MORAL HAZARDS AND GEOENGINEERING: EVIDENCE FROM A LARGE-SCALE ONLINE EXPERIMENT

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Geoengineering (especially solar radiation management) may help to reduce the negative outcomes of climate change by minimising or reversing global warming. However, many express the worry that geoengineering may pose a moral hazard, i.e., that information about geoengineering may lead to a reduction in climate change mitigation efforts. In this paper, we report a large-scale pre-registered, money-incentivised, online experiment with a representative US sample (N=2500). We compare actual behaviour (donations to climate change charities and clicks on climate change petition links) as well as stated preferences (support for a carbon tax and self-reported intentions to reduce emissions) between participants who receive information about geoengineering with two control groups (a salience control that shows information about climate change generally and a content control that shows information about a different topic). Behavioural choices are made with an earned endowment, and stated preference responses are incentivised via the Bayesian Truth Serum. We fail to find a significant impact of receiving information about geoengineering, and based on equivalence tests, we provide evidence in favour of the absence of such an effect. We take this to provide evidence for the claim that there is no moral hazard in this context.

I. Introduction[1]

Climate change continues to pose serious challenges to societies across the globe as the international community fails to adequately address its root causes. Aside from mitigation strategies aimed at reducing greenhouse gas emissions, geoengineering approaches are increasingly being considered. These are intentional interventions in the climate system with the aim of minimising, reducing, or reversing the damaging effects of climate change. A prominent example of geoengineering is solar radiation management (SRM), which attempts to reflect back or otherwise neutralise a fraction of sunlight. This can be achieved via marine cloud brightening (Latham et al. 2012) or stratospheric interventions (Hulme 2012), among others. What SRM methods have in common is reducing ground-level solar radiation in a way that some believe could relatively cheaply and easily reduce short-term global warming. However, such options come with technical downside risks and do not address the root cause or other chemical effects of greenhouse gas emissions (Mahajan, Tingley, & Wagner 2019).

However, even if SRM would work as hoped, risks remain. One such risk has been called "moral hazard" (Gardiner 2017; Svoboda 2017; Hale 2012), which has been called a "prominent challenge" to geoengineering (Pamplany et al., 2020). Moral hazard, as Baker (1996) discusses, refers to the "tendency for insurance against loss to reduce incentives to prevent or minimize the cost of loss" (239), i.e., moral hazard refers to the effect by which individuals' incentives regarding some behaviour are altered if the majority of the downside risk is borne by others, e.g., insurers. For instance, if a property is insured against fire, the property owners may

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be less likely to take the necessary steps to further reduce fire risks as the majority of the risk is borne by the insurance company. This effect has been studied before in contexts such as health insurance (Zweifel & Manning 2000), worker's compensation coverage (Butler & Worrall 1991), natural disasters (Hudson, Botzen, Czajkowski, & Kreibich 2017), crop insurance (Quiggin, Karagiannis, & Stanton 1993), and bank deposits (Martin 2006). Lin (2013) proposes that the model of moral hazard may also apply to geoengineering: They analogise the insurance policy that is typically at the heart of a moral hazard to geoengineering research, the insurer to the government that supports this research, and the insured to the public. On Lin's account, the government's decision to engage in geoengineering research is perceived to help protect the public from the consequences of climate change, who may then reduce motivation or behaviour to reduce emissions. As Lin puts it, "geoengineering is analogous to insurance in that geoengineering may cause behaviors and policy preferences to shift in a manner that creates additional risk" (Lin 2013, 689).

While the moral hazard objection has received significant attention (e.g., Pamplany et al.'s (2020) recent review found 33 papers on the topic) and is plausibly a central concern in the social science literature, empirical work on this context remains relatively sparse. Further empirical evidence could help indicate whether or to what extent moral hazard is generated by introducing geoengineering in general (and SRM in particular) to the public, which is largely unaware that there are alternatives to conventional mitigation (Mahajan, Tingley & Wagner, 2019). If individuals were apprised of an alternative they were unaware of, this could reduce both stated support for climate measures and personal behaviour, e.g. because this lessens the perceived threat of climate change (Campbell-Arvai et al., 2017), or because it weakens resolve (Austin & Converse, 2021). Thus, it could vindicate and support the importance of the moral hazard concern if there was an effect on stated preferences or behaviour. In contrast, it could lessen the importance of moral hazard if no effect on stated preferences or behaviour was found.

Overall, the results presented in the literature so far are mixed.[2] In line with the theoretical predictions outlined by Lin (2013), Raimi, Maki, Dana, & Vandenbergh (2019) find that reading about geoengineering leads to a reduction in mitigation support in a US sample (irrespective of the framing of the problem), though the magnitude and significance of the effects varied depending on the description of SRM. Contrary to this finding, however, Cherry, Kallbekken, Kroll, & McEvoy (2021) find that information about SRM leads to an increase in support for a national carbon tax. This is corroborated by a similar result in a German sample by Merk, Pönitzsch, & Rehdanz (2016), who find that reading about SRM increases willingness to invest in mitigation. Further, Fairbrother (2016) finds no effect on the receiving of an introduction to SRM on the willingness to pay taxes. Lastly, in a climate disaster game, Andrews, Delton & Kline (2022) find no moral hazard amongst "citizens", but that "policymakers" somewhat anticipate that "citizens" *will* be subject to moral hazard.

We believe that the present results are inconclusive and that this is, at least in part, because of some methodological choices made in the literature. Specifically, we identify three areas of potential methodological improvement for research that our paper aims to improve upon. First, we believe that it is important in experiments like this that the research purpose is not overt to the study participants to minimise the threat of experimenter demand and self-selection effects. To achieve this in this present study, we presented all participants with several texts, quizzes, and hypothetical and actual choice scenarios relating to

[2] While we lack the space to be comprehensive, the experimental literature on carbon dioxide removal shows similarly mixed results (Campbell-Arvai et al., 2017; Austin & Coverse, 2021). As we do, Hart et al. (2022) control for salience in the case of carbon dioxide removal and, as we do with SRM, find no moral hazard effect.

various topics to ensure they were unaware of the target of this study. Second, we claim that in order to properly identify the effect of geoengineering on stated preferences and behaviour (in either direction), one has to control not only for the content of the intervention, but also for salience (of the topic of climate change generally). To test for this specifically, we include a salience control condition, in which participants are presented with a text about climate change generally (with no mention of geoengineering). Third, coming back to Lin (2013), we argue that it is crucial that any research in this area focuses on both stated preferences and behaviours, not just stated preferences. This is because leaving out behaviour reduces external validity in a context in which laboratory experiments already have to deal with substantial criticism relating to external validity, and where this criticism is exceedingly relevant to public policy recommendations. To address this worry, this present study is the first to also introduce several behavioural measures in its design, while also incentivising the stated preference measures via an incentive-compatible mechanism aimed at incentivising honest reporting of subjective data: the Bayesian Truth Serum (Prelec 2004).

Our experimental design relies on the following mechanism of moral hazard: While participants are plausibly quite familiar with climate change and standard mitigation techniques (like those discussed in Salience Control), the SRM treatment text should still provide novel information or, at least, bring to the fore a topic they had not considered for a while or had not considered to this extent. The treatment text outlines the potential efficacy of SRM approaches, and, consistent with previous literature that has used the same type of set-up, we argue that participants may react to this knowledge treatment with behaviour akin to a moral hazard. Importantly, we understand that our main treatment is couched in a lot of additional texts, a trade-off of our design, but we argue that this is exactly the type of situation that citizens may find themselves in when they discover SRM. Specifically, they may come across the topic for the first time only in a short text that is presented alongside other texts and media. This is why we hope that our research design allows for the estimation of an effect that is as close as possible to the type of effect expected in the ‘real world’.

We acknowledge that our methodological approaches come with trade-offs. For example, by focusing on keeping the study purpose opaque, we may bias our results towards the null. Further, as our main treatment text is quite short and our intervention thus quite subtle, this may again bias the results towards a null. However, given the methodological choices made by the majority of other work in the literature, we argue that our design choices, even accounting for the consequent downsides, are justified in producing a well-diversified set of empirical results that, jointly, may be able to inform public policy.

The two main pre-registered null hypotheses that we pre-registered are:

Null Hypothesis I: Information about geoengineering does not reduce (or increase) policy support for mitigation measures.

Null Hypothesis II: Information about geoengineering does not reduce (or increase) behaviours related to mitigation measures.

We understand the following patterns of data as providing the attendant evidence in favour (or against) the existence of a moral hazard in the context of geoengineering: Strong evidence in favour of the existence of a moral hazard effect involves the rejection of both null hypotheses. Weak evidence in favour of the existence of a moral hazard effect involves the rejection of only one of the two null hypotheses. Failing to reject both null hypotheses, observation of an increase, or finding evidence in favour of a null (with equivalence tests) will be taken as evidence against the existence of a moral hazard effect.

Our results indicate a failure to reject either null hypothesis while also providing evidence in favour of a null effect via exploratory equivalence tests and

Bayesian analyses. As such, our conclusion is that our study provides evidence for the claim that there is no moral hazard.

II. Methods

Participants

We pre-registered this study on the Open Science Framework,[3] and it has received ethics approval.[4] For this study, we recruited 2500 participants via Prolific that were representative of the US population with regards to age, sex, and ethnicity based on US Census Bureau data. We collected these data between March 5, 2022 and March 12, 2022 on Prolific. Participants received a participation reward of £1.25.[5] During the experiment, they could earn an endowment of up to £0.45 depending on their choices. To calculate the sample size, we conducted an a priori power analysis via G*Power. In order to have .95 power to detect the smallest effect size of interest at the global effect of f^2=.01 with an alpha of .01 (adjusting for multiple comparisons) in a multiple regression model with over 10 predictors, the required total sample size is 1785. We recruit 750 participants per control condition and 1000 in the treatment (totalling 2500), which is within the maximum deliverable representative sample size of 2500 and allows this study to have a high level of power almost regardless of the size of exclusions. To avoid self-selection, we advertised this study as a study regarding current topics and did not emphasise the focus on climate change.

[3] Pre-registration available at the Open Science Framework: https://osf.io/n6vt3/?view_only=3358f4414543401caf79d3331e9240d9.

[4] Ethics approval has been granted by the University Teaching and Research Ethics Committee (UTREC) at the University of St Andrews (SA15687)

[5] All participant rewards on Prolific are denominated in GBP (£). As such, even a US sample like the one we draw on in this paper is well acquainted with this currency and we do not anticipate this to have any impact on our results.

Procedures and Measures

Participants were randomly selected into one of three conditions (Treatment, Salience Control, and Content Control). Thirty percent of participants were randomly assorted into either of the two Control conditions, while the remaining forty percent were selected into the Treatment condition. Our design had two control groups to allow us to control for potential salience effects that mere exposure to a climate change stimulus may bring about.

In order to keep the main research question opaque, all participants were shown texts on three different topics, including texts on abortion and CRISPR in addition to each group's treatment specific texts. All three texts within each group were presented randomly to avoid order effects. After one of the texts, participants were presented with an attention check that instructed them to respond with 'Disagree' on a Likert-scale item asking them how they have enjoyed reading the texts so far. For a visual depiction of which text was shown in which condition and the overall experimental design, cf. Figure 1.

The Treatment condition's specific text was a text introducing geoengineering. This text included both an introductory paragraph on climate change generally and a specific paragraph explaining SRM outlining the potential upsides and risks. As outlined above, this functions as our central intervention and is phrased neutrally to mimic the type of information most likely to be received in a 'real world' context. The Salience Control's specific text was the same text on the topic of standard climate change mitigation techniques as in the Treatment. This paragraph was followed by another paragraph focused exclusively on standard mitigation techniques. This text consisted of plausibly already known material, allowing us to control for the salience of climate change generally. For full treatment texts, see Appendix E. In the Content Control condition, participants were presented with a text on racism.

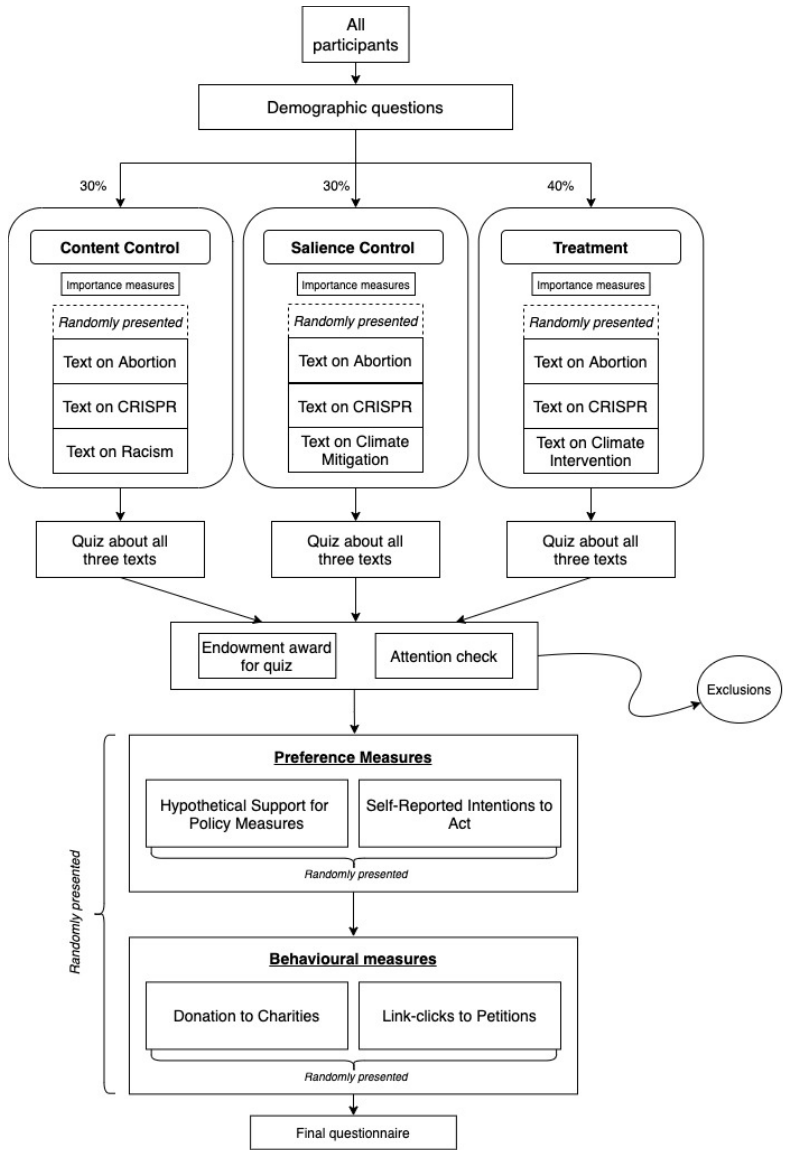

FIGURE 1. EXPERIMENTAL PROCEDURE

Notes: Full experimental procedure, including sample sizes of all conditions and an overview of all measures collected.

Directly under each text, participants were quizzed on the content of the text to ensure that they read the texts carefully and to verify comprehension of the treatment text. Participants received £0.10 for each correctly answered question (for a total of up to £0.30)[6], making up the earnings for their experimental endowment. In case the question was answered incorrectly, participants were told this after answering all three quiz questions and were provided with the text and the same question again, explaining to them that they had answered this question incorrectly while giving them the opportunity to retake the questions. If they answered the question incorrectly again, we coded this as failing the comprehension check and removed them from all analyses. Overall, we excluded all participants who failed either the comprehension check, the attention check, or both.

For each of the topics shown to participants, we collected two types of data: *stated preference measures* and *behavioural measures*. However, our primary variables of interest relate to climate change mitigation preferences and behaviours, and all additional measures (e.g., one's desire to attend a social justice march or one's donation to a global poverty charity) do not enter into our analyses. We randomised the order in which people were presented with questions about stated preference measures and behavioural measures to control for order effects, minimising this potential source of bias. We incentivised behavioural measures by having participants make choices with the previously earned real endowment, thus increasing ecological validity.

Further, we incentivised stated preference measures by applying the Bayesian Truth Serum (Prelec 2004), an incentive-compatible mechanism aimed at incentivising honest reporting of subjective data, which has been adopted in a number of contexts such as energy commodity price forecasts (Zhou et al. 2019). The Bayesian Truth Serum works by asking participants to answer the survey questions while also asking them to estimate the frequency of other participant

[6] We have chosen relatively small stakes because previous research has shown that in donation contexts, participant behaviour is relatively invariant to stake sizes, with the primary exception being cases of extremely high stakes, in which hyper-altruistic behaviour (donating all of one's endowment) vanish (Brañas-Garza et al., 2021).

choices. Drawing on the Bayesian claim about population frequencies, namely that one's own view ought to be underestimated by others, because it functions as an informative sample of one, the method rewards surprisingly common answers. Participants who scored in the top third according to this post-hoc compensation algorithm were rewarded with an additional £0.15 after the experiment.

The first set of items (*stated preference measures*) had two components: First, we collected *stated policy preferences* on FDA regulation, the filibuster, and a carbon tax. As before, we collected these additional measures to keep the purpose of our study opaque while truthfully stating the topic of our study to participants[7]. Their support was measured on a 5-point Likert scale ranging from '1 – Strongly oppose' to '5 – Strongly support'. All three sets of questions were incentivised by the Bayesian Truth Serum (Prelec 2004), meaning participants were also asked to estimate the average frequency of all five respective response options across all sampled participants.

Second, we collected *reported intentions to act* from participant responses on a number of items where we asked them to state on a 5-point Likert scale ranging from '1 – Very unlikely' to '5 – Very likely' whether they were planning to undertake any of the following actions within the next twelve months: attend a protest march to address social justice, donate to charity to reduce global poverty, reduce carbon emissions, quit one's job, or stop eating meat. Our two exclusive variables of interest in this section were the support for a carbon tax and the self-reported intention to personally reduce carbon emissions within the next twelve months.

The second set of items (*behavioural measures*) also had two components: First, we presented participants with three charities that were all drawn from *The*

[7] However, note that while these additions helped to keep the study purpose opaque, they are not necessarily completely unrelated to climate change.

Founders Pledge, a charitable initiative aiming to promote effective charitable giving. This was done to hold constant the factors that may influence donor behaviour such as brand recognition, trust, or previous familiarity with the organisation. We collected their donation choices with respect to the following three charities: The Global Health and Development Fund (global poverty), the Climate Change Fund (climate change), and the Patient Philanthropy Fund (long-term future of humanity).

Second, we collected behaviour measures relating to participant interest in *signing petitions* to address pressing social issues. We presented participants with three real and active petitions and measured whether they clicked the links to those petitions. We chose to use actual petitions and only measured clicks (as opposed to creating new petitions and measuring actual signing) as the former had higher ecological validity while also preserving participant anonymity to a much higher degree (as we did not track whether they signed the petitions). Further, we believe that interest in a petition is theoretically interesting in itself and connects up directly to the research question of a potential geoengineering moral hazard. The three petitions included a petition on access to abortions, climate change action, and a reform of the filibuster. Our exclusive variables of interest in this section were the frequency and size of donation to the climate charity, and clicks on the climate petition.

Lastly, we collected data on a number of *additional variables*. Those were used as control variables in our main pre-registered analyses. In addition to the demographic variables (age, gender, ethnicity), we also collected data on level of education, political identity, belief in anthropocentric climate change, previous knowledge of geoengineering, subjective financial well-being, rurality/urbanicity, trust in government, and trust in science (on top of further variables aimed at keeping the study's purpose opaque such as views on political polarisation and abortion, as well as previous knowledge of CRISPR and the Senate filibuster).

Overall, our design has the following methodological strengths: First, from recruitment all through the end of the study, participants were told (truthfully) that the study is concerned with a multitude of topics. They could not know that we were only interested in their attitudes and behaviours relating to climate change. As such, the actual purpose of the study was kept largely opaque, allowing us to mitigate experimenter demand worries to a significant extent. Second, we employed a variety of outcome measures, in terms of both stated preferences and behaviours. This allowed us to capture a wide spectrum of participant responses. This also made it less likely that our design omitted plausible outcomes while also leading to higher external validity as the central response of interest is actual behaviour, i.e., we did not only rely on hypothetical measures, giving our study a higher level of external validity. Third, by properly adjusting for multiple comparisons, we reduced the consequent risk of false positives that an increase in outcomes for hypotheses tests brings with it (Barnett et al. 2022). Fourth, by the introduction of our Salience Control group, we disentangled a potential effects driven simply by making climate change salient, again a feature that was not always properly controlled for in all previous work.

III. Results

In total, we excluded 216 participants for failing either the attention check or one or more comprehension questions at the second attempt (or both). All analyses in this paper are reported with the remaining 2284 participants.[8] In Table 1, we display the demographics of our final sample. Compared with the US Census Bureau (2021), our sample is roughly within 1-percentage point of the actual population

[8] The results in all five main regressions are robust to this exclusion decision; results do not differ if all participants are included.

distribution regarding ethnicity and gender. For example, our sample has 75.7% White and 12.3% Black participants, while the overall share is 76.3% and 13.4% respectively. Further, our sample has 51.1% female participants, contrasted with a 50.8% share in the actual population. However, our sample differs slightly with regards to age in being slightly younger: Where we have 48.8% of participants between the ages of 21 and 44, the population frequency is 41.27%.

TABLE 1—DEMOGRAPHICS

	%		%
Age		*Ethnicity*	
18-28	21.4	White	75.7
29-38	20.2	Black	12.3
39-48	17.2	Asian	6.7
49-58	18.0	Mixed	3.2
59 and above	23.2	Other	2.1
Gender		*Political Affiliation*	
Male	47.2	Liberal	57.4
Female	51.1	Conservative	23.5
Other	1.7	Independent	19.1
Education		*Financial Wellbeing*	
High school	30.5	Finding it very difficult	8.6
Undergraduate	48.0	Finding it quite difficult	13.4
Graduate/Professional	21.5	Just about getting by	27.8
		Doing alright	36.1
Urbanicity/Rurality		Living comfortably	14.1
Urban	63.5		
Rural	36.5	*Anthrop. Climate Change*	
		Strongly agree	41.2
Knowl. Of Climate Interventions		Agree	36.4
Strongly agree	18.8	Neither disagree nor	11.2
Agree	38.5	Disagree	7.0
Neither disagree nor agree	10.8	Strongly disagree	4.2
Disagree	21.2		
Strongly disagree	10.7		

Notes: Demographics for the full n=2284 sample after exclusions.

Between these variables, we find relationships that are very much in line with previous literature. For example, we find that conservatives show both lower belief in anthropogenic climate change, $r=-.486$, $p<.001$, and lower trust in government, r=-.230, p<.001, and science, r=-.388, p<.001, while being older, r=.144, p<.001, and whiter, r=.102, p<.001. We also find that those who knew about climate interventions prior to this study showed higher trust in science, r=.111, p<.001, were younger, r=-.078, p<.001, and more male, r=-.089, p<.001. We also observe a strong relationship between trust in government and trust in science, r=.422, p<.001.

Our design allows us to collect five central outcomes: frequency of donation (to a climate change charity), frequency of link clicks (to a climate change petition), size of donation with a maximum of £0.30 (to a climate change charity), support for a carbon tax (on a 5-point Likert scale), and intention to reduce emissions (on a 5-point Likert scale). In Table 2, we outline these variables split by our three conditions and report mean, frequency, and standard deviation.

TABLE 2—OUTCOME MEASURES

	Control	Salience Control	Treatment
Frequency of Donation to Climate Charity	14.8%	14.3%	15.4%
Link Clicks to Climate Petition	15.8%	16.2%	15.3%
Size of Donation to Climate Charity	3.16 (8.48)	3.32 (8.91)	3.40 (8.84)
Size of Donation (if donation)	21.34 (9.93)	23.18 (9.73)	22.08 (9.79)
Support for Carbon Tax	3.80 (1.34)	3.81 (1.33)	3.71 (1.38)
Intention to Reduce Emissions	3.29 (1.42)	3.45 (1.36)	3.25 (1.45)
Sample Size	696	678	910

Notes: Outcome measures for all three conditions with frequency and mean (standard deviation).

Investigating the relationships between our behavioural outcome variables, we find that the two charity measures (frequency of giving and amount of giving) are highly correlated, r=.902, p<.001, while both also correlate significantly with clicking the link, r=.144, p<.001, r=.143, p<.001 respectively. Between the two stated preference measures (support for a carbon tax and intention to reduce emissions), we also find a significant relationship, r=.462, p<001. Importantly though, all our dependent variables show some level of correlation, suggesting that they all measure roughly one type of overall behaviour, i.e. climate change mitigation behaviour/preferences.

Below, we present five pre-registered regression models testing our two null hypotheses. For all these models, we control for a variety of factors. First, we control for standard demographic characteristics like age, gender (with '1 = Female'), ethnicity (with 'White' as the comparison group), and education (with 'High School' as the comparison group). Further, we also control for political orientation via two dummies; conservatism (with '1 = Conservative') and liberalism (with '1 = Liberal') with independents and the respective other being coded as '0'. Further, we also control for urbanicity/rurality (with '1 = Urban') as well as subjective financial well-being (which is coded as a 5-point Likert scale). Further, we add two further central control variables: prior knowledge of geoengineering (as a 5-point Likert scale where increasing scores denote increasing knowledge) and belief in anthropogenic climate change (as a 5-point Likert scale where increasing scores denote increasing belief in anthropogenic climate change).

Because our dependent variables are quite distinct (ranging from binary variables to continuous variables of different ranges), all reported coefficients below are in standardized form to allow for easier cross-model comparisons. For all analyses below, the pre-registered threshold for significance is set to p=.01 to adjust for multiple comparisons following the Bonferroni method. While we do designate p=.05 with '*' in the regression tables, we will only interpret p-values below .01 as significant and will treat any values at or above .01 as unequivocally non-significant.

In order to provide evidence regarding our first null hypothesis, we present three OLS regression models with our behavioural outcome variables. Model (1) has the choice to donate to a climate change charity as its dependent variable (with '1' if such a donation is made, and '0' if a donation is made to another type of charity or if no donation is made). Model (2) predicts behaviour with regard to the size of the donation to a climate change charity (donations can be up to £0.30, and donations to different charities as well as no donations at all are coded as '0').[9] Lastly, Model (3) predicts link clicks to a climate change petitions (with '1' if the link has been clicked, and '0' if a link to a different type of petition has been clicked or if no link has been clicked at all). In Appendix A, we present pre-registered logit models as robustness checks for our OLS regressions with binary dependent variables,[10] finding virtually identical results, suggesting that the results in Table 3 are not sensitive to this model choice.

In order to provide evidence regarding our second null hypothesis, we present two further OLS regression models with our stated preference outcomes in Table 3. In Model (4), support for a carbon tax is our dependent variable (with '1 = Strongly Oppose' and '5 = Strongly support). For Model (5), the dependent variable is self-reported intention to reduce carbon emissions over the next 12 months (with '1 = Very Unlikely' and '5' = Very Likely').

TABLE 3— OLS REGRESSION RESULTS FOR ALL FIVE OUTCOME VARIABLES

	(1) Donation Freq.	(2) Donation Amount	(3) Link Click	(4) Carbon Tax Support	(5) Intention to Reduce Emissions
Treatment	.009 (.026)	.014 (.025)	-.005 (.023)	-.030 (.018)	-.011 (.023)
Salience Control	-.006 (.029)	.008 (.025)	.009 (.024)	.008 (.019)	.050* (.022)

[9] If the outcome variable would be size of donation to a climate charity conditional on making a donation, the treatment effect is of the same size and also non-significant at roughly the same level.

[10] We decided to report OLS models in the main document so that we can present more easily interpretable and comparable results, and because results are generally insensitive to model choice (Angrist & Pischke, 2008; Hellevik, 2009; Gomila, 2020).

Control Variables	Yes	Yes	Yes	Yes	Yes
R^2	.074	.073	.034	.502	.237
Sample size	2284	2284	2284	2284	2284

Notes: Standardised coefficients and standard errors. *p<.05, **p<.01, ***p<.001

The results suggest that the treatment showed no statistically significant effect on either behavioural outcomes, as evidenced by Models (1) to (3), or stated preferences, as shown by Models (4) and (5). In terms of behavioural outcomes, the coefficients for treatment in relation to both charity decisions and petition link clicks were not statistically significant, with βs ranging from -.005 (SE = .023, for link clicks) to .014 (SE = .025, for donation amount). Likewise, for stated preferences, neither the support for a carbon tax nor self-reported intentions to reduce emissions were significantly affected by the treatment, with βs of -.030 (SE = .018) and -.011 (SE = .023), respectively. The salience control condition also showed no statistically significant effects across all five outcomes, with the exception of intention to reduce emissions (β = .050, SE = .022); however, this association did not withstand adjustment for multiple comparisons. These findings collectively point to a failure to detect moral hazard. For a visual representation of all standardised coefficients alongside their standard errors for the treatment condition, please refer to Figure 2.

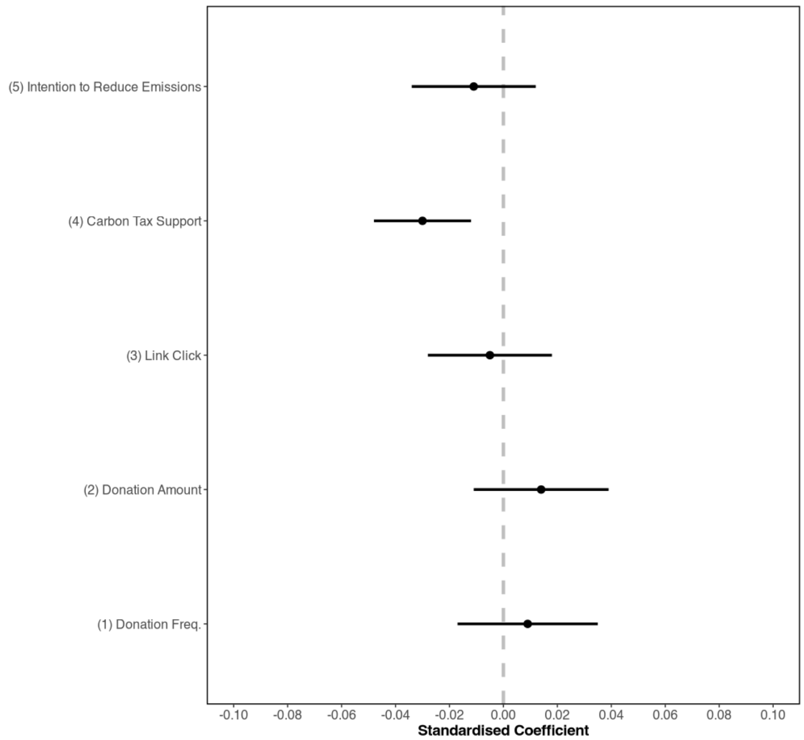

FIGURE 2. Forest Plot

Notes: Standardises coefficients and standard errors for all five outcome variables

One may worry that given our large number of control variables, that our estimates may be subject to overcontrol bias (Li, 2021). To show that our results are robust to our inclusion of that many (pre-registered) control variables, we report the same set of main regressions in Appendix B without any control variables, finding no difference in magnitude, size, or significance of the treatment effect coefficients.

Overall, the above results for Models (1) – (5) suggest that we did not find evidence that would allow us to reject either null hypothesis as we fail to find that the geoengineering treatment significantly predicts either behaviour (on any of the three models) or stated preferences (on either of the two models). This is true both before and after adjusting for multiple comparisons and after conducting robustness checks (like logit models for binary dependent variables).

However, we also provide direct evidence in favour of the null. This is something that a regression analysis in a null-hypothesis testing framework technically cannot provide, which is why we present pre-registered equivalence test results to not only provide evidence in favour of a failure to reject the null hypothesis, but instead in favour of the null itself (Lakens et al. 2020). We conduct a number of tests of equivalence that basically amount to two one-sided tests (TOST) against two equivalence bounds that allow for the conclusion that the estimate is null or negligibly small, i.e. within these bounds.

Following Alter & Counsell (2021), we conduct these tests on the standardised coefficients from Models (1) to (5) and test them against a range of plausible upper and lower equivalence bounds, all in standardised coefficients to allow for easier cross-model comparisons. We pre-registered this approach to enable scrutiny of our results across a variety of potentially interesting levels. This approach is akin to a sensitivity analysis across potential equivalence bounds. Specifically, we report results for bounds of (-).01, (-).05, and (-).075. We do not report results of larger equivalence bounds as we already have strong evidence in favour of a negligibly small effect at the present parameters, even after adjusting for multiple comparisons by putting the level of significance at the 1% level, see Table 4.

TABLE 4—TOST FOR STANDARDISED TREATMENT EFFECTS

	-.01	.01	-.05	.05	-.075	.075
Model (1) Std. Treatment Effect	.73	.04	2.27**	1.58*	3.23***	2.54**
Model (2) Std. Treatment Effect	.96	-.16	2.56***	1.44	3.56***	2.44**
Model (3) Std. Treatment Effect	.22	.65	1.96*	2.39**	3.03***	3.48***
Model (4) Std. Treatment Effect	-1.11	2.22*	1.11	4.44***	2.50**	5.83***
Model (5) Std. Treatment Effect	-.04	.91	1.70*	2.65**	2.78**	3.74***

Notes: All t-statistics for TOST procedures on a variety of lower and upper equivalence bounds (in standardized coefficients) of treatment effects from Models (1)-(5). *p<.05, **p<.01, ***p<.001

Our results suggest that while we do not have evidence in favour of a null (or a negligibly small effect) at the tight equivalence bounds of .01, we do already provide such evidence at an 5% alpha-level at the .05 equivalence bounds for standardised treatment effects of Model (1), Model (3), and Model (5). Most centrally, all our standardised treatment effect estimates fall within the equivalence bounds of standardised coefficients -.075 and .075 at the 1% level of significance, indicating strong evidence in favour of a null effect as big as .075.

In unstandardised and more easily interpretable terms, this means that the treatment effect in Model (1) is smaller than a 5-percentage point increase or decrease in the probability of donating to a climate-related charity and amounts to an effect smaller than a donation increase or decrease of 1.3 pence in Model (2) (out of a maximum donation of 30 pence). In Model (3), the treatment effect is smaller than a 6-percentage point increase or decrease in the probability of clicking on a link to a climate-related petition. For Model (4), our treatment effect is smaller than a .21-point increase or decrease in support with a carbon tax on a 5-point Likert scale. For Model (5), the treatment effect is smaller than a .20-point increase or decrease in self-reported intention to reduce greenhouse gas emissions over the next twelve months on a 5-point Likert scale. These results suggest that the effect of being provided with information about geoengineering does not impact behaviour or preferences to an extent exceeding these estimates. While estimations of policy-relevance are always difficult to make, we argue that these results provide robust evidence in favour of a null at these specified bounds.

We also run exploratory Bayesian aimed at testing the sensitivity of our results by not only relying upon the frequentist approach. In Appendix C, we report Bayes factor model odds for the null models on a number of different priors (uniform, beta binomial, and Wilson), showing strong evidence in favour of the null for Models (1)-(4). The model averaged coefficients similarly replicate our frequentist regression results of showing a null effect. For further details, please see Appendix C.

IV. Discussion

The data collected here do not allow for the rejection of either null hypothesis. Based on pre-registered equivalence tests, however, we are able to provide strong evidence in favour of the null that information about geoengineering does not lead to a reduction (or increase) in climate change mitigation behaviours or stated preferences, and thus as such does not constitute a moral hazard. Moreover, we are able to concretely specify the bounds of these null effects, suggesting that the treatment effect of being informed about SRM is either null or rather small and thus unlikely to be relevant to public policy. These results are corroborated by exploratory Bayesian analyses, providing evidence in favour of the null from a non-frequentist framework.

Previous work has found conflicting results, with Raimi, Maki, Dana, & Vandenbergh (2019) finding a reduction in mitigation support, Cherry, Kallbekken, Kroll, & McEvoy (2021) finding the converse, and Fairbrother (2016) finding no effect. We argue that our results may differ because of distinct methodological and statistical approaches. Not all previous work has properly adjusted for multiple comparisons where it would be necessary. The present paper instead adjusts for this false positive rate via the Bonferroni method. The results showing an increase in support by Cherry, Kallbekken, Kroll, & McEvoy (2021), on the other hand, do not control for salience by only having a no-information baseline and an information treatment, which means that they are unable to distinguish their effects from a simple salience effect, something which we control for. Further, almost all of the current literature does not incentivise their participant responses and does not collect behavioural measures, while we do both, which, we posit, leads to higher external validity. While our results are largely in line with Fairbrother (2016), it is worth pointing out that their results only show a failure to find an effect, and that they do not report analyses in favour of a null effect, while we offer equivalence tests for this.

Importantly, our results also show a number of secondary relationships that strengthen the validity of our outcome variables and thus our results. For example, we find that those who self-identify as conservative (in the US-political sense) are significantly less likely to support a carbon tax or report intentions to reduce their own emissions. Similarly, higher trust of government also predicts both of these outcomes while higher trust in science only predicts support for a carbon tax. Further, belief in anthropogenic climate change stands in a positive relationship to donating to a climate change charity and to the size of that donation, as well as both support for a carbon tax and intention to reduce emissions. Lastly, previous knowledge of geoengineering only impacts self-reported intentions to reduce emissions positively. This is in line with previous literature.

In conclusion, our study was designed to mimic an environment as realistically as possible, where a variety of different information was provided and a plurality of outcome measures were collected. Overall, we did not find evidence that being provided with information about SRM significantly impacts either stated preferences nor actual behaviour. Specifically, we argue that our paper substantially contributes to this literature by adding new statistical and methodological approaches, and by presenting a robust null effect. Because our design had several design strengths over previous work—e.g., usage of behavioural measures (like donations to climate change charities and clicking on links to climate change related petitions), incentivising preference measures (via the Bayesian Truth Serum), keeping the topic of the study opaque (by including a large number of additional questions and texts), and by controlling for salience (by including an intervention only providing information about climate change generally), and because the design was highly powered, pre-registered, and used a representative sample alongside appropriate statistical methods to draw conclusions regarding a null effect—we believe that the work presented here represents the best evidence available regarding the question whether SRM information poses a moral hazard. Our answer is that it does not (or that its effect is negligibly small). We hope that this will contribute to a more nuanced discussion where, instead of talking about moral hazards of particular climate measures in isolation, we discuss risks in terms of packages of climate measures (Markusson, McLaren & Tyfield, 2018; Jebari et al., 2021).

While we have outlined the strengths of our design above, we also want to draw attention to the limitations and downsides of our approach. In most cases, optimising for one parameter (like experimenter demand) may lead to trade-offs with other worthwhile design goals. For example, one limitation of this design is inherent in its focus on reducing experimenter demand. Specifically, by having aimed so heavily on ensuring that experimenter demand concerns are minimised, the current design may in fact be biasing the results towards the null by making the stimuli themselves too subtle. While we argue that the treatments themselves provided ample reason to think that there was a relatively high chance of a detectable very small effect (recall that the a priori power analysis indicated enough power to detect small global effect of f^2=.01), we want to point out this trade-off so that readers are directly informed about this limitation.

A further, similar, limitation of our design is that by putting so much effort on minimising experimenter demand, this may have induced participant fatigue, which may have driven the results towards a null. While this is certainly possible, we argue that our low failure rates at the comprehension quizzes (at 3.96%) provides some evidence against this worry. One further limitation of our study is that it was conducted entirely in the (online) lab and did not include field experiment aspects. While this is in line with the literature as a whole (Fairbrother 2016; Cherry, Kallbekken, Kroll, & McEvoy 2021), it is a weakness worth noting. Furthermore, one may worry that more compelling and evocative treatment texts may have led to a significant effect. While this may be true, we argue that our choice of treatment text was motivated by having it be neutral and similar to informative media one may encounter in the 'real world'. While we do acknowledge this limitation, we argue that our approach was justified on these grounds.

In simple terms, we conclude that providing US Americans with information about climate interventions does not meaningfully impact their behaviour or their stated preferences regarding climate change mitigation behaviour and, therefore, our results indicate that climate intervention information does not constitute a moral hazard.

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VI. Appendix

Appendix A – Logit Robustness Check

Here, we report two pre-registered robustness checks, i.e. logit models of Model (1) and Model (3) respectively. These are reported to show that our results are not sensitive to model choice as the outcome variables are binary and as such both approaches would be valid. The results indicate that there is no difference in estimation of treatment effect.

APPENDIX TABLE 1—LOGISTIC REGRESSION ROBUSTNESS CHECKS

	(6)	(7)
Treatment	.053 (.148)	-.030 (.142)
Salience Control	-.008 (.160)	.057 (.150)
Conservativism	-.753** (.268)	-.602** (.221)
Liberalism	.010 (.180)	.107 (.165)
Belief in Anthropogenic Climate	.650*** (.103)	.138 (.079)
Knowledge of Climate Interventions	.057 (.049)	.022 (.047)
Trust in Government	.276** (.095)	-.007 (.086)
Trust in Science	.365* (.155)	.291* (.127)
Urbanicity	-.085 (.130)	-.217 (.123)
Subj. Financial Wellbeing	.063 (.057)	.021 (.054)

Gender (Female)	.184 (.126)	.221 (.120)
Age	.013** (.004)	.001 (.004)
Undergraduate Education	-.066 (.153)	-.011 (.143)
Postgraduate Education	.062 (.179)	.256 (.170)
Ethnicity – Asian	.189 (.235)	.247 (.227)
Ethnicity – Black	-.258 (.230)	.032 (.194)
Ethnicity – Mixed	.143 (.351)	.377 (.307)
Ethnicity - Other	-.749 (.616)	1.257*** (.326)
Cox & Snell R^2	.089	.035
Sample size	2284	2284

Notes: Log odds and standard errors. *p<.05, **p<.01, ***p<.001

Appendix B – Robustness Check for Main Regressions without Controls

We also investigate the results of our central regressions without any control variables to test the robustness of our result to a potential overcontrol bias (Li, 2021). We find that coefficients show the same directionality throughout all five models, and the same significance level throughout four models, with Model (11) no longer being significant at the 10% level (which was not interpreted either way due to our move to a 1% level because of adjustment for multiple comparisons). Further, the magnitude difference of all five coefficients is also negligibly small, with .009 in the original regression turning into .008 when all controls are dropped, and .014 into .013, -.005 into -.007, -.30 into -.034, and -.011 into -.016 for the four other models respectively. This suggests that our results are not influenced by overcontrol bias.

Appendix C – Bayesian Analyses

We also conduct additional exploratory Bayesian analyses (Rouder & Morey 2012) using Bayesian linear regression analyses that draw on Bayesian model averaging (e.g., Hinne et al., 2019, Maier et al., 2022) to provide further evidence that does not rely on a frequentist framework. We report results for our five basic models that include our treatment dummies as covariates. Below we report for simplicity's sake only the null model's results. Though note that each analysis for each outcome variable actually includes four models: the null model (only intercept and error term), a model with only the treatment dummy, one with only the control salience dummy, and one with both.

We report results with three sets of priors to show the sensitivity of our results to different model prior choices. First, we report the Bayes factor model odds for null model results with a uniform model prior at .25. Second, we report the same analyses using a beta binomial model prior at .33 which is not biased against sparse and dense models. Third, to provide an even harsher test we also report results with a Wilson prior that is a variant of the beta binomial prior that assigns more mass to models with fewer predictors (Van Den Bergh et al. 2020). For the Wilson prior, we set α=1 and λ=2 (with β=λ*predictors), and the model prior for the null model is thus set at .667. As before, (1) refers to frequency of donation to a climate charity, (2) to the amount of that donation, (3) to clicks on a link to a climate petition, (4) to support for a carbon tax, and (5) to the self-reported intentions to reduce emissions.

APPENDIX TABLE 2—BAYESIAN LINEAR REGRESSION BAYES FACTOR MODEL ODDS FOR NULL MODEL

	Uniform Prior	Beta Binomial Prior	Wilson Prior
Freq. of Donation to Charity (1)	26.799	34.459	22.753
Donation to Climate Charity (2)	29.241	37.523	24.852
Petition Link Clicks (3)	27.766	35.715	23.570
Support for Carbon Tax (4)	10.823	13.759	9.252
Intention to Reduce Emissions (5)	.915	1.138	.791

Notes: Bayes Factor Model Odds for the Null Model with Uniform Prior, Beta Binomial Prior, and Wilson Prior.

The results in Appendix Table 2 suggest that in four of our five models (1) – (4), the odds in favour of the model being the null model after observation of the data have increased by a factor of between 9.252 for Model (4) on a Wilson prior to 37.523 for Model (2) on a beta binomial prior. Model (5), predicting self-reported intentions to reduce emissions, is markedly different in that the Bayes factor model odds to not suggest that the data fit the null model, with a Bayesian model odds factor of between .791 at the Wilson prior and 1.138 at the beta binomial prior.

Below we report the model averaged coefficients that allow us to deal with uncertainty over the estimates as well as uncertainty over model choice. We report coefficients as well as 95% credible intervals that represent a weighted average (weighted by the posterior probability of predictor inclusion). We use a JZS parameter prior with the default r scale of .354 (Liang et al. 2008) and use the uniform model prior to compute the model averaged results.

APPENDIX TABLE 3—BAYESIAN LINEAR REGRESSION COEFFICIENTS AND 95% CREDIBLE INTERVALS

	Treatment	Salience Control
Freq. of Donation to Charity (1)	.0004 [-.0015, .0021]	-.0004 [-.0020, .0000]
Donation to Climate Charity (2)	.0083 [-.0639, .0099]	.0015 [.0000, .0244]
Petition Link Clicks (3)	-.0004 [-.0047, .0000]	.0004 [-.0001, .0011]
Support for Carbon Tax (4)	-.0154 [-.1208, .0000]	.0034 [.0000, .0550]
Intention to Reduce Emissions (5)	-.0138 [-.1401, .0024]	.1224 [.0000, .2803]

Notes: Model averaged coefficients and 95% credible intervals.

The results in Appendix Table 3 provide additional evidence in favour of a null effect for the treatment condition across all five outcome variables, though note that for the self-reported intentions to reduce emissions, these results suggest a notable influence of the salience control condition, which we did not observe in our pre-registered null-hypothesis testing results reported in the main text (and which is also captured in the Bayes factor model odds above, explaining the divergence of results in Model (5)). However, the central estimate of interest is the treatment effect, which is why we take our exploratory Bayesian analyses to provide strong evidence in favour of the claim that there is no moral hazard with regard to being presented with information about climate interventions.

Appendix D – Additional Analyses

We also report the following non-pre-registered analysis. In our pre-registered regression models, we compare the Treatment and Salience Control to the Content Control. However, in exploratory analyses, we do find a statistically significant effect when we use the Salience Control as the comparison group. Running the same specifications for all five Models (1)-(5), we find that for Model (5) – Intention to Reduce Emissions, the standardised coefficient is significant at the adjusted significance level with B=-.063 (SE=.022). However, this effect does not in itself constitute evidence for a moral hazard because it actually captures the fact that, empirically, those in Salience Control M=3.45 (SD=1.358) show a higher intention to reduce emissions than both the Treatment M=3.25 (SD=1.452) and the Control M=3.29 (SD=1.416). Because we do not find that the Treatment is lower than the Control, we do not take this as evidence for a moral hazard, but wanted to outline this pattern of results nonetheless.

Appendix E – Treatment Materials

Below we present the treatment text for Treatment and Salience Control, as well as the questions that we used for the five main outcome variables.

Treatment Text

According to the Intergovernmental Panel of Climate Change (IPCC), climate models project global warming is likely to exceed 1.5°C above pre-industrial levels by 2041-2060. This brings with it a number of risks, such as: increases in mean temperatures, hot extremes in most inhabited regions, heavy precipitation in several regions, and the probability of drought. Further, sea-levels are projected to rise around 0.1 metre by 2100 and most of these risks have direct negative impacts on human health, livelihoods, food security, water supply, human security, and economic growth.

In order to reduce global warming, a number of new approaches have been proposed. One of them is called 'climate intervention' or 'geoengineering'. Climate interventions aim to deliberately engineer the Earth's climate to reduce global warming. One such climate intervention is Solar Radiation Management (SRM), where small particles are sent into the stratosphere in order to reflect inbound sunlight back into space. These particles can be delivered via aeroplanes or large balloons. SRM can potentially be both fast and inexpensive in reducing global warming, allowing more time to reduce emissions. However, these efforts potentially involve continuous injection to maintain the same effect; also, local and global environmental side-effects are poorly understood. Regardless, climate interventions could be an important contributor to our climate responses.

How does solar radiation management (SRM) work?

- ○ It reflects energy back down to earth.
- ○ It reduces the amount of energy the sun radiates.
- ○ It reflects sunlight back into space.

Control Salience Text

According to the Intergovernmental Panel of Climate Change (IPCC), climate models project global warming is likely to exceed 1.5°C above pre-industrial levels by 2041-2060. This brings with it a number of risks, such as: increases in mean temperatures, hot extremes in most inhabited regions, heavy precipitation in several regions, and the probability of drought. Further, sea-levels are projected to rise around 0.1 metre by 2100 and most of these risks have direct negative impacts on human health, livelihoods, food security, water supply, human security, and economic growth.

In order to reduce global warming and mitigate the negative outcomes of climate change, countries, companies, and individuals have to engage in a number of mitigation strategies. These strategies include decarbonising electricity generation, reducing transport emissions by driving less, reduction in energy consumption (both for companies and for individuals), increases in investments into renewable energy sources, and better planning of urbanisation. These mitigation efforts may involve behavioural change as well as involve some economic costs. However, all these mitigation strategies can make some contribution to lessening climate change risks. If countries, companies, and individuals all take significant mitigation action over a prolonged period, the most substantial risks can be reduced.

Which of the following is an important social measure for reducing the risk of climate change?

- ○ Switching to solar and wind energy
- ○ Recycling plastic and glass
- ○ Eating locally grown food

Donation Choice

In this section of the survey, you can decide here to donate all, some, or none of **your earned budget of £0.30** to one of the charities below. The remaining amount that you decide not to donate will be paid out directly to you as a bonus via Prolific.

We present three charities below. All three charity funds are administered by 'The Founders Pledge', an organisation that researches and champions evidence-led and thoughtful approaches to having an impact.

The Global Health and Development Fund
Nearly half the world lives on less than $2.50 a day, yet giving by the world's richest often overlooks the world's poorest and most vulnerable. Despite the average American household being richer than 90% of the rest of the world, only 6% of US charitable giving goes to charities which work internationally.

This Fund is focused on helping those who need it most, wherever that help can make the biggest difference.

The Climate Change Fund
Current levels of emissions are contributing to millions of deaths annually from air pollution and causing irrevocable damage to our planet. In addition, millions worldwide do not have access to modern energy technology, severely hampering development goals.

This Fund is committed to finding and funding sustainable solutions to the emissions crisis that still allow growth, freeing millions from the prison of energy poverty.

The Patient Philanthropy Fund
This fund focuses on how we can collectively grow our resources to support the long-term flourishing of humanity. It addresses a crucial gap: as a society, we spend much too little on safeguarding and benefiting future generations. In fact, we spend more money on ice cream each year than we do on preventing our own extinction. However, people in the future - who do not have a voice in their future survival or environment - matter. Lots of them may yet come into existence and we have the ability to positively affect their lives now, if only by making sure we avoid major catastrophes that could destroy our common future.

This Fund aims at protect the long-term future of humanity.

Please indicate below which of these charities (if any) you would like to donate to.

- ○ The Global Health and Development Fund
- ○ The Climate Change Fund
- ○ The Patient Philanthropy Fund
- ○ I do not want to donate

Petition Choice

The **Stand Up for Real Climate Action** petition calls on world leaders in the U.S. and beyond to "act so we do not lose ground in combating climate change".

You can find the petition here: Link.

Support for Carbon Tax

In 2021, Senators Sheldon Whitehouse, Brian Schatz, and five co-sponsors introduced the Save Our Future Act. It proposes a carbon tax at an initial tax rate of $54 per metric ton of carbon emissions with an annual adjustment of 6% + inflation.

This carbon tax would come into effect in 2023 and would be one significant step towards mitigating climate change. This tax would make polluting more expensive and as such reduce our overall emissions.

Do you support the introduction of the Save Our Future Act?

	Strongly oppose	Somewhat oppose	Neutral	Somewhat support	Strongly support
Please indicate your support or opposition for the Save Our Future Act here.	○	○	○	○	○

For this question, please estimate the average proportion of responses other participants will give. For example, if you think that 10% of other participants will select 'Neutral', then you should enter '10' under 'Neutral'. Importantly, all entries have to sum to '100'.

Strongly oppose	0
Somewhat oppose	0
Neutral	0
Somewhat support	0
Strongly support	0
Total	0

Self-Reported Intentions to Act

In this section of the survey, we are interested in what **actions you personally plan to undertake within the next 12 months.** Please answer honestly.

Please indicate how likely you are to personally engage in each of the following actions.

	Very unlikely	Somewhat unlikely	Neither likely nor unlikely	Somewhat likely	Very likely
Attend a protest march to address societal injustices.	○	○	○	○	○
Donate to charity to reduce global poverty.	○	○	○	○	○
Reduce carbon emissions (for example by reducing plane and car travel).	○	○	○	○	○
Quit my job and search for a more fulfilling new job.	○	○	○	○	○
Stop eating meat (for example by buying meat-substitutes)	○	○	○	○	○