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Can competing demands affect pro-environmental behaviour: a study of the impact of exposure to partly related sequential experiments

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ABSTRACT

The study of human behaviour is central to the development of appropriate policies for sustainability. We argue that mathematical models of human choice behaviour may produce biased results if they fail to account for the possibility of spillover effects, in particular the possibility that individual behaviour may change as a result of interventions along with competing demands (multiple demands), such as in the sequential exposure to partly related choice contexts. Using a sample of 751 individuals and a carefully constructed experiment, we develop mathematical models that jointly explain the choice between different pro-environmental actions and the willingness to donate money for environmental causes, and at the same time, allow us to test the indirect effect of exposure to multiple demands. We find that the strength of preferences for behavioural changes leading to greater CO_2 reductions is (causally) shaped by participants previously considering other similar behavioural changes. The kind of spillover effects we find are relatively complex and often subtle, and thus warrant further replication studies. Care was taken to account for variation of tastes, formatting, order and learning effects, thus reducing the risk of the spillover-related results being influenced by differences across individuals in environmental preferences. Our study demonstrates the existence of a specific type of spillover effects, namely how prior exposure to related choice contexts may affect behaviour in subsequent settings and showcased the effectiveness of discrete choice models to test for it. Given our results, we believe that spillover effects need to be taken into account in the broader choice modelling literature, and at the same time we showcase a useful experimental framework for environmental psychologists and economists.

1. Introduction

Climate change is a reality of global concern that represents the greatest threat to the sustainability of life on the planet. This is exacerbated by human activity, with the carbon footprint produced by humans being 50% higher in 2021 than the corresponding level in 1990 (UNDP, 2022). Changes in human behaviour and consumption habits can play a decisive role in reducing greenhouse gases and therefore human impact on climate change, since their actions, habits and consumption can contribute to the reduction or increase of the carbon footprint which is responsible for much of the abrupt changes in climate (Ivanova et al., 2020). As part of the strategy to counteract this, a call has been made for climate action and responsible consumption as part of the Sustainable Development Goals (SDG) (United Nations, 2022). However, the development of new policies to protect the environment relies on understanding human behaviour and consumer demand (United Nations Environment Programme, 2017).

Understanding human behaviour and the psychology of the individual is complex, since it can be influenced by many factors that can be heterogenous. Previous studies have made use of experimental settings to understand pro-environmental preferences and their role in individual decision making on the basis of observable characteristics of the individual (e.g., gender, age), beliefs and attitudes (Čapienė et al., 2021; Klöckner, 2013; Verain et al., 2015), characteristics of goods and services, including the effect of incentives and advertising suggestions and spatial effects (Donmez-Turan and Kiliclar, 2021). The issue has also

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received attention in the environmental economics literature, where a popular approach has been to use stated preference (SP) data and analyse it with discrete choice models. It is in this context that research in environmental economics has also shown that displaying extra information or presenting choice situations in a different order can impact decision making (see for example: Dekker et al., 2014). However, a factor that has not been widely studied is that preferences of respondents, and hence the outcome of their decision processes can be affected both by their exposure to competing demands (multiple demands) and the decisions they make therein. While this topic has been overlooked in a choice modelling context, it has attracted considerable attention in the environmental psychology literature (Maki et al., 2019), where there has been extensive investigation into how a previous intervention (experiment performed to encourage behaviour change) can trigger an impact on other behaviour - described as *spillover effects* (Thøgersen, 1999).

The emergence of that research line in environmental psychology has motivated researchers from other disciplines to use methodologies and combinations of factors that have led to divergent conclusions about whether environmental behaviours have a positive or negative indirect effect that influences the probability of taking additional pro-environmental behaviours (Truelove et al., 2014). Broadly, *spillover effects* have been associated with either external motivators (e.g., rewards, punishment) or internal motivators (Nilsson et al., 2016; Thøgersen and Ölander, 2003; Thomas et al., 2016; Truelove et al., 2014). Teasing out the role of these effects is a complex empirical undertaking, and it is in that context that the flexibility of discrete choice models has made it possible to uncover numerous possible dimensions along which to study spillover effects. For example, there has been a focus on measuring environmental externality spillovers (Concu, 2009), multi-attribute benefits (Yang and Le, 2023), or even the interdependence in different alternatives and the role of motivational spillover (Said et al., 2022). The focus of previous studies has been on possible triggers of spillover behaviour. A potentially overlooked topic in that context is whether there are also spillover effects caused by the exposure of individuals to different types of decision-making situations. So, there may be value in conducting simultaneous experiments to better understand what different focuses in the same experiment can lead to.

In this research we use stated preference (SP) surveys to capture information on potential respondent behaviour. SP surveys are a widely used approach to understand consumer preferences (Louviere et al., 2000) and are increasingly popular in environmental economics (Mariel et al., 2021). While stated preference surveys are inherently hypothetical in their nature, careful framing of the scenarios reduces the risk of hypothetical bias and especially stated choice (SC) methods are seen as more reliable than direct approaches such as contingent valuation (Mariel et al., 2021). SC surveys produce data on choices in hypothetical settings, and to understand the influences on those choices, mathematical models of behaviour known as discrete choice models are used. These models are grounded in micro-economic theory, while also giving substantial flexibility to capture behavioural effects (Train, 2009). They allow analysts to understand the trade-offs that people are willing to make between the different attributes of options that are available to them, and to estimate the impact that changes in an attribute have on the overall appeal (utility) of an option (Train, 2009).This would not be possible with simpler methods, such as basic regression. Choice models are flexible in the type of "options" that are considered, looking at the choice between different products, different policies, or, in our case, different types of behaviour change. The analytical tool also has the clear advantage of allowing us to test a wide range of effects and control for possible confounding factors (e.g., individual characteristics, order effect) in order to isolate the specific effect of interest, i.e. spillover effect.

In contrast to the existing literature, our work goes further by not looking at the order of individual tasks in a set of experiments of the same type, but rather at how exposure to one type of experiment may influence later behaviour in a different type of experiment. We intend to show that the proposed framework has the advantage of allowing us to empirically test the hypothesis of whether interventions (experiment performed to encourage behaviour change) and small variations in the survey design affect the individuals' behaviour and their further decisions, and to quantify the size of these changes. This topic has real world relevance as it would allow us to understand whether repeated demands of partially related actions and an intervention can lead to a strengthening or weakening of preferences. In other words, if an individual is faced with different ways of contributing to reducing environmental impacts, will each new but different demand be met with an independent response or will the previous exposure to other contexts matter, either positively or negatively?

We argue that if such spillover effects play a role, then a failure to consider their impact in models of human behaviour may lead to biased findings for the more *direct* factors, i.e. the impact of product and consumer characteristics as well as the choice context. As a first step in that direction, our work here focusses not on *spillover* in terms of past choices influencing future decisions but looks in particular at the possibility that previous exposure alone to related choice contexts may impact the behaviour in later settings.

A key novel feature of our work is that, under the same overall experimental setup, we use variations in the type of experiment format, the choice tasks, and the order of experiments across individual respondents. We compared the preferences in a specific experiment between "experienced" participants with prior experience in an earlier experiment and "naive" participants without such prior experience. We use three types of experiments that are described in more detail in the following section. Briefly, these include two experiments looking at environmental actions (E1 and E2) that differ in their presentation, and a donation experiment (E3). Our empirical work tests for each one of these experiments whether and how the behaviour differs as a function of the previous exposure to one or both of the other experiments. As each individual was exposed to each type of scenario, we can in the model then account for ordering effects, without risking the spillover-related results being influenced by differences across individuals in environmental preferences.

In the remaining sections of this paper, we discuss in turn the details of the methods and data used, followed by the specification of the model and estimates. We also make reference to work from a variety of disciplines that supports our results.

2. Methods and data

2.1. Data

2.1.1. Study area and sample

The survey was administered online using the Prolific platform, sampling from a group of people living in the United Kingdom (UK). We used a convenience sample with quotas only for gender, as budget constraints did not allow recruiting a fully representative sample. The information was collected on two different dates, but the sample is well-balanced in terms of gender and shows some degree of representativeness for different age groups. The modelling process tested for any differences caused by the collection of information on two separate dates, a point we return to later in the paper. The size of the sample, the dates of collection and the main sociodemographic characteristics are shown in Table 1.

Table 1
Sample sizes, survey dates and retention rate.

		Data collection date 1	Data collection date 2	Total
	Date of collection	17/11/2021	05/01/2022	
	Total sample	458	293	751
		n (%)	n (%)	n
Gender	Female	220 (48%)	135 (46%)	355
	Male	232 (51%)	148 (51%)	380
	Non-binary	5 (1%)	8 (3%)	13
	Prefer not to say	1 (0%)	2 (1%)	3
Age	Up to 29	163 (36%)	103 (35%)	266
	Between 30 and 39	150 (33%)	75 (26%)	225
	Between 40 and 49	79 (17%)	36 (12%)	115
	Between 50 and 59	55 (12%)	50 (17%)	105
	Over 60	11 (2%)	29 (10%)	40

2.1.2. Survey materials

To allow us to study the impact of previous exposure to related but different decision contexts, we used three different types of experiments as part of the survey. The first two (E1 and E2) differ from each other in the presentation format, but both are focused on the choice between different green actions that require varying levels of commitment/effort and lead to different levels of reduction in the carbon footprint. The third experiment (E3) focuses on monetary donations.

The emphasis of our spillover study on environmental behaviour and preferences for green actions and CO_2 reduction is framed by the importance of contributing to the understanding of human behaviour to achieve Sustainable Development Goals (SDG). The inclusion of the donation experiment as an intervention builds on previous work linking donations and environmental behaviour using the choice modelling approach (e.g. Reichl et al., 2021). What makes donations attractive to be used as an intervention on behaviour is that donations are one of the few valid direct measures of behaviour that have been used in environmental psychology (Carrico et al., 2018; Lange and Dewitte, 2019, 2023).

Stated preference component

Before participants made their choices, information was shown about the annual emissions of CO_2 per-capita, the negative effects of their accumulation in the Earth's atmosphere and the implications of this for human life. It was also mentioned that there is a way to help reduce this impact by minimizing CO_2 emissions and that planting six trees are required to counteract 1 t of CO_2.[1] The latter was mentioned to make easier the understanding of the impact of the reduction.

When presenting the scenarios, the choice options were framed around the following hypothetical scenario: *"Given that you know that the annual CO_2 emissions (*per capita*) in the UK are 6 t per person/year (taken from the worldbank organization*[2]*), would you be willing to change your routine a bit by making one of the following changes to your behaviour in order to counteract your CO_2 emissions?"*

We now look separately at the context and experimental design used for the action experiments (E1 and E2) and the donations experiment (E3). Example scenarios for each of the three experiments are shown in Fig. 1.

In an SP component, and in particular one of the stated choices (SC) type, respondents make hypothetical choices between mutually exclusive options, requiring an analyst to decide on the choice setting, alternatives, and attributes of these alternatives. In this study, we used a labelled experiment, with three different green actions as alternatives to reduce CO_2 emissions in experiments E1 and E2, based on the activities with the highest annual CO_2 mitigation potential, namely travel behaviour and dietary choices (Ivanova et al., 2020). In particular, we gave respondents the option of reducing the number of days on which they use cars, the option of moving to a vegetarian diet for some days, the option of moving to a vegan diet for some days, or the option of no change in behaviour. Alongside the type of action, the alternatives were characterized mainly by two attributes: (i) frequency and (ii) reduction in CO_2 emissions in tonne/year. However, a third attribute called "same impact as planting [specified number] trees", was included to better explain CO_2 emissions in experiment E2 only. The donations experiment (E3) gave a choice between different levels of donations, where this was contextualised by showing the corresponding number of trees that would be planted. Each respondent faced 4 scenarios in each of the E1 and E2 experiments, alongside a single donation task (E3).

Experimental design

An experimental design was used to determine which combination of levels was shown in a given scenario for a given experiment, where the approach differed between E1/E2 and E3. The example for the experiments shown in Fig. 1 already highlights the role of the experimental design in that for the same frequency of engaging in a given activity, the reductions in CO_2 were not constant across individuals, so as to allow us to separately identify the impact of each attribute. These differences were controlled by the experimental design, with a view to ensuring parameter identifiability. The scenarios force respondents to make trade-offs between the attributes as well as the type of action, and there is not a clear dominant option in any single scenario.

The experimental design process relates to selecting the combinations of attribute levels (see Table 2) for each given choice scenario, for example leading to the scenario presented in Fig. 1. We used a D-efficient experimental design with uninformative priors that only recognise the directionality of the CO_2 (positive) and frequency (negative) attributes. This design process generates meaningful trade-offs while also maintaining attribute-level balance in the data (Rose and Bliemer, 2014). The design included 16 choice scenarios that were subdivided into four blocks of four scenarios each (with one block randomly assigned to each respondent). For a detailed introduction to experimental design see Bliemer and Rose (2010).

We now look at these separately for E1/E2 and E3 and explain the experimental design at the same time.

For E1/E2, the attributes were the frequency of conducting the activity, the resulting CO_2 reductions, and the equivalent number of trees (see Table 2). For the frequency of conducting an activity, two levels were used (three times per week or every day), where these were varied independently across the actions. Two levels of CO_2 reductions were used, where the levels differed between the actions, with corresponding (but not perfectly correlated) differences in the number of trees that would need to be planted. The levels correspond to the maximum, minimum and average level of mitigation potential of CO_2 if a change is made in the routine in terms of the consumption options shown for transport, vegan and vegetarian meals. That information was taken from a literature review on this subject (cf. Ivanova et al., 2020). The higher levels of CO_2 emission reduction are associated with a higher frequency of the activity (carrying out the activity every day) while the lowest correspond to a lower frequency (carrying out the activity for three days

[1] *https://climate.selectra.com/en/news/co2-tree*

[2] *https://data.worldbank.org/indicator/EN.ATM.CO2E.PC?locations=GB*

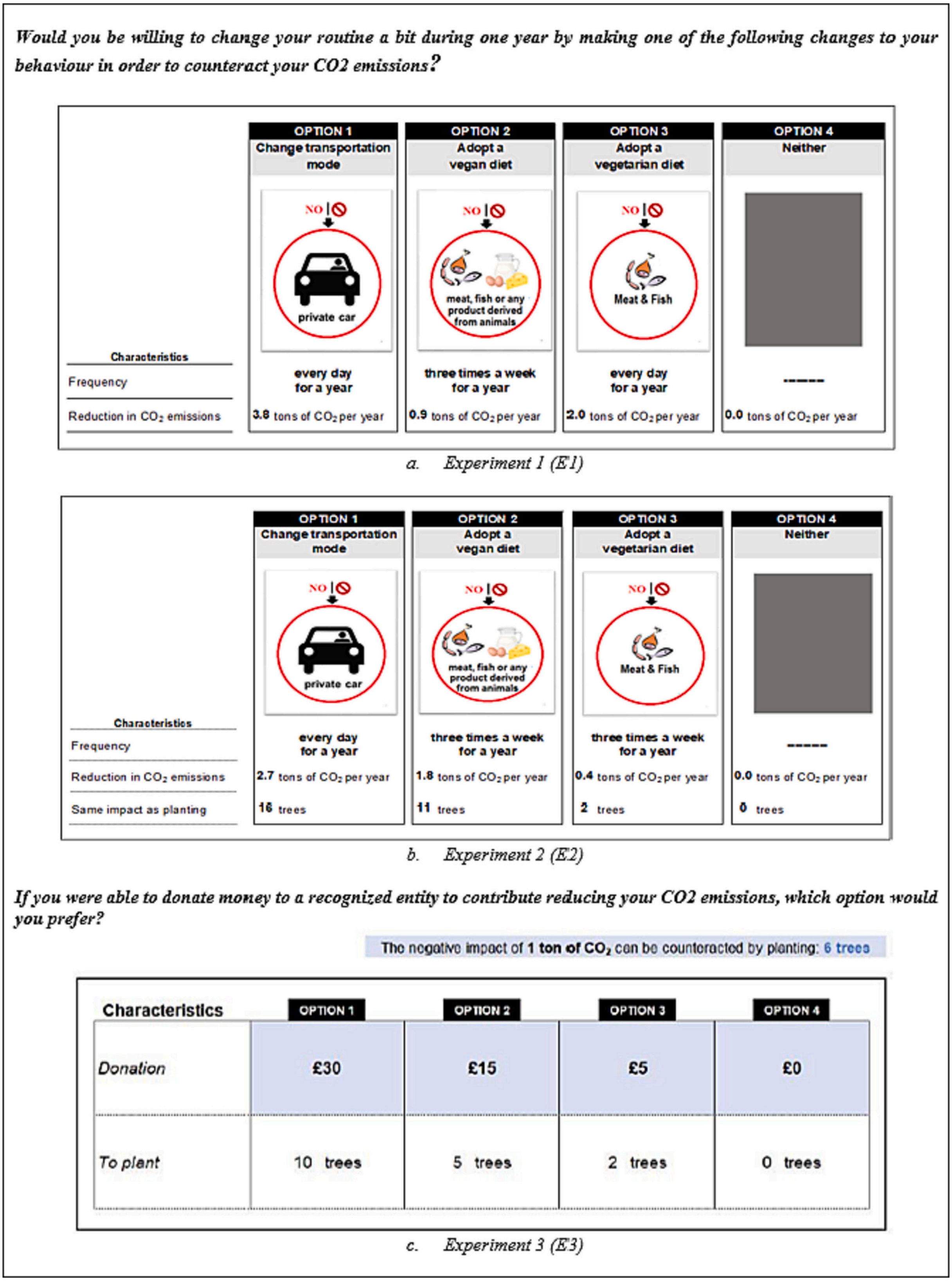

Fig. 1. Example choice scenarios.

Table 2
Attributes and levels.

	Attributes	Levels	OPTION 1	OPTION 2	OPTION 3	OPTION 4
EXPERIMENTS E1 & E2			Change transportation mode	Adopt a vegan diet	Adopt a vegetarian diet	Neither
	Frequency	1	Three times a week for a year	Three times a week for a year	Three times a week for a year	n/a
		2	Every day for a year	Every day for a year	Every day for a year	n/a
	Reduction in CO_2 emissions	1	0.9	0.9	0.4	0
		2	1.5	1.8	0.8	0
		3	2.7	2.5	1.5	0
		4	3.8	3.2	2	0
	Same impact as planting	1	5 trees	5 trees	2 trees	0 trees
		2	9 trees	11 trees	5 trees	0 trees
		3	16 trees	15 trees	9 trees	0 trees
		4	23 trees	19 trees	12 trees	0 trees
		Version	OPTION 1	OPTION 2	OPTION 3	OPTION 4
EXPERIMENT E3	Donation (Pounds)	1	£0	£5	£15	£25
		2	£0	£5	£15	£30
		3	£25	£15	£5	£0
		4	£30	£15	£5	£0
	To plant in one year	1	0 trees	2 trees	5 trees	8 trees
		2	0 trees	2 trees	5 trees	10 trees
		3	8 trees	5 trees	2 trees	0 trees
		4	10 trees	5 trees	2 trees	0 trees

in a week).

For the donation experiment (E3), the attributes used to describe the four possible donations were the amount donated (in £) and the number of trees that would be planted. For each attribute, four levels were used, where there were two different versions of the ranges that differed only for the highest level (£25 vs £30, and 10 trees vs 8 trees). Respondents were allocated randomly to one of four versions of the experiment, where these differed in terms of which order of donations (increasing and decreasing) was used, and what the maximum level of donation/trees presented was. The data provided in the donation part (E3) was based on real information taken from the National Trust.[3]

2.2. Model specification and estimation

2.2.1. Introduction

Econometric methods belonging to the family of discrete choice models (Train, 2009), and specifically those based on the theory of random utility (cf. Marshack, 1960) were used to analyse the choices of green actions and donations. These models seek to mathematically explain the choices in individual scenarios, on the basis of the attributes describing those scenarios. In addition, we also incorporate the role of the order of the experiments and consider the impact of the way in which information is presented to participants. By estimating parameters to study the impact on choices of exposure to earlier scenarios, we thus measure spillover effects. Of course, this study does not focus solely on the analysis of spillover but considers it as part of the drivers of individual behaviour, thus helping us to disentangle the different influences on choices.

The data used in this analysis come from two separate types of experiments, a set of stated choice (SC) scenarios in which respondents faced choices between different types of CO_2 reducing actions (or no action), split into experiments E1 and E2 above, and a single donation scenario, described as experiment E3 above. As discussed earlier, the CO_2 scenarios were further divided into scenarios describing environmental benefits solely in terms of CO_2 reductions (E1), and those quantifying those reductions in terms of the equivalent number of trees (E2), while for the scenarios about donations, the order of the levels of donation and the maximum donation were varied across respondents. Both of these factors influence the model specification, as outlined below.

[3] https://www.nationaltrust.org.uk/features/plant-a-tree

The dependent variables in the two overall types of experiments are of different types, requiring the use of a different econometric approach. For experiments E1 and E2, the choice is between four mutually exclusive alternatives (car, vegan, vegetarian, no action) without a clear ordering, motivating the use of a multinomial choice model. On the other hand, for experiment E3, the dependent variable is ordinal (going from no donation to the highest presented level), motivating the use of an ordered choice model. We will now look at the specification of the two structures in turn before turning to model estimation.

2.2.2. Specification of model for CO_2 experiments

Let U_{jnt} be the utility of alternative j for individual n in task t, where this is decomposed into two components, namely V_{jnt} (often referred to as the deterministic or observed part of utility) and a type I extreme value (also known as Gumbel) distributed error term, ε_{jnt}, such that

$$U_{jnt} = V_{jnt} + \varepsilon_{jnt} \tag{1}$$

We have that j is an index of alternatives, which in order are (1) car, (2) vegan, (3) vegetarian, and (4) no action. For the first three alternatives, we have that the utilities are given by:

$$V_{jnt} = \delta_{jn} + \Delta_{everyday,jn} \cdot x_{daily,jnt} + \beta_{log-CO2,nt} \cdot log\left(x_{CO_2\ reduction\ per\ day,jnt}\right) + \xi_{jn}. \tag{2}$$

We will now look at the individual terms in turn:

- δ_{jn} is an alternative specific constant for action j, where the subscript n relates to the fact that we capture deterministic heterogeneity across individual respondents, linked to their socio-demographics;
- $\Delta_{everyday,jn}$ is a shift in the utility for action j for person n (again allowing for heterogeneity) if this activity it has to be performed every day instead of just three days a week (where the attribute $x_{daily,jnt}$ is equal to 1 if action j has to be performed on a daily level in task t for person n);
- $\beta_{log-CO2,nt}$ is the marginal utility parameter associated with the natural logarithm of the daily CO_2 reductions[4] for action j in task t for person n ($x_{CO_2\ reduction\ per\ day,jnt}$), where the subscripts n and t now

[4] The use of a logarithmic transform implies decreasing marginal returns, i.e. each additional unit of CO_2 reductions matters less than the one before. The use of such a non-linear specification was found to improve model fit and parameter robustness and the finding about non-linear utilities is common in many applications, supported by evidence in behavioural economics.

relate to the fact that we allow for heterogeneity across respondents as well as for order effects, a point we return to below; and
- ξ_{jn} is a random disturbance capturing correlations across choice tasks for the same individual, which is distributed identically but independently across alternatives and individuals (but constant within individuals) with an estimated standard deviation σ.

The utility for the no action alternative is simply given by:

$$V_{4nt} = \delta_4 + \xi_{4n}, \tag{3}$$

where, for identification, δ_4 is fixed to zero (meaning the utilities for the three actions are expressed relative to no action).

One term in Eq. [2] that warrants further attention is the parameter for CO_2 reductions, where this was specified in such a way as to capture the impact of experiment type and order. In particular, we have that:

$$\beta_{log-CO2,nt} = \beta_{log-CO2,base,n} \cdot \left(\sum_{s=1}^{5} \kappa_{s,E1} \cdot x_{c_s,E1,nt} + \kappa_{c,E2} \cdot x_{c_s,E2,nt} \right) \tag{4}$$

This specification allows for different multipliers on the base CO_2 sensitivity by experiment (E1 vs E2) and by 5 different "cases" relating to the order of presentation of experiments (for full details, see Table 3), where we use E1 as the base after previously facing E2 (i.e. $\kappa_{2,E1} = 1$ in Table 3). The subscript n on $\beta_{log-CO2,base,n}$ relates to the incorporation of deterministic heterogeneity across individuals, linked to observed socio-demographics.

With this specification, the probability of individual n choosing alternative i (with $i = 1, \ldots, 4$) in task t is given by:

$$P_{int} = \frac{e^{\mu_n V_{int}}}{\sum_{k=1}^{4} e^{\mu_n V_{knt}}}, \tag{5}$$

where

$$\mu_n = \mu_{wave1} x_{wave1,n} + \mu_{wave2} x_{wave2,n}. \tag{6}$$

This scale term μ_n captures differences between respondents in error variance of utility depending on the data collection wave, where $x_{wave1,n}$ is equal to 1 if respondent n was sampled in data collection wave 1 (and zero otherwise), with a corresponding specification for $x_{wave2,n}$. We normalise μ_1 to 1, meaning that the estimate of μ_2 relates to the relative scale (which is inversely proportional to the variance of ε_{jnt}) in data collection wave 2 relative to data collection wave 1.

It should be noted that Eq. [5] is conditional on the random term ξ_{jnt} (for $j = 1, \ldots, 4$) included in $V_{jnt}, \forall j$, and the likelihood of the observed sequence of choices for person n is thus given by integration over the distribution of ξ_{jnt}, such that, with j^*_{nt} being the alternative chosen by individual n in task t, we have:

$$LC_n = \int_{\xi_n} \prod_{t=1}^{T} P_{j^*_{nt}} f(\xi_n) d\xi_n \tag{7}$$

2.2.3. Specification of model for donation experiments

To model the response to the donation experiment (E3), we used an ordered logit model (cf. Greene, 2014), with the likelihood given by:

$$LD_n = \sum_{p=1}^{4} (D_n == p) \left(\frac{e^{\tau_{D_p,n} - \mu_n \lambda_n}}{1 + e^{\tau_{D_p,n} - \mu_n \lambda_n}} - \frac{e^{\tau_{D_{p-1},n} - \mu_n \lambda_n}}{1 + e^{\tau_{D_{p-1},n} - \mu_n \lambda_n}} \right) \tag{8}$$

where the term $(D_n == p)$ is equal to 1 if and only if respondent n answers with level p to the donation question D_n, where $p = 1,\ldots,4$. The τ_{D_p} parameters are thresholds to be estimated (with the normalisation that $\tau_{D_0,n} = -\infty$ and $\tau_{D_4,n} = +\infty$). All threshold parameters are generic across individuals for except for $\tau_{D_3,n}$, i.e. we have that $\tau_{D_l,n} = \tau_{D_l}, \forall n, l = 0, 1, 2, 4$. For the third threshold parameters, separate values were estimated depending on whether the highest level of donations was equal to £25 or £30, with

$$\tau_{D_3,n} = \tau_{D_3,£25} \cdot (x_{maxD,n} == £25) + \tau_{D_3,£30} \cdot (x_{maxD,n} == £30) \tag{9}$$

where $x_{maxD,n}$ shows the maximum donation level shown to respondent n. The expectation is in this case that the estimate $\tau_{D_3,£30} > \tau_{D_3,£25}$, meaning that when faced with the higher maximum donation, respondents are less likely to choose it.

The component λ_n captures the impact of the specific setup of the donation experiment as well as its position in the overall sequence of experiments, while μ_n is the same scale parameter as in Eq. [6]. In particular, we have that:

$$\lambda_n = \sum_{s=1}^{5} \lambda_s \cdot x_{c_s,D,n} + \lambda_{dec} \cdot x_{dec_D,n}, \tag{10}$$

where once again we have 5 specific ordering cases (as explained in Table 3), where the base this time is case 1, in which the donation experiment is seen before any CO_2 experiments. We have an additional shift term for the case where the donation levels are shown in decreasing (as opposed to increasing order), with any shift in the utility captured by λ_{dec} (multiplying the term $x_{dec_D,n}$, which is 1 if donations are presented in decreasing order for respondent n).

2.2.4. Model estimation

The estimation process for discrete choice models consists of finding the parameter values that best explain the choices in the data, where this is achieved by maximising the log-likelihood of the model.[5] In our case, we have two model components, namely the one explaining the answers to the action scenarios (with likelihood LC_n for person n given by Eq. [7]), and the one explaining the answer to the donation task (with likelihood LD_n for person n given by Eq. [8]). These two models share a single common parameter, namely $\mu_{wave\ 2}$, and we thus find parameter estimates by maximising the combined log-likelihood of both components at the sample level, given by:

$$LL = \sum_{n=1}^{N} log(LC_n \cdot LD_n) \tag{11}$$

An alternative approach would of course have been to estimate separate models by experiment type. This would have implied differences in utilities for the three actions between E1 and E2, but no such differences were found when comparing separate models. Differences in the response to CO_2 emissions between E1 and E2 are already captured in the joint model. The only parameter shared between E1/E2 and E3 is the scale parameter $\mu_{wave\ 2}$, and the fact that the estimate for this is no different from 1 (see later results) again means that separate and joint estimation would lead to the same findings.

Eq. [11] includes LC_n, which, as shown in Eq. [7] is given by an integral over the distribution of ξ. This integral does not have a closed

[5] Each observed choice has a probability in the model, and the log-likelihood is the sum across all observations of the logarithms of the probabilities of the chosen alternatives. The log-likelihood is used instead of the likelihood for numerical reasons, where the fact that the logarithm is a monotonic transform ensures that the maximum of the likelihood and log-likelihood is at the same parameter values. In a purely deterministic model the log-likelihood would be 0 (with all choices having a probability of 1), while in a purely random model, the log-likelihood would be $N \cdot log\left(\frac{1}{J}\right)$, where J is the number of alternatives. The latter is known as the log-likelihood at zero - LL(0). A measure of the goodness of fit of a choice model is given by the adjusted ρ^2 measure (McFadden, 1973), which shows how far estimation has moved from LL(0) toward a perfect model, with $adj.\rho^2 = 1 - \frac{LL(\beta) - K}{LL(0)}$, where $LL(\beta)$ is the log-likelihood at convergence, and K is the number of estimated parameters. While there are no absolute guidelines, values in the range of 0.2 to 0.4 are typically seen as providing a very good fit.

form solution, and we thus approximate the log-likelihood by using numerical methods using simulation techniques, and in particular the use of 500 Halton draws (cf. Bhat, 2001).

All our models were estimated using Apollo v 0.2.8 (Hess and Palma, 2019). The process of fitting a mathematical model to empirical data involves making decisions on model specification, i.e. which parameters to retain. This process considers (i) formal statistical tests, relating to whether new parameters lead to significant improvements in fit (likelihood ratio tests) and whether their estimates can reject the null hypothesis of no effect (t-ratios), and (ii) more informal tests such as examining the sign of the estimated coefficient, to judge whether it conforms to a priori notions or theory.

3. Results and discussion

3.1. Overview

Our specification search tested many different versions of the model adding sources of preference gradually and further flexibility to the model by capturing variation in preferences linked to socio-demographics, and the impact of exposure to earlier scenarios/experiments, i.e. the order of the components of the survey (i.e., CO_2 only, CO_2 with trees, donations).

The modelling work for this analysis followed a careful specification search, where we gradually removed parameters from a full specification of the model. Decisions on the removal of parameters, in particular the socio-demographic interactions, were based on statistical tests, not just in relation to the individual parameter tests of statistical significance (t-ratios), but also in terms of the impact on model fit of removing these parameters (likelihood ratio tests). This is standard practice in choice modelling with a view to avoiding a proliferation of parameters. It should be noted our analysis however did not simply impose some strict criteria but combined statistical significance with behavioural insights when searching for the final specification. We thus retained some parameters that did not strongly reject the null hypothesis of no effect, but which nevertheless provided us with behavioural insights. This final specification has a log-likelihood of −4189.07 and an adjusted ρ^2 of 0.31, offering the best fit of all specifications tested after accounting for the number of parameters.

The tables displayed in this section show the estimates for model parameters obtained by maximising the log-likelihood. To test statistical significance, we have used robust t-ratios against 0 for the main parameters (green action, frequency, CO_2 daily, and sigma), and robust t-ratios against 1 for parameters related to order effects (given that these were used as multipliers in the model, such that the base for comparison is 1).

We look in turn at separate sets of parameters that relate to individual findings of our analysis, focussing first on the findings independent of the presentation order.

Table 3
Detailed estimates green action.

General description	Estimate	Robust stand. error	Robust t-ratio (0)
(1) Action			
ASC no action	0.000	-fixed-	-fixed-
ASC vegetarian	4.162	0.43	9.72
… shift male	−1.548	0.33	−4.65
… shift age between 30 and 39 years	−1.040	0.43	−2.42
ASC car	3.291	0.32	10.17
… shift age between 30 and 39 years	−1.030	0.34	−2.99
… shift age over 59 years	−2.011	0.88	−2.28
ASC vegan	2.205	0.33	6.65
… shift male	−1.715	0.37	−4.66

<u>Result 1: People are willing to undertake emission reducing lifestyle changes.</u>

We first look at the estimates related to the baseline preferences for the different actions, shown in Table 3. An estimate with a negative sign denotes a reduction in utility (i.e., an undesirable attribute), while the opposite is true for a positive estimate. By definition, participants are more likely to choose alternatives that give them greater utility (cf. Train, 2009, Chapter 2).

The results shown in Table 3 reveal that, with "no action" as the base, making any change in the routine to reduce CO_2 emissions on three days a week causes an increase in individuals' utility, independently of which action is chosen, and across all socio-demographic groups, noting that, for each of the three actions, none of the negative interactions (or combinations of them) are large enough to change the sign of the overall effect. For both the main effects and the interactions, we report standard errors and robust t-ratios, where, for a two-sided test, the critical value for 95% confidence would be 1.

Significant differences in tastes were also found in some socio-demographic groups for the different green actions. First, note that the utility of making dietary changes by adjusting the usual eating pattern to move toward vegetarian or vegan alternatives is lower for men than women (4.162–1.548 = 2.614 vs 4.162 for vegetarian, 2.205–1.715 = 0.49 vs 2.205 for vegan). Second, the utility for moving to a vegetarian diet is lower for respondents aged between 30 and 39 years than for other age groups (a decrease by 1.04), while the utility for reducing car travel is also lower in that age group (a decrease by 1.03) and for the oldest group of respondents, i.e. those aged over 59 (a decrease by 2.011).When looking at the socio-demographic effects together, we can note the following through some simple combination of the estimates. For women, a move to a vegetarian diet is the option with the highest utility across all age groups. A reduction in the days where cars are used is the second most preferred option for all age groups for women, except those aged over 59, where the move to a vegan diet is preferred ahead of a reduction in car travel. For men on the other hand, a move to not using car three days per week is the action with the highest utility ahead of a move to a vegetarian diet, except in the over 59 group, where a vegetarian diet on three days per week has a higher utility than not using car. Across all age groups, a vegan diet has the lowest utility for men.

An interesting aspect to consider in these results is that different green actions require varying levels of commitment/effort. So, finding that the shift to a vegan diet three days a week has the lowest utility of the three actions overall is not unexpected, since this requires greater effort and a higher degree of commitment. In the case of sociodemographic influence, it could be speculated that the marked preference of men to reduce the use of private vehicles three times a week and of women to adopt a vegetarian diet instead could be related to the fact that women drive less than men (Department for Transport, 2023) and therefore prefer alternative options. Other explanations are possible, such as reductions in driving not being a feasible option for all individuals due to mobility restrictions, working hours, family obligations, ease of access to public transport, the distance to work, among others (Ho et al., 2020). The preference of individuals for changing diet to vegetarian is not surprising since this is a practice that has become more common in the daily routine in recent years for different reasons, including an environmental and healthy lifestyle (Kim et al., 2020; Rosenfeld, 2018) and also because it does not imply as many food restrictions as a vegan diet.

So far, we have looked only at the baseline utilities for different types of green actions. Of course, the impact on utility provided by each option is also influenced by the attributes or characteristics that describe it. Therefore, we now proceed with a description of the impacts of these characteristics.

<u>Result 2: Emission-reducing actions that have to be performed rigorously every day yield lower utility than the same actions performed on three days a week.</u>

Table 4
Detailed estimates considering frequency.

General description	Estimate	Robust stand. error	Robust t-ratio (0)
(2) Frequency			
2.1. vegetarian			
shift for vegetarian every day	−0.634	0.16	−3.86
… age between 50 and 59 years	−0.918	0.44	−2.10
… age over 59 years	−2.095	0.78	−2.69
2.2. car			
shift for car every day	−0.411	0.26	−1.60
… male	−0.814	0.32	−2.54
… age between 50 and 59 years	−1.853	0.50	−3.70
… age over 59 years	−1.399	0.80	−1.76
2.3 vegan			
shift for vegan every day	−0.707	0.17	−4.07

The experiments include a frequency attribute that captured the differential impact on utility of having to conduct an activity seven times a week as opposed to only on three days. The corresponding estimates are shown in Table 4. Result 2 is obtained by noting that having to take the same action every day decreases the utility of all actions and all individuals. While the positive effects found in Table 3 show that people would like to take an action, we now note that this willingness decreases when having to take that action every day. This is again related to the level of effort and commitment, this result being in line with previous findings on the negative effect of behavioural costs and behavioural difficulty (Brick et al., 2017; Truelove and Gillis, 2018; Vesely and Klöckner, 2018).

Socio-demographic characteristics also play a role in how frequency affects the utility of making changes in one's routine. Changing to a vegetarian diet every day of the week is less attractive than doing so on only three days for all individuals, but compared to the reference group this effect is much stronger in individuals over 50 years, especially for individuals over 59 years (−2.729 vs −0.634). When looking at reducing car travel, having to do so on a daily basis has a more negative impact on the utility for male respondents and for those aged over 50 years, where this impact is strongest for individuals between 50 and 59 years.

One contribution we make to the literature here is that we manipulate behaviour costs in terms of the required time commitment/strict adherence to a type of behaviour, which usefully complements previous studies relying for example on difficulty self-reports (Brick et al., 2017), contextual proxies (Diekmann and Preisendörfer, 2003; Keuschnigg and Kratz, 2018), or expert assessment (Andersson and von Borgstede, 2010). The best way of operationalizing behaviour costs is still debated, and in all likelihood, a combination of different approaches, including operationalizing it as time commitment, will lead to the most reliable conclusions.

Result 3: Participants prefer lifestyle changes leading to greater CO_2 reductions.

The next attribute we look at relates to CO_2 reductions. Before describing the results, it is important to clarify that although carbon labelling can influence some cases of daily decision making (Brunner et al., 2018; Rondoni and Grasso, 2021), this should not be taken for granted since it is something that does not occur in all cases (Ran et al., 2022; Soregaroli et al., 2021), perhaps due to misperceptions that people may have about carbon emissions (e.g., Holmgren et al., 2019; Truelove and Parks, 2012; Wynes et al., 2020). In our study, the emission mitigation potential of each alternative was clearly stated, which may have contributed to these mitigation potentials being able to influence choices; when emission reduction potentials are more opaque, it is likely that they will exert less of an influence (Emberger-Klein and Menrad, 2018).

Table 5 reports the detailed estimates for the impact on utility of the logarithm in daily CO_2 reductions.[6] The positive sign of the parameter associated with the logarithm of daily CO_2 reductions (equal to 1.478) shows that an increase of one unit of CO_2 abatement increases the utility for the associated action. This is relevant as it reflects the increased likelihood of respondents choosing options associated with greater emission reductions. This finding is consistent with research on perceptions of positive environmental impact being an important motivational driver (e.g., Truelove and Parks, 2012). While the impact on utility of CO_2 reductions is positive for all individuals, it is lower for individuals between 30 and 49 years.

Table 5
Detailed estimates daily CO_2 reductions.

General description	Estimate	Robust stand. error	Robust t-ratio (0)
(3) Log of daily CO_2 reductions			
CO2 daily log	1.478	0.26	5.58
… age between 30 and 39 years	−0.698	0.26	−2.73
… age between 40 and 49 years	−0.687	0.24	−2.85

3.2. Impact of order and presentation mode

So far, we have focussed on the preferences without considering the impact of presentation. Even though estimating the CO_2 parameter is on its own highly relevant in the context of our study (see results above), we have gone beyond this point to also determine (i) the effect of the order of presentation of experiments, (ii) the way how the CO_2 attribute is displayed, and (iii) possible spillover caused by the inclusion of the donation experiment.

Given that we have used three different experiments (E1: no Trees, E2: Trees, and E3: Donations – as explained in Section 3), we consider for each experiment five "cases", depending on where in the sequence of the

Table 6
Detailed estimates effect of order and presentation mode.

General description	Multiplier of CO_2	Robust stand. error	Robust t-ratio (1)
(4) Effect of order and presentation mode			
4.1 Experiment 1 (E1) – presentation format without trees shown			
Case 1: no prior experiments faced	0.603	0.14	−2.93
Case 2: after facing the experiment with trees (base)	1.000	-fixed-	-fixed-
Case 3: after facing the experiment about donations	0.604	0.17	−2.36
Case 4: after facing the experiment with trees and then donations	0.718	0.15	−1.87
Case 5: after facing the experiment about donations and then the experiment with trees	0.868	0.19	−0.69
4.2 Experiment 2 (E2) – presentation format with trees shown			
Case 1: no prior experiments faced	0.722	0.10	−2.90
Case 2: after facing the experiment with NO trees	0.856	0.17	−0.87
Case 3: after facing the experiment about donations	0.753	0.20	−1.26
Case 4: after facing the experiment with NO trees and then donations	0.617	0.16	−2.34
Case 5: after facing the experiment about donations and then the experiment with NO trees	0.552	0.16	−2.75

[6] The rationale for this specification was elaborated on in Section 2.2.2.

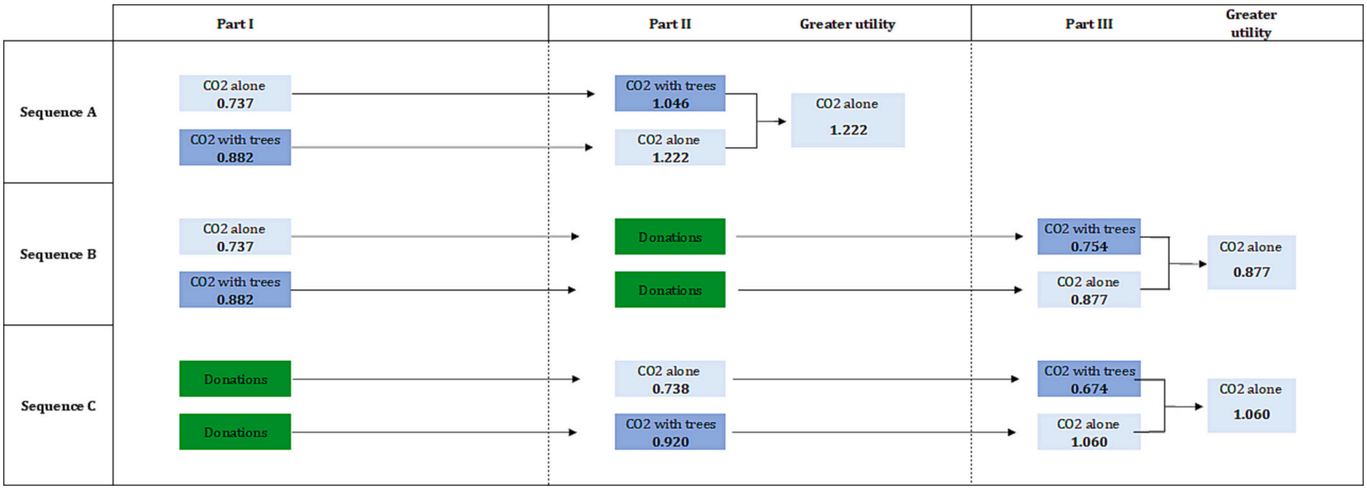

Fig. 2. Sequence of experiments and marginal utility considering weighted average of the utilities across age groups. *Light blue – experiment 1 (E1); Dark blue – experiment 2 (E2); Green – experiment 3 (E3).*

three experiments it was faced by participants. For each experiment, Case 1 is the situation where the experiment is faced first in the sequence, while Case 5 is the situation where it is faced last out of the three experiments. These effects are captured by multipliers on CO_2 utility, as shown in Eq. [4].

Detailed estimates of the effect of order and presentation mode on the sensitivity to CO_2 reductions are shown in Table 6. While there are some cases where we cannot reject the null hypothesis of no effect, overall, it can be noticed that the order, the context and the information that is displayed affect behaviour in both experiments and do so differently between the two experiments. As we now look at multipliers, the reference value is 1 rather than 0, and this is reflected in statistical tests against a base value of 1.

Each case will be explained in detail in the description of the Results 5 and 6 where the effect of the variation of tastes for the different sociodemographic groups will be also included.

To further visualize these findings, Fig. 2 looks at the key sequence effects of experiments. To better understand Fig. 2, it is relevant to note that the graph is divided into four vertical panels. The first panel shows the type of sequence being analysed, and the remaining panels concern the findings at different steps in that sequence. The first sequence (A) shown in Fig. 2 is the easiest to understand, as it depicts a single experiment. There, we see that when faced as a first experiment, seeing CO_2 with trees generates a higher utility than seeing CO_2 on its own (described in detail in Result 4 below). In particular, Part II in Sequence A in in Fig. 2 shows how the utility for CO_2 reductions changes in the second experiment, as a function of the format of the experiment. The same rationale applies for Sequences B and C, where we additionally look at the utilities for CO_2 reduction in subsequent experiments i.e., after seeing a CO_2 experiment and the donations experiment, in either order. Note that, as the "cases" in Table 6 are experiment-specific, the three "sequences" in Fig. 2 should not be confused with these cases that incorporate sociodemographic effects.

Result 4: Preferences for lifestyle changes leading to greater CO_2 reductions become descriptively stronger when CO_2 reduction potentials are displayed in an illustrative format.

Our model incorporates heterogeneity in preferences across different subgroups of the population, where for CO_2 reductions, we found differences as a function of age (cf. Table 5). To analyse the overall behavioural patterns, we proceed with a weighted average of the results across age groups, using the weights from the sample used in estimation.

To test if competing demands (multiple demands) in consumers affect pro-environmental behaviour, we first look at the effect of the format in both experiments related to green actions. We establish this first result by comparing Case 1 of experiments E1 and E2, which is the situation where no other experiment is faced before.

Results for the baseline in which individuals had not been exposed to earlier experiments show a stronger influence of CO_2 reductions when the presentation format includes the CO_2 equivalence in planted trees (0.882 vs 0.737, shown in Fig. 2, Part I, Sequences A and B), although this result needs to be viewed as suggestive as we cannot reject the null hypothesis of no difference. However, we report this result since both discovering a significant effect (in either direction) or a non-significant effect of the presentation format should be considered a relevant data point in the context of the broader literature and could be useful to understand if the spillover effect caused by the exposure of the experiment of donations is weaker or stronger.

Additionally, looking at Sequence C in Fig. 2, where subjects have participated in the donations experiment, but have not faced a prior CO_2 experiment, leads to the same conclusion, i.e., participants' preferences for lifestyle changes leading to greater CO_2 reductions become descriptively stronger when CO_2 reduction potentials are displayed in an illustrative format (coefficients on CO_2 reductions of 0.738 and 0.920, respectively). This is consistent with an investigation carried out by Zheng et al. (2021) which suggests that ecological compensation does have spillover effects.

These headline results relate to weighted averages across age groups. Fig. 3 shows the separate effect of different ages on the perceived benefit of green actions that lead to greater CO_2 reductions. Looking at the baseline (Case 1 for E1 and E2), the estimates show that individuals under 30 and over 49 years perceive a greater utility than individuals in other age categories, where this effect being slightly stronger in the experiment with trees (see Case 1 in Fig. 3).

Result 4 could thus be seen to give tentative support to the idea that presentation format can influence decisions, in our case by making it easier to visualize the decisions' real-life impact in the Trees condition, see e.g. Asensio and Delmas (2015); Rodemeier and Löschel (2021). However, looked at more critically, Result 4 is consistent with findings of limited or no relevance of presentation format (e.g., d'Adda et al., 2022; Wolske et al., 2018).

Result 5: The strength of preferences for lifestyle changes leading to greater CO_2 reductions is affected by participants' previous exposure to choice situations.

Results 5 and 6 are the core of our research, especially Results 5b and 5c that involve the spillover effect produced by the intervention. Below, we report three specific instances of Result 5:

a) *The strength of preferences for lifestyle changes leading to greater CO_2 reductions increases as a result of facing previous scenarios about lifestyle changes.*

Support for this result comes mainly from comparing the magnitude of estimates for the impact of CO_2 reductions in Case 1 and Case 2, independently of whether we look at experiments E1 or E2. Note that exposing individuals to a second set of decision screens with CO_2 reductions has a positive influence on the utility/benefit perceived by the individuals in the subsequent experiment (see Sequence A in Fig. 2), independent of which order is used (E1 then E2, or E2 then E1). However, it is important to highlight that the impact depends on the type of information/choice-tasks that were shown. For example, when the first choice tasks shown were from the experiment with trees and then the experiment without trees, the utility of the experiment without trees goes from 0.737 to 1.222 when compared to seeing E1 first, while in the opposite case, the utility of the experiment with tree equivalence increases from 0.882 to 1.046 when compared to seeing E2 first. Both results show that preferences appeared to be more strongly influenced by CO_2 reduction potentials of available behavioural alternatives when subjects already had previous experience with making decisions about lifestyle changes.

In Fig. 2, note also that the greatest impact on the perceived utility/benefit by individuals is given by Case 2 in E1 (1.222), i.e., the choice tasks where the potential CO_2 reduction without the equivalence of trees is shown after making choices in the tasks that showed the equivalence in trees (sequence A, part II). That means that although respondents did not see the equivalence in trees in the second experiment/set of tasks, they were more sensitive or aware of the changes in CO_2 that could be achieved by choosing the actions.

Although the findings have wide confidence intervals, our results lead us to consider that competing demands could affect choices and imply a learning effect or a spillover effect given the subject's exposure to another choice experiment, whereby making choices involving the possibility of CO_2 reductions makes people more likely to consider and value CO_2 reductions in subsequent choices. This is in line with evidence of positive spillover across environmentally relevant decisions (Galdeano-Gómez et al., 2008; Geiger et al., 2021; Maki et al., 2019; Truelove et al., 2014) and broadly consistent also with the behavioural economics literature on learning across decision settings (e.g., Cooper and Kagel, 2008; Sausgruber and Tyran, 2005).

b) *Learning effect and spillover. The strength of preferences for lifestyle changes leading to greater CO_2 reductions decreases as a result of previous opportunities to donate to offset carbon emissions.*

One of our key results is the finding of potential spillover effects on choices. Remember that we define spillover as an effect of an intervention (donation experiment) performed to encourage behaviour change on the subsequent behaviour not targeted. Support for this result comes from comparing instances where participants first faced hypothetical opportunities to donate to offset carbon emissions to equivalent cases without prior donation opportunities (Fig. 2, Sequence B, Part II and Sequence C, Parts II and III). Specifically:

In sequence B, Parts I and III, note that the donation experiment did not cause a spillover effect. The effect that is observed can be associated more with a learning effect. So, for example, when comparing the marginal utilities of the experiments before and after the donations, no clear differences are observed, whereas sequence A showed an increase.

In sequence C, Parts II and III, if we compare first the utilities in Part II of Sequence C with the utilities of the experiments without any intervention (Part I of sequences A or B), it can be concluded that there is no spillover effect on the subsequent experiment (CO_2 alone 0.738 vs 0.737 and CO_2 with trees 0.920 vs 0.882). However, when comparing the utilities in the same sequence but in Part III with the utilities of the experiment that considers the same order (Sequence A, which tests the format effect), a number of observations in relation to behavioural spillover can be made. First, we observe a negative spillover effect on the experiment without trees (E1) with a drop of 0.162 (1.060 vs 1.222). Second, a negative spillover effect is observed in the experiment with trees (E2) with a drop of 0.372 (0.674 vs 1.046), which is greater than the drop in experiment with no trees (E1). In the second case, we can for the first time note a decrease in the utility of the experiment with the format that shows the equivalence of CO_2 in trees.

From these findings, it could be speculated that priming of financial concerns in the donations experiment temporarily weakened pro-environmental and pro-social motives, subsequently leading to less pro-environmental choices (Lindenberg and Steg, 2007; Stajkovic et al., 2022). This result should nevertheless be considered only as suggestive due to the lack of statistical significance (similarly see Capaldi and Zelenski, 2016). An alternative explanation is that that respondents may feel that they have already done their share of contributing to the environment by donating money and no longer need to change their behaviour.

c) *Variation in tastes across different subgroups of the population, where for CO_2 reductions, we found differences as a function of age.*

Fig. 3 shows the utility patterns of the five cases analysed but considering different age groups for the two experiments about green actions. The overall trends remain the same as the tendencies already

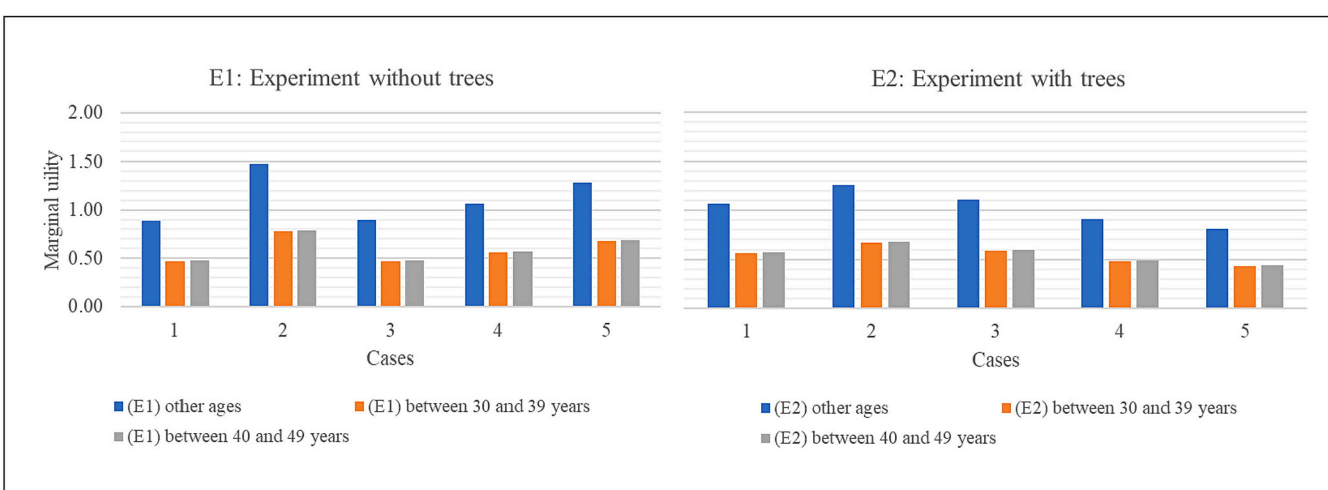

Fig. 3. Marginal utility of the different cases considering different age groups.

described in Result 5b, in which the weighted average of the utilities across age groups was considered. However, unlike what was previously described, it is shown here that these trends may be more marked for different age groups. For example, in sequence A, the gap between utilities when the individual faces Case 1 vs Case 2 (Sequence A in Fig. 2) is larger for individuals under 30 and over 49 regardless of the presentation format (E1, E2).

Other more specific differences can be observed too. First, in E1 for all ages, the largest utility for lifestyle changes leading to greater CO_2 reductions occurs after facing the experiment with trees (Case 2 in Fig. 3), followed by Case 5 (in which the individual faces the experiment about donations and then the experiment with trees). On the other hand, in E2, the largest utilities are presented again for Case 2, but this time followed by Case 3 (after facing the experiment about donations). Second, the utilities vary across the different age groups, and are lowest for individuals between 30 and 49 years of age. Finally, across all age groups, the utility in the experiment without the equivalence of CO_2 reduction in trees (E1) is lower than in experiment 2 only in Cases 1 and 3, which refer to the case where no prior experiments were faced and the case after facing the experiment about donations, respectively.

3.3. Impact on donations

For the model that seeks to explain willingness to donate money to counteract CO_2 emissions, we can study the impact of three experimental settings, namely: (i) the position in the sequence of experiments for the donation choice task, (ii) whether the possible levels of donations were shown in increasing or decreasing order, and (iii) whether the highest level was set to £25 or £30. Table 7 shows the estimates for the ordered logit model. We look below in detail at the behavioural findings, noting first that, as required by an ordered logit model, the threshold parameters are monotonically increasing, showing that a higher latent utility for donating would be required to choose a higher level of donation.

<u>Result 6: Experiments related to green actions (E1 and E2) and the order in which all the experiments were displayed did not influence donations.</u>

Our models also made provision for impacts of exposure to the CO_2 experiments on subsequent donation experiments, but as can be seen from Table 7, no such effects were observed, with no single impact being able to reject the null hypothesis of no difference at any reasonable level of confidence (all t-ratios <1.54).

<u>Result 7: Displaying donation options in decreasing (vs increasing) order leads to higher willingness to donate, especially for individuals between 40 and 49 years.</u>

Statistical evidence showing that the order of donation levels matters is reported in Table 7. This appears to be an instance of anchoring (see Epley and Gilovich, 2006; Tversky and Kahneman, 1974), where participants use the suggested choice options that they see first as anchors or cues for how much to donate. Our finding suggests that the usual display of donation options on fundraising websites (from lowest to highest) may lead to lower donations than the same display in reverse order. Follow-up field experiments can more accurately establish the practical relevance of this finding.

<u>Result 8: Displaying a higher maximum donation impacts the willingness to donate.</u>

Offering a higher maximum donation level (£30 vs £25) reduces the likelihood of choosing the maximum donation, as expected (threshold increases from 1.443 to 1.558 (as shown in Table 7). This however can still be counteracted overall by the fact that for those respondents who are willing to make that higher level of donation, the donation increases from £25 to £30.

To better understand these three findings for the donation experiment, we used the estimated model to make predictions of the expected level of donation under different scenarios, using the estimates from Table 7, and notwithstanding the fact that cases did not significantly impact donations. Table 8 shows the predicted donations as a function of

Table 7
Detailed estimates effect on experiment 3.

General description	Estimate	Robust stand. error	Robust t-ratio (0)
4.3 Experiment 3 (E3) – impact on donations			
Case 1: no prior experiments faced	0.000	-fixed-	-fixed-
Case 2: after facing the experiment with NO trees	0.239	0.23	1.03
Case 3: after facing the experiment with trees	0.167	0.20	0.83
Case 4: after facing the experiment with NO trees and then with trees	0.222	0.22	1.03
Case 5: after facing the experiment with trees and then with NO trees	0.316	0.21	1.54
Impact of seeing donations in decreasing order	0.194	0.13	1.48
...shift age between 40 and 49	0.537	0.21	2.50
Threshold 1 (£5)	−2.184	0.20	−10.95
Threshold 2 (£15)	0.309	0.16	1.88
Threshold 3 version 1 (£25)	1.443	0.19	7.74
Threshold 3 version 2 (£30)	1.558	0.19	8.27

Table 8
Predicted donation levels by Case.

	Donations shown in <u>increasing</u> order		Donations shown in <u>decreasing</u> order	
	Maximum at £25	Maximum at £30	Maximum at £25	Maximum at £30
Donation choice task…				
Case 1: … before any other experiment	£11.30	£12.09	£12.71	£13.75
Case 2: … after seeing E1	£11.83	£12.70	£13.26	£14.39
Case 3: … after seeing E2	£11.46	£12.28	£12.89	£13.95
Case 4: … after seeing E1 and E2	£11.74	£12.60	£13.17	£14.28
Case 5: … after seeing E2 and E1	£12.22	£13.16	£13.67	£14.87

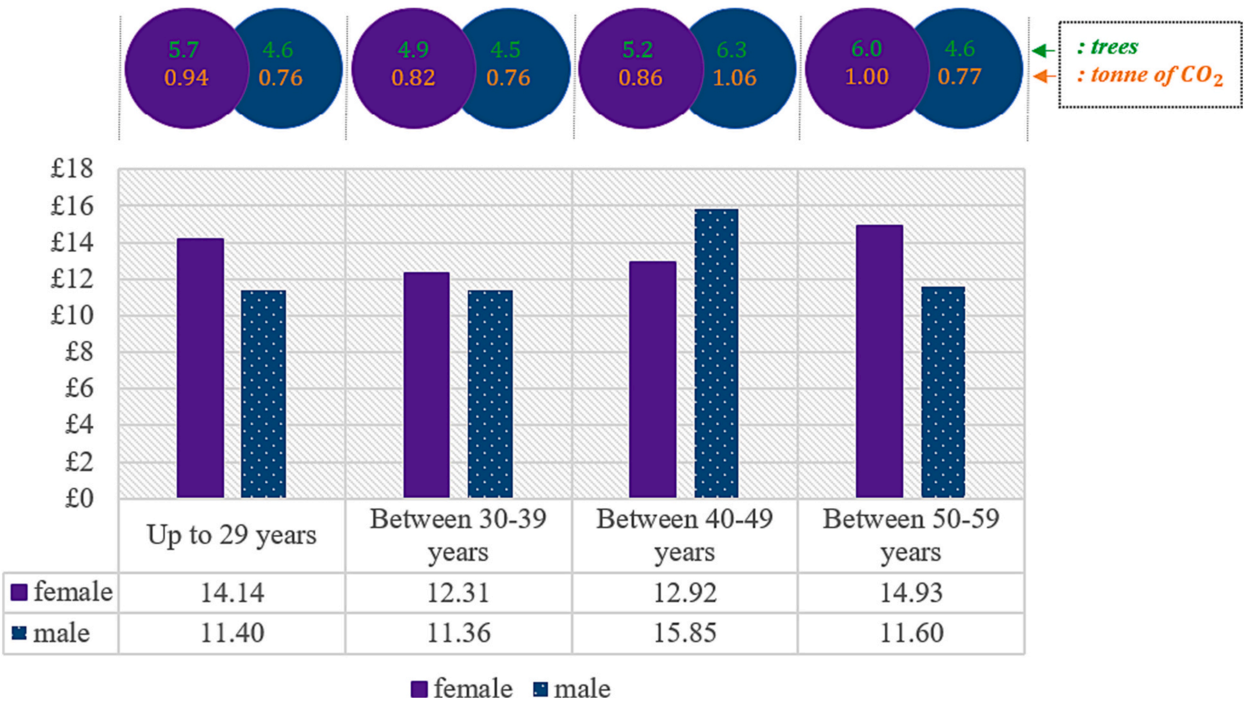

Fig. 4. Average donation levels by socio-demographic profile and equivalence in trees and tonne of CO_2.

the order of experiments, the use of increasing and decreasing levels, and depending on whether £25 or £30 is used as the highest level. In monetary terms, the expected donation differs slightly between cases, with donations lower when the donation experiment is shown before any other. Note that the donations are slightly higher after facing the two experiments related to green actions (both E1 and E2). Also, showing a higher maximum lead to a small additional increase in expected donations (e.g. Table 8, Case 4, with donations shown in increasing order, the expected donation goes from £11.74 to £12.60). Similarly, the expected donation is higher when donations are shown in decreasing order (e.g. Case 4 with a maximum set to £30, the expected donations increase from £13.16 to £14.87 when shown in decreasing as opposed to increasing order).

From a policy perspective, it is of course also of interest to understand how potential donation intentions vary across different socio-demographic groups. While this is not at the heart of the present analysis, Fig. 4 presents an overview of how average donations (across all survey presentation formats) vary by age and gender. Across most age groups, a greater willingness to donate is evident among women in comparison to men. However, this trend deviates for men aged between 40 and 49 years, who exhibit the highest average willingness to donate in our sample, at £15.85. Finally, Fig. 4 also shows the impact of these donations in terms of their benefits for potential afforestation efforts and the consequential mitigation of CO_2 emissions.

3.4. Additional results relating to model structure (error variance and correlation)

Data collection date: We tested for differences across the two times of data collection in the variance of the error terms in the logit model used for the choice between actions. This is achieved by estimating a scale parameter for one of the two waves and fixing the scale parameter for the other wave to 1, where, for a type I extreme value distribution, the variance is inversely proportional to the scale parameter. Using data collection date 1 as the base, no statistically significant differences were found between the two data collection dates with an estimate of $\mu_{wave\ 2}$=1.148, and a t-ratio (against 1) of 1.49. This indicates that the level of noise in behaviour (from the perspective of the analyst) is similar for the two data collection dates.

Correlation across choices: Our model included a normally distributed error term to capture correlation across choices in E1 and E2. We obtain an estimate of $\sigma_{\xi} = 2.68$ for the standard deviation for that error term. With a t-ratio (against 0) of 16.31, we can convincingly reject the hypothesis that this is zero, showing evidence of a pseudo panel effect. This indicates a strong correlation in the error terms across the different scenarios for the same respondent in the model explaining the choice between actions.

4. Conclusions and limitations

4.1. Conclusions

The flexibility of the discrete choice models and stated preference tools has allowed us to test the influence of competing demands in three partially related experiments and specifically the influence of this on choices. The donation experiment was included as an intervention to encourage behaviour change on subsequent behaviour. By jointly accounting for formatting, order effects, learning effects and variation of tastes, as well as capturing correlation across choices and potential differences in error across data collection waves, we can with more certainty interpret our findings without concern of misattribution.

Using a stated choice survey in which participants faced a series of decisions about lifestyle changes and about donations to offset carbon emissions (our experiments E1, E2 and E3), we uncovered a number of typically small effects that suggest a causal impact on pro-environmental behaviours (or stated preferences for these behaviours, in our case) due to earlier exposure to related choice contexts. Specifically, we found that preferences for emission reducing lifestyle changes (Result 5a) and for carbon offsets became stronger for participants who were previously faced with decisions about lifestyle changes (evidence of positive spill-over). In contrast, preferences for emission reducing lifestyle changes became weaker for participants who were previously faced with scenarios involving hypothetical donations to offset carbon emissions (evidence of negative spillover, see Result 5b).

That the above effects are small is consistent with the broader literature on pro-environmental behaviour spillover (Maki et al., 2019; Geiger et al., 2021). That the spillover effects were sometimes positive (spillover from earlier lifestyle change decisions to subsequent lifestyle change or donation preferences) and sometimes negative (spillover from donation decisions to lifestyle change preferences) helps extend existing spillover research. Existing research suggests that the difficulty (Gneezy et al., 2012; van der Werff et al., 2014) and similarities between the initial and subsequent behaviours (Margetts and Kashima, 2017; Thøgersen, 2004), co-determine whether spillover occurs and in what direction. The positive (Results 5a) and negative (Result 5b) spillover effects that we observe may be consistent with the notion that more difficult (vs easier) initial choices are more likely to lead to positive spillover, assuming that the donation choices in our experiment were easier than the lifestyle choices. The pattern of results is less consistent with an explanation that positive spillover to similar (vs dissimilar) subsequent behaviours is more likely. Subsequent research can systematically manipulate the difficulty and similarity of studied behaviours to shed further light on the conditions under which positive and negative spillover occur.

Aside from the empirical results, this work has served to highlight the potential benefits of SC approaches for data collection and choice models for behaviour analysis in a behaviour spillover context. Indeed, the carefully constructed experiments allow us to separate out the effect of different factors in decision-making, related both to the characteristics of the possible behavioural actions (level of CO_2 emissions and scale of donations), while the use of different orders of experiments across individuals permits us to infer the role of spillover without the insights being biased by possible learning or sequencing effects.

In addition to our main findings on spillover, our experiments demonstrate that people are generally motivated to implement emission reducing lifestyle changes in areas of transportation and diet (Result 1), and that the magnitude of the emission reduction potentials of these changes drives motivation up (Result 3), especially if emission reduction potentials are communicated in an easy to understand format (Result 4). People are more willing to implement lifestyle changes that are less demanding (Result 2). Finally, we report anchoring (Result 7) and choice set effects (Result 8) in the context of donations to offset carbon emissions, which can be potentially leveraged in environmental fundraising. These findings are discussed in more detail in the previous sections.

4.2. Limitations

Of course, like any study, the work in this paper has some limitations, related to both the data and the modelling. First, the sample used is comparatively small and lacks representativeness, meaning that the results should be seen as a proof of concept rather than being easily generalisable. Second, the number of choice tasks faced by each respondent is limited, and not framed around their real-world experiences, where future studies could for example show larger potential CO_2 reductions for respondents who drive more than others, or incorporate a consideration of the relationship between the spatial geolocation and how that affects the behaviour to reduce CO_2 emissions as stated by Lv and Li (2021). Third, consideration should be given to incentive compatibility of the experiments, such as using actual donations (e.g. allowing respondents to use part of their compensation in the donations experiments). More generally, the possibility of hypothetical bias is inherent in the stated choice methodology, and our results should therefore be seen to have greater validity in the domain of intentions or willingness to act than in the domain of actual behaviour (where intentions are known to correlate with behaviour, but only imperfectly). Nevertheless, evidence from environmental behaviour research suggests that neither self-reported intentions nor self-reported behaviours are substantially biased in the direction of social desirability, and hypothetical bias may therefore be interpreted mainly as the presence of unsystematic noise in the data (e.g., due to imperfect planning or imperfect recall, see Vesely and Klöckner, 2020). Finally, an important avenue for future modelling work in this context is to capture heterogeneity across individual respondents, allowing for the possibility that the level of susceptibility to spillover effects varies across individuals, including the role of attitudes and perceptions.

The empirical results in our work clearly suggest that accounting for the potential existence of spillover effects can provide additional insights into decision-making, and this should serve as a motivation for future work across different disciplines.

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Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

The authors do not have permission to share data.

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