Summary of the Online Appendix for "Beating Expectations: Turning Polarized Beliefs into Support for Sustainable Initiatives" by Ole-Andreas Elvik Næss

Section A presents supplementary tables and figures, while Section B discusses practical considerations for implementation. Section C provides additional theoretical results. Section D reports further findings from Experiment 1, and Section E presents the internal-validity robustness test (Experiment 9). Section F reproduces the pre-analysis plans, and Sections beginning at G compile all experimental instructions.

A Additional Tables and Figures

Table A1: Descriptives by views on carbon tax and state-specific choices (Experiment 1)

	Supporters	Skeptics	No tax	Tax, bonus if not hot	Tax, bonus if hot
Education	2.937	2.667	2.701	2.782	2.865
Income	2.236	2.057	2.055	2.124	2.179
Leftwing	3.905	3.050	2.904	3.563	3.826
Probability 2024 is warmest ever	77.773	61.320	59.784	60.821	81.422
Lobby success	3.675	3.137	2.932	3.479	3.624
Male (=1)	0.504	0.469	0.473	0.450	0.510
Age	45.595	45.980	48.029	44.061	45.802
White (=1)	0.656	0.653	0.709	0.597	0.668

Note: This table reports descriptive demographic statistics from Experiment 1. The first and second columns separate respondents into carbon tax supporters and skeptics. The third, fourth, and fifth columns further divide respondents assigned to the state-specific treatment according to whether they initially preferred no carbon tax (column 3), a carbon tax with a bonus if next year is not the hottest ever (column 4), or a carbon tax with a bonus if next year is the hottest ever (column 5). Education is coded from 1 to 4, where 1 denotes primary education and 4 denotes a postgraduate degree. Income takes values 1 (under $50,000), 2 ($50,000–100,000), 3 ($100,000–150,000), 4 ($150,000–200,000), and 5 (over $200,000). Leftwing is coded from 1 (strongly right-wing) to 5 (strongly left-wing). Probability that 2024 is the warmest ever ranges from 0 to 100. Lobby success was measured on a five-point scale with the following categories: 0%, 0–0.01%, 0.01–0.1%, 0.1–1%, and more than 1%, as displayed in the figure. Male and White are binary indicators for male gender and white ethnicity, respectively, while Age is measured in years.

Table A2: Robustness WTP table (Experiment 1)

Dropping zero probability respondents

	Stage 1			Stage 2 (No market prob.)			Stage 2 (Market prob.)		
	(1) All	(2) Supporters	(3) Skeptics	(4) All	(5) Supporters	(6) Skeptics	(7) All	(8) Supporters	(9) Skeptics
State-specific	1.986**	-1.412*	6.217***	3.493***	-0.491	6.672***	1.318	0.499	4.526***
	(3.01)	(-2.22)	(7.34)	(3.70)	(-0.43)	(5.33)	(1.37)	(0.49)	(3.37)
Constant	2.673***	13.27***	-7.295***	1.401*	10.33***	-6.092***	4.092***	11.90***	-4.198***
	(5.45)	(30.65)	(-12.23)	(2.04)	(12.88)	(-7.15)	(5.80)	(16.85)	(-4.18)
PosWTP(control)	0.60	0.93	0.29	0.58	0.84	0.35	0.63	0.85	0.40
PosWTP(state-specific)	0.72	0.92	0.56	0.69	0.85	0.55	0.68	0.86	0.54
Observations	2082	966	1116	1056	487	569	1026	479	547

t statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Outcome variable: Expected value of implementing lobby package

	Stage 1			Stage 2 (No market prob.)			Stage 2 (Market prob.)		
	(1) All	(2) Supporters	(3) Skeptics	(4) All	(5) Supporters	(6) Skeptics	(7) All	(8) Supporters	(9) Skeptics
State-specific	1095.3***	-267.3	2645.0***	1327.2***	130.6	2281.4***	798.1*	7.432	2146.9***
	(4.37)	(-0.99)	(7.57)	(3.73)	(0.31)	(4.53)	(2.22)	(0.02)	(4.09)
Constant	76.92	3307.1***	-2960.4***	44.53	2742.2***	-2220.4***	503.8*	2815.9***	-1951.0***
	(0.43)	(18.83)	(-12.57)	(0.18)	(9.22)	(-6.71)	(1.97)	(9.98)	(-5.22)
PosWTP(control)	0.60	0.93	0.29	0.58	0.84	0.35	0.63	0.85	0.40
PosWTP(state-specific)	0.72	0.92	0.56	0.69	0.85	0.55	0.68	0.86	0.54
Observations	2082	966	1116	1056	487	569	1026	479	547

t statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Different measure of carbon tax supporters

	Stage 1			Stage 2 (No market prob.)			Stage 2 (Market prob.)		
	(1) All	(2) Supporters	(3) Skeptics	(4) All	(5) Supporters	(6) Skeptics	(7) All	(8) Supporters	(9) Skeptics
State-specific	1.823**	0.0674	5.226***	3.050***	1.861	5.263***	1.802*	0.0753	5.245***
	(2.91)	(0.10)	(5.97)	(3.37)	(1.89)	(3.72)	(1.98)	(0.08)	(3.58)
Constant	1.622***	8.552***	-12.12***	0.569	6.182***	-10.49***	2.800***	9.152***	-9.872***
	(3.49)	(17.95)	(-21.65)	(0.87)	(8.47)	(-11.48)	(4.22)	(13.90)	(-9.90)
PosWTP(control)	0.56	0.78	0.13	0.55	0.71	0.22	0.60	0.78	0.23
PosWTP(state-specific)	0.68	0.84	0.35	0.65	0.80	0.35	0.66	0.80	0.39
Observations	2386	1588	798	1200	798	402	1186	790	396

t statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table presents results similar to those in Table 1, showing that the results in Experiment 1 are robust to changes in measurement. In the first panel, respondents who reported a zero subjective probability that the lobby package would lead to the implementation of a carbon tax in the US ($Prob_i = 0$) are excluded. In the second panel, the outcome variable otherwise in the paper represented by WTP_i, is replaced by $\frac{WTP_i}{Prob_i}$. Respondents with $Prob_i = 0$ are excluded, and expected values are capped at $\pm$\$20,000. In the third panel, respondents who indicated they are uncertain or would abstain from voting on a US carbon tax are classified as supporters rather than skeptics.

Table A3: Correlation Between Subjective Probabilities and Payout Choices (Experiment 1)

	Payout Choices					
	Carbon tax with hot payout			Carbon tax with not-hot payout		
	(1) Hot	(2) Hot	(3) Hot	(4) NotHot	(5) NotHot	(6) NotHot
Underestimate	-0.341***			0.191***		
	(-11.84)			(7.33)		
Large underestimate		-0.358***			0.153***	
		(-15.25)			(4.57)	
Carbon tax skeptic			-0.355***			-0.0475*
			(-13.14)			(-1.73)
Observations	1,196	1,196	1,196	1,196	1,196	1,196
Controls	No	No	No	No	No	No
Mean in the other group	0.58	0.44	0.57	0.19	0.28	0.35

t statistics in parentheses

* p<0.1, ** p<0.05, *** p<0.01

Note: This table shows the correlation between subjective probabilities and payout choices in Experiment 1 using OLS regression with robust standard errors, restricting the sample to respondents receiving the state-specific treatment in Stage 1. "Underestimate" is a binary variable equal to 1 for respondents whose subjective probability that 2024 will be the hottest year on record, as measured in Stage 0, is lower than the market probability. "Large underestimate" is a binary variable equal to 1 for respondents whose subjective probability is below 50 percent. Carbon tax skeptic is a binary variable equal to 1 for respondents who did not support a hypothetical carbon tax in Stage 0. "Hot" is a binary variable equal to 1 for respondents who want to implement the lobby package for carbon taxation while receiving the bonus of $24 if 2024 is the hottest year on record. "NotHot" is a binary variable equal to 1 for respondents who want to implement the lobby package while receiving the bonus of $110 if 2024 is not the hottest year on record.

Table A4: Descriptives by support for policies (Experiment 2)

	Supp(W)	NoSupp(W)	Supp(S)	NoSupp(S)	Supp(C)	NoSupp(C)
Male	0.496	0.485	0.515	0.461	0.561	0.454
Age	45.040	49.280	44.634	48.512	44.143	47.378
Democrat	0.340	0.180	0.389	0.159	0.407	0.233
Republican	0.232	0.390	0.192	0.398	0.205	0.315

Note: This table reports descriptive statistics for Experiment 2 respondents split by whether they support each of the three policies. The first two columns divide the sample into supporters (*Supp*) and non-supporters (*NoSupp*) of the Wildfire program in Scenario 1 (W); the next two columns split the sample by support for solar panels in Scenario 2 (S); and the last two columns split the sample by support for congestion pricing in Scenario 3 (C). Male is a binary indicator for being male, and Age is measured in years. Democrat and Republican are binary indicators for whether respondents identify as a Democrat or a Republican.

Table A5: Support for government programs from different parties (Experiment 6)

All Respondents

	Wildfire prevention			Congestion pricing		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Top 50	Top 10	All	Top 50	Top 10
Treatment	0.163***	0.143***	0.231**	0.0941***	0.0665*	0.104
	(6.03)	(3.62)	(2.55)	(3.61)	(1.79)	(1.29)
Observations	1,470	732	153	1,470	732	153
Controls	No	No	No	No	No	No
Control group mean	0.55	0.54	0.45	0.31	0.33	0.33
Probability of failure	0.38	0.39	0.40	0.50	0.51	0.55

t statistics in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Own party

	Wildfire prevention			Congestion pricing		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Top 50	Top 10	All	Top 50	Top 10
Treatment	0.0972**	0.102*	0.155	0.0944**	0.0538	0.00363
	(2.23)	(1.76)	(1.25)	(2.04)	(0.93)	(0.03)
Observations	472	296	74	480	312	75
Controls	No	No	No	No	No	No
Control group mean	0.70	0.69	0.67	0.44	0.47	0.53
Probability of failure	0.25	0.23	0.22	0.42	0.41	0.43

t statistics in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Not own party

	Wildfire prevention			Congestion pricing		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Top 50	Top 10	All	Top 50	Top 10
Treatment	0.193***	0.158***	0.271**	0.120***	0.113**	0.278***
	(5.81)	(3.09)	(2.32)	(3.97)	(2.55)	(3.22)
Observations	998	436	79	990	420	78
Controls	No	No	No	No	No	No
Control group mean	0.48	0.45	0.27	0.23	0.20	0.080
Probability of failure	0.44	0.50	0.57	0.55	0.59	0.66

t statistics in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Note: Each column reports OLS coefficients (robust standard errors) of a state-specific bonus indicator (*Treatment*) on binary support for the policy. "All Respondents" uses the full sample. "Own party" ("Not own party") restricts to respondents for whom the labeled party matches (does not match) their preferred party. Because a sizable share identifies as independent/neutral, the "Not own party" panel contains more observations. For heterogeneous effects, columns labeled "Top 50" and "Top 10" restrict to respondents in the top half and top decile of the affective polarization distribution. The first three columns refer to wildfire prevention; the last three to congestion pricing. Control group means are reported in the tables. Probability of failure is the average probability that the program fails for respondents included in the analysis. No additional covariates are included.

Table A6: Trust, voting intentions, and perceived appropriateness (Experiment 6)

All Respondents

	Wildfire prevention					Congestion pricing				
	(1) All	(2) Top 50	(3) Top 10	(4) Vote	(5) Appr.	(6) All	(7) Top 50	(8) Top 10	(9) Vote	(10) Appr.
Treatment	0.0889**	0.0860	0.264	0.0938*	-0.201***	0.0186	-0.0747	0.0291	-0.0598	-0.113*
	(2.01)	(1.25)	(1.42)	(1.82)	(-2.92)	(0.41)	(-1.12)	(0.18)	(-1.22)	(-1.73)
Observations	1,470	732	153	1,470	1,470	1,470	732	153	1,470	1,470
Controls	No	No	No	No	No	No	No	No	No	No
Control group mean	3.04	3.03	2.98	3	3.53	2.85	2.90	2.88	2.89	3.02

t statistics in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Own party

	Wildfire prevention					Congestion pricing				
	(1) All	(2) Top 50	(3) Top 10	(4) Vote	(5) Appr.	(6) All	(7) Top 50	(8) Top 10	(9) Vote	(10) Appr.
Treatment	0.0421	0.129	0.286	0.0849	-0.255**	0.184**	0.0946	0.324	0.139*	-0.0806
	(0.61)	(1.41)	(1.40)	(1.11)	(-2.32)	(2.45)	(0.99)	(1.57)	(1.77)	(-0.73)
Observations	472	296	74	472	472	480	312	75	480	480
Controls	No	No	No	No	No	No	No	No	No	No
Control group mean	3.34	3.33	3.50	3.46	3.97	3.07	3.15	3.19	3.20	3.34

t statistics in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Not own party

	Wildfire prevention					Congestion pricing				
	(1) All	(2) Top 50	(3) Top 10	(4) Vote	(5) Appr.	(6) All	(7) Top 50	(8) Top 10	(9) Vote	(10) Appr.
Treatment	0.109**	0.0284	0.156	0.0941	-0.180**	-0.00550	-0.117	-0.0649	-0.0770	-0.0575
	(2.02)	(0.31)	(0.64)	(1.53)	(-2.14)	(-0.10)	(-1.38)	(-0.30)	(-1.33)	(-0.73)
Observations	998	436	79	998	998	990	420	78	990	990
Controls	No	No	No	No	No	No	No	No	No	No
Control group mean	2.91	2.84	2.55	2.78	3.32	2.72	2.66	2.48	2.69	2.81

t statistics in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Note: Outcome variables are five-point Likert indices: *Trust* (columns 1–3 and 6–8), *Voting intention* (columns 4 and 9) and *Appropriateness* (columns 5 and 10). Trust is measured by asking how the proposal would affect respondents' trust in the assigned party to handle the policy, on a five-point scale from 1 ("trust much less") to 5 ("trust much more"), with 3 indicating no change. Voting intention is measured by asking how likely the proposal would make respondents vote for the assigned party, on a five-point Likert scale from 1 ("much less likely") to 5 ("much more likely"), with 3 indicating no change. Perceived appropriateness is measured on a five-point scale from 1 ("completely inappropriate") to 5 ("completely appropriate"), with 3 as neutral. "All/Top 50/Top 10" refer to the full sample and the top 50%/10% by affective polarization, respectively. Treatment is a binary indicator for seeing the state-specific bonus. Specifications are OLS with robust standard errors and no additional controls. Panels split respondents by whether the proposing party matches their preferred party.

Table A7: Conspiracy beliefs and trust in eight outcome-certifying sources (Experiment 7)

	USgov	IntlBody	AcadCons	FinExch	AuditFirm	BChainOrac	PredMkt	NatNews
All	0.65	0.72	0.83	0.72	0.79	0.49	0.50	0.55
Top 50	0.56	0.65	0.81	0.67	0.74	0.55	0.53	0.47
Top 10	0.46	0.58	0.75	0.55	0.66	0.55	0.49	0.36

Note: The table shows the share of respondents rating each source "moderately" (3) or higher. The first row is the full sample; the next two rows report the top 50% and top 10% most conspiracy-minded (by CMQ). Sources (by column): (1) U.S. government statistics agency; (2) international body; (3) independent academic consortium; (4) regulated financial exchange; (5) large audit/accounting firm; (6) blockchain oracle service; (7) blockchain prediction-market platform; (8) major national news outlets. Appendix Figure A8 presents the full distribution of responses.

Table A8: Government vs. private implementation of state-specific bonuses (Experiment 8)

All Respondents

	Appropriate		Legitimacy		Trust	
	(1)	(2)	(3)	(4)	(5)	(6)
Private	-0.0281		-0.0250		0.0312	
	(-0.34)		(-0.27)		(0.39)	
Profit-motive		-0.693***		-0.477***		-0.446***
		(-7.79)		(-5.14)		(-5.80)
Observations	640	669	640	669	640	669
Controls	No	No	No	No	No	No
Government group mean	3.85	3.85	3.37	3.37	2.85	2.85

t statistics in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Democrats

	Appropriate		Legitimacy		Trust	
	(1)	(2)	(3)	(4)	(5)	(6)
Private	-0.288**		-0.277*		-0.126	
	(-2.18)		(-1.78)		(-0.88)	
Profit-motive		-0.831***		-0.712***		-0.449***
		(-5.51)		(-4.42)		(-2.95)
Observations	187	183	187	183	187	183
Controls	No	No	No	No	No	No
Government group mean	4.18	4.18	3.72	3.72	2.99	2.99

t statistics in parentheses
* p<0.1, ** p<0.05, *** p<0.01

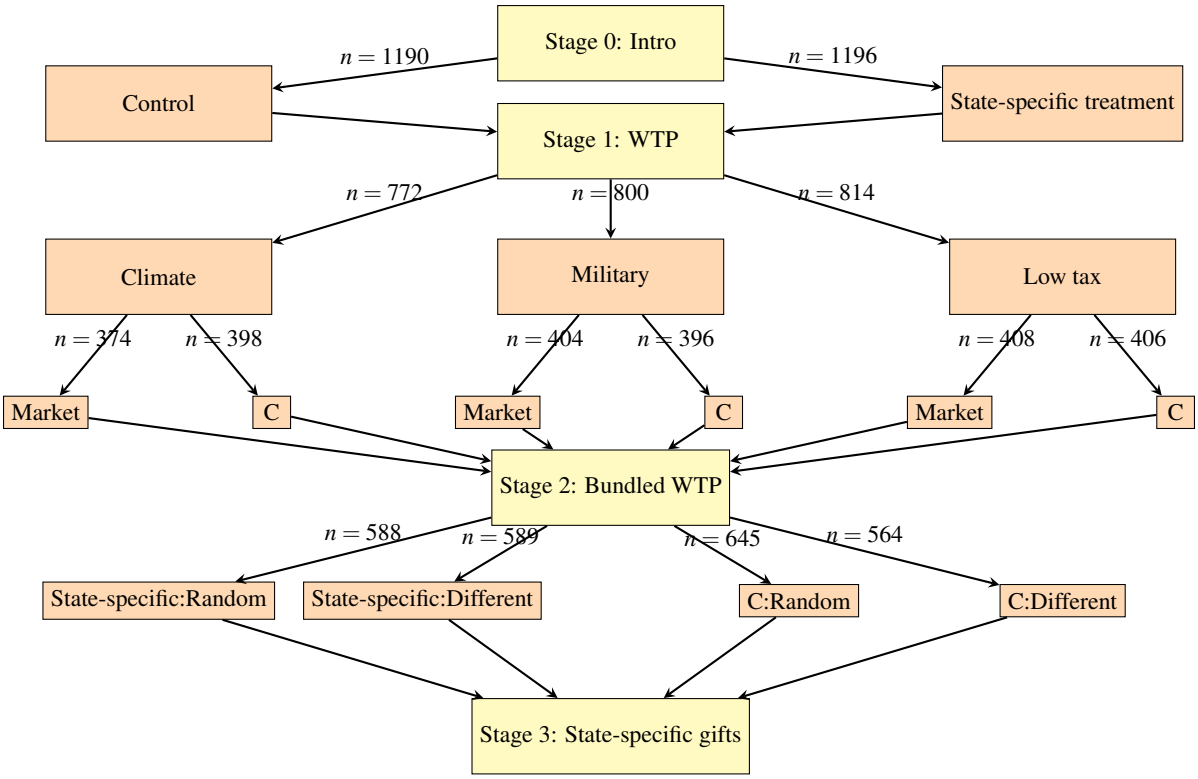
Republicans

	Appropriate		Legitimacy		Trust	
	(1)	(2)	(3)	(4)	(5)	(6)
Private	0.126		0.0331		0.222	
	(0.76)		(0.18)		(1.40)	
Profit-motive		-0.832***		-0.448**		-0.387***
		(-4.47)		(-2.38)		(-2.64)
Observations	188	179	188	179	188	179
Controls	No	No	No	No	No	No
Government group mean	3.72	3.72	3.21	3.21	2.67	2.67

t statistics in parentheses
* p<0.1, ** p<0.05, *** p<0.01

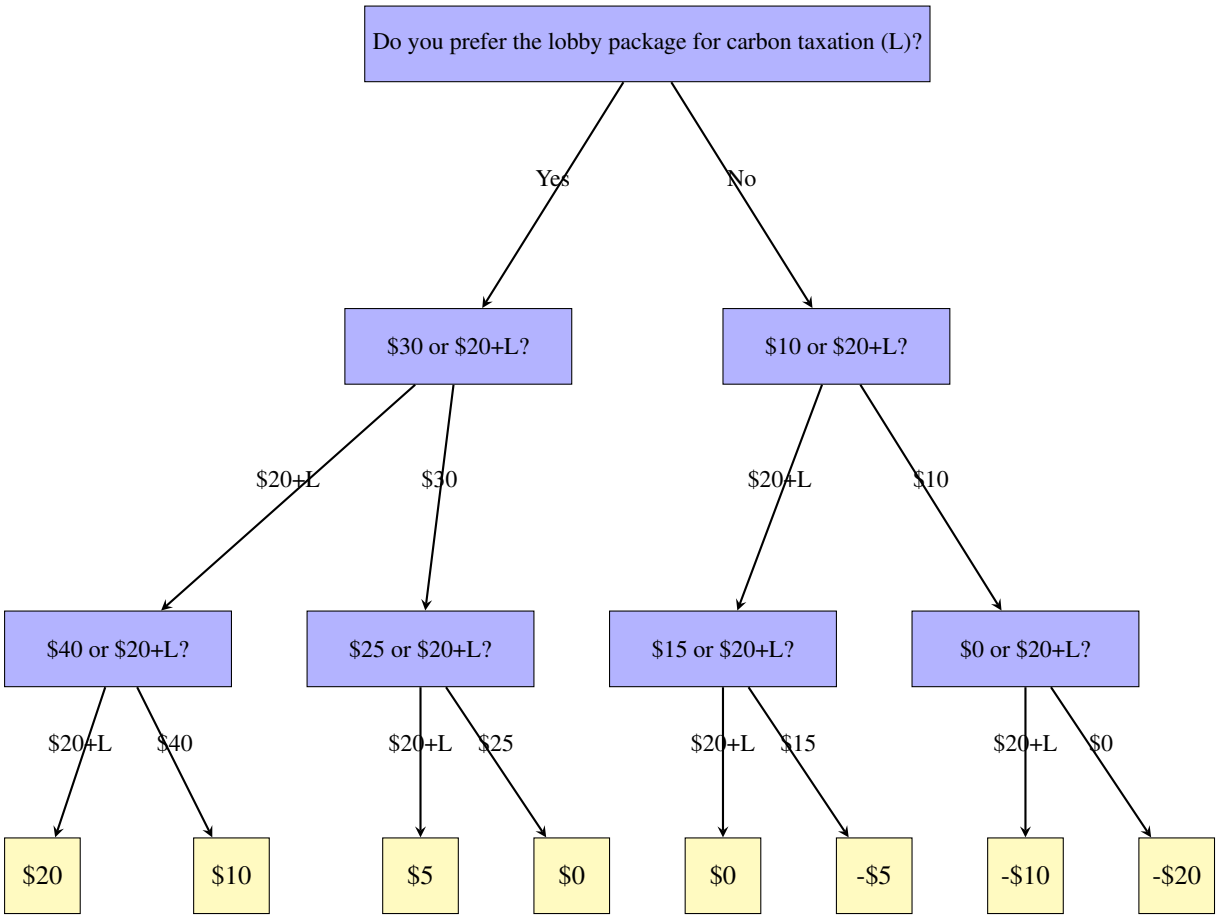
Note: Each column reports OLS treatment effects (robust standard errors) relative to the Government baseline. **Private** and **Profit motive** are binary indicators for the corresponding treatments. Outcomes are five-point Likert indices: *Appropriate* (columns 1–2), *Legitimate* (3–4), and *Trust* (5–6). Higher values indicate greater appropriateness, legitimacy, and trust (or more positive change in trust). Subpanels split the sample by self-identified partisanship. No additional controls are included.

Figure A1: Structure of Experiment 1



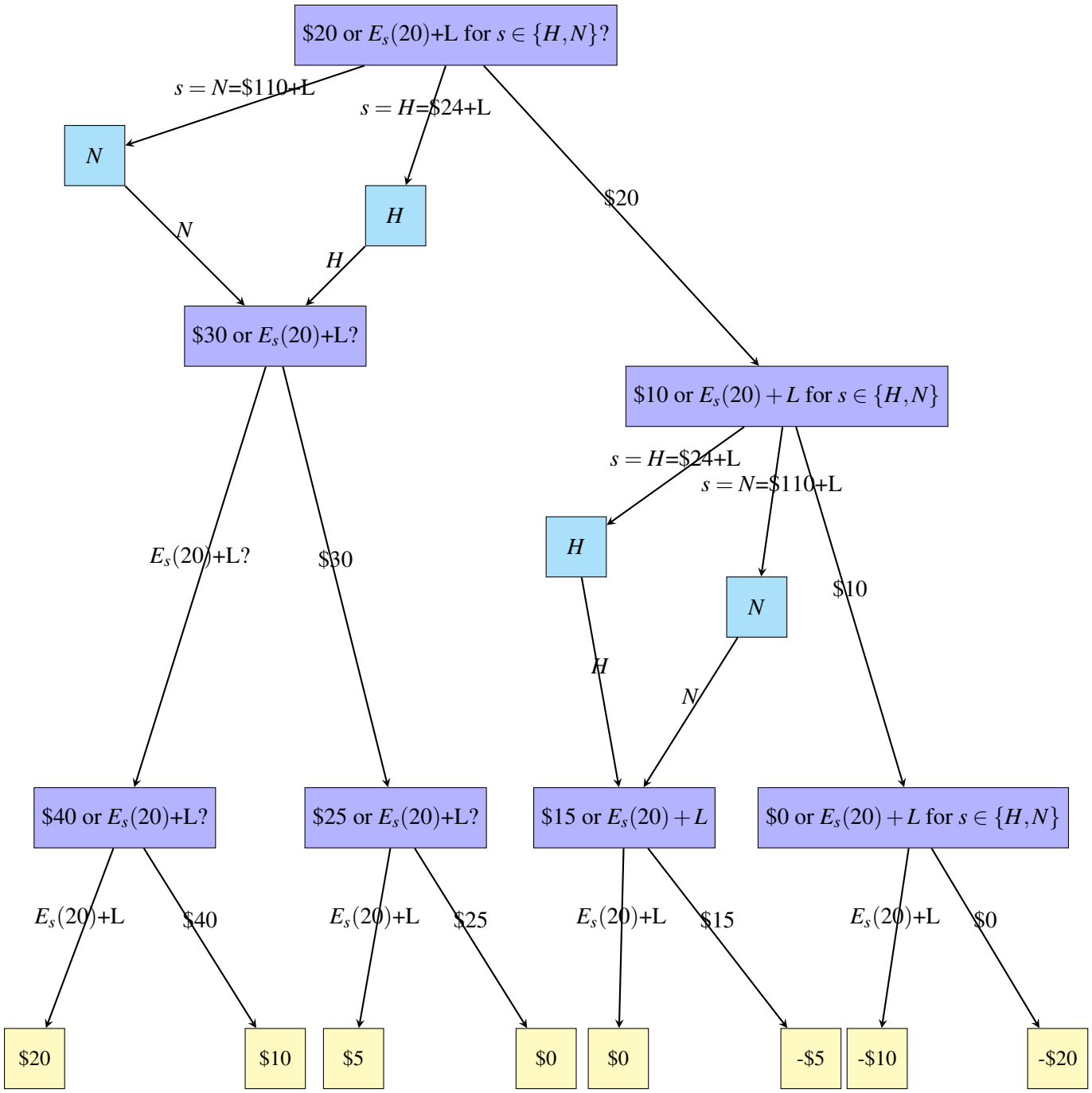
Note: This figure illustrates the design of Experiment 1. After Stage 0, respondents are randomized into a control treatment (Control), where the payout is fixed, and a state-specific payout treatment. After Stage 1, respondents are further randomized into three different policy bundles: Climate, Military, and Low Tax. For each bundle, half of the respondents receive additional information in the form of a market probability treatment (Market), while the others receive no additional information (C). After Stage 2, respondents are randomly assigned to one of four gift treatments. Two of these treatments involve state-specific payouts (one with a randomly assigned recipient and the other with a recipient holding opposing views on carbon taxation), while the other two serve as control groups where the gift payout is fixed.

Figure A2: Experiment 1: WTP Measurement algorithm



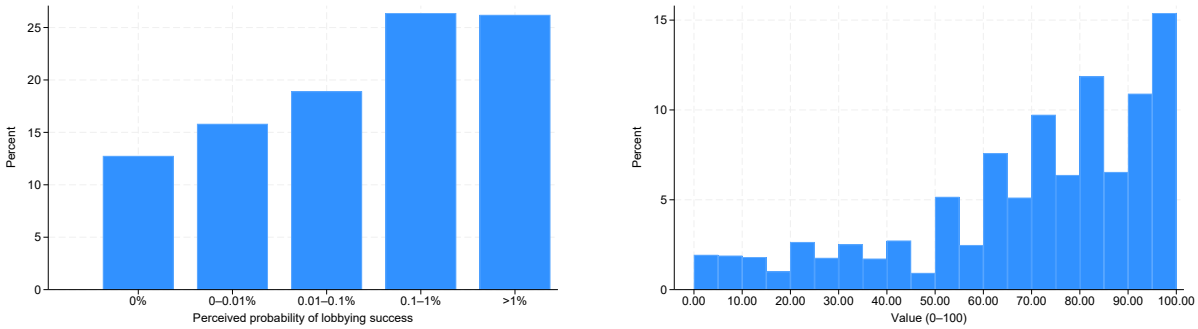
Note: This figure illustrates the algorithm used to elicit the willingness to pay (WTP) for the carbon tax lobby package in Experiment 1. Initially, respondents are asked whether they would like the researchers to implement the carbon tax lobby package (denoted as L), as described in Section 3.1.2. Based on their initial response, they are presented with a series of binary choices. For any given value X, the prompt "$X or $20 + L?" represents a choice between: (a) receiving a personal bonus of $X, or (b) implementing the lobby package for carbon taxation along with a personal bonus of $20. The highest amount of money a respondent is willing to forgo (ranging from -$20 to $20) is used as a measure of their WTP.

Figure A3: Experiment 1: WTP measurement algorithm for state-specific payouts



Note: This figure illustrates the algorithm used to elicit the willingness to pay (WTP) for the carbon tax lobby package for the state-specific payout treatment in Experiment 1. Respondents are initially asked whether they prefer: (a) $110 if 2024 is not the hottest year on record (denoted as $E_N(20)$) along with the lobby package for carbon taxation (L); (b) $24 if 2024 is the hottest year on record (denoted as $E_H(20)$) along with L; or (c) $20 without L. Respondents who choose option (a) are assigned $s = N$, while those who choose option (b) are assigned $s = H$. These respondents then proceed to choose between $E_s(20)$ and various alternative bonuses. Respondents selecting option (c) are subsequently asked if they prefer $10 or one of the previous alternatives, either (a) or (b). If they prefer $10, they are then asked to choose between $0 and alternatives (a) or (b). If a respondent prefers $E_s(20)+L$ over $10, they are next asked to choose between $15 and $E_s(20)+L$ for their assigned value of s. The WTP is measured as the highest dollar amount they are willing to forgo for $E_s(20)$.

Figure A4: Experiment 1: Perceived probabilities for lobbying success and 2024 being hottest year on record

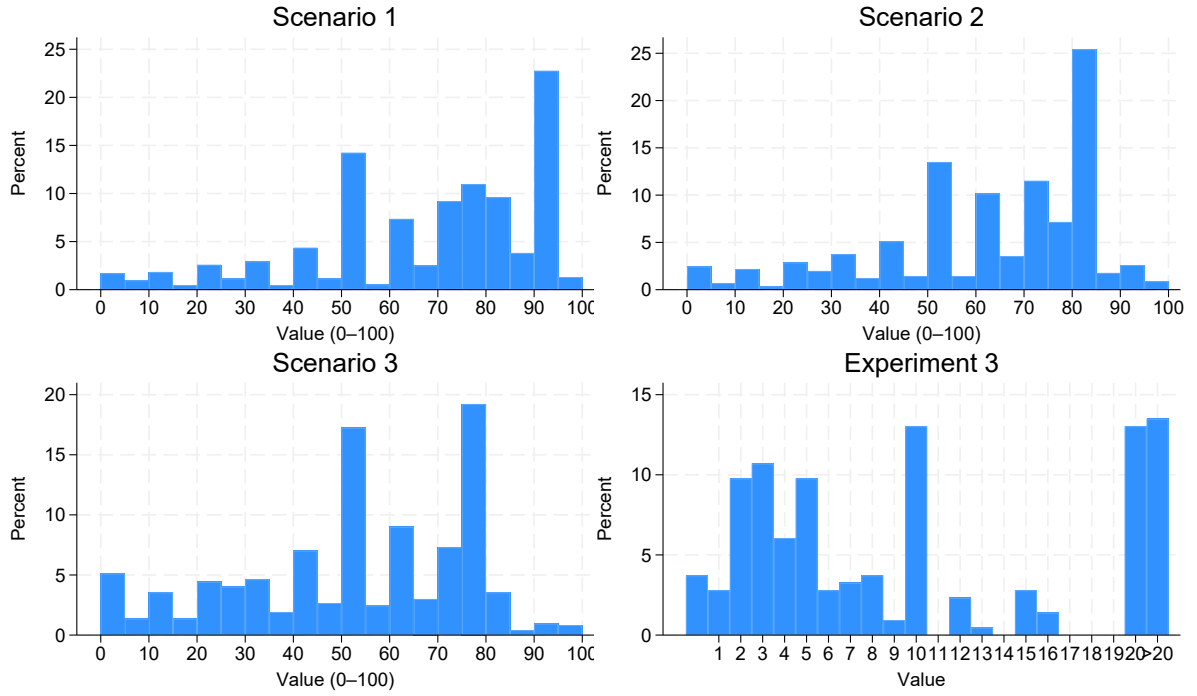


(a) Perceived probability of lobbying success

(b) Probability that 2024 will be the warmest year on record

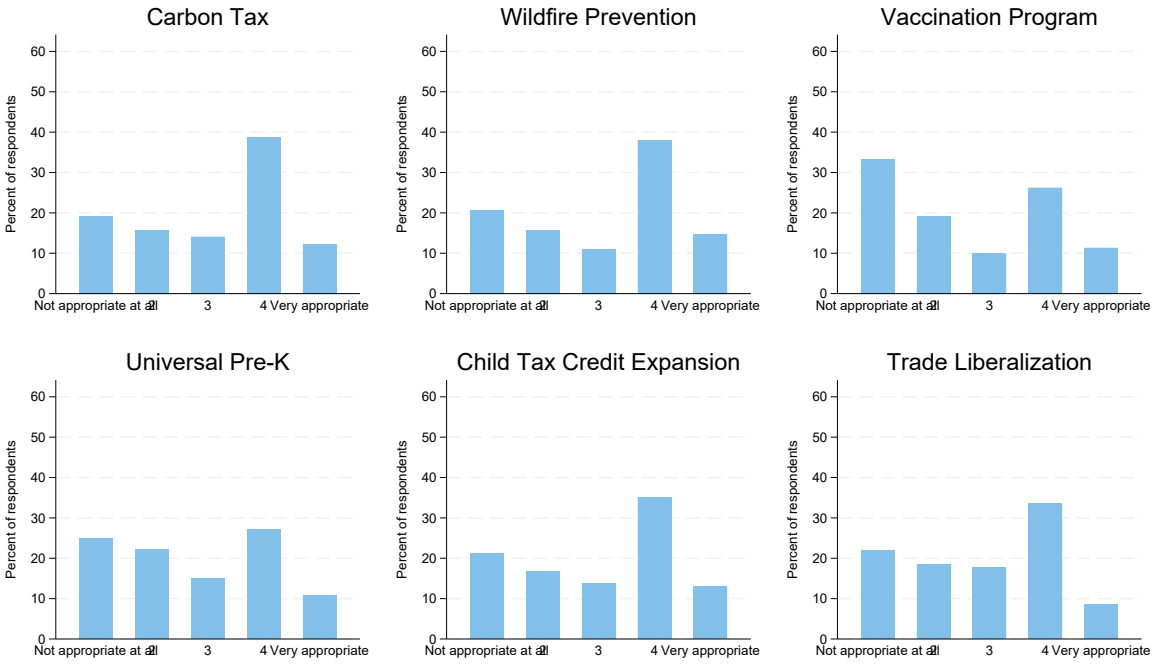
Note: The first panel shows the distribution of respondents' perceived probabilities that the lobby package will succeed in implementing a carbon tax in the United States. Responses were given on a five-point scale, with the following categories: 0%, 0–0.01%, 0.01–0.1%, 0.1–1%, and more than 1%, as displayed in the figure. The second panel displays the distribution of stated probabilities that 2024 will be the warmest year on record. Respondents answered by entering a number between 0 and 100.

Figure A5: Distribution of subjective probabilities in Experiment 2 and 3



Note: The first three histograms display the distributions of perceived success probabilities for the three scenarios in Experiment 2: wildfire prevention (Scenario 1), solar panels (Scenario 2), and congestion pricing (Scenario 3). The fourth histogram displays the distribution of respondents' expectations for the number of extreme price days in the Norwegian setting in Experiment 3. The histogram is capped at 20; responses above this value are grouped into a "> 20" category. Notably, only 4 percent of respondents believe there will be zero extreme price days in 2025.

Figure A6: Appropriateness of state-specific payouts across policy domains (Experiment 5)



Note: Respondents rated, on a five-point Likert scale (1 = "Not appropriate at all," 5 = "Very appropriate"), how appropriate it is for the government to use state-specific bonuses to increase support for six policies: carbon taxes, wildfire prevention, vaccination, universal pre-K, child tax credit expansion, and trade liberalization. The figure plots the distribution of responses for each domain.

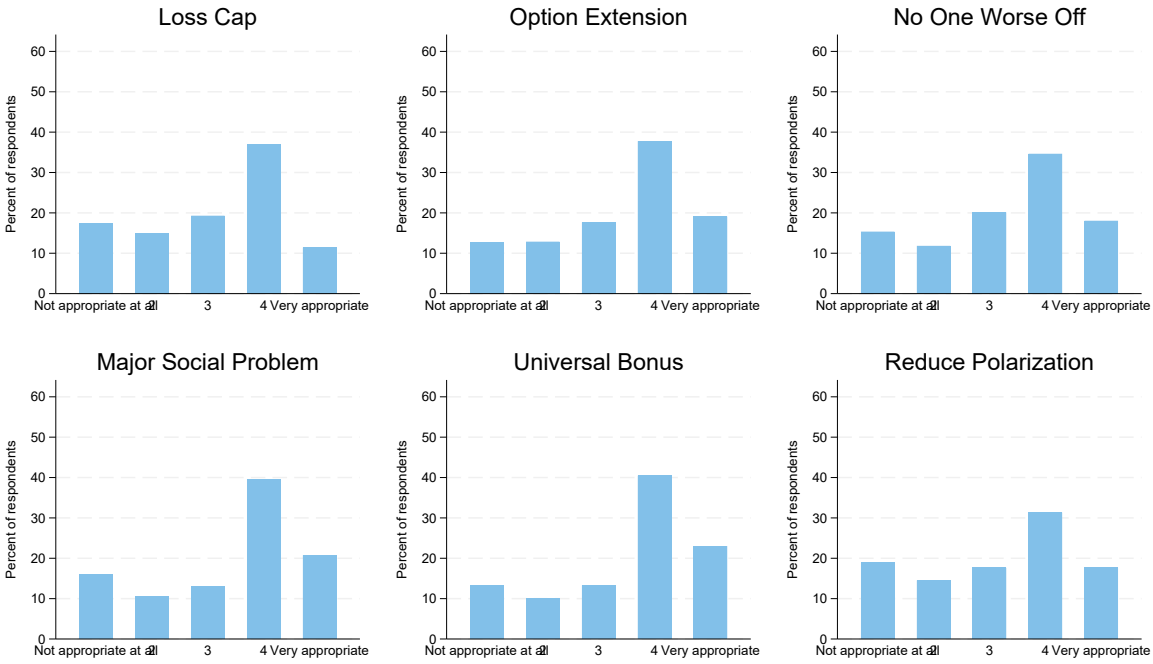
B Practical implementability and market liquidity

B.1 Theoretical framework

An important question is whether a designer can *practically* hedge the state–contingent transfers implied by the mechanism. This subsection formalizes the designer's payment in each state and derives a simple bound on the maximum exposure that must be hedged.

Government outlays. Recall that $F(q) \equiv \Pr(q_i < q)$ is the share of citizens who *under*-estimate the market probability $q = P(I = 1)$. For ease of exposition, I consider the setting where $r = 0$. Under the state–specific scheme (Definition 2.1) the total payouts for the designer are $v\frac{1-F(q)}{q}$ if $I = 1$ and $v\frac{F(q)}{1-q}$ if $I = 0$. For comparison, the uniform (baseline) rebate always costs v.

Figure A7: Support for structural constraints on state-specific payouts (Experiment 5)



Note: Respondents used a five-point Likert scale (1 = "Not appropriate at all," 5 = "Very appropriate") to rate how appropriate it would be for governments to use state-specific bonuses under six design constraints: loss caps, option extension, no one worse off, major social problem, Universal Bonus, and reduced polarization. The figure displays the rating distributions for each constraint.

Proposition B.1. *Let $W(q)$ be defined by*

$$W(q) := v \max\left\{ \frac{F(q)}{1-q}, \frac{1-F(q)}{q} \right\}.$$

Then:

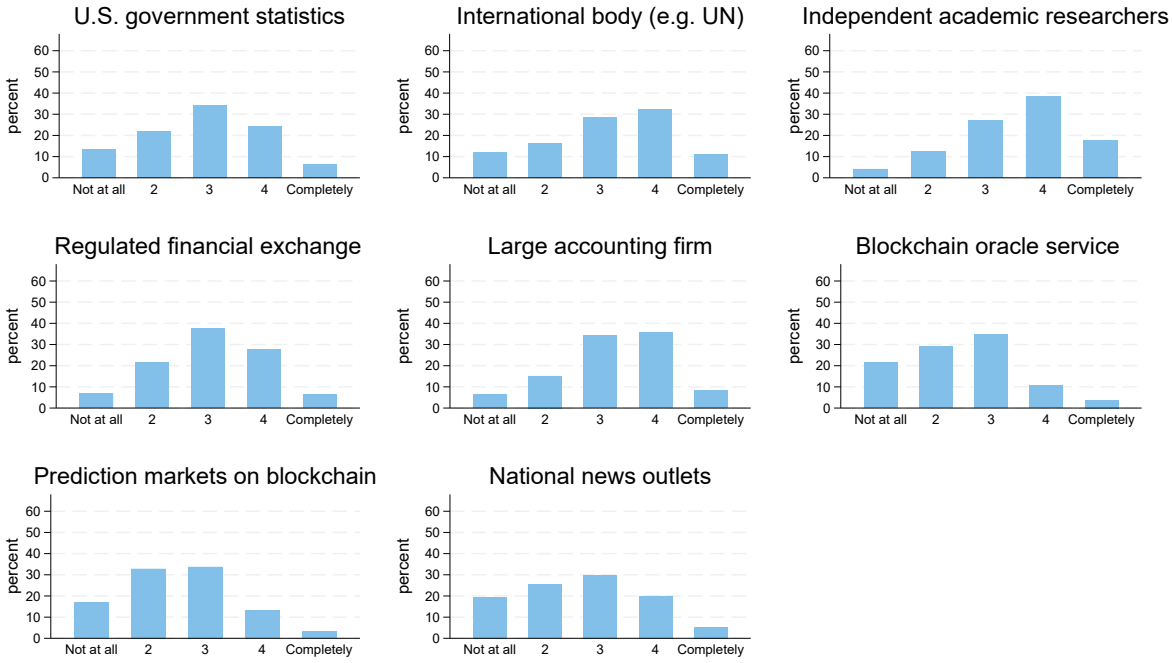
1. *$W(q)$ equals the maximum total cash outlay the designer may face in either state under the state–specific payout scheme.*

2. *Exposure is minimized when $\frac{F(q)}{1-q} = \frac{1-F(q)}{q}$.*

Proof. Immediate from the definition of $W(q)$ and the payout expressions above.

□

Figure A8: Trust in potential outcome-certifying bodies (Experiment 7)



Note: Respondents in Experiment 7 rated how much they would *trust* each of eight potential certifying bodies to verify whether the bonus–triggering event occurs. All answers were given on a 1–5 Likert scale (1 = *Not at all*, 2 = *Slightly*, 3 = *Moderately*, 4 = *Very*, 5 = *Completely*). The eight sources were: (1) a U.S. government statistics agency (e.g., Census Bureau, Bureau of Economic Analysis); (2) an international body (e.g., United Nations, World Meteorological Organization); (3) an independent academic research consortium (university scientists); (4) a regulated financial exchange (e.g., Kalshi, Chicago Mercantile Exchange); (5) a large audit/accounting firm (e.g., Deloitte, PwC); (6) a blockchain oracle service that records the result on-chain (e.g., Chainlink); (7) A prediction-market platform on the blockchain (e.g., Polymarket); and (8) major national news outlets reporting the result (e.g., AP, Reuters)..

The quantity $W(q) - v$ measures the maximum payment obligation relative to the baseline cost v. When the two state payments coincide, $W(q) = v$ and no hedge is required. Proposition B.1 shows that for intermediate values of q and $F(q)$ the designer's unhedged exposure is *automatically dampened.*

Lemma B.2 shows how individuals preferring payouts in different states restrict the amount of risk that the designer needs to hedge.

Lemma B.2 (Two–sided exposure bound)**.** *Fix $P \in (0, \frac{1}{2}]$. Suppose both the market probability and the share of citizens choosing each state-contingent payout satisfy*

$$P \;\le\; q \;\le\; 1-P, \qquad P \;\le\; F(q) \;\le\; 1-P. \tag{$\star\star$}$$

Then

$$\frac{W(q)}{v} \;=\; \max\Big\{\tfrac{F(q)}{1-q},\, \tfrac{1-F(q)}{q}\Big\} \;\le\; \Upsilon(P) := \frac{1-P}{P}.$$

Proof. Immediate from Proposition B.1. □

For example, if $P = \frac{1}{3}$, then $\Upsilon(P) = 2$, meaning the designer's maximum liability in any of the states is at most twice the flat rebate cost. As P increases, the cap tightens; as $P \to 0$, the cap grows without bound.

Universal Bonus limits. The Universal Bonus corresponds to the polar cases $F(q) = 1$ or $F(q) = 0$, where everyone selects the same state. One—arguably unlikely—case actually eliminates state risk: if everyone chooses the *more likely* state, then the larger of the two liabilities in Proposition B.1 satisfies $W(q) \to v$.[32] By contrast, if everyone selects the *unlikely* state, the unhedged exposure would diverge as that state becomes vanishingly rare (e.g. $F(q) \to 0$ with $q \to 0$), which is the rare–event setting emphasized by the Norwegian grid–integration example (Experiment 3). To avoid divergence in practice, the Universal Bonus can include a loading parameter $\delta > 0$, as in Section 2.2.1. The choice of δ therefore reduces worst–case liability while preserving the mechanism's appeal.

[32] If $F(q) \to 1$ with $q \to 0$ (or symmetrically $F(q) \to 0$ with $q \to 1$), all citizens select the payout in the most likely state and $W(q) \to v$. The scheme then entails no more risk than a flat rebate.

In sum, natural hedging is perfect when $F(q)/(1-q) = 1$ and remains moderate as long as q *and* $F(q)$ stay within the bounds in Lemma B.2. Universal Bonuses push the mechanism to its insurance frontier. I now turn to the practical question of how designers can purchase insurance-like financial products to protect against these risks.

C More theoretical results

This section examines three topics: (i) state-specific policies, (ii) state-specific payouts with risk-averse voters, and (iii) the relation to No-Betting Pareto Dominance (NBPD). Throughout this section, I set $r = 0$ (no participation fee) to highlight intuition, so the per-capita budget equals $b = v$. I also normalize population size to $N = 1$ and denote aggregate resources by v.

C.1 State-Specific Policies

The state-specific mechanism extends to settings when revenue funds alternative public programs rather than direct cash transfers.

Assumption 1 (Separability)**.** *The designer (in this case the government) can partition the population into two groups G_0 (share $F(q)$ of the population) and G_1 (share $1-F(q)$) and pre-design two mutually exclusive programs P_0 and P_1 such that:*

1. *P_0 benefits only G_0 and P_1 benefits only G_1;*
2. *Event I with market probability q determines which program receives the entire budget v;*
3. *Every member of G_1 assigns a higher subjective probability than q to $I = 1$, and every member of G_0 assigns a lower one.*

Corollary C.0.1. *Under Assumption 1, the government can implement* state-specific policy bundles *that replicate the effects of the monetary payouts in Section 2.1.*

Intuition. Invest $vF(q)$ in the $I=0$ claim, which pays $\frac{1}{1-q}$ in that state. This yields $\frac{vF(q)}{1-q}$, exactly enough for each of the $F(q)$ individuals to receive $\frac{v}{1-q}$. The argument is symmetric for $I=1$.

□

If we interpret G_0 and G_1 as two distinct regions, and P_0 and P_1 as transfers allocated to these regions respectively, then we require an event for which individuals in one region systematically underestimate the probability, while those in the other overestimate it.

C.2 State-Specific Payouts with Risk-Averse Voters

The baseline model assumed risk–neutrality. This section shows that the qualitative advantage of state-specific payouts survives *standard* levels of risk aversion. I derive a simple "distance-from-market" condition under which a risk-averse individual still prefers the state-specific design to an equivalent baseline (uniform) rebate.

Preferences and notation. Let each citizen have CRRA utility

$$U(c)=\begin{cases}\dfrac{c^{1-\gamma}}{1-\gamma}, & \gamma\neq 1,\\[2ex] \ln c, & \gamma=1,\end{cases}\qquad \gamma\in(0,\bar{\gamma}]$$

Let W denote consumption (equivalently, wealth) *absent the policy*. The policy contributes a deterministic component w_i to consumption, so define

$$W_0 \;:=\; W+w_i,$$

and evaluate risk around the level W_0.[33] I present the case $q_i > q$ (the case $q_i < q$ is symmetric).

Baseline (uniform) rebate. Utility if the policy is implemented with a flat transfer is $U(W_0+v)$.

[33] CRRA utility is defined over total consumption; evaluating curvature on a loss domain (as in prospect theory) would require a reference-dependent model, which is beyond the scope of this paper.

State-specific rebate. The voter selects $z_i = 1$ and obtains expected utility

$$EU^{\mathrm{SS}} \;=\; (q+\eta)\,U\Big(W_0+\tfrac{v}{q}\Big) \;+\; (1-q-\eta)\,U(W_0), \qquad \eta := q_i - q > 0.$$

Define the difference

$$\Delta U(v,\eta) \;:=\; EU^{\mathrm{SS}} - U(W_0+v).$$

A positive ΔU implies the voter strictly prefers the state-specific scheme.

Proposition C.1. *For every fixed $q \in (0,1)$ and $W_0 > 0$ there exists a strictly positive constant*

$$f(q,\gamma,\eta) \;:=\; \frac{2\eta/q}{\gamma\Big(\dfrac{q+\eta}{q^2}-1\Big)}$$

such that

$$\boxed{\Delta U(v,\eta) > 0 \quad \Longleftarrow \quad 0 < \frac{v}{W_0} < f(q,\gamma,\eta)}.$$

Hence, whenever the bonus is small enough relative to wealth and the optimism parameter η is large (while still $< 1-q$), the optimistic agent strictly prefers the state-specific payout to the baseline rebate.

Details. Take a second-order Taylor expansion of $U(\cdot)$ around W_0:

$$U\Big(W_0+\tfrac{v}{q}\Big) = U(W_0) + U'(W_0)\tfrac{v}{q} + \tfrac{1}{2}U''(W_0)\big(\tfrac{v}{q}\big)^2 + O(v^3),$$

$$U(W_0+v) = U(W_0) + U'(W_0)v + \tfrac{1}{2}U''(W_0)v^2 + O(v^3).$$

Insert into ΔU. The zeroth-order terms cancel, giving

$$\Delta U(v,\eta) = U'(W_0)\,\frac{\eta}{q}\,v \;+\; \frac{1}{2}\,U''(W_0)\Big(\tfrac{q+\eta}{q^2}-1\Big)v^2 + O(v^3).$$

For CRRA, $U'(W_0) = W_0^{-\gamma} > 0$ and $U''(W_0) = -\gamma W_0^{-\gamma-1} < 0$, hence

$$\Delta U(v,\eta) = W_0^{-\gamma} v \left[\frac{\eta}{q} - \frac{\gamma}{2} \frac{v}{W_0} \left(\tfrac{q+\eta}{q^2} - 1 \right) \right] + O(v^3).$$

The bracketed term is positive exactly when $v/W_0 < f(q,\gamma,\eta)$. In that range the leading linear term dominates the $O(v^3)$ remainder for sufficiently small v, so $\Delta U > 0$. □

Interpretation. The larger the belief gap $\eta = q_i - q$, the bigger a payment v (relative to resources W_0) a risk-averse citizen is willing to accept in state-specific form. Importantly, the curvature parameter acts on *wealth*, not on the gain itself: a \$200 bonus is practically riskless against a \$50 000 consumption baseline.

Calibration. Consider a very risk-averse agent ($\gamma = 3$) with moderate optimism ($\eta = 0.1$) relative to the market probability of $q = 0.5$ leading to $f(q = 0.5, \gamma = 3, \eta = 0.1) = 0.0952$. Even this very risk averse individual still prefers state-specific payout over the baseline rebates as long as the rebate/bonus (v) is less than 10 percent of wealth or consumption. In all experiments used throughout this paper, the amounts used for the state-specific payouts are small compared to overall wealth.

C.3 Relation to No-Betting Pareto Dominance

The *No-Betting Pareto Dominance* (NBPD) criterion of Gilboa et al. (2014) says that f *no-betting-Pareto dominates* g if and only if:

1. **Own-belief dominance:** Each person i weakly prefers f to g under her own belief q_i.

2. **Common-belief robustness:** There exists a single belief $\tilde{q} \in [0,1]$ such that, keeping the originally chosen claim fixed — $z_i = 1$ if $q_i \geq q$ (event-bonus), $z_i = 0$ if $q_i < q$ (non-event-bonus) — everyone still weakly prefers f to g when both are evaluated under $\tilde{q}$.

I now analyze when this is satisfied for the state-specific payout mechanism analyzed in this paper (using risk-neutral individuals). Let H denote the set of individuals who choose $z_i = 1$ (event bonus) and

L those who choose $z_i = 0$ (non-event bonus).

Proposition C.2 (NBPD criterion under the status-quo)**.** *Let $g = 0$ denote the status-quo (zero payoff). NBPD holds for the state-specific payout f if and only if:*

1. *Own-belief dominance: For all i,*

$$w_i + v \cdot \max\left\{ \frac{q_i}{q}, \frac{1-q_i}{1-q} \right\} \geq 0.$$

2. *Common-belief robustness: The common-belief interval*

$$\underline{q}^* \leq \overline{q}^*,$$

where

$$\underline{q}^* := \max\Big\{0, \sup_{i:z_i=1} \big(-\tfrac{q}{v} w_i\big)\Big\}, \quad \overline{q}^* := \min\Big\{1, \inf_{i:z_i=0} \big(1 + \tfrac{1-q}{v} w_i\big)\Big\},$$

is non-empty. If the relevant set is empty, the corresponding bound is dropped (i.e. if $H = \{i : z_i = 1\}$ is empty, set $\underline{q}^* = 0$; if $L = \{i : z_i = 0\}$ is empty, set $\overline{q}^* = 1$).

Special case: Universal Bonus. *If all individuals choose the same z (i.e. H is full or L is full), then condition (i) implies condition (ii):*

- *If H is full (L empty), condition (ii) reduces to*

$$\tilde{q} \in \big[\, \max\{0, \sup_i(-\tfrac{q}{v} w_i)\},\ 1 \,\big],$$

which is non-empty whenever condition (i) holds (choose $\tilde{q} = 1$).

- *If L is full (H empty), condition (ii) reduces to*

$$\tilde{q} \in \big[0,\ \min\{1, \inf_i(1 + \tfrac{1-q}{v} w_i)\} \,\big],$$

which is non-empty whenever condition (i) holds (choose $\tilde{q} = 0$).

Sketch of Proof. Step (i): Own-belief dominance. With the status quo g as comparison (zero payoff), each person compares the policy f to g using her own belief q_i. Under the state-specific payout, the best a person can do is choose the side (event or non-event) that gives the higher expected value under her own belief. This yields the condition

$$w_i + v \cdot \max\left\{ \frac{q_i}{q}, \frac{1-q_i}{1-q} \right\} \geq 0$$

for all i. This is exactly the "own-belief dominance" requirement.

Step (ii): Common-belief robustness. Now suppose there is a single common belief $\tilde{q}$ used by everyone, but each person keeps the *same* side choice z_i she originally made under her own q_i.

- If $z_i = 1$ (chose the event bonus), the payout under $\tilde{q}$ is $v \cdot \frac{\tilde{q}}{q}$. This is preferred to the status quo iff

$$w_i + v \cdot \frac{\tilde{q}}{q} \geq 0 \quad \Longleftrightarrow \quad \tilde{q} \geq -\frac{q}{v} w_i.$$

- If $z_i = 0$ (chose the non-event bonus), the payout under $\tilde{q}$ is $v \cdot \frac{1-\tilde{q}}{1-q}$. This is preferred to the status quo iff

$$w_i + v \cdot \frac{1-\tilde{q}}{1-q} \geq 0 \quad \Longleftrightarrow \quad \tilde{q} \leq 1 + \frac{1-q}{v} w_i.$$

For the common-belief condition to hold, there must exist some $\tilde{q}$ satisfying *all* such inequalities simultaneously. This is equivalent to requiring that the *common-belief interval*

$$\tilde{q} \in \left[\underbrace{\max\left\{0, \sup_{i:z_i=1}\left(-\tfrac{q}{v} w_i\right)\right\}}_{\underline{q}^*}, \quad \underbrace{\min\left\{1, \inf_{i:z_i=0}\left(1 + \tfrac{1-q}{v} w_i\right)\right\}}_{\overline{q}^*} \right]$$

is non-empty, i.e. $\underline{q}^* \leq \overline{q}^*$.

Special case: Universal Bonus. If all individuals choose the same side (H full or L full), then condition (ii) is automatically satisfied whenever condition (i) holds: one can simply choose $\tilde{q} = 1$ (if H full) or $\tilde{q} = 0$ (if L full) to meet all inequalities. □

For the general case, when both H and L are non-empty, condition (ii) may fail even if condition (i) holds. Intuitively, this happens when the beliefs and net benefits of the two groups are so far apart that no single common belief $\tilde{q}$ can simultaneously make both sides at least as well off as under the status quo. In such cases, the lower bound $\underline{q}^*$ implied by the H-group lies above the upper bound $\overline{q}^*$ implied by the L-group, leaving no overlap.

Numerical example Let $q = 0.5,\ v = 2$, and consider two agents: A with $q_A = 0.9$ ($z = 1$), and B with $q_B = 0.1$ ($z = 0$).

Failing NBPD: (Let $w_A = w_B = -2.2$)

$$\text{Step (i):} \quad w_A + v(q_A/q) = -2.2 + 2(0.9/0.5) = 1.4 \quad \text{(same for B)}.$$

$$\text{Step (ii):} \quad \underline{q}^* = \max\{0, -0.5/2 \cdot (-2.2)\} = 0.55,$$

$$\overline{q}^* = \min\{1, 1 + 0.5/2 \cdot (-2.2)\} = 0.45.$$

Here, $\underline{q}^* > \overline{q}^*$, so the interval $[0.55, 0.45]$ is empty $\Rightarrow$ NBPD fails.

Passing NBPD: (Let $w_A = -2.2,\ w_B = -0.2$)

$$\text{Step (i):} \quad \text{as above, both} \geq 0.$$

$$\text{Step (ii):} \quad \underline{q}^* = 0.55 \text{ (A)}, \quad \overline{q}^* = 0.95 \text{ (B)}.$$

Now, the interval $[0.55, 0.95]$ is non-empty $\Rightarrow$ NBPD holds.

D Experiment 1: Additional results

This section presents the experimental design and results for portions of Experiment 1 not included in the main manuscript. It covers a deeper analysis of the bundling results from Stage 2, as well as the effects of state-specific gifts in Stage 3.

D.1 Experimental design: Stage 2 (Bundling)

Bundling policies In Stage 2 of Experiment 1, respondents are randomly assigned to one of three conditions, where they are asked how much they value a carbon taxation lobby package together with making a financial donation to one of the following organizations: (a) **The Climate Group**, which advocates for climate change mitigation by influencing policymakers (**Climate** bundle); (b) **Military Officers Association of America (MOAA)**, which supports the rights and benefits of military personnel, veterans, and their families (**Military** bundle); or (c) **Americans for Tax Reform**, which promotes lower taxes by influencing policymakers (**Low Tax** bundle).

I here present results on how the different bundles impact WTP on respondents with differing views on carbon taxation.

D.2 Experimental design: Stage 3 (State-Specific Gifts)

If state-specific payouts provide benefits by aligning with individuals' beliefs, such that they pay more in scenarios that respondents deem more likely to occur, then this mechanism might have broader applications. Therefore, I test whether this same mechanism applies within a binary version of the dictator game.

Control group respondents are asked to choose between receiving a $1 personal bonus or giving a $20 bonus to another respondent. In contrast, treated respondents are given the choice between receiving a $1 personal bonus, giving a $24 bonus to another respondent if 2024 is the hottest year ever, or giving a

$110 bonus to another respondent if 2024 is not the hottest year ever. Respondents are informed that one of their answers will be implemented. Additionally, within both groups, half of the respondents are told that the recipient will be a person with opposing views on carbon taxation ("Different"), while the other half are informed that the recipient will be a randomly selected individual ("Random"). This creates four treatment groups: State-Specific (Random or Different) and Control (Random or Different).

D.3 Results: Stage 2 (Bundling)

Descriptive results The second row of the first panel in Table A9 shows that none of the three policy bundles results in a higher WTP than the baseline WTP of $1.6 for the carbon tax lobby package alone.[34]

Among the bundles, the lowest WTP is observed for the combination of carbon taxes with reduced other taxes, with an average WTP of $-0.3. Supporters exhibit a consistently positive WTP for all three bundles, while skeptics demonstrate a negative WTP across all bundles. Notably, skeptics exhibit the highest (least negative) WTP for the bundle combining carbon taxes with increased military spending.

Effect of bundles on WTP Since WTP is measured for the same respondents twice—first in Stage 1 for the carbon tax lobby package alone, and then in Stage 2 for the bundled policy—I use paired t-tests to evaluate the individual effects of the bundling treatments. These results are presented in Table A9, with separate analyses for respondents in the control and state-specific treatment groups. The analysis reveals a significant $2.77 increase in WTP for the military bundle among skeptics, offset by a significant $3.04 decrease in WTP among carbon tax supporters.[35] Results for the low-tax bundle show a broadly similar pattern, though the reduction in WTP among carbon tax supporters is more pronounced in this case.

[34] Columns 2 to 5 in Table A9 present results exclusively for respondents not assigned to the market probability treatment.

[35] The coefficient derived from paired t-tests may differ slightly from the difference in means between the unbundled policy and the military bundle due to methodological differences and minor variations in the sample.

D.3.1 The effect of state-specific treatment on support for carbon tax bundles

The state-specific payout treatment raises the average WTP for the military bundle to \$3.80, an increase of \$3.3 compared to the military bundle without state-specific payouts ($p = 0.035$). This combination achieves the highest WTP estimate across all bundles in this study, though it is only slightly higher than the WTP from the unbundled state-specific treatment from Stage 1 (\$3.4) and the state-specific treatment with the climate bundle (\$3.79). While the state-specific treatment does not meaningfully affect carbon tax supporters, it increases the WTP of skeptics by \$4.6 compared to the military bundle alone ($p = 0.024$).

For the combination of state-specific payouts with lowering other taxes, the WTP increases by \$3.57 relative to the low-tax bundle alone ($p = 0.022$). Once again, this effect is driven by carbon tax skeptics. Among skeptics, the combination of the Low tax bundle and state-specific payouts raises WTP by \$7.6, compared to an average WTP of \$-7.1 without state-specific payouts ($p < 0.001$). However, this combination still fails to achieve broader support or a higher overall WTP compared to other bundles, primarily due to its strong negative effect on carbon tax supporters.

Table A9: WTP for Carbon Tax Lobby Packages: Bundled and Unbundled (Experiment 1)

All Respondents

	All		Bundles		
	(1)	(2)	(3)	(4)	(5)
	Unbundled	Combined bundles	Climate	Military	Low tax
State-specific	1.823***	3.050***	2.292	3.301**	3.571**
	(2.91)	(3.37)	(1.43)	(2.11)	(2.30)
Constant	1.622***	0.569	1.502	0.500	-0.296
	(3.49)	(0.87)	(1.31)	(0.44)	(-0.26)
Positive share (control)	0.56	0.55	0.61	0.54	0.50
Positive share (state-specific)	0.68	0.65	0.65	0.65	0.65
Paired t-test (control)		-0.12	0.47	0.47	-1.28
P-value t-test (control)		0.797	0.374	0.551	0.177
Paired t-test (state-specific)		-0.13	1.15	-0.43	-1.06
P-value t-test (state-specific)		0.764	0.064	0.556	0.190
Observations	2,386	1,200	398	396	406

t statistics in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Carbon tax supporters

	All		Bundles		
	(1)	(2)	(3)	(4)	(5)
	Unbundled	Combined bundles	Climate	Military	Low tax
State-specific	-1.531**	-0.842	-1.529	0.000727	-0.848
	(-2.47)	(-0.76)	(-0.97)	(0.00)	(-0.40)
Constant	13.12***	10.24***	12.99***	9.367***	8.077***
	(31.21)	(13.28)	(13.19)	(6.27)	(5.41)
Positive share (control)	0.92	0.83	0.93	0.81	0.75
Positive share (state-specific)	0.91	0.84	0.89	0.83	0.80
Paired t-test (control)		-2.55	0.15	-3.04	-5.00
P-value t-test (control)		0.001	0.840	0.036	0.002
Paired t-test (state-specific)		-1.87	0.56	-2.64	-3.67
P-value t-test (state-specific)		0.011	0.535	0.033	0.025
Observations	1,051	526	186	166	174

t statistics in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Carbon tax skeptics

	All		Bundles		
	(1)	(2)	(3)	(4)	(5)
	Unbundled	Combined bundles	Climate	Military	Low tax
State-specific	5.833***	6.199***	6.368***	4.647**	7.640***
	(7.64)	(5.36)	(3.13)	(2.28)	(3.93)
Constant	-8.258***	-7.050***	-9.009***	-5.289***	-7.098***
	(-15.47)	(-8.96)	(-6.67)	(-3.94)	(-5.14)
Positive share (control)	0.25	0.32	0.31	0.36	0.29
Positive share (state-specific)	0.51	0.51	0.45	0.51	0.55
Paired t-test (control)		1.80	0.75	2.77	1.74
P-value t-test (control)		0.001	0.302	0.002	0.100
Paired t-test (state-specific)		1.22	1.65	1.33	0.75
P-value t-test (state-specific)		0.010	0.056	0.126	0.318
Observations	1,335	674	212	230	232

t statistics in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Note: *Constant* represents the average WTP for respondents not assigned to the state-specific treatment, while *State-specific* denotes the treatment effect of this treatment using OLS regression with robust standard errors and no control variables. T-statistics are in parentheses, and * p<0.1, ** p<0.05, *** p<0.01. *Positive share* indicates the proportion of respondents with strictly positive WTP. Column (1) presents the results of WTP for the carbon tax lobby package (Stage 1), while Columns (2) to (5) analyze the WTP for the bundled carbon tax lobby package (Stage 2), where the sample is restricted to respondents not receiving information about market probabilities. The first panel presents the results for all respondents, while the second and third panels separate the results into carbon tax supporters and skeptics. *Paired t-test* and *P-value (t-test)* denote the individual difference between WTP for bundled and unbundled carbon taxes and the associated p-value.

D.3.2 Results: Stage 3 (State-Specific Gifts)

Treatment Effects In Stage 3, 55% of control group respondents chose to keep the $1 personal bonus, but this share decreases by 7 percentage points ($p = 0.001$) when respondents are assigned to one of the state-specific treatments, reflecting a 12% reduction. Appendix Table A10 provides more detailed information on this effect.

Appendix Table A11 shows that respondents are more likely to give gifts aligned with states they perceive as more likely. For example, respondents underestimating the probability of 2024 being the hottest year are 10 percentage points less likely to give a bonus contingent on 2024 being the hottest year, but 7 percentage points more likely to give a bonus if 2024 is not the hottest year. These correlations suggest that one reason state-specific gifts increase giving is that givers perceive a higher expected value when the gift is tied to a state they believe is more likely to occur.

Table A10: State-specific treatment effect on choosing personal bonus (Experiment 1)

Share keeping

	Full sample			Supporters			Non-supporters		
	(1) Combined	(2) Random	(3) Different	(4) Combined	(5) Random	(6) Different	(7) Combined	(8) Random	(9) Different
State-specific, combined	-0.0684***			-0.0924***			-0.0491*		
	(-3.35)			(-3.01)			(-1.80)		
State-specific, random		-0.0287			-0.00856			-0.0367	
		(-1.03)			(-0.21)			(-0.97)	
State-specific, different			-0.128***			-0.177***			-0.0890**
			(-4.59)			(-4.25)			(-2.35)
Observations	2,386	1,233	1,153	1,051	545	506	1,335	688	647
Controls	No	No	No	No	No	No	No	No	No
Control group mean	0.55	0.41	0.71	0.53	0.34	0.74	0.55	0.46	0.67

t statistics in parentheses

* p<0.1, ** p<0.05, *** p<0.01

Note: This table presents the OLS regression results with robust standard errors. The outcome variable is a binary indicator equal to 1 if the respondent chooses a personal bonus, and 0 if the respondent chooses to give to others. The first row ("State-specific, combined") represents the combined effect of the two state-specific treatments. The second row isolates the treatment effect for the state-specific treatment where the recipient is randomly assigned, while the third row isolates the effect for the treatment where the recipient holds differing views on carbon taxation. Columns (1) to (3) show the effects for the entire sample, while Columns (4) to (6) and Columns (7) to (9) display the effects for carbon tax supporters and skeptics, respectively.

E Experiment 9: Robustness

This study extends Experiment 2 to test whether the effect of *state-specific bonus* can occur from framing effects. Two distinct framing channels are evaluated:

Table A11: Correlation Between Subjective Probabilities and State-Specific Gifts (Experiment 1)

	Gift with hot payout			Gift with not-hot payout		
	(1)	(2)	(3)	(4)	(5)	(6)
	Hot	Hot	Hot	NotHot	NotHot	NotHot
Underestimate	-0.0959***			0.0735***		
	(-3.73)			(2.71)		
Large underestimate		-0.178***			0.0429	
		(-8.08)			(1.34)	
Not supporting carbon tax			-0.0827***			0.0199
			(-3.37)			(0.74)
Observations	1,177	1,177	1,177	1,177	1,177	1,177
Controls	No	No	No	No	No	No
Mean in the other group	0.28	0.26	0.26	0.26	0.29	0.29

t statistics in parentheses

* p<0.1, ** p<0.05, *** p<0.01

Note: This table shows the correlation between subjective probabilities and gift choices using OLS regression with robust standard errors. "Underestimate" is a binary variable equal to 1 for respondents whose subjective probability that 2024 will be the hottest year, as measured in Stage 0, is lower than the market probability. "Large underestimate" is a binary variable equal to 1 for respondents whose subjective probability is lower than 50 percent. "Not supporting carbon tax" is a binary variable equal to 1 for respondents who did not support a hypothetical carbon tax in Stage 0. "Hot" is a binary variable equal to 1 for respondents who wish to give a gift with an expected value of $20 if 2024 is the hottest year. "NotHot" is a binary variable equal to 1 for respondents who wish to give a gift with an expected value of $20 if 2024 is not the hottest year.

1. **Cost–scope signaling.** In the original state-specific bonus treatment arm, the quoted cost of the program is higher ($90 for wildfire prevention; $275 for congestion pricing) than in the control arm ($50 and $150, respectively). Respondents might infer that a more expensive program is larger, raising support even without the bonus.

2. **Large-number attraction or contingency neglect.** The state-specific bonus arm pairs a substantial cost with a *much* larger contingent payment. If respondents neglect the contingency, the larger numbers may draw support. To isolate this, I create a *placebo* bonus equal in size and probability to the true bonus but tied to an outcome unrelated to policy success.

3. **Responsiveness to costs.** As an additional robustness test, I examine the extent to which higher costs affect support for the policy. All else equal, we should expect higher costs to reduce support. If respondents misunderstand the question or answer randomly, however, this effect would be absent. To test this, I include a treatment identical to the control group except that it shows a higher cost ($90 vs. $50).

E.1 Experimental design

In June 2025, $N = 989$ U.S. adults were recruited on Prolific, representative of the population on party identification, age, and gender.[36] Each respondent evaluated the wildfire-prevention and congestion-pricing scenarios from Experiment 2, but with additional treatment arms.

E.1.1 Treatments

Within each scenario, respondents were randomized to one of four conditions:

1. **State-Specific** — identical to Experiment 2.
2. **Control** — identical to Experiment 2.
3. **High-Cost** — the control treatment, but with the same cost of the policy as in the State-specific treatment:
   (a) *Wildfire*: "The program would cost $90 per taxpayer. Would you support it?"
   (b) *Congestion*: "The program would cost $275 per taxpayer. Would you support it?"
4. **Placebo bonus** — same cost and expected value as the State-Specific treatment arm, but bonus triggered by an irrelevant event.
   (a) *Wildfire*: "The program would cost $90 per taxpayer. If it rains in San Francisco on 1 October 2025—an event that occurs roughly once in ten years—the government will pay $400 to each taxpayer. Would you support this program?"
   (b) *Congestion*: "The program would cost $275 per taxpayer. In addition, the government will run a one-in-four lottery; if successful it will pay $500 to each taxpayer. Would you support this program?"

[36] Pre-registered at AsPredicted #234521. The pre-analysis plan and full instructions appear in Appendix F and J.5.

E.1.2 Outcome measures

After each scenario respondents answered:

- **Support** — binary support for the program (as in Experiment 2);
- **Scope** — perceived scale of the program on a 5-point Likert scale (*very small* to *very large*).

E.1.3 Analytic strategy

Comparison of the **Scope** ratings in the State-Specific and Control arms reveals whether the stated cost alters respondents' perception of the scope of the program. Contrasting the *Support* outcomes in the High-Cost and Control conditions isolates the pure effect of quoting a higher cost. Finally, to detect any "large-number" framing, I compare each of the State-Specific and Placebo arms with the Control group. For both scenarios, the Placebo treatment arm provides respondents with an offer that has the same expected value and distribution as the state-specific treatment for respondents holding the same beliefs as government. However, in the Placebo treatment, the event determining the outcome is unrelated to subjective beliefs, and we should hence not expect the treatment effect to differ by beliefs if it is the state-specific mechanism that drives the effect.

E.2 Results

E.2.1 Cost–scope cue

Table A12, columns (1) and (3), shows no significant difference in **Scope** between the State-Specific and Control arms, implying the higher quoted cost does *not* make the program seem larger. Hence, we should arguably not be worried that the state-specific treatment increases support by affecting perceptions of the scope of the policy in Experiment 2. Columns (2) and (4) indicate that the High-Cost arm all else equal *reduces* support by roughly 11 percentage points. Thus, the positive bonus effect in Experiment 2 should arguably not be attributed to cost-induced scope perceptions.

E.2.2 Large-number/bonus attraction

Table A13 reports that the genuine State-Specific bonus raises support by 9 percentage points for wildfire ($p = 0.019$) and 7.5 percentage points for congestion ($p = 0.106$). The placebo bonus, by contrast, *reduces* wildfire support by 7 percentage points ($p = 0.073$) and has no effect on congestion. Moreover, the true state-specific bonus is most effective among respondents who believe the policy is likely to fail, whereas placebo effects are flat across belief types. For example, among those with pessimistic beliefs on the success of congestion pricing, the state-specific bonus raises support by 12.6 pp ($p = 0.017$) while the placebo does not.

Together, these findings make simple framing stories less likely: higher quoted costs do not inflate perceived scope, and large numbers per se do not attract support unless the bonus is *belief-relevant.*

Table A12: Cost framing: High-cost vs. State-Specific (Experiment 9)

	Wildfire prevention		Congestion pricing	
	(1)	(2)	(3)	(4)
	Scope	Support	Scope	Support
State-specific Treatment	-0.0384		0.0202	
	(-0.47)		(0.23)	
High Cost Treatment		-0.115***		-0.114***
		(-2.69)		(-2.64)
Observations	457	500	467	507
Controls	No	No	No	No
Control group mean	4.27	0.70	3.83	0.45

t statistics in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Note: **Scope** is measured on 5-point Likert scale (higher value equals larger perceived scale). **Support** is the binary support variable, measured as in Experiment 2. "High Cost" and "State-Specific" are treatment indicators; control is the low-cost fixed-bonus arm. OLS with robust standard errors.

Table A13: Placebo bonus vs. belief-aligned bonus (Experiment 9)

All Respondents

	(1) Wildfire prevention	(2) Wildfire prevention	(3) Congestion pricing	(4) Congestion pricing
State-specific Treatment	0.0944**		0.0745	
	(2.35)		(1.62)	
Placebo Treatment		-0.0755*		0.0330
		(-1.80)		(0.72)
Observations	469	508	473	475
Controls	No	No	No	No
Control group mean	0.70	0.70	0.45	0.45

t statistics in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Pessimistic beliefs

	(1) Wildfire prevention	(2) Wildfire prevention	(3) Congestion pricing	(4) Congestion pricing
State-specific Treatment	0.0905*		0.126**	
	(1.80)		(2.39)	
Placebo Treatment		-0.0808		0.0750
		(-1.59)		(1.42)
Observations	325	364	347	343
Controls	No	No	No	No
Control group mean	0.66	0.66	0.36	0.36

t statistics in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Optimistic beliefs

	(1) Wildfire prevention	(2) Wildfire prevention	(3) Congestion pricing	(4) Congestion pricing
State-specific Treatment	0.0841		-0.0238	
	(1.32)		(-0.28)	
Placebo Treatment		-0.0626		-0.0524
		(-0.87)		(-0.62)
Observations	144	144	126	132
Controls	No	No	No	No
Control group mean	0.78	0.78	0.66	0.66

t statistics in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Note: Outcome is binary support. "State-Specific" and "Placebo" are treatment indicators; control is the low-cost fixed arm. Pessimists (optimists) assign a lower (higher or equal) success probability than the government benchmarks of 90 and 75 percent. OLS estimates with robust standard errors.

F Pre-analysis plans

F.1 Experiment 1

CONFIDENTIAL - FOR PEER-REVIEW ONLY

Carbon tax preferences (#184328)

Created: 07/25/2024 09:01 AM (PT)

This is an anonymized copy (without author names) of the pre-registration. It was created by the author(s) to use during peer-review.
A non-anonymized version (containing author names) should be made available by the authors when the work it supports is made public.

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1) Have any data been collected for this study already?
No, no data have been collected for this study yet.

2) What's the main question being asked or hypothesis being tested in this study?
The main question is how to increase support for carbon taxation through state-specific payments and policy bundles.
Opposition to carbon taxes often stems from differing beliefs about future climate outcomes. By tailoring policy bundles to these beliefs, support for carbon taxation can be increased. For example, offering payments in states where climate change is less damaging than expected may be extra beneficial, and enhance support, among skeptics. This study will particularly analyze how state-specific payments impact carbon tax opponents, and if willingness to pay (WTP) for carbon taxation rises when individuals can choose a personal bonus if 2024 is -or is not -an extraordinarily warm year.
Additionally, the study will examine opposition arising from non-uncertainty-related beliefs or preferences. If opposition to carbon taxes is preference-based, policy bundles combining a carbon tax with favored policies (lower taxes, more military spending, or climate change mitigation efforts) might increase support. The hypothesis will be tested by eliciting respondents' WTP for these bundles and analyzing whether the share of positive WTP increases, focusing on both supporters and opponents of carbon taxation.
Finally, the study will investigate if opposition is due to belief differences where carbon tax supporters can use non-preferred policies as signaling devices, meaning that by accepting a policy they dislike in exchange for a carbon tax, then opponents of the carbon tax may become more certain that climate change is serious.

3) Describe the key dependent variable(s) specifying how they will be measured.
The key dependent variable is the WTP for a lobby package for carbon taxation, both alone and bundled with three other policies (lower taxes, more military spending, and climate change mitigation efforts). This will be measured through a series of questions: first, a binary question to determine if WTP is positive. If the response is positive, the follow-up questions will ask if they are willing to give up $10, and if affirmative, then $20. If the response is no after $10, the follow-up will ask about $5. For those with an initial negative WTP, the questions will assess if they prefer losing $10 to implementing the policy, and if still negative, then $20. If the response is negative followed by yes, the follow-up will ask about $5.

4) How many and which conditions will participants be assigned to?
First, half of the respondents will be assigned to a conditional WTP condition, where they can choose a payment conditional on 2024 being the hottest or not the hottest year. Second, after eliciting WTP for carbon taxation, respondents will be split into three groups to elicit WTP for different policy bundles, with each respondent valuing only one bundle. Additionally, to understand the underlying mechanisms behind bundling, there will be further randomization. Half of the respondents will be informed that the expected value is equal for payments conditional on 2024 being the hottest or not the hottest year. To examine state-specific payments' effects on behalf of others, respondents will be divided into four groups: half will choose between a personal bonus and a payment to others, while the other half will choose between a personal bonus and a state-specific payment to others. In each case, half will be told the other person is random, and the other half will be told the other person holds opposing views on carbon taxation. Additionally, half of the respondents will receive information about the number of carbon tax supporters who prefer or do not prefer the bundled policy.

5) Specify exactly which analyses you will conduct to examine the main question/hypothesis.
I will first conduct OLS regressions with average WTP and the share of positive WTP as dependent variables and the assignment to state-specific treatment as the independent variable, particularly analyzing treatment effects for respondents who do not generally support carbon taxes. Second, I will analyze within-subject differences by comparing individual WTP for carbon tax alone versus carbon tax bundled with one of the three policies (lower taxes, more military spending, climate change mitigation). The main goal is to identify which policy combinations maximize the share of respondents supporting carbon taxation, whether through bundling, state-specific payments, or both.

6) Describe exactly how outliers will be defined and handled, and your precise rule(s) for excluding observations.
I will not exclude any observations. Most variables are either binary or Likert scale variables. The main WTP variable ranges from -20 to 20. One question asks about the perceived probability of their choice impacting the implementation of the lobby package; if this probability is near zero, even a small WTP can imply an arbitrarily large value of being pivotal. To avoid this, I cap the maximum value of being pivotal at $10,000.

7) How many observations will be collected or what will determine sample size? No need to justify decision, but be precise about exactly how the number will be determined.
I will aim for a sample size of 2500 from a representative US sample collected through Prolific. If I do not receive 2500 observation within August 4st, 18:00 CET I will close the data collection at this time.

F.2 Experiment 2

State-specific government programs (#207101)

Author(s)
This pre-registration is currently anonymous to enable blind peer-review.
It has one author.

Pre-registered on: 01/09/2025 05:07 AM (PT)

1) Have any data been collected for this study already?
No, no data have been collected for this study yet.

2) What's the main question being asked or hypothesis being tested in this study?
This study investigates whether structuring payoffs to favor less likely outcomes increases public demand for government projects.

Participants are presented with three hypothetical scenarios, each including the government's estimated probabilities of the projects' success. In each scenario, half of the participants (control group) are shown an estimated project cost
. The other half (treatment group) are shown the same expected cost, assuming the government's probability estimates are accurate, but with a modified structure that includes a bonus payout if the project fails. The core hypothesis is that if participants perceive the likelihood of success to be lower than the government's estimates, they will prefer a payout structure that offers greater rewards in the event of project failure.

3) Describe the key dependent variable(s) specifying how they will be measured.
Three binary variables takings value 1 if respondents support the government project and 0 otherwise.

4) How many and which conditions will participants be assigned to?
For each of the three scenarios, participants are randomly assigned to either the treatment or control group.

5) Specify exactly which analyses you will conduct to examine the main question/hypothesis.
An ordinary least squares (OLS) regression is used, where the independent variable is the binary treatment indicator and the dependent variable is as described above. The primary specification estimates the overall treatment effect for the entire sample. Additionally, I will examine heterogeneous effects based on participants' subjective probabilities of project failure. The assumption is that participants who align with the government's probability estimates are likely to exhibit a smaller treatment effect.

6) Describe exactly how outliers will be defined and handled, and your precise rule(s) for excluding observations.
All observations that pass the attention check will be included in the analysis. Since the main variables are binary, there is no concern about the presence of outliers.

7) How many observations will be collected or what will determine sample size? No need to justify decision, but be precise about exactly how the number will be determined.
The sample size is set at 1,500 participants, representative of the U.S. population in terms of age, sex, and political affiliation. Data collection will close on January 16th at 23:59 CET, unless the target number of participants is reached earlier.

8) Anything else you would like to pre-register? (e.g., secondary analyses, variables collected for exploratory purposes, unusual analyses planned?)
No

F.3 Experiment 3

Norway state-specific energy support. (#206366)

Author(s)
This pre-registration is currently anonymous to enable blind peer-review.
It has one author.

Pre-registered on: 01/03/2025 07:17 AM (PT)

1) Have any data been collected for this study already?
No, no data have been collected for this study yet.

2) What's the main question being asked or hypothesis being tested in this study?
This is a brief follow-up to #184328. The focus is on exploring whether support for clean energy exchange in Europe can be enhanced by offering individuals a payoff in the (unlikely) scenarios where electricity prices become very high. The core mechanism relies on leveraging the disparity between market-assessed probabilities and individual perceptions. Specifically, by providing a payout in scenarios that markets deem unlikely but individuals perceive as more likely, it may be possible to increase policy support by aligning payouts with states that people overestimate the likelihood of occurring.

3) Describe the key dependent variable(s) specifying how they will be measured.
A binary variable of whether respondents prefer energy exchange or not.

4) How many and which conditions will participants be assigned to?
Half of the respondents are additionally informed that the energy exchange includes a personal payoff of NOK 1,000 per household, which will be distributed on days when prices are extremely high.

5) Specify exactly which analyses you will conduct to examine the main question/hypothesis.
OLS regression of the treatment on support for energy exchange.

6) Describe exactly how outliers will be defined and handled, and your precise rule(s) for excluding observations.
All respondents passing the attention check will be included.

7) How many observations will be collected or what will determine sample size? No need to justify decision, but be precise about exactly how the number will be determined.
The data collection will close on January 12th at 23:59 CET. With approximately 300 individuals in the potential sample active on Prolific, I anticipate receiving no more than 100–200 responses.

8) Anything else you would like to pre-register? (e.g., secondary analyses, variables collected for exploratory purposes, unusual analyses planned?)
Nothing else to pre-register.

F.4 Experiment 4

State-specific payouts and greenwashing disagreement (#240576)

Author(s)
This pre-registration is currently anonymous to enable blind peer-review.
It has one author.

Pre-registered on: 2025/07/30 - 06:24 AM (PT)

1) Have any data been collected for this study already?
No, no data have been collected for this study yet.

2) What's the main question being asked or hypothesis being tested in this study?
The study examines whether a firm can increase consumer support for an ESG-oriented product by offering a state-specific refund that pays out only if the ESG goal is not met.

3) Describe the key dependent variable(s) specifying how they will be measured.
Primary variable: a 5-point Likert scale variable asking about the likelihood of buying the product. Two further outcomes measure future buying intentions and whether the company takes all customers seriously.

4) How many and which conditions will participants be assigned to?
Participants are randomly assigned to one of two groups:
- Control – standard Rainforest Safe bar at a fixed price.
- Treatment – higher-priced bar bundled with a $5 refund if independent monitors later confirm the deforestation target was not achieved.

Assignment is 50/50.

5) Specify exactly which analyses you will conduct to examine the main question/hypothesis.
The main test is an OLS regression of the purchase decision on a treatment dummy, with robust standard errors. I will explore heterogeneity by (i) the respondent's stated probability that the company will succeed and (ii) their prior belief that green product claims are mostly greenwashing.

6) Describe exactly how outliers will be defined and handled, and your precise rule(s) for excluding observations.
No outliers passing the attention check will be excluded.

7) How many observations will be collected or what will determine sample size? No need to justify decision, but be precise about exactly how the number will be determined.
1000.

8) Anything else you would like to pre-register? (e.g., secondary analyses, variables collected for exploratory purposes, unusual analyses planned?)
No

F.5 Experiment 5

State specific payouts: Ethical consideration (#234386)

Author(s)
This pre-registration is currently anonymous to enable blind peer-review.
It has one author.

Pre-registered on: 2025/06/19 - 07:14 AM (PT)

1) Have any data been collected for this study already?
No, no data have been collected for this study yet.

2) What's the main question being asked or hypothesis being tested in this study?
This is an exploratory descriptive survey (no experimental manipulations). Its sole aim is to document public perceptions of the appropriateness of using state-specific bonuses across different policy domains and under various safeguard designs.

3) Describe the key dependent variable(s) specifying how they will be measured.
For each vignette, respondents rate the appropriateness of using state-specific bonuses on a 5-point Likert scale (1 = Not appropriate at all, 5 = Very appropriate).

4) How many and which conditions will participants be assigned to?
None. All participants complete the same set of survey questions.

5) Specify exactly which analyses you will conduct to examine the main question/hypothesis.
I will report descriptive statistics for each policy domain and safeguard condition.

6) Describe exactly how outliers will be defined and handled, and your precise rule(s) for excluding observations.
All respondents who pass the pre-registered attention check will be retained. No additional outlier exclusions will be applied.

7) How many observations will be collected or what will determine sample size? No need to justify decision, but be precise about exactly how the number will be determined.
I will recruit 500 Prolific participants from a panel matched to U.S. census benchmarks. Based on prior experience, I anticipate >90 % will pass the attention check, yielding more than 450 analysable responses.

8) Anything else you would like to pre-register? (e.g., secondary analyses, variables collected for exploratory purposes, unusual analyses planned?)
No

F.6 Experiment 6

ASPREDICTED

Wharton CREDIBILITY LAB
UNIVERSITY of PENNSYLVANIA

State-specific: Republican vs Democrat (#236094)

Author(s)
This pre-registration is currently anonymous to enable blind peer-review.
It has one author.

Pre-registered on: 2025/06/30 - 11:51 AM (PT)

1) Have any data been collected for this study already?
No, no data have been collected for this study yet.

2) What's the main question being asked or hypothesis being tested in this study?
I test whether a state-specific bonus is more persuasive when it is presented by a voter's favoured party versus a non-favoured party.
To identify this, I replicate two scenarios from preregistration #207101 but vary the source cue: half of treated respondents see the programme proposed by "the Republican Party," the other half by "the Democratic Party."
Before any treatments we administer an affective-polarisation thermometer (in-party vs out-party feeling), allowing heterogeneous-treatment-effect analysis by partisan sentiment.

3) Describe the key dependent variable(s) specifying how they will be measured.
A binary Support variable for the policy (Yes = 1, No = 0) – identical to #207101. Additionally, three 5-point Likert items assessing perceived legitimacy, trust in the proposing party, and likelihood of voting for that party.

4) How many and which conditions will participants be assigned to?
Respondents are randomised into three groups for each of the two scenarios:

State-specific bonus, Republican source

State-specific bonus, Democratic source

Control (flat-cost policy, no bonus)

5) Specify exactly which analyses you will conduct to examine the main question/hypothesis.
I will use OLS regression with robust standard errors without control variables using the experimental treatment as independent variables. I will use the binary support variable and Likert scale questions as dependent variables.

6) Describe exactly how outliers will be defined and handled, and your precise rule(s) for excluding observations.
All respondents who pass the built-in attention check(s) will be retained; no observations will be dropped as outliers.

7) How many observations will be collected or what will determine sample size? No need to justify decision, but be precise about exactly how the number will be determined.
1500

8) Anything else you would like to pre-register? (e.g., secondary analyses, variables collected for exploratory purposes, unusual analyses planned?)
No

F.7 Experiment 7

State Specific: Conspiracy beliefs (#236612)

Author(s)
This pre-registration is currently anonymous to enable blind peer-review.
It has one author.

Pre-registered on: 2025/07/03 - 06:47 AM (PT)

1) Have any data been collected for this study already?
No, no data have been collected for this study yet.

2) What's the main question being asked or hypothesis being tested in this study?
This study examines whether people - especially those who score high on conspiracy thinking (measured with the 5-item CMQ) - behave in line with their stated beliefs when faced with state-specific bonuses linked to President Trump's tariff-revenue target. Concretely we test: (i) Do respondents pick the bonus option that maximises their own subjective expected utility, as revealed by an incentivised probability estimate and a real-money lottery choice? (ii) Is any departure from expected-utility behaviour larger among high-CMQ individuals? In addition, we explore which certifying bodies (government agency, exchange, blockchain oracle, etc.) respondents trust to verify such outcomes and how that trust co-varies with conspiracy thinking.

3) Describe the key dependent variable(s) specifying how they will be measured.
(a) Policy support: binary Yes / No for the tariff package (baseline, flat $90 rebate, or $600 state-specific rebate).
(b) Subjective probability : 0–100 % slider scored with a quadratic (Brier) rule for a cash bonus.
(c) Lottery choice: pick either $60 for sure or $70 if tariff revenue < $250 bn.
(d) Trust ratings: 1–5 Likert for each potential certifying source.
(e) CMQ score: sum of the five 0–100 items divided by 5.

4) How many and which conditions will participants be assigned to?
A 3 × 2 design:
• Bonus arm (random 1/3 each): No bonus / Flat $90 / $600 state-specific bonus.
• Information arm (random ½ each): sees market probability (15 %) vs. sees no market information.
Total six cells; every participant completes one cell only.

5) Specify exactly which analyses you will conduct to examine the main question/hypothesis.
The primary analyses will be descriptive and regression-based. We will:

• compute means for each dependent variable by treatment cell;
• estimate simple OLS regressions of policy support, probability reports, and lottery choice on (i) treatment indicators and (ii) the CMQ conspiracy-belief index (constructed as the average of the five items, following Bruder et al. 2013), including interaction terms to see whether treatment effects vary with CMQ;

6) Describe exactly how outliers will be defined and handled, and your precise rule(s) for excluding observations.
No outliers passing the attention check will be excluded

7) How many observations will be collected or what will determine sample size? No need to justify decision, but be precise about exactly how the number will be determined.
1000 (excluding those not passing the attention check)

8) Anything else you would like to pre-register? (e.g., secondary analyses, variables collected for exploratory purposes, unusual analyses planned?)
No

F.8 Experiment 8

ASPREDICTED

Wharton CREDIBILITY LAB
UNIVERSITY of PENNSYLVANIA

State-specific payouts: Governments vs private firms (#235538)

Author(s)
This pre-registration is currently anonymous to enable blind peer-review.
It has one author.

Pre-registered on: 2025/06/26 - 07:03 AM (PT)

1) Have any data been collected for this study already?
No, no data have been collected for this study yet.

2) What's the main question being asked or hypothesis being tested in this study?
This study examines how people perceive state-specific bonus schemes when they are offered by governments versus private firms, and how explicit profit motives shape those perceptions. Specifically, I ask whether it is acceptable for a private company to offer bonuses on events whose probabilities people tend to overestimate when the company's goal is to increase profits.

Two experimental scenarios will be run.
• Scenario 1 presents the same bonus scheme as coming from (a) a government agency, (b) a private firm, or (c) a private firm that explicitly states it is profit-motivated.
• Scenario 2 elicits hypothetical demand for a product under three conditions: (a) a standard offer (control), (b) a one-sided, state-specific bonus tied to an improbable event, and (c) the same as (b) but with an explicit profit motive.

3) Describe the key dependent variable(s) specifying how they will be measured.
Scenario 1: Three Likert-scale items measure whether the scheme is viewed as (a) appropriate, (b) legitimate, and (c) trustworthy. An open-ended question gathers participants' immediate reactions.
Scenario 2: Three Likert-scale items measure (a) likelihood of purchase, (b) perceived appropriateness, and (c) interest in future products. An open-ended question records any additional comments.

4) How many and which conditions will participants be assigned to?
Scenario 1: Three treatments-government, private, and private with explicit profit motivation.
Scenario 2: Three treatments-control, one-sided bonus, and one-sided bonus with explicit profit motivation.

5) Specify exactly which analyses you will conduct to examine the main question/hypothesis.
For each dependent variable we will estimate ordinary least squares regressions with robust standard errors.
• Scenario 1 comparisons: (a) government vs. private; (b) private vs. private with explicit profit motive.
• Scenario 2 comparisons: (a) control vs. one-sided bonus; (b) one-sided bonus vs. one-sided bonus with explicit profit motive.

6) Describe exactly how outliers will be defined and handled, and your precise rule(s) for excluding observations.
All observations that pass the attention check will be retained; no statistical outliers will be removed.

7) How many observations will be collected or what will determine sample size? No need to justify decision, but be precise about exactly how the number will be determined.
I will survey 1,000 respondents selected to be broadly representative of the U.S. population on key demographic characteristics.

8) Anything else you would like to pre-register? (e.g., secondary analyses, variables collected for exploratory purposes, unusual analyses planned?)
No

F.9 Experiment 9

State specific: Internal vailidty (#234521)

Author(s)
This pre-registration is currently anonymous to enable blind peer-review.
It has one author.

Pre-registered on: 2025/06/20 - 01:58 AM (PT)

1) Have any data been collected for this study already?
No, no data have been collected for this study yet.

2) What's the main question being asked or hypothesis being tested in this study?
The original study (#207101) found that belief-aligned state-specific payouts raised support for public policies.This follow-up tests whether that effect could be explained by framing rather than belief alignment.
We examine two potential framing channels:
*H1 (Gain-frame) – Simply adding a "lottery-style" bonus (same expected value, unrelated event) boosts support.
*H2 (Cost-signal) – Stating a higher headline cost ($90 vs $50 or $275 vs $150) increases perceived scope/quality.

3) Describe the key dependent variable(s) specifying how they will be measured.
Each respondent will answer two outcome questions after reading their assigned vignette.
*Policy support – a binary yes/no item: "Would you support this program?"
*Perceived scope/quality
These two measures are collected separately for the wildfire-prevention vignette and for the congestion-pricing vignette.

4) How many and which conditions will participants be assigned to?
For each vignette respondents are randomly assigned to one of four arms:
*Control-LowCost – baseline version stating the lower cost only ($50 for wildfire, $150 for congestion) with no bonus.
*Control-HighCost – identical description except the higher cost is shown ($90 or $275) and no bonus is offered. This isolates any effect of the headline cost.
*Placebo Bonus – the higher cost ($90 or $275) plus a state-specific bonus that pays $400/$500 if an unrelated event with a 10 % probability occurs (e.g., rain in San Francisco on a specific date). This tests whether a generic lottery frame drives support.
*Belief-Aligned Bonus – the higher cost ($90 or $275) plus a state-specific bonus of the same size that pays only if the program itself fails. This replicates the original belief-alignment mechanism.

5) Specify exactly which analyses you will conduct to examine the main question/hypothesis.
The main analysis testing the "gain-framing" hypothesis compares the effects of the Belief-Aligned Bonus and the Placebo Bonus treatments to the Control-LowCost group.
To do so, I will conduct two parallel analyses:
*One comparing the Belief-Aligned Bonus treatment to the Control-LowCost group (i.e., the key comparison from the main experiment).
*One comparing the Placebo Bonus treatment to the Control-LowCost group.
In both analyses, I will examine heterogeneous effects based on subjective beliefs about policy success. Specifically, I will split the sample into two subgroups depending on whether the respondent's subjective probability of success is lower than the government's stated probability.
I expect that the Belief-Aligned Bonus will have a stronger effect among respondents who are skeptical (i.e., assign low subjective probability of success), while the Placebo Bonus should not display this pattern.

To assess whether cost framing alone influences outcomes, I will compare the Control-HighCost group to the Control-LowCost group, focusing on both dependent variables.

All analyses will use OLS regression with robust standard errors and no additional control variables.

6) Describe exactly how outliers will be defined and handled, and your precise rule(s) for excluding observations.
No outliers will be excluded and all respondents passing attention check will be kept in the final sample.

7) How many observations will be collected or what will determine sample size? No need to justify decision, but be precise about exactly how the number will be determined.
I aim for 1000 respondents (pre attention check), representative of US population according to Prolific.

8) Anything else you would like to pre-register? (e.g., secondary analyses, variables collected for exploratory purposes, unusual analyses planned?)
No

G Experiment 1: Survey instructions

This section provides parts of the instructions for the survey. Since there are various treatments and each group of respondents receives slightly different WTP questions, it is not feasible to include every version of every question here. Instead, I have included key treatment variations along with other core questions. The full survey is available at the following link: `https://nhh.eu.qualtrics.com/jfe/form/SV_2rjQAbzdb9HKQSy`

G.1 Stage 0

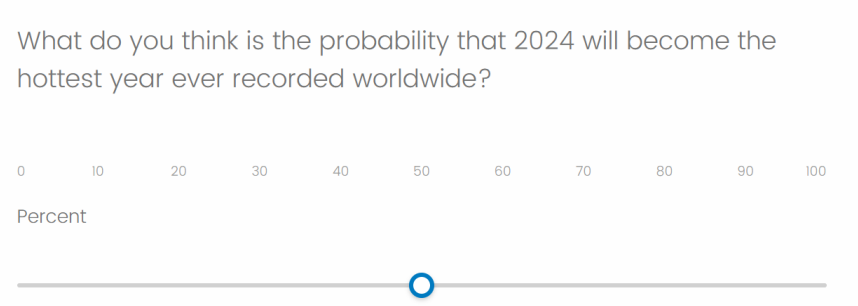

Figure A9: Subjective probability that 2024 will be the hottest year on record

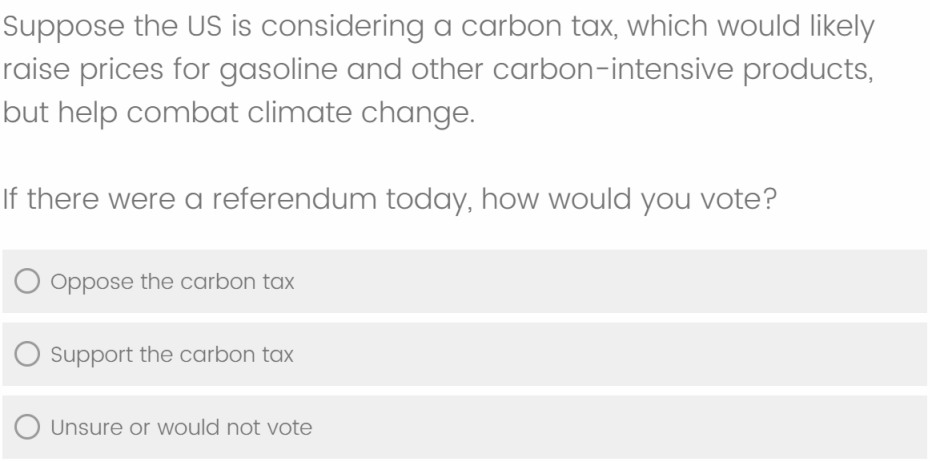

Figure A10: Support for a carbon tax in the United States

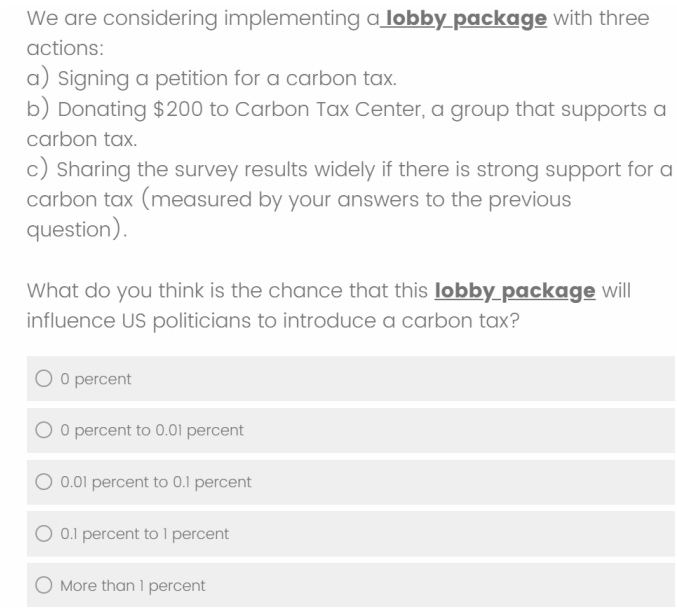

Figure A11: Lobby package: description and likelihood of influencing policy

G.2 Stage 1

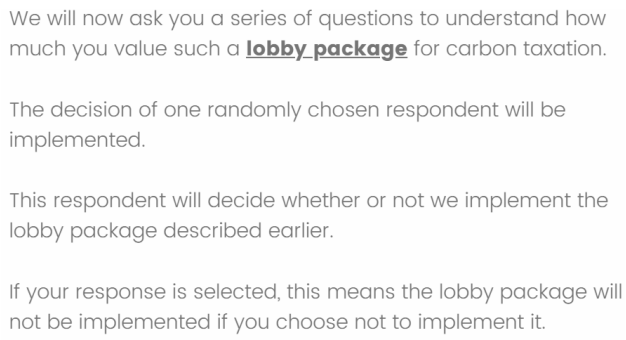

Figure A12: Overview of valuation for the lobby package

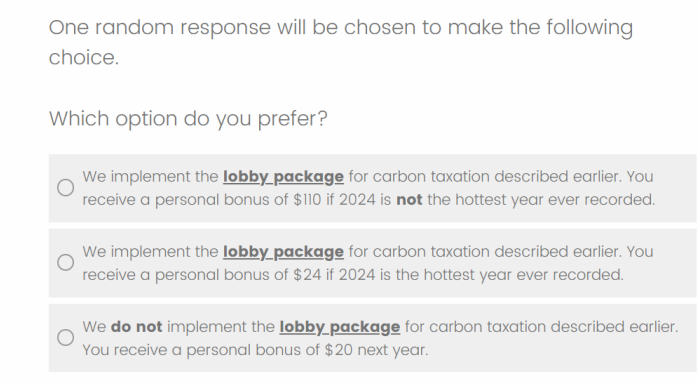

Figure A13: Initial valuation question (for **State-specific treatment** respondents only)

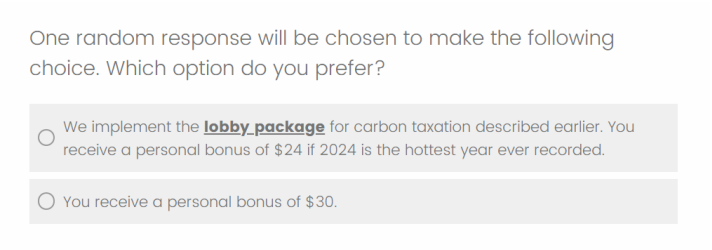

Figure A14: Valuation question after selecting initial preferences for the lobby package and payout choice if 2024 is the hottest year on record (for **State-specific treatment** respondents only). The remainder of the valuation algorithm is outlined in Appendix Figure A2.

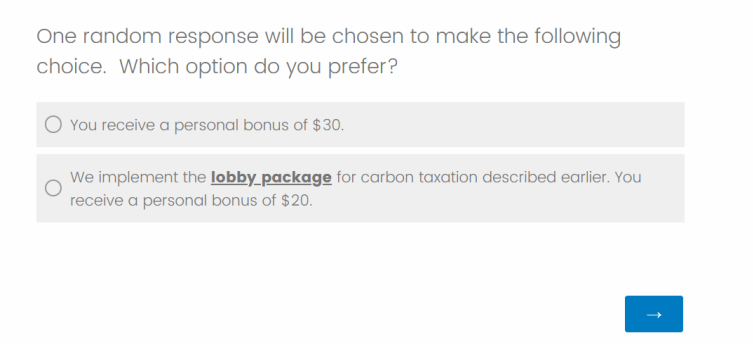

Figure A15: Valuation question following initial preferences for the lobby package (for respondents *not* in the **State-specific treatment**)

G.3 Stage 2

Figure A16: Information treatment on market probabilities (provided to a random half of respondents)

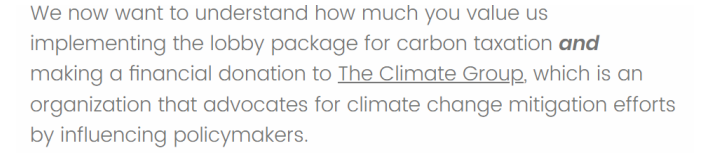

Figure A17: Introduction to eliciting WTP for a bundle of climate change mitigation and carbon tax (provided only to **Climate** treatment group)

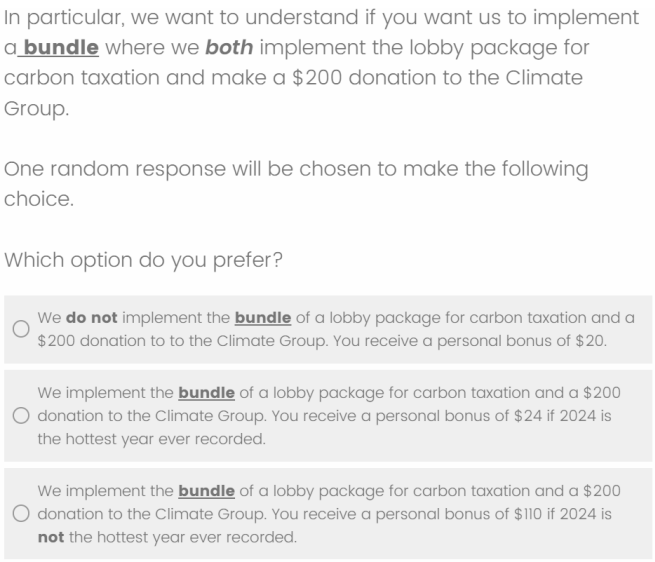

Figure A18: Initial WTP question for the climate change mitigation and carbon tax bundle (for **Climate** and **State-specific** treatment groups)

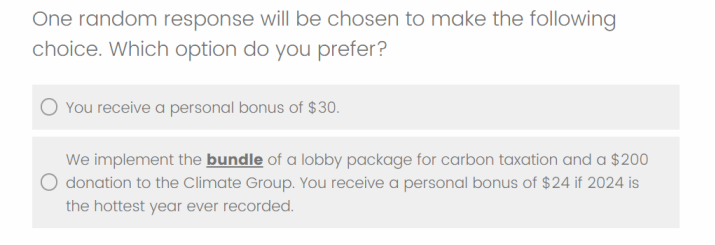

Figure A19: Valuation question following initial preferences for the lobby package and payout selection if 2024 is the hottest year on record (for **Climate** and **State-specific** treatment groups). The rest of the valuation algorithm is described in Appendix Figure A2.

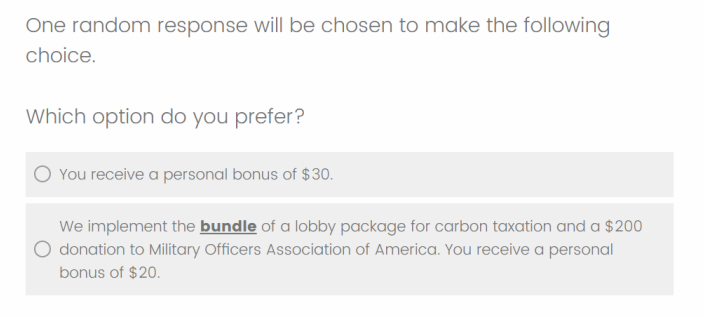

Figure A20: Valuation question after initial preferences for the lobby package (for **Military** group respondents *not* in the **State-specific** treatment).

G.4 Stage 3

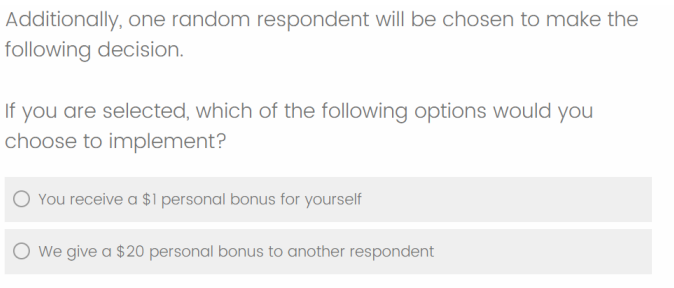

Figure A21: State-specific gifts (**Control group**)

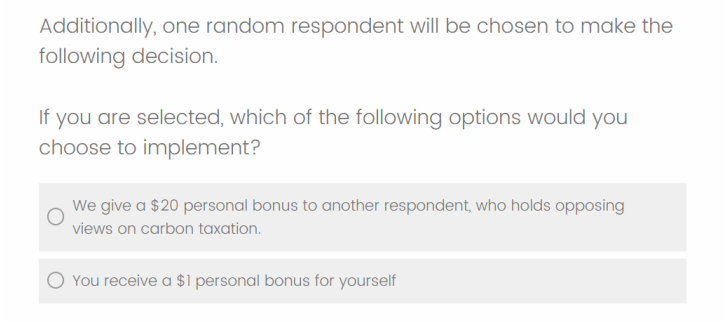

Figure A22: State-specific gifts (**Control** and recipient holds different views on carbon taxation)

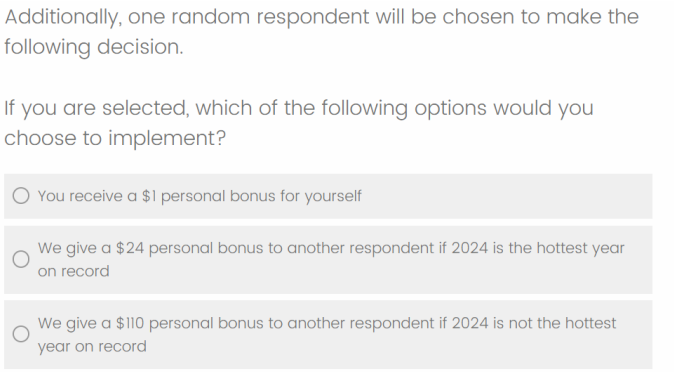

Figure A23: State-specific gifts (**Treatment group**)

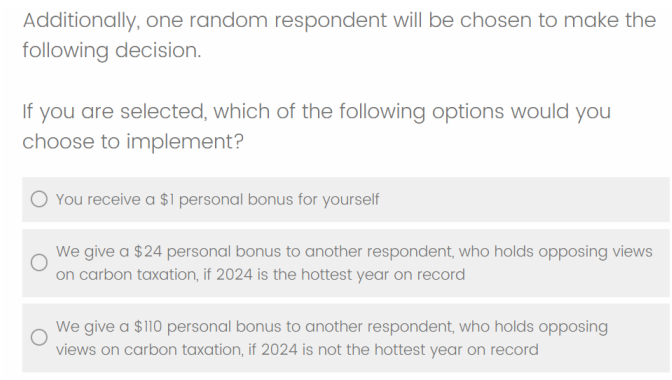

Figure A24: State-specific gifts (**Treatment** and recipient holds different views on carbon taxation)

H Experiment 2 and 3: Survey instructions

H.1 Experiment 2: Instructions

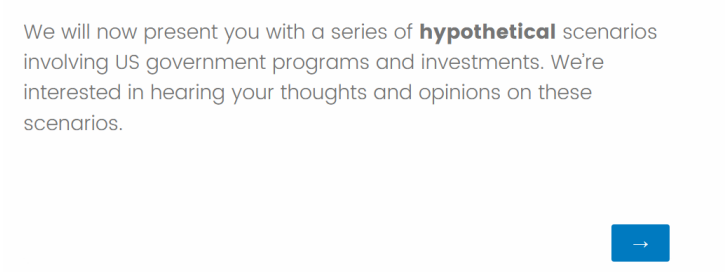

Figure A25: Introduction

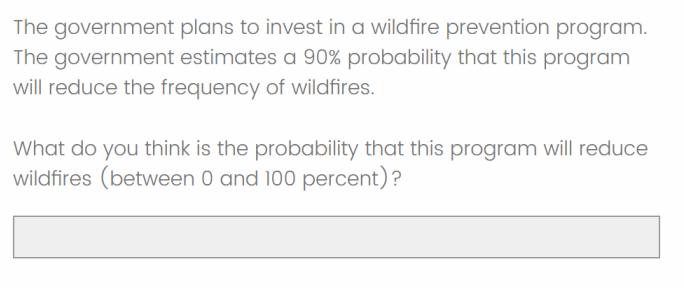

Figure A26: Beliefs about success of wildfire prevention program

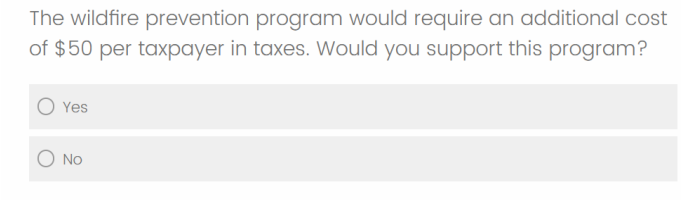

Figure A27: Support for wildfire prevention (control)

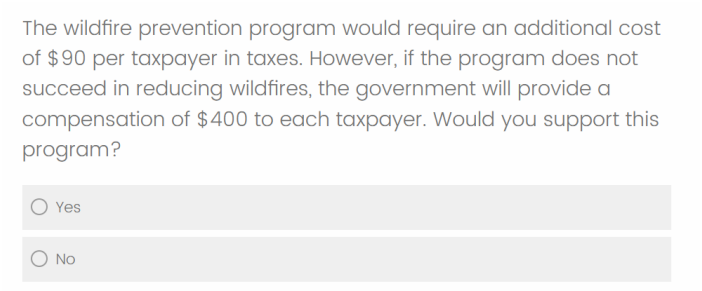

Figure A28: Support for wildfire prevention (state-specific treatment)

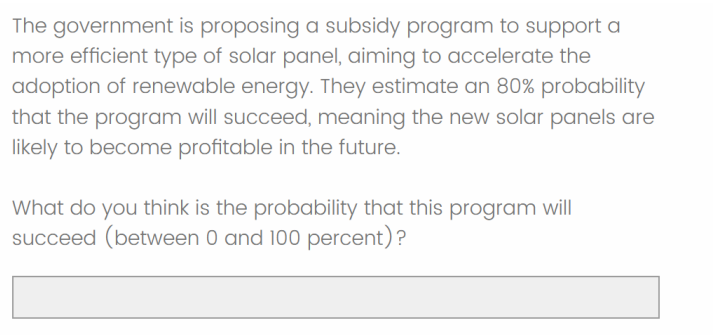

Figure A29: Beliefs about success of solar energy subsidies

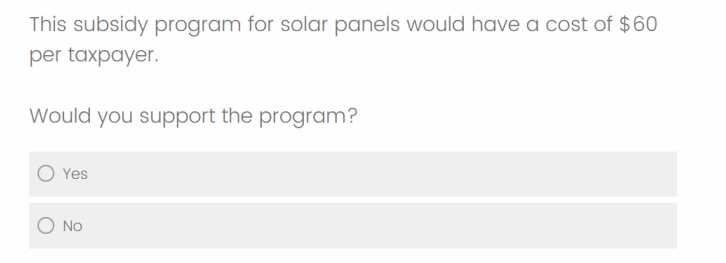

Figure A30: Support for solar energy subsidies (control)

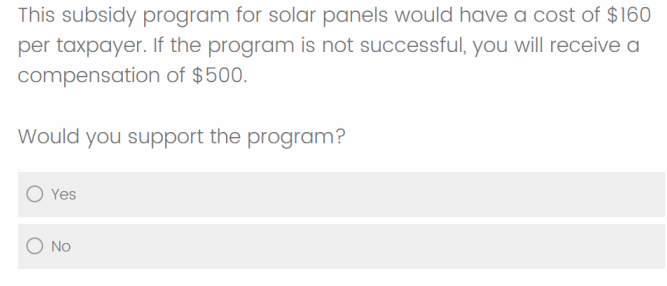

Figure A31: Support for solar energy subsidies (state-specific treatment)

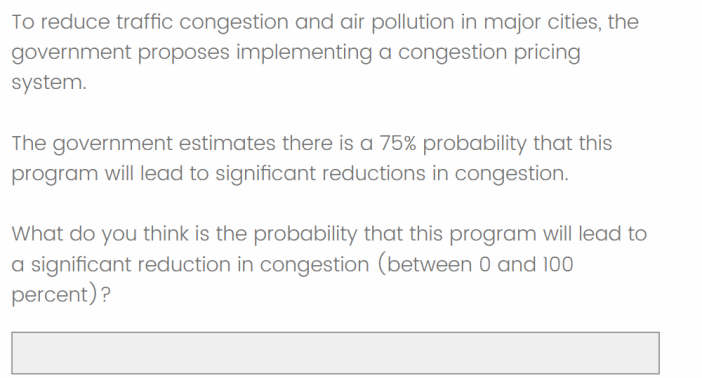

Figure A32: Beliefs about success of congestion pricing

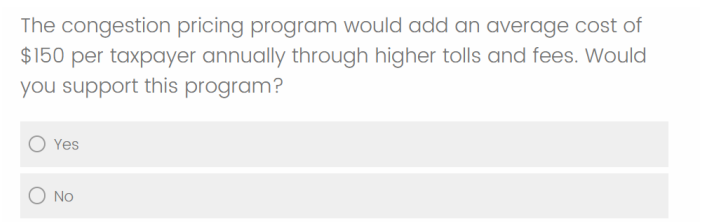

Figure A33: Support for congestion pricing (control)

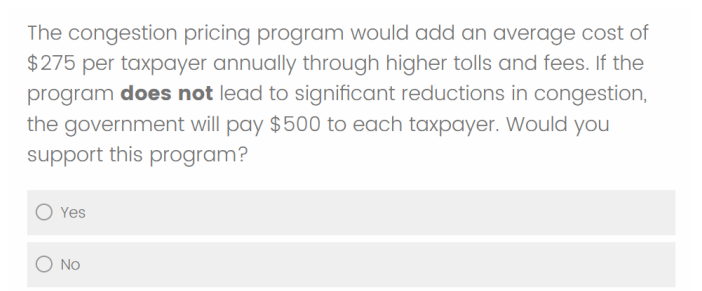

Figure A34: Support for congestion pricing (state-specific treatment)

H.2 Experiment 3: Instructions

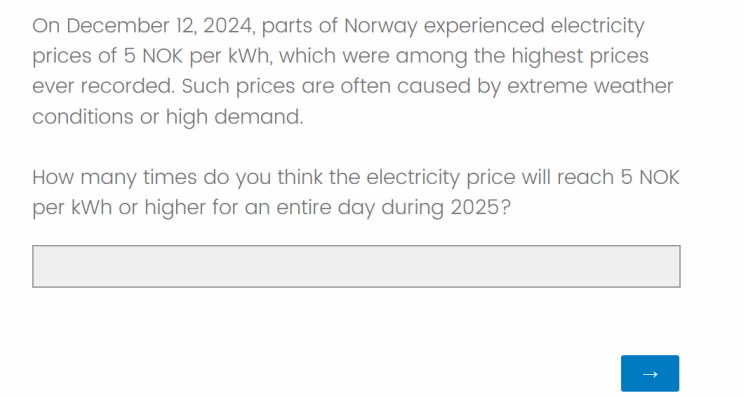

Figure A35: Beliefs about electricity prices

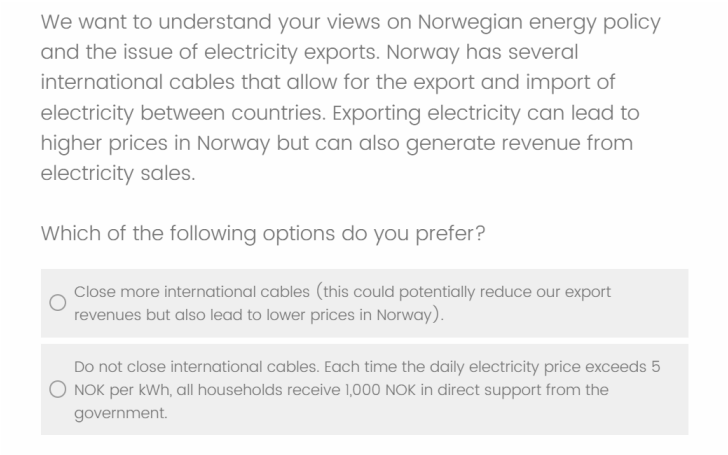

Figure A36: Support for electricity integration (state-specific treatment)

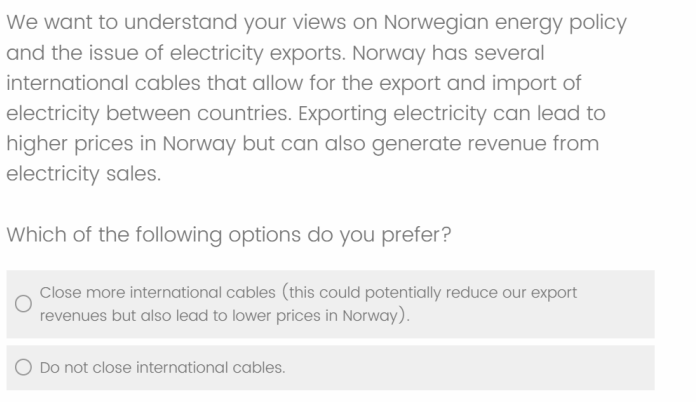

Figure A37: Support for electricity integration (control group)

I Experiment 4: Instructions

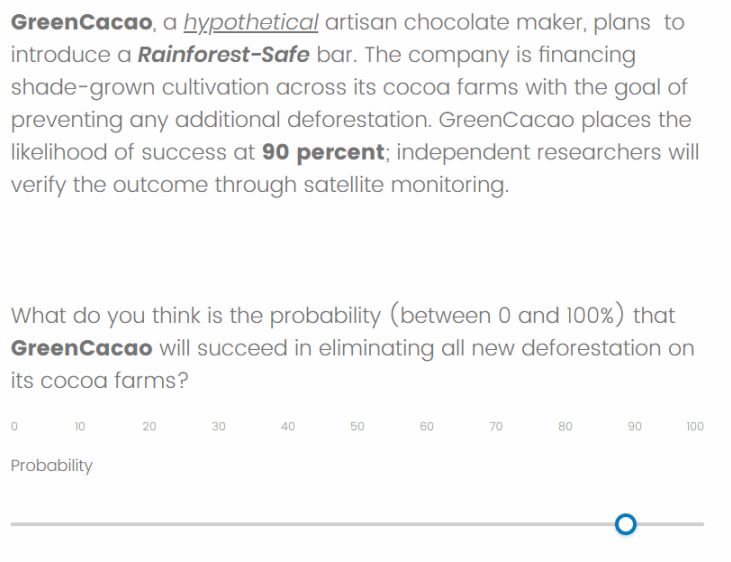

Figure A38: Probability of success

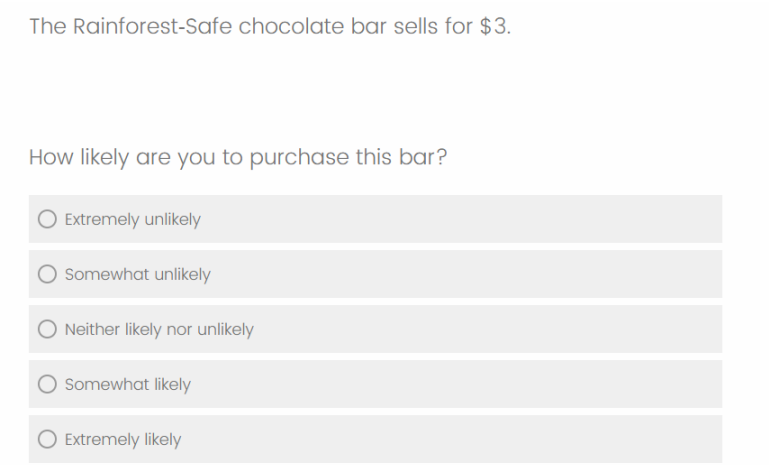

Figure A39: Purchase Likelihood: Control

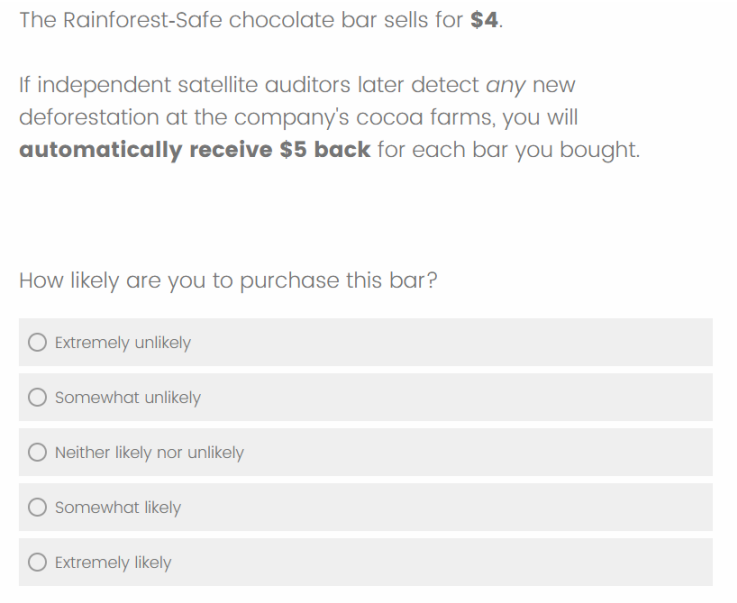

Figure A40: Purchase Likelihood: Space-specific Treatment

Figure A41: Perceived Inclusiveness and Future Purchase Likelihood

J Experimental instructions (Experiment 5 to 9)

J.1 Experiment 5: Instructions

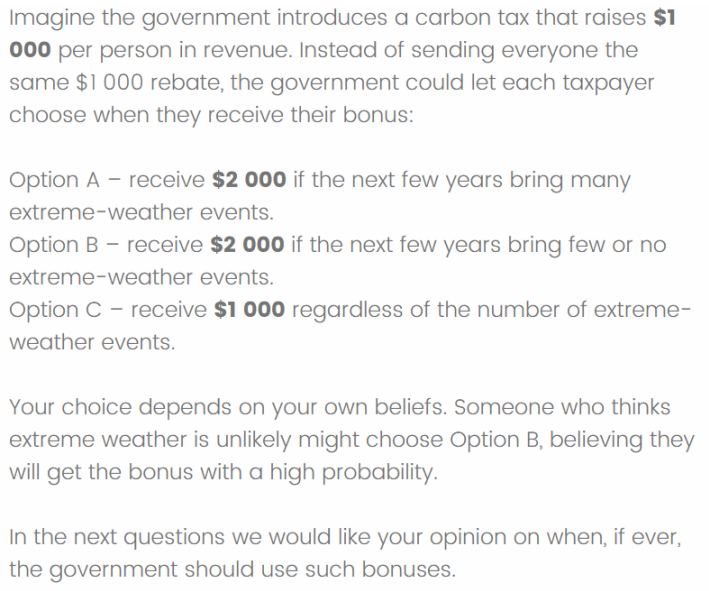

Figure A42: Introduction to carbon tax scenario

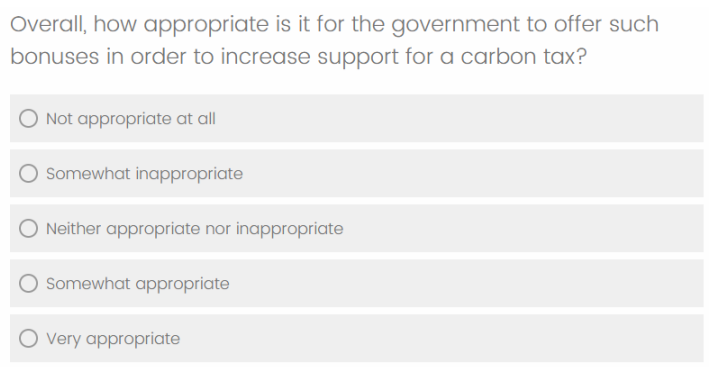

Figure A43: Appropriateness: Carbon tax

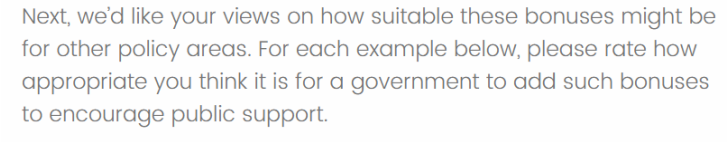

Figure A44: Transition

Some voters doubt that extra spending on wildfire prevention really reduces fires. **As part of the wildfire prevention package,** the government could offer a state-specific bonus that pays each taxpayer a bonus if fires do not decrease after the program. Because skeptics believe fires are unlikely to decline, they would expect to receive this bonus with a higher probability.

How appropriate do you think it is for governments to use such bonuses to increase support for wildfire-prevention programs?

- ◯ Not appropriate at all
- ◯ Somewhat inappropriate
- ◯ Neither appropriate nor inappropriate
- ◯ Somewhat appropriate
- ◯ Very appropriate

Figure A45: Appropriateness: Wildfire Prevention

A minority believe a new vaccine will not reduce illness. **As part of the vaccination program**, the government could pay each taxpayer a payment if illness rates do not fall. Those who distrust the vaccine would expect this payment to be more likely.

How appropriate do you think it is for governments to use such bonuses to increase support for vaccination programs?

- ◯ Not appropriate at all
- ◯ Somewhat inappropriate
- ◯ Neither appropriate nor inappropriate
- ◯ Somewhat appropriate
- ◯ Very appropriate

Figure A46: Appropriateness: Vaccination Program

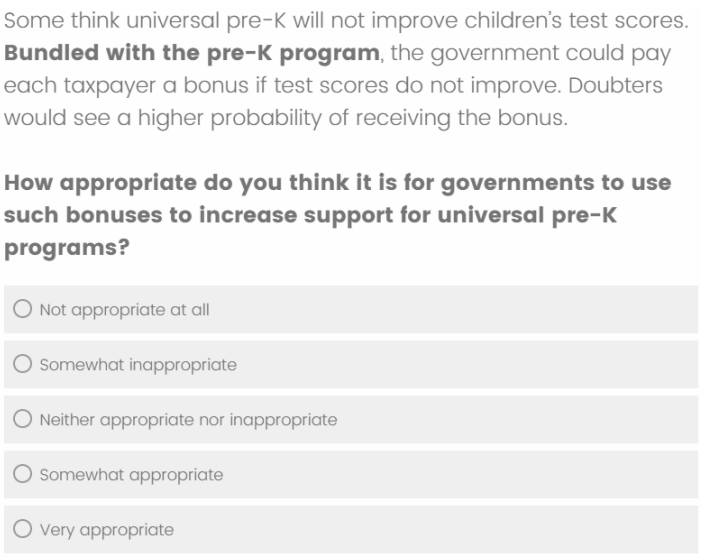

Figure A47: Appropriateness: Universal Pre-K

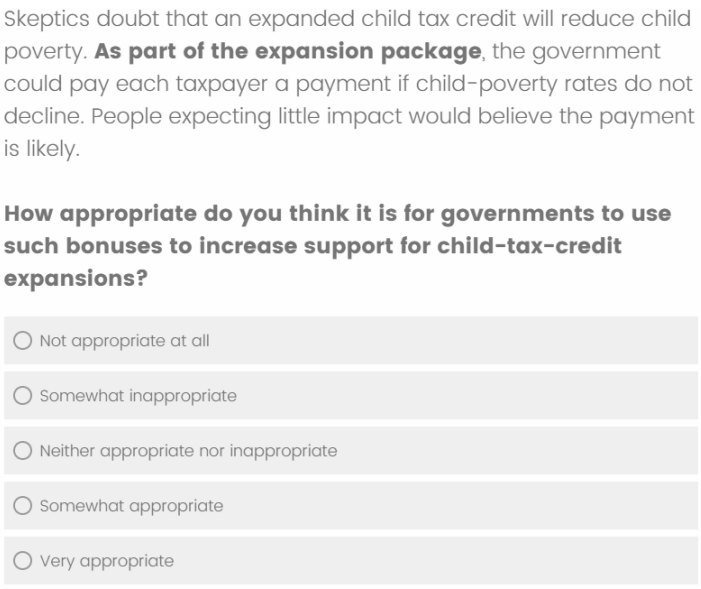

Figure A48: Appropriateness: Child Tax Credit Expansion

Figure A49: Appropriateness: Trade Liberalization

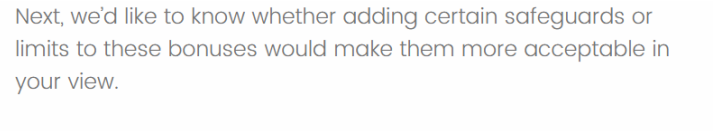

Figure A50: Transition

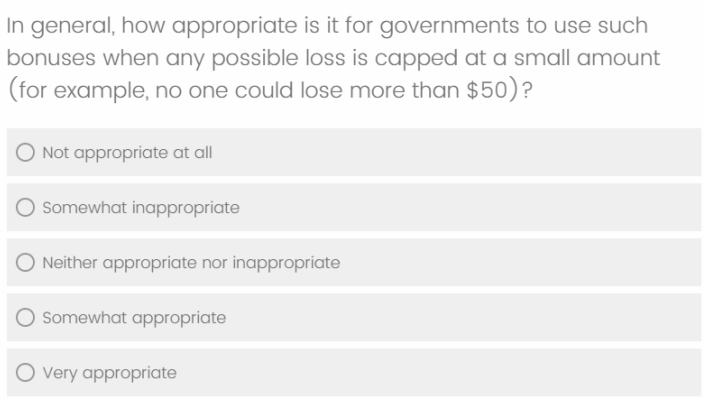

Figure A51: Appropriateness: Loss Cap

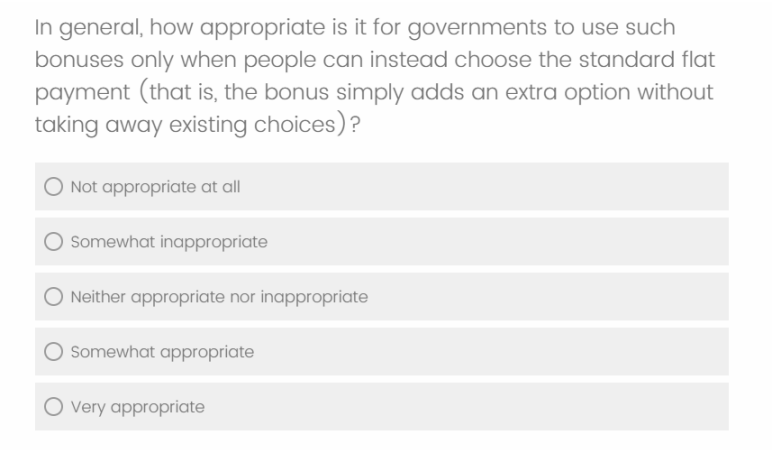

Figure A52: Appropriateness: Option Extension

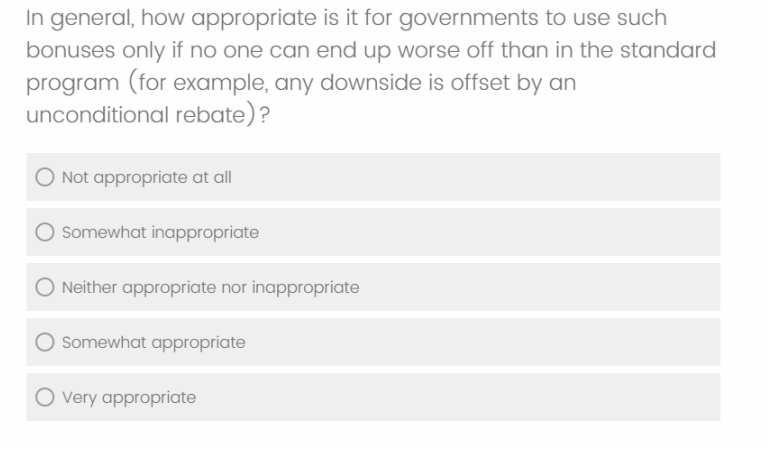

Figure A53: Appropriateness: No One Worse Off

Figure A54: Appropriateness: Major Social Problem

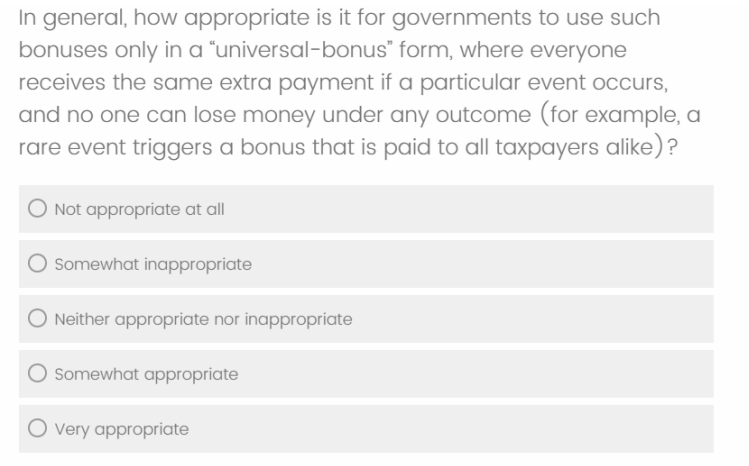

Figure A55: Appropriateness: Universal Bonus

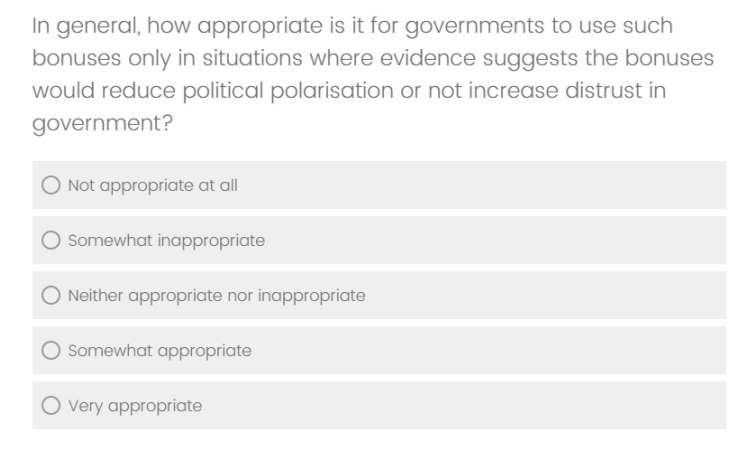

Figure A56: Appropriateness: Reduce Polarization

J.2 Experiment 6: Instructions

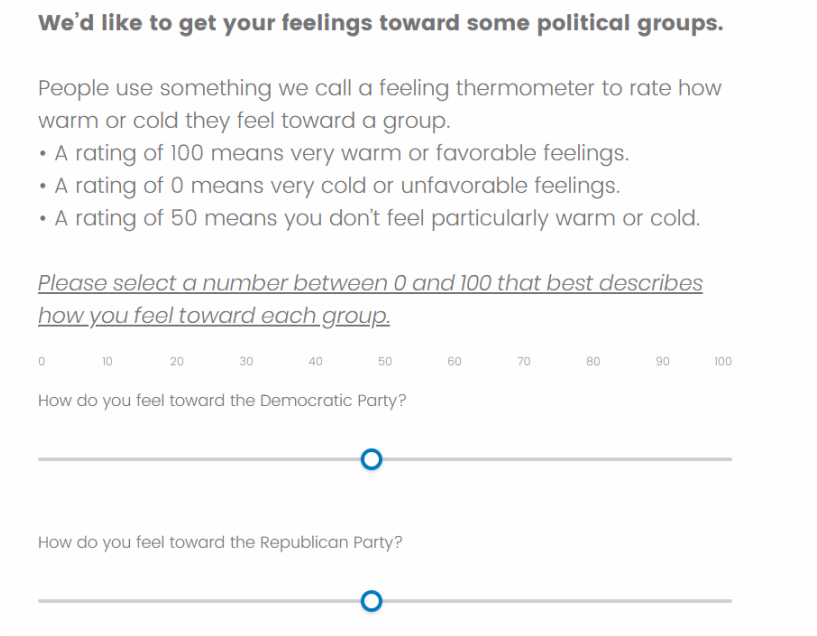

Figure A57: Affective Polarization (AP)

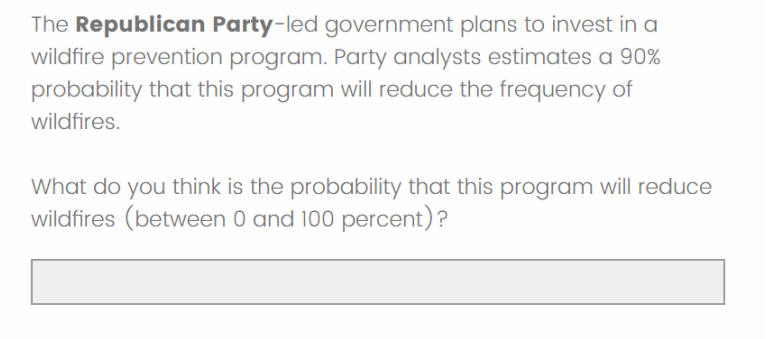

Figure A58: Beliefs about success of wildfire prevention program

Note: Half respondents see "Democratic Party" rather than "Republican Party"

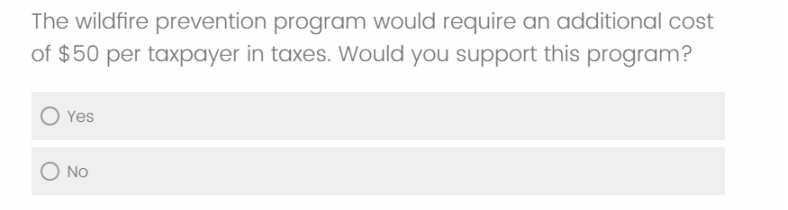

Figure A59: Support for wildfire prevention (control)

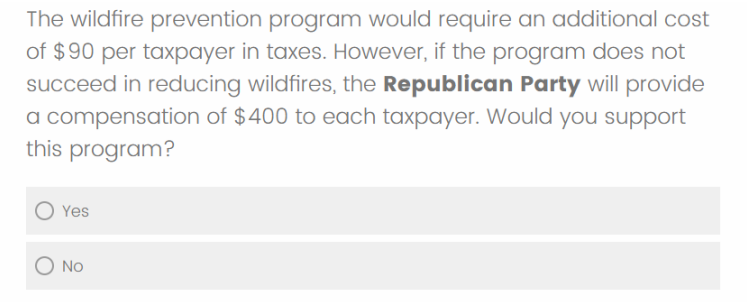

Figure A60: Support for wildfire prevention (state-specific treatment)

Note: Half respondents see "Democratic Party" rather than "Republican Party"

Figure A61: Trustworthiness question

Note: Half respondents see "Democratic Party" rather than "Republican Party"

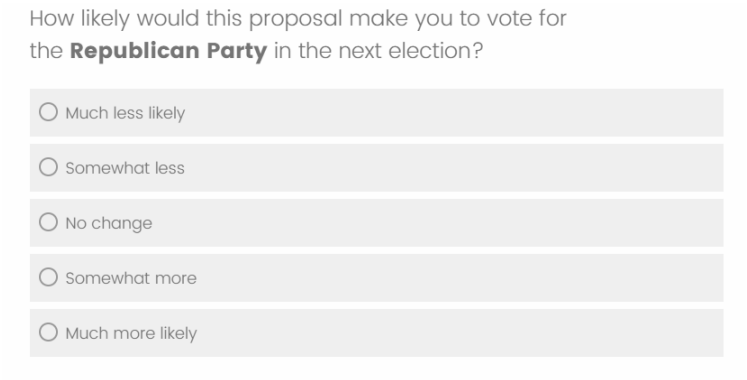

Figure A62: Voting question

Note: Half respondents see "Democratic Party" rather than "Republican Party"

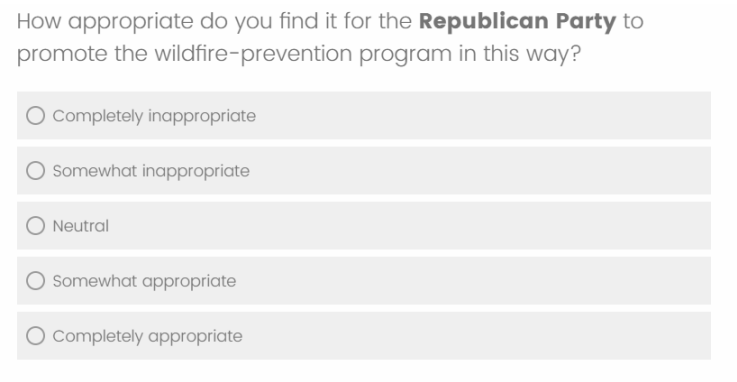

Figure A63: Appropriateness question

Note: Half respondents see "Democratic Party" rather than "Republican Party"

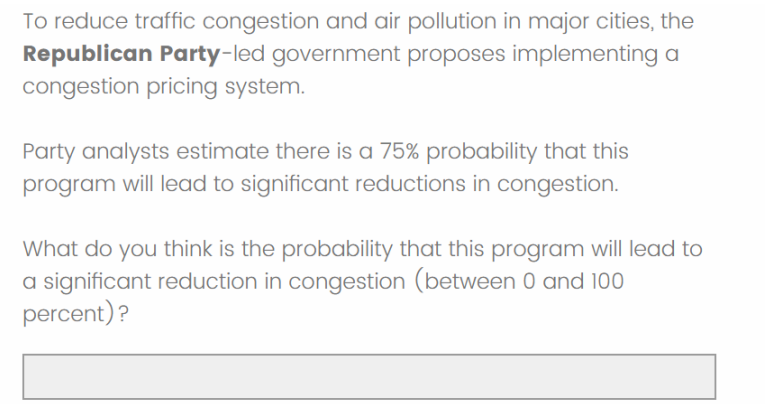

Figure A64: Beliefs about success of congestion pricing

Note: Half respondents see "Democratic Party" rather than "Republican Party"

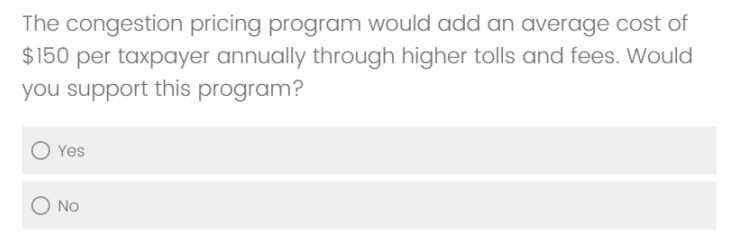

Figure A65: Support for congestion pricing (control)

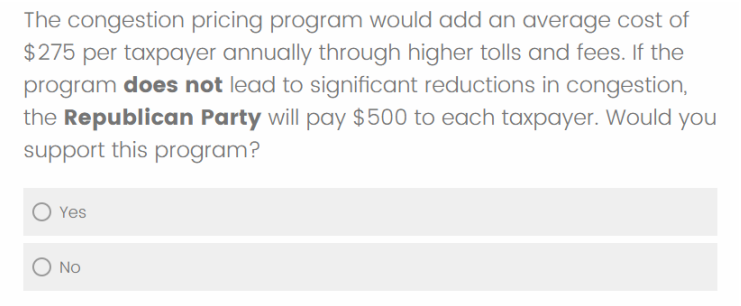

Figure A66: Support for congestion pricing (state-specific treatment)

Note: Half respondents see "Democratic Party" rather than "Republican Party". The following trustworthiness, voting, and appropriateness questions were presented in the same format as in the wildfire prevention scenario, with only the scenario name changed to “congestion pricing.”

J.3 Experiment 7: Instructions

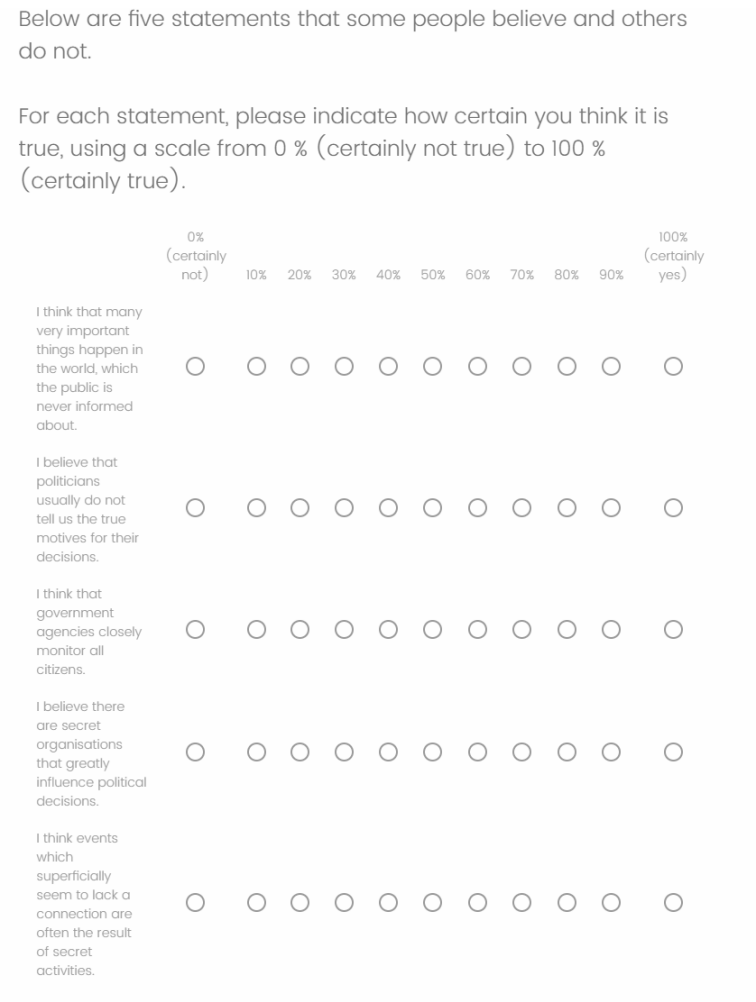

Figure A67: Conspiracy Mentality Questionnaire (Bruder et al. 2013)

Note: Only the experimental instructions for the parts covered in the paper are included here.

When a bonus depends on whether a specific event occurs, someone has to certify the outcome. Below are different organisations that could perform this role.

Please indicate how much you would trust each source to measure and report the outcome accurately.

	Not all	Slightly	Moderately	Very	Completely
A U.S. government statistics agency (e.g., Census Bureau, Bureau of Economic Analysis)	○	○	○	○	○
Major national news outlets reporting the result (e.g., AP, Reuters)	○	○	○	○	○
A regulated financial exchange (e.g., Kalshi, Chicago Mercantile Exchange)	○	○	○	○	○
A blockchain oracle service that records the result on-chain (e.g., Chainlink)	○	○	○	○	○
A large audit/accounting firm (e.g., Deloitte, PwC)	○	○	○	○	○
An international body (e.g., United Nations, World Meteorological Organization)	○	○	○	○	○
An independent academic research consortium (university scientists)	○	○	○	○	○
A prediction-market platform on the blockchain (e.g., Polymarket)	○	○	○	○	○

Figure A68: Trust in different verifying sources

J.4 Experiment 8: Instructions

The Norwegian government proposes proposes investments in grid integration. Experts expect the country to gain overall, but short-run electricity prices may rise. Many citizens worry about frequent "extreme-price days".

Analysts estimate such extreme prices will be **very rare**.

To address this concern, the government could bundle grid integration with a **bonus:** every household would receive 100 USD for each "extreme-price day". Because extreme days are unlikely, the programme should cost little while making the reform more attractive to sceptics.

Figure A69: Government Treatment

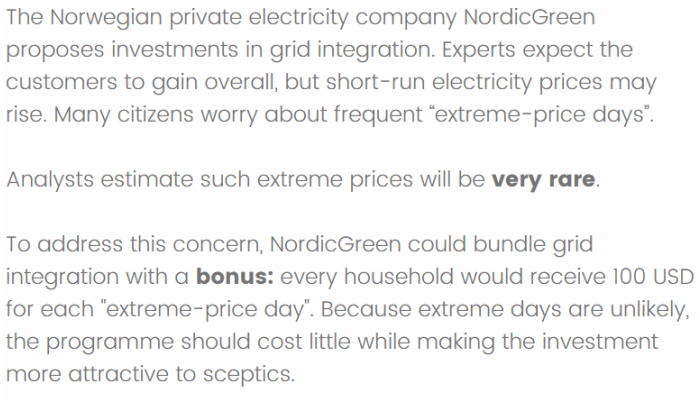

Figure A70: Private Treatment

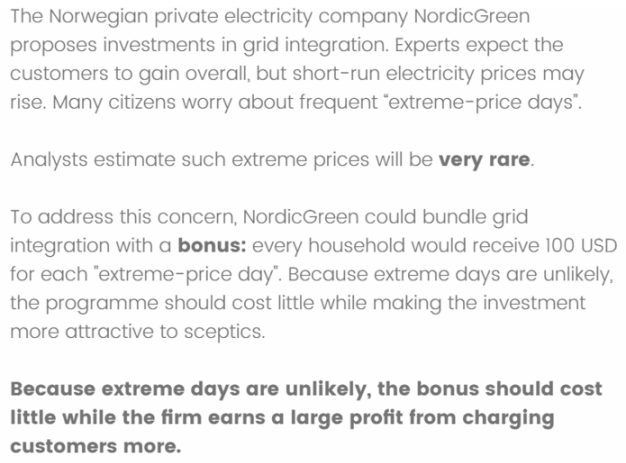

Figure A71: Profit-motive Treatment

Figure A72: Appropriateness outcome

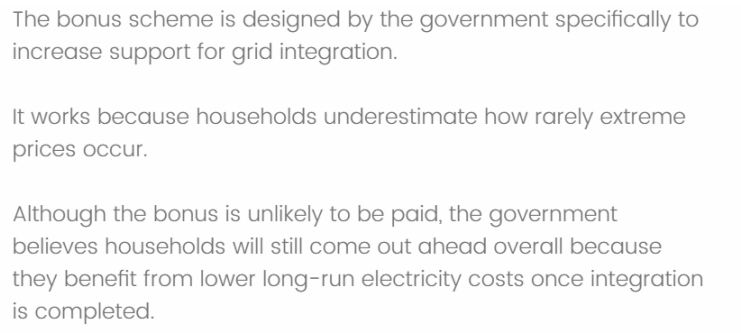

Figure A73: Detailed explanation (for Government Treatment)

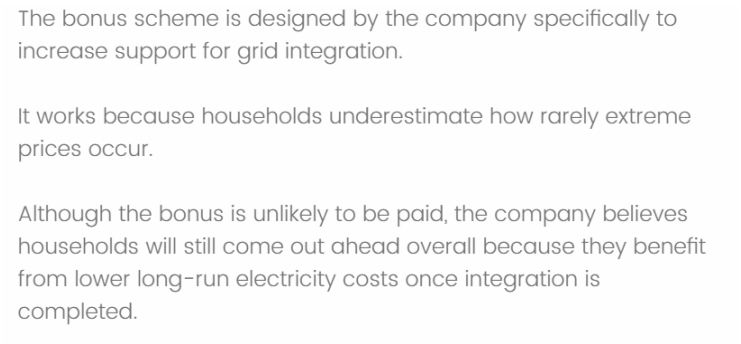

Figure A74: Detailed explanation (for Private Treatment)

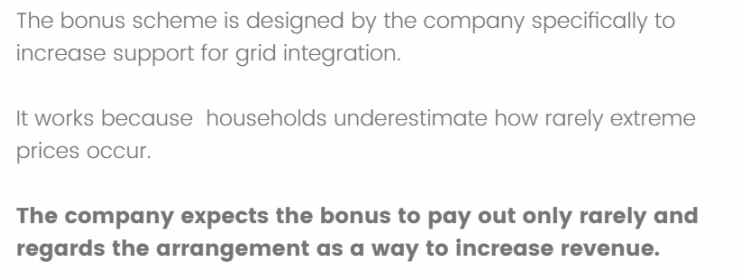

Figure A75: Detailed explanation (for Profit-motive Treatment)

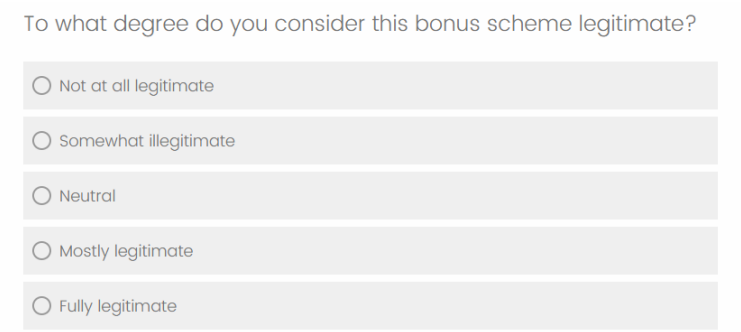

Figure A76: Legitimacy outcome

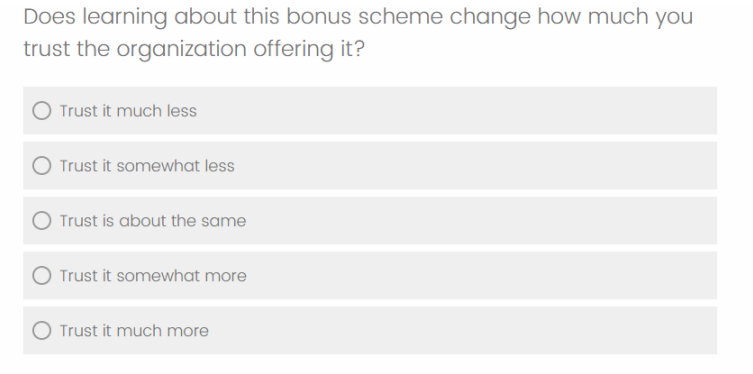

Figure A77: Trustworthiness outcome

J.5 Experiment 9: Instructions

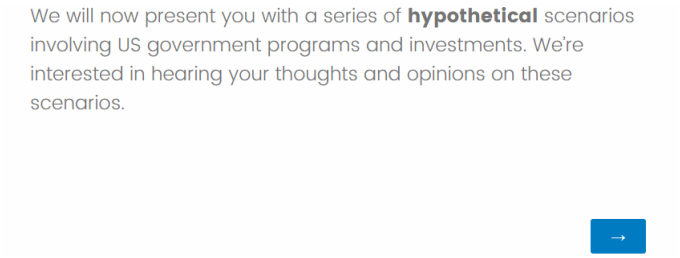

Figure A78: Introduction

J.5.1 Wildfire prevention scenario

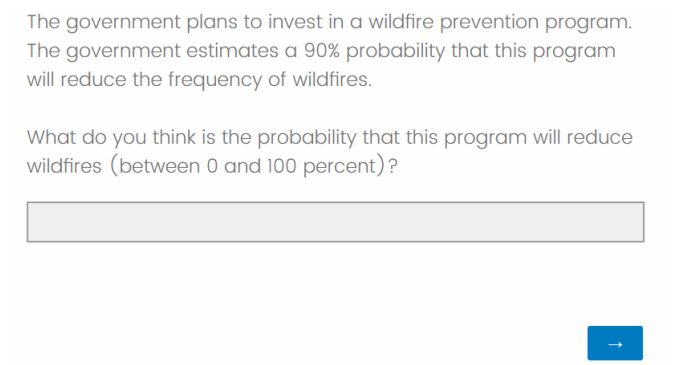

Figure A79: Beliefs about success (identical to Experiment 2)

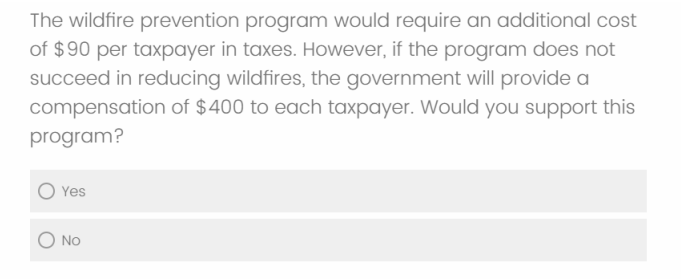

Figure A80: State-specific Treatment (identical to Experiment 2)

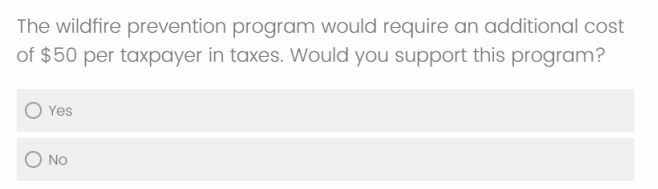

Figure A81: Control treatment (identical to Experiment 2)

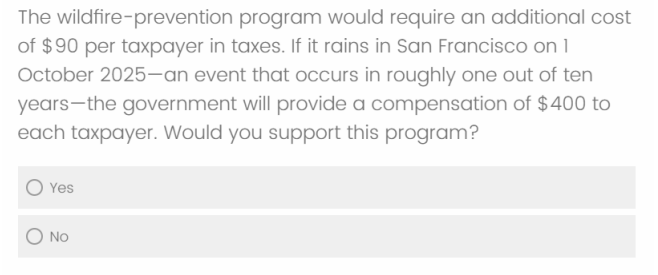

Figure A82: Placebo bonus treatment

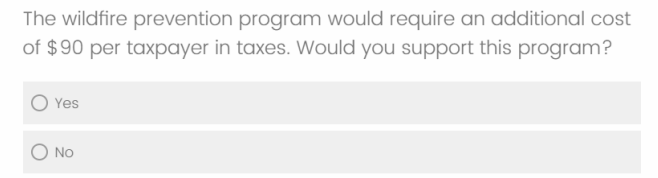

Figure A83: High-Cost treatment

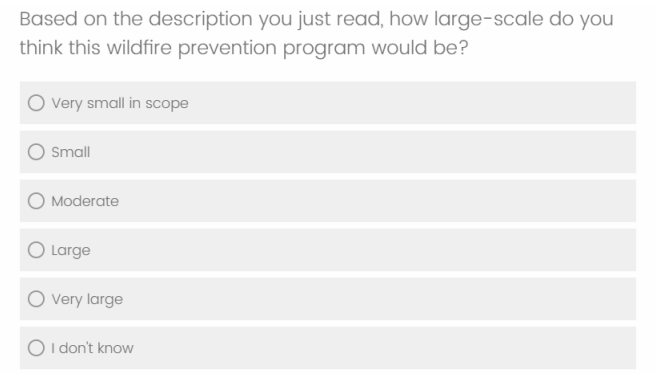

Figure A84: Scope of program

J.5.2 Congestion pricing scenario

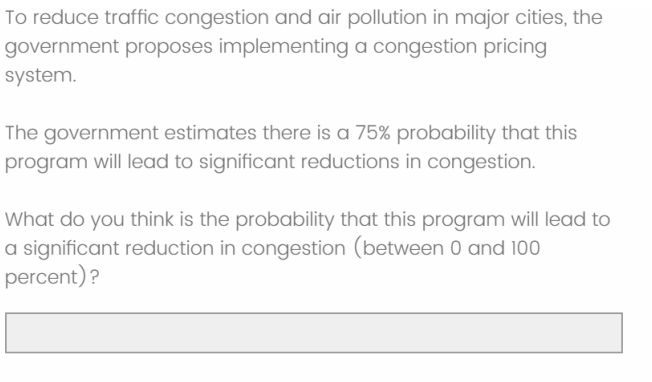

Figure A85: Probability of success (identical to Experiment 2)

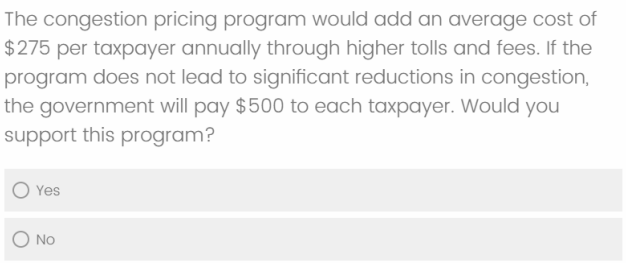

Figure A86: State-specific treatment (identical to Experiment 2)

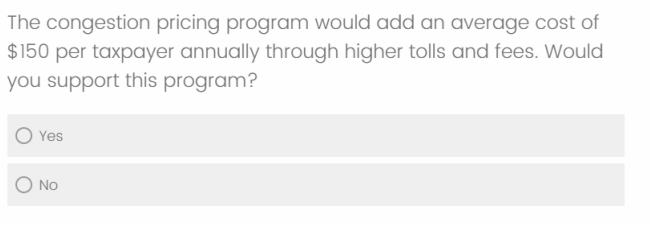

Figure A87: Control treatment (identical to Experiment 2)

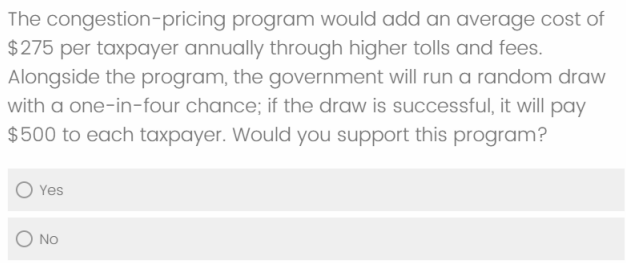

Figure A88: Placebo bonus treatment

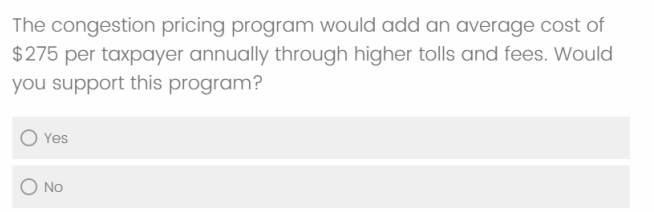

Figure A89: High-Cost treatment

Figure A90: Scale of program