instead, we
 21 incorporated a conservative Bonferroni correction into the power calculation
 22 used to select our sample size (see Online Appendix for details). We use Stata 17
 23 to conduct statistical analyses and the Quanteda package in R for text analysis.

24 **Results**

25 The resulting experimental data satisfies our preregistered balance tests and
 26 shows expected levels of demographic diversity (see Online Appendix for details
 27 and summary statistics). Overall, respondent intentions to get the booster dose
 28 for themselves are generally high across all conditions (mean of 4.7 on a six-
 29 point Likert scale). Respondents' policy approval is centred around the midpoint
 30 of the scale (mean of 6.3 on a 11-point scale). We begin our analysis with Figure
 31 ??, which shows mean values and 95% confidence intervals for the two outcome
 32 measures across the four different experimental conditions.

33 [INSERT FIGURE 1 HERE]

34 Next, table 3 presents intent to treat effects on vaccination intentions and
 35 approval of policy. Contrary to our expectations, the nudge intervention of

1 default vaccination enrolment reduced respondents' intention to get a booster
 2 dose by 0.065 units on a six-point scale ($p < .005$) — in other words, respondents
 3 who were nudged into a default enrolment were 0.016 standard deviations less
 4 likely to accept the vaccine compared to those who were left to schedule their
 5 own booster vaccination. The think condition also produces a negative effect on
 6 behavioural intentions to get a booster (-0.058 or -0.014 s.d., $p < .005$).¹ By
 7 contrast, defaulting people into vaccine enrolments or encouraging them to
 8 reflect on the vaccine policy produces no measurable effect on approval versus
 9 self-scheduling in the control condition (nudge = 0.037, n.s.; think = 0.035, n.s.).
 10 Most importantly, encouraging respondents to reflect on the default appointment
 11 policy in the nudge+ condition further decreased intentions to vaccinate relative
 12 to the negative effect observed in the nudge condition (-0.125 or -0.031 s.d., $p <$
 13 $.005$ versus controls; -0.059, $p < .005$ versus nudge; -0.066, $p < .005$). Due to a
 14 coding error, respondents were required to answer the approval question in the
 15 nudge+ condition but not in other conditions. However, missingness was less
 16 than 1% in the control (48 responses), nudge (44 responses), and think (3
 17 responses) conditions and our ITT results are robust to randomly dropping 1–
 18 5% of the nudge+ observations in percentage-point intervals (see Tables S14 and
 19 S15 in Online Appendix). Nonetheless, the nudge+ intervention also reduced
 20 policy approval (-0.150 or -0.021 s.d., $p < .005$ versus controls; -0.112, $p < .05$
 21 versus nudge; -0.184, $p < .005$). Findings from the exploratory analysis, in which
 22 we re-specify the model as an interaction between default enrolment and
 23 reflection, is reported in Table S19 and are equivalent to those reported in Table
 24 3.

25 [INSERT TABLE 3 HERE]

26 We examine compliance rates to see whether respondents received the
 27 treatment as intended. We find that manipulation check passage rates by
 28 condition vary between 45% (nudge+) and 69% (nudge), indicating that many
 29 respondents were unable to comprehend fully the government policy in
 30 question.

31 Noncompliance was statistically uncorrelated with respondent inattention in a
 32 preregistered attention check ($F=0.40$, $p=0.75$; see Online Appendix for question
 33 wording). An exploratory analysis of compliers following Marbach and
 34 Hangartner [23] shows that compliance with the nudge or think is not
 35 significantly associated with respondent gender, parenthood, city/town type,
 36 religious beliefs, prior COVID-19 incidence, trust in vaccines, or prior vaccine
 37 and booster uptake. However, we find that adults without children and those

¹ Note that this effect is significant in the following countries but not ..

1 who live in smaller towns/cities are more likely to successfully receive the
2 nudge+ treatment (see Figure S4 in the Online Appendix). We therefore follow
3 our preregistered approach to estimate complier average causal effects. These
4 models use indicators for random assignment as instruments for endogenous
5 measures of treatment receipt. For nudge, the endogenous measure of treatment
6 receipt is answering the manipulation check question correctly. For think and
7 nudge+, we use the number of sentences written in the open text prompt (either
8 as an integer or an exploratory measure standardised by country) and the
9 square root of the total number of characters written standardised by country. A
10 more detailed analysis of the textual responses is provided in the Online
11 Appendix (see subsection Textual Analysis).

12 The main effects of treatments among compliers, which are reported in Table
13 S16 in Online Appendix, are consistent with the ITT estimates in Table 3 across
14 instrumental variable specifications. The nudge, think, and nudge+ treatments all
15 reduce booster vaccination intentions relative to the control condition. As in the
16 ITT analysis, the effects on approval are null for the nudge and the think, and
17 negative for nudge+ versus the control condition. The nudge+ consistently
18 lowers vaccination intentions policy approval versus the nudge and the think
19 conditions. These ITT effects (controlling for lasso variables and country fixed
20 effects) are shown in Figure ??.

21 [INSERT FIGURE 2 HERE]

22 Finally, as preregistered, we conduct exploratory checks for robustness of
23 these treatment effects, across each country, which are reported in Tables S17
24 and S18 in the Online Appendix. The negative effects on booster intentions in the
25 pooled sample are statistically detectable in the U.S. and Germany for nudge; the
26 U.K. and Japan for think; and all countries but France and Italy for nudge+. The
27 nudge+ condition does not significantly increase the backlash effect of the nudge
28 in any country. Further, we find the nudge measurably increases policy approval
29 versus the control condition in the United Kingdom and Japan, while decreasing
30 it in the United States. The think increases policy approval versus the control in
31 the United States and France, while it decreases it in the U.K. and Italy. The
32 nudge+ decreases approval versus the control condition in every country, except
33 Italy where we find a null, and U.K. and Japan where we see a positive effect.
34 There is no statistical evidence to suggest that the nudge, think, or nudge+
35 conditions increase vaccination intentions in any G7 country versus the status
36 quo in the control condition. All within country effects for vaccination intentions
37 are broadly consistent with our pooled findings in Table 3. We also report
38 exploratory heterogeneity in estimated treatment effects in the Online Appendix
39 (see Table S21 and Figure S5).

1 **Discussion and conclusion**

2 We present experimental evidence on the role of reflective transparency in
3 behavioural public policy [4]. Contrary to expectations, we find that a
4 hypothetical default opt-out nudge does not increase survey respondents'
5 willingness to get a COVID-19 vaccine booster and support is even lower when
6 actively asked to reflect on the policy. In other words, reflecting on the nudge
7 diminishes approval, which better aligns policy approval with intended
8 behaviour under the influence of the nudge. These findings suggest that
9 reflective transparency may help citizens think through government actions and
10 generate more informative signals about policy efficacy and likely compliance.
11 For instance, future research should test if reflecting on a nudge that successfully
12 changes intended behavior (unlike what we find here) generates increased
13 approval for the policy.

14 Our findings generate insights on how to most effectively use
15 experimentation with citizen feedback in developing behavioural public policy.
16 For example, we contribute to conversations around open, democratic
17 governments as “laboratories for policy experimentation” [8, 9] that search for
18 better policies [14]. Our research suggests citizen reflection might inform “test-
19 learn-adapt” approaches to behavioural policy development [18, 17] while
20 avoiding public reactance [44]. Specifically, policymakers can use nudges that
21 encourage citizen reflection to avoid false signals of public support for policies
22 which are likely to be rejected by the public.

23 In the context of vaccines, default appointments represent an interesting case
24 as they are not fully coercive yet seek to shape people’s behavior. Reflection on
25 such a policy can therefore lead to different policy outcomes as we show. A
26 nudge+ enables policy-makers to ascertain underlying preferences when there is
27 an opt-out that might otherwise be disguised. Without this moment of active
28 reflection, policy-makers might be puzzled at citizen reaction to opt-out
29 approaches. However, we strongly caution that we are unable to test whether the
30 effects we observe on behavioral intentions would translate into real-world
31 behaviors, which should be validated in future studies if such an approach were
32 undertaken (to date, democratic governments have not sought to automatically
33 enrol people in COVID-19 vaccines).

34 Our findings on the negative effects of a hypothetical default nudge also
35 contribute to a wider debate on the extent to which nudge effects are sensitive to
36 context. [39] and [48] find, for instance, that defaults can increase vaccination
37 intentions and behaviours in samples from the U.S. and Italy, respectively.
38 Consistent with prior evidence suggesting defaults can fail [44], though, we
39 instead observe negative effects of automatic scheduling of hypothetical booster

1 appointments on vaccination intentions. In some cases, nudges like these might
 2 be seen as intervening too aggressively; related work finds backlash effects of
 3 nudges with organ donation, for instance [22]. Practitioners should be more
 4 attentive to how to use nudges given the context of the social problem and seek
 5 to use reflection as a tool to generate policy signals from citizens.

6 Further research should assess the external validity of these findings in other
 7 times and contexts. It is possible that our findings were influenced by the specific
 8 design of the nudge (which cannot create a true default in the same manner as a
 9 real-world nudge) and the nature of the reflection task. Prior research suggests
 10 that treatment effects may vary by types of disclosures [31] and frames of
 11 evaluation [13] used. Further, nudge+ interventions may also differ in the type of
 12 reflection embedded in the nudge [3]. Heterogeneity in uptake of these
 13 interventions by different target populations should also be studied. For
 14 example, our exploratory analysis suggests that male participants, people
 15 without a booster, and those who are less trusting in institutions and more right-
 16 leaning are less likely to have a positive reaction to nudge+, for both vaccination
 17 intentions and policy approval.

18 Several limitations of our study must be noted. First, we note that the
 19 magnitude of our estimated effect sizes are small (0.02–0.03 standard
 20 deviations). Second, we cannot measure the effects of actual nudge policies on
 21 vaccination behaviour; future studies should extend this research to test the
 22 effects of nudge+ interventions in real-world settings before scaling up nudge
 23 policies which can have negative effects. Third, our exploratory findings showing
 24 heterogeneity across countries should be investigated further. The effects of
 25 reflection can also vary with other nudges. The deployment of our proposed
 26 nudge+ policy can be logistically and financially challenging. Further research is
 27 required how to most cost-effectively encourage reflection in the public
 28 effectively (see Keppeler et al. [19] who recently deployed a nudge+ like
 29 mechanism in Germany to improve vaccination behaviours). Fourth, our study
 30 took place after the peak pandemic but during a period in which the public was
 31 still worried about COVID after the Omicron variant. Further research should
 32 assess the external validity of these findings in other times and contexts. Finally,
 33 our study is based on cross-sectional data; future research should consider how
 34 these vaccination behaviours and policy effects change over time. Despite these
 35 limitations, we believe our findings are novel and informative for future tests of
 36 nudge+ interventions.

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Table 1: Text of experimental vignettes

Treatment	Vignette description
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Control	In this scenario, the government leaves it to every adult living in your country to choose whether they should get this vaccine booster shot or not. If you want a booster, you will have to call your local clinic to schedule a booster appointment.
Nudge <i>Default enrolment</i>	In this scenario, the government announces that every adult living in your country will be automatically enrolled to receive this vaccine booster shot at a local clinic. Your local clinic will call you to schedule a booster appointment at a convenient date and time. You can opt out of this automatic enrolment if you wish.
Think <i>Reflection</i>	<p>In this scenario, the government leaves it to every adult living in your country to choose whether they should get this vaccine booster shot or not. If you want a booster, you will have to call your local clinic to schedule a booster appointment.</p> <p>Please think about the government's actions in this scenario. Do you think this approach is appropriate? Do you think this approach will work for you? In at least one or two sentences, please write down your thoughts. [text box]</p>
Nudge+ <i>Default enrolment & reflection</i>	<p>In this scenario, the government announces that every adult living in your country will be automatically enrolled to receive this vaccine booster shot at a local clinic. Your local clinic will call you to schedule a booster appointment at a convenient date and time. You can opt out of this automatic enrolment if you wish.</p> <p>Please think about the government's actions in this scenario. Do you think this approach is appropriate? Do you think this approach will work for you? In at least one or two sentences, please write down your thoughts. [text box]</p>

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Table 2: Experimental design

Default enrolment	
No	Yes

Reflection	No Yes	Control Think	Nudge Nudge+
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Table 3: Intent to treat effects on vaccination intentions and policy approval

	Intentions	Approval
Nudge	-0.065*** 0.020	-0.037 0.046
Think	-0.058*** 0.020	0.035 0.049
Nudge+	-0.125*** 0.021	-0.150*** 0.048
Controls	✓	✓
Country FE	✓	✓
N	24,164	24,115

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Figure Legends

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Figure 1: Confidence interval bar plots of Intentions to get the booster dose (Panel A) and Approval of actions of the government (Panel B).

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Figure 2: Coefficient plot of intent-to-treat effects for Intentions to get the booster dose (Panel A) and Approval of actions of the government (Panel B).

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14 from all participants. All methods were performed in accordance with the
15 relevant guidelines and regulations

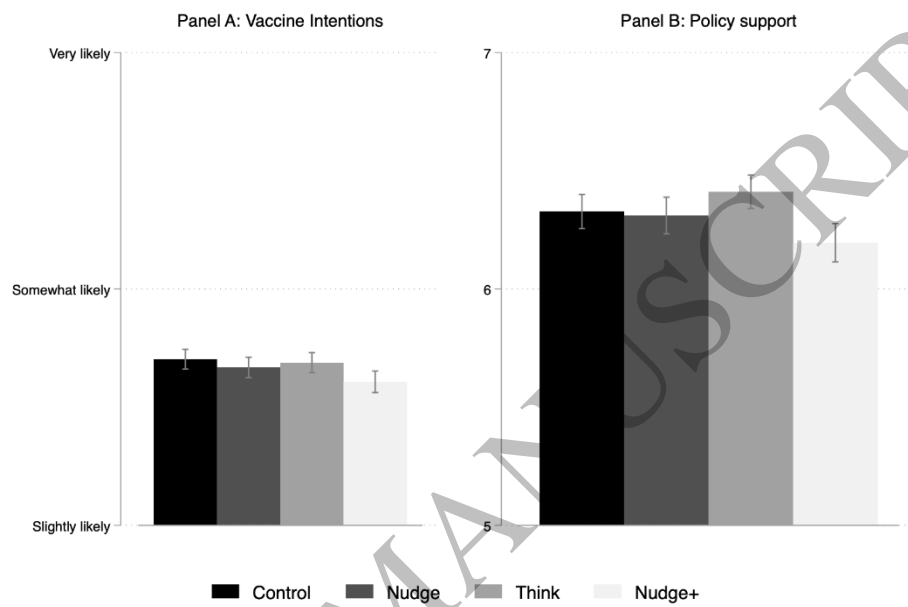


Figure 1
307x204 mm (x DPI)

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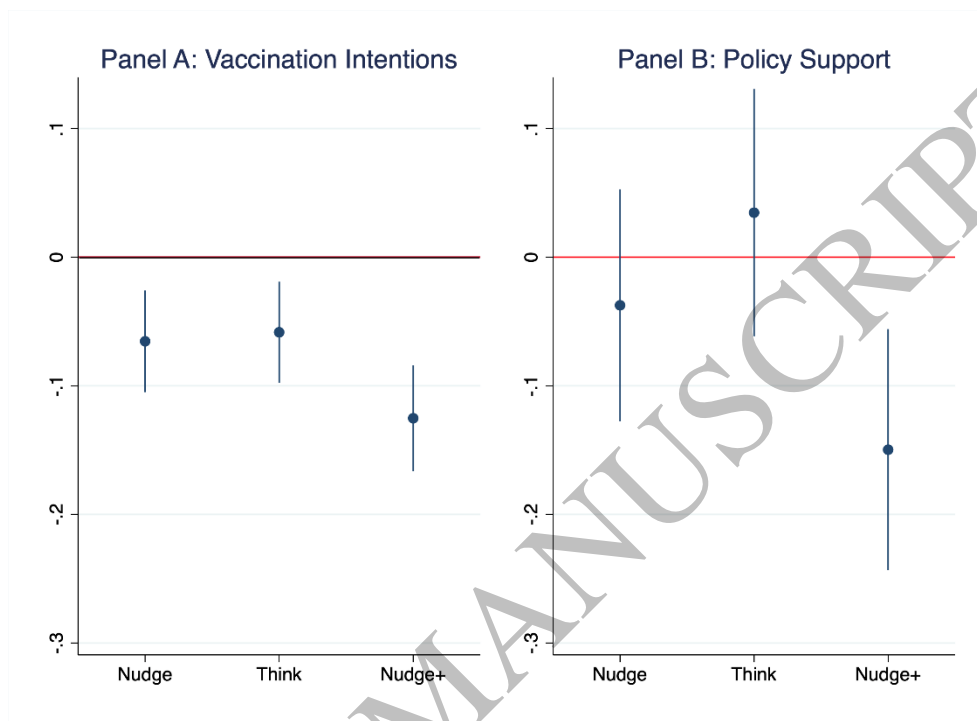


Figure 2
489x356 mm (x DPI)

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