Resource and Energy Economics 57 (2019) 135–155

Contents lists available at ScienceDirect

Resource and Energy Economics

journal homepage: www.elsevier.com/locate/ree

Revisiting cost vector effects in discrete choice experiments

Klaus Glenk[a,*], Jürgen Meyerhoff[b], Faical Akaichi[a], Julia Martin-Ortega[c]

[a] Department of Rural Economy, Environment and Society, Scotland's Rural College (SRUC), Edinburgh, UK
[b] Institute for Landscape and Environmental Planning, Technische Universität Berlin, Germany
[c] Sustainability Research Institute, School of Earth and Environment, University of Leeds, UK

ARTICLE INFO

Article history:
Received 27 April 2018
Received in revised form 2 May 2019
Accepted 5 May 2019
Available online 11 May 2019

JEL Classifications:
H51
D61
H41

Keywords:
Environmental valuation
Stated preferences
Context dependence
Anchoring
Decision strategies

ABSTRACT

The estimation of marginal utility of income in discrete choice experiments is of crucial importance for the estimation of willingness to pay (WTP) and welfare estimates. Despite this central importance, there are only few investigations into the impact of the design of the cost attribute vector on choices and WTP estimates. We present a conceptual framework that describes why cost vector effects might occur in choice experiments, and investigate cost vector effects empirically drawing on data from a choice experiment in the context of peatland restoration in Scotland. This study employs a split sample approach with three different cost vectors that vary considerably in the cost levels offered to respondents, and investigates differences between treatments with respect to marginal WTP estimates, status quo choice, use of systematic decision strategies and attribute non-attendance. A key finding is that the choice of cost vectors can affect the incidence of decision strategies. After accounting for the differential use of a decision strategy that might not be consistent with random utility modelling, cost vectors that are higher in magnitude result in higher WTP, in line with an anchoring hypothesis. We find weak support that marginal WTP of lower income respondents is affected differently compared to higher income respondents through the use of different cost vectors. Differences in welfare estimates resulting from the use of different cost vectors might change outcomes of cost-benefit analyses. We therefore recommend that researchers include tests of sensitivity of welfare estimates to different cost vectors in their study design.

1. Introduction

The estimation of marginal utility of income in choice experiments is of crucial importance for the estimation of willingness to pay (WTP) and welfare estimates. It requires the inclusion of a monetary or cost attribute with a series of levels – the cost vector – to be defined by the researcher. Numerous studies have been concerned with (optimal) bid selection in closed-ended contingent valuation (e.g., Cooper and Loomis, 1992; Boyle et al., 1998; Veronesi et al., 2011). Regarding cost vector design in choice experiments, Carlsson and Martinsson (2008, 167) noted: "[a] similar discussion on which attribute levels to attach to the cost attribute is relatively absent in the choice experiment literature". This statement is still valid after a decade that has seen a surge in choice experiment applications for non-market valuation. Specific information on the process of selecting the cost vector (i.e. on the number of levels to use and their values) is rarely reported in environmental (public good) applications of the discrete choice experiment literature. According to Hanley et al. (2005, 228), in discrete

* Corresponding author.
E-mail address: Klaus.glenk@sruc.ac.uk (K. Glenk).

https://doi.org/10.1016/j.reseneeco.2019.05.001
0928-7655/

Table 1
Cost vectors investigated in previous literature.

	Currency	Vector	Cost vector level						
			1	2	3	4	5	6	7
Ryan and Wordsworth (2000)	GBP	1.	2	8	20	35			
		2.	7	30	40	60			
Hanley et al. (2005)	GBP	1.	2	5	11	15	24		
		2.	0.67	1.67	3.67	5	8		
Carlsson and Martinsson (2008)	SEK	1.	125	200	225	275	375		
		2.	325	400	425	475	575		
Mørkbak et al. (2009)	DKK	1.	20	26	38	51	65	80	
		2.	20	26	38	51	65	120	
Kragt (2013a)	AUD	1.	30	60	200	400			
		2.	50	100	300	600			
Su et al. (2017)	USD	1.	0.4	0.6					
		2.	0.8	1.2					
Svenningsen and Jacobsen (2018)	DKK	1.	10	20	40	60	90	120	200
		2.	100	200	400	600	900	1,200	2,000

choice experiments "[t]he researcher typically specifies [the] levels [of the cost vector] based on an educated guess as to the underlying distribution of WTP". Mørkbak et al. (2010) have been somewhat more specific and report, citing Garrod and Willis (1999), that the highest level of the cost vector should be chosen using a 'rule of thumb' that alternatives with the highest cost level should not be selected in more than 5%–10% of the choice tasks in which it is present. While a clear explanation for this rule is lacking, it appears to have been used to recognise that measurement of demand for a good requires identification of the choke price (i.e. the price at which demand is zero). Recent guidance on the use of stated preference methods focuses on the type of information that ought to be conveyed via the payment vehicle, but does not comment on the choice of cost or bid vector magnitude beyond the statement that "[a]mounts and payment vehicles must be credible and salient to respondents" (Johnston et al., 2017, 328).

The lack of concrete and cohesive guidance on the definition of the cost vector is of concern to choice experiment practitioners if decisions regarding cost vector design have implications for the validity of derived welfare estimates. In other words, an 'educated guess' may suffice if it can be established that welfare estimates derived from choice experiments are invariant to decisions regarding cost vector design. There are relatively few investigations into the impact of cost vector design on WTP estimates, which provide mixed evidence (Ryan and Wordsworth, 2000; Hanley et al., 2005; Carlsson and Martinsson, 2008; Mørkbak et al., 2010; Kragt, 2013a; Su et al., 2017). Given the central importance of marginal utility of income for the estimation of welfare effects and the increase in the use of choice experiments for non-market valuation, a more robust evidence base on the role of cost vector design should be a high research priority. We conceive this article to be an important step towards this aim.

We use a split sample approach to investigate whether the magnitude of the cost vector affects choice behaviour and WTP, drawing on data from a choice experiment on habitat restoration. While we do not aim to develop and test a formal model of choice in response to differences in cost vectors, we provide a reasoned argument on why choice behaviour and/or WTP estimates might differ if cost vectors vary, i.e. what kind of cost vector effects one might expect to find (Section 2.2). Our study therefore provides further empirical evidence on cost vector effects, and an indication of reasons underpinning these to inform future research in this area.

This study differs from previous research on cost vector effects in that it specifically investigates the role that respondents' income has on choice behaviour in the face of different cost vectors, thus offering first insights on whether cost vector effects are homogenous across socio-economic dimensions. Additionally, unlike previous studies, this paper explores whether variation in cost vectors shown to respondents affects respondents' decision and information processing strategies that can, in turn, affect WTP estimates. While a wide range of strategies may be considered for this purpose, this study focuses on a reduced set of strategies that can be observed through systematic choice patterns found in the data, specifically serial non-participation (Kragt, 2013a; Hanley et al., 2005) and systematic choice of non-status quo alternatives with lowest or highest cost levels, as well as on attribute non-attendance (AN-A).

The paper is structured as follows. Section 2 provides an overview of the literature on cost vector effects in choice experiments, followed by a discussion of why and what kind of cost vector effects might occur. Section 3 describes the experimental approach, presents hypotheses to be tested and the econometric modelling approach used. Section 4 introduces the data set. Results are reported in Section 5, followed by a discussion and conclusions drawn from the analysis (Section 6).

2. Cost vector effects in discrete choice experiments

2.1. Literature review

We are only aware of seven studies that used split samples to investigate how varying cost vectors affect welfare measures (Table 1). Ryan and Wordsworth (2000), analysing preferences for cervical cancer screening, varied the levels of three out of six attributes presented to two different respondent groups. They found significant differences for most marginal WTP estimates but no differences between welfare estimates for bundles of attributes across treatments.

Comparing two split samples that received either a cost vector with a lower or three times higher magnitude in a choice experiment for river health improvement, Hanley et al. (2005) found a significant increase in status quo choice if the cost vector contained higher values. After allowing for differences in error variance between their two samples, an observed increase in WTP associated with higher values of the cost vector was found to be not significant. However, compared to the split sample that used a cost vector with higher values, using a lower cost vector resulted in a reduction in estimated benefits of water quality improvements by 45%. As the authors point out, this difference could well tip outcomes of a cost-benefit analysis, and might thus be politically significant.

In a different context—Swedish households' marginal WTP to reduce power outages—Carlsson and Martinsson (2008) report that increasing the magnitude of each of the cost vector levels by a constant value significantly increased households' marginal WTP for two independent samples. The other context variations investigated in the paper (number of choice sets and the design of the first choice set presented to respondents) did not have a significant impact on marginal WTP results, indicating that different cost vectors created a stronger context effect.

Mørkbak et al. (2010) examined the impact of defining the highest level of the cost attribute to potentially exceed the choke price on consumers' preferences and WTP for a market good. The authors found that increasing the maximum cost level by 50% significantly increased respondents' WTP (up to 68%) for all non-cost attributes and denoted this effect as 'choke price bias'. They concluded that yeah-saying is likely to be the main driver of the observed increase, but acknowledge that other contributing factors may be important such as anchoring, attribute non-attendance to the cost attribute and insensitivity to price changes particularly of respondents with a high level of income.

Comparing two split samples, Kragt (2013a) investigated the effect of varying both the range and the magnitude of cost vectors on participants' WTP in a choice experiment on catchment natural resource management in Australia. No differences were found with respect to systematic choice of the status quo alternative (serial non-participation) and the proportion of choices observed at any of the cost levels, irrespective of their absolute magnitude. Cost levels that were higher in magnitude resulted in a significant increase in WTP for only one of the attributes. Kragt (2013a) concluded that participants in the choice experiment seemed to be more sensitive to relative cost levels than absolute cost levels, thus extending similar findings for a non-cost attribute reported in Luisetti et al. (2011).

Testing whether alternative elicitation methods give similar WTP estimates, Su et al. (2017) compared estimates from an experimental auction and a discrete choice experiment, both non-hypothetical, regarding rice with different qualities. Respondents were randomly allocated to a low price and a high-price group. Prices (cost vector levels) in the high price-group were twice as high as prices shown to respondents in the low price group. WTP estimates were significantly lower for the low price group, with WTP estimates for the high price group being closer to results from the auctions. The authors attribute these differences partially to anchoring effects triggered by the use of different levels of the cost vector.

Svenningsen and Jacobsen (2018) tested the effects of changes in the payment vehicle (tax *versus* donation), the elicitation format (stated *versus* revealed [real donation mechanism]) and the magnitude of the cost vector in a choice experiment study concerned with distributional outcomes of climate policies. Using a 10 fold difference in the magnitude of two cost vectors (the highest so far in the literature), the authors found differences in WTP between their split samples and concluded that those differences must be due to either the cost vector range or the elicitation format: because their overall experimental design, effects of cost vector differences and elicitation format are confounded, making it impossible to identify the cause of the observed differences in WTP to either of the two factors.

In sum, the literature on cost vector effects indicates a tendency of finding higher WTP if cost vectors of greater magnitude are used, but differences in marginal WTP are not always found to be statistically significant for all or some of the choice experiment attributes. These mixed results provide motivation for revisiting cost vector effects in this paper. Additionally, the design of our study differs from previous studies based on split-sample as shown in Table 1 in important ways. First, the lowest cost level is of the same magnitude across cost vectors in our split samples to ensure that disutility associated with moving from zero cost to the lowest level is captured in the same way across split sample treatments, an aspect which has been shown to affect WTP estimation (Hess and Beharry-Borg, 2012) but that has only been considered by Mørkbak et al. (2014) in the context of food choice. Second, the study is (to our knowledge) the first to use both lower and higher cost vectors relative to an average cost vector treatment that reflects our own best 'educated guess'.

2.2. Conceptual framework

2.2.1. Cost vector effects and standard behaviour

Economic demand theory predicts that an increasing price, all else constant, has a negative impact on demand for a normal good, be it private or public. Applied to choice experiments, an increasing cost of an alternative should decrease the probability that this alternative is chosen. If all levels of the non-monetary attributes are equal across alternatives, the alternative with the lowest price should therefore be selected[1]. If there are differences in the non-monetary attributes

[1] This expectation, based on choice from a set of alternatives in which one alternative is clearly dominant, has been exploited for tests of consistency of respondent behaviour with rationality assumptions (e.g., Hanley et al., 2002).

between alternatives, the choice depends on the individual's trade-offs between the differences in non-monetary attributes and the difference in cost. If the cost of all alternatives with non-zero cost in a choice set are too high compared to the utility obtained from the non-monetary attributes (typically representing environmental improvements over the status quo) or compared to an individual's budget constraint, then the zero price (or status quo) alternative included to allow estimation of welfare effects is chosen.

Theoretically, the comparison of "benefits" *versus* "costs" of an alternative should be independent of the magnitude of offered cost vector levels as a rational individual, having well-defined preferences, is capable of "finding" the maximum amount of costs that would equal the benefits provided by an alternative. Therefore, if all respondents of a choice experiment survey adhere to the theoretical assumptions that (i) all alternatives are independently evaluated by their attributes, that (ii) they choose the alternative that maximizes their utility, and that (iii) they have stable preferences that are exogenous to the hypothetical market (Braga and Starmer, 2005), it is to be expected that varying the cost vector will not affect respondents' WTP (Mørkbak et al., 2010). This implies that acceptance of alternatives with particular cost vector levels will differ across cost vector treatments, with generally a lower acceptance rate to be expected for higher cost vector treatments. We would also expect a greater incidence of status quo choices as the magnitude of cost vectors increases, because the likelihood of a cost vector level to exceed respondents' maximum WTP increases.

2.2.2. Deviations due to non-standard behaviour

While is not straightforward to attribute *specific* behavioural mechanisms to potential context effects regarding cost vector differences, we can outline a number of reasons why they might occur in the valuation of environmental goods based on empirical evidence. A main reason derives from evidence that challenges the assumption that individuals have well-defined and stable preferences. Critiques of neoclassical theory posit that preferences may be ill-formed and malleable, and constructed throughout the choice process depending on context-specific information (Slovic, 1995; Bettman et al., 1998; Payne et al., 1999; Hoeffler and Ariely, 1999). This hypothesis draws mainly on a large amount of experimental evidence starting with Tversky and Kahneman's (1974) seminal work, which finds that the presence of uninformative anchors (prior cues) significantly affects subsequent valuations. Following the idea of coherent arbitrariness (Ariely et al., 2003), respondents to choice experiments may have a range of acceptable values in mind rather than specific values. This corresponds well with stated preference literature on value uncertainty (e.g., Ready et al., 1995; van Kooten et al., 2001; Hanley et al., 2009; Bateman et al., 2008). Given the uncertainty about values, respondents' choices might be sensitive to a range of factors including framing of choice options, choice context and anchoring. The influence of such factors adds arbitrariness within acceptable value bounds to respondents' *absolute* valuations. Ariely et al. (2003) have shown that consumer valuations can be internally coherent despite being subject to arbitrariness. This means that individuals' *relative* valuation of different amounts or qualities of a good appear sensible: asked to subsequently value a lower and a higher quality good, individuals tend to place a higher value on the higher quality good, despite having uncertain and potentially overlapping value ranges for both goods.

Ambiguity enhances the arbitrariness of valuations (Ariely et al., 2006). Consequently, experience with a good is expected to have a mitigating effect on the role of anchoring (see Alevy et al., 2015 for evidence from a field experiment supporting this notion)[2] . In the context of environmental public good valuation, it is common that some respondents have limited prior experience with the good, and that most respondents have no or only limited experience with valuing changes to the good (Brown et al., 2008). We would thus expect that respondents of choice experiment surveys conducted in the environmental public good domain are likely to be subject to context effects, including anchoring or starting point effects: respondents use information, including on cost, provided in instructional choice sets (Meyerhoff and Glenk, 2015) or initial choice sets or bids (Boyle et al., 1985; Herriges and Shogren, 1996; Ladenburg and Olsen, 2008; Carlsson and Martinsson, 2008) as cues that affect choices in subsequent choice sets.[3]

The above discussion suggests that different cost vectors may provide different anchors (or prior value cues) for respondents. In fact, most studies empirically investigating cost vector effects in choice experiments (Hanley et al., 2005; Carlsson and Martinsson, 2008; Mørkbak et al., 2010; Kragt, 2013a; Su et al., 2017) refer to anchoring as an important reason why differences in the cost vector would result in different WTP estimates. In line with these studies and an anchoring hypothesis, we might then expect that cost vectors of a higher magnitude result in higher estimates of WTP, *ceteris paribus*. Based on the concept of coherent arbitrariness, we would expect that choices will be coherent within each sample that is offered the same cost vector despite differences in absolute valuations. Thus, we might expect that the probability to choose an alternative will decrease as its cost increases *within* each sample receiving the same cost vector, but that the rate of acceptance for the *n*th level of the cost vector does not necessarily differ *between* samples that are shown different cost vectors. Kragt (2013a) indeed finds that respondents base their decisions on relative cost differences between alternatives rather than on absolute

[2] Hanley et al. (2009) find that experience (beyond a threshold level) has a significant impact on reducing the uncertainty (i.e., the acceptable 'value range') in respondents' stated WTP in a payment card contingent valuation study. This finding corresponds well with the notion that experience may mitigate anchoring effects if the degree of arbitrariness is believed to be positively correlated with a propensity to be sensitive to anchoring.

[3] Due to the repeated nature of choices with varying cost and non-cost attribute levels in discrete choice experiments, more complex ordering effects may arise beyond anchoring of values based on the cost or bid in the initial choice task (Day and Pinto Prades, 2010).

cost levels. If respondents are primarily concerned with relative cost differences, this should equally apply if the cost vector is lower or higher in magnitude compared to a reference cost vector.

2.2.3. Cost vector effects due to decision and information processing strategies

If respondents adhere to standard assumptions outlined in Section 2.2.1, there is no theoretical reason to expect that respondents behave differently by employing different decision and information processing strategies if exposed to different cost vectors. However, there is ample empirical evidence in the choice experiment literature on the impact of decision context on choice behaviour. Examples include dimensionality of alternatives, attributes and levels and associated task complexity (Oehlmann et al., 2017), hypothetical versus non-hypothetical (i.e., incentivised) choice experiments (Mørkbak et al., 2014), presence or absence of instructional choice sets (Meyerhoff and Glenk, 2015), the consumption context (Blasch and Farsi, 2013), whether virtual realities are used instead of verbally describing the choice alternatives (Bateman et al., 2009; Matthews et al., 2017), whether specific attributes are added to the choice sets (Caputo et al., 2017), or whether a price attribute is present or not (Aravena et al., 2014; Carlsson et al., 2007; van Zanten et al., 2016).

A wide range of decision and information processing strategies might be considered in relation to cost vector effects. Strategies that can be observed through systematic choice patterns found in the data that are of interest to this study are i) serial non-participation, investigated in previous studies on cost vector effects (Kragt, 2013a; Hanley et al., 2005); ii) systematic choice of non-status quo alternatives with lowest or highest cost levels; and iii) attribute non-attendance (AN-A). Serial non-participation is characterised by exclusively choosing the status-quo or opt-out alternative in all choice tasks of a choice experiment. This can arise if respondents express 'protest' against the valuation exercise, or if respondents' genuine preferences are reflected by zero WTP. Typically, follow-up questions are used to distinguish protest respondents from 'genuine zeros'.

Serial non-participation may increase as the magnitude of the cost vector increases because respondents are less likely to afford higher cost levels associated with alternatives on offer; however, this effect should be negligible if there are no differences at the lower bound of applied cost vectors. Systematic choice of the most expensive alternative has been investigated by Kragt (2013a) and may occur if respondents are keen to seeing the proposed policy change implemented, and believe that indicating higher WTP to policy makers would increase the likelihood of policy implementation. Systematically choosing the most expensive alternative may also be a simplifying strategy for respondents who believe that higher cost would always be associated with greater levels of improvement (price-quality heuristic; e.g., Rao and Monroe, 1989; Gneezy et al., 2014). Systematic choice of the cheapest non-status quo alternative might entail an elimination-by-aspects procedure where alternatives are eliminated from the choice set if they exceed a certain acceptable cost threshold, followed by a choice of the cheapest of the remaining non-status quo alternatives (if all non-status quo alternatives exceed the acceptable threshold in a choice set, the status quo is chosen). This choice pattern might occur if respondents want to indicate general support for policy change (e.g., conservation action), irrespective of the environmental improvements on offer.

Given the above, an increase in the magnitude of the cost vector might result in an increase in systematically choosing the most expensive or the cheapest alternative in all choice sets. Such behaviour may be indicative of non-trading, i.e., that respondents do not trade-off cost and benefits as shown in the choice sets consistent with a random utility paradigm, and may make welfare estimation based on such response patterns difficult or impossible (Hess et al., 2018).[4]

AN-A as an information processing strategy has been widely studied in the choice experiment literature (e.g., Hensher et al., 2005; Campbell et al., 2008; Scarpa et al., 2009; Campbell et al., 2011; Colombo and Glenk (2013); Kragt, 2013b; Glenk et al., 2015; Koetse, 2017) and is mentioned by Mørkbak et al. (2010) as a possible reason for cost vector effects. Instead of drawing on all the information available in choice sets, respondents may resort to using a sub-set of the information and ignore information on one or more attributes when making their choices. Use of this semi-compensatory choice heuristic can considerably affect WTP estimates (e.g., Glenk et al., 2015). AN-A may arise for a variety of reasons (Alemu et al., 2013), including coping strategies in the face of complex choice tasks to reduce the cognitive cost associated with choosing, irrelevance of an attribute to a particular choice situation, and aspects of the experimental design (Puckett and Hensher, 2008; Hensher et al., 2012). Estimates of the incidence of AN-A are sensitive to the methodological approach used for inference (stated versus inferred AN-A, see Scarpa et al. (2009) and Scarpa et al. (2013)). Nevertheless, AN-A can be a useful indicator of choice behaviour to compare different choice experiment versions and investigate context effects (e.g., Weller et al., 2014; Mørkbak et al., 2014). Here, a main focus lies on assessing if there are considerable differences in stated and inferred AN-A depending on cost vector magnitudes rather than on reliably identifying the absolute magnitude of AN-A.

Several environmental valuation studies find considerable non-attendance to the cost attribute. For example, Campbell et al. (2008) reports that 31% of respondents stated to have ignored cost, Scarpa et al. (2009) infers AN-A for cost to be between 53% and 90% depending on the modelling approach used, and Kragt (2013b) finds stated AN-A for cost to be 22% and AN-A inferred through latent class modelling to be 40%. Hess et al. (2013) argue that AN-A derived analytically through modelling may be confounded with low sensitivities reflecting low (rather than no) perceived importance of attributes. In their own words, "our designs do not include sufficient scenarios in which the given attribute can influence the choice" (Hess et al. 2013, 606). This argument implies that perceived importance and hence influence of the cost attribute on choice

[4] The systematic choice patterns above may also represent simplifying decision strategies related to the complexity of the valuation exercise and therefore cognitive effort required in association with low engagement with the survey.

should increase (and hence AN-A decrease) if the upper bound of the cost vector increases. Cameron and DeShazo (2010) present a model of differential allocation of attention across attributes depending on the expected marginal benefits and marginal costs of further information processing, and thus on the mix of attribute levels in a choice set. According to their model, for which they find empirical support, a greater difference in the range between alternatives of levels for an attribute is associated with a greater likelihood that an individual will take this attribute into account, *ceteris paribus*. Increasing the upper bound (and hence the range) of the cost vector may thus result in a greater likelihood of attention to cost, and thus a reduced incidence of AN-A for cost vectors that are higher in magnitude.

2.2.4. Fat tails in discrete choice experiment data

Another motivation for investigating cost vector effects in discrete choice experiments relates to concerns about the presence of 'fat tails' in the WTP distribution derived from contingent valuation studies, where logit and probit estimators are found to be sensitive to the highest bids included (e.g. Desvousges et al., 2015; Ready and Hu, 1995). A paper by Parsons and Myers (2016) finds strong evidence for the presence of 'fat tails' in a contingent valuation study. The authors wonder "whether there is a fat-tails-equivalent for choice experiments [manifested] through [lack of] sensitivity of willingness to pay estimates to the maximum bid level used for the payment attribute in the choice experiment" (Parsons and Myers 2016, 217). Fat tails in a choice experiment context might thus be understood as an inability to effectively choke off demand with the maximum cost level. Suppose that respondents are randomly allocated to two different cost vectors that differ in their upper bounds (i.e., the maximum cost level). The maximum level of the first cost vector is chosen to reflect the (known) choke price, the maximum level of the second cost vector is twice as high in magnitude. If respondents choose in accordance with standard economic assumptions and have constant marginal utility of income, we do not expect to see a difference in estimated WTP, as discussed in 2.2.1.

Now suppose that the above applies to 80% of the sample. The remaining 20% select alternatives with the maximum cost level irrespective of its magnitude due to yeah-saying or the price-quality heuristic, for example. In this case, WTP estimates derived from both samples are biased, and WTP estimates derived from the sample using the higher maximum cost level will be higher. Empirical findings from 'bid acceptance curves' of the percentage of alternatives chosen depending on the magnitude of cost suggest that a residual number of respondents appears to continue to choose alternatives at maximum cost levels even if their magnitude increases considerably over a baseline (Kragt, 2013a; Mørkbak et al., 2010). Mørkbak et al. (2010) demonstrate that choosing alternatives with the maximum cost level beyond a point where bid acceptance decreases only marginally with increasing cost results in a significant increase in WTP estimates. This is akin to the problem of 'fat tails' in contingent valuation (CVM) studies. Continued demand despite a very high cost of alternatives may be legitimate if respondents have high levels of wealth. However, this should plausibly only apply to a relatively small proportion of the sample.[5]

2.2.5. Differences in cost vector effects depending on respondents' income

The discussion in Sections 2.2.2 and 2.2.3 rests on an assumption of constant marginal utility of income. If we assumed decreasing marginal utility of income, we would expect that, *ceteris paribus*, increasing the magnitude of the cost vector results in higher estimates of mean marginal disutility of cost and thus lower WTP estimates (Mørkbak et al., 2010). It is often argued that constant marginal utility of income may be a reasonable assumption if utility associated with proposed environmental changes can be compensated by a relatively small change in income. Whatever may be considered a small change in this context, it is plausible that a higher amount of the maximum cost vector level is more likely to represent a more substantial change in income for low income respondents compared to respondents belonging to higher income groups. As a consequence, the assumption of constant marginal utility of income over the range of cost observed in the choice experiment may be more likely to be violated for lower income respondents if the magnitude of the maximum cost level increases.

Another reason for potential differences in WTP estimates between income groups is related evidence that not all anchors are effective. Sugden et al. (2013) find that anchoring effects only occur if the anchor value is perceived to be a plausible price for the good for which the individual is a potential buyer. One of the reasons behind this empirical finding is related to respondents using anchor values as informative cues. This means that the (implied) question 'Would you choose environmental changes proposed in alternative A at a cost of £X?' may consciously or subconsciously be interpreted by respondents as '£X is a typical and reasonable cost in exchange for changes proposed in alternative A'. The less plausible £X appears as a cost for the proposed change, the less likely it is that respondents make this inference and thus anchor their choices in £X. Within the choice experiment application presented in this paper, we did not vary cost vector levels to become wholly implausible at their upper bound. However, it is still conceivable that respondents with comparatively lower levels of income may be less likely to find maximum cost vector levels to be a plausible reflection of the value of the proposed change if they are greater in magnitude.

[5] Other reasons fall into the behavioural domain and include cut-off violations (Colombo et al., 2015), anchoring (e.g., Chien et al., 2005), yeah-saying (e.g., Brown et al., 1996) and non-attendance to cost (e.g., Scarpa et al., 2009).

Table 2
Cost vectors used in the three split samples in GBP per year.

Treatment label	Level 1	Level 2	Level 3	Level 4	Level 5	Level 6
LOW$	10	15	30	45	90	150
AVERAGE$	10	25	50	75	150	250
HIGH$	10	40	80	120	240	400
Factor of relative increase over previous level			2	1.5	2	1.67

3. Method

3.1. Experimental approach

In this study, respondents were randomly allocated to one of three split samples with different cost vectors, LOW$, AVERAGE$ and HIGH$ (Table 2). The same relative rate of increase was applied from cost level 2 onwards across cost vector treatments. The AVERAGE$ cost vector was selected based on results of preparatory focus groups aimed at testing the survey instrument and a pilot study. The maximum cost vector level of the $AVERAGE treatment was set above the highest WTP value expressed in the focus groups. We then calculated bid acceptance curves from pilot study data (N = 100) to check if the maximum cost vector level was set in line with the 'rule of thumb' that the highest price should not be selected in more than 5%–10% of the cases when it is offered (Mørkbak et al., 2010). The magnitude of the remaining cost vectors (above level 1) was chosen to be 60% (LOW$) and 160% (HIGH$) of the cost vector magnitude in the AVERAGE$ treatment. Based on income data collected in the survey, three income groups are created.

3.2. Hypotheses

Drawing on the baseline economic theory assumptions outlined in Section 2.2.1, we derive the following series of specific hypotheses:

Hypothesis 1. The proportion of respondents choosing alternatives that contain the *n*th cost level across treatments is lower if the absolute magnitude of the *n*th cost level is greater, i.e., respondents' choices are sensitive to changes in the absolute magnitude of cost levels. Denote the proportion of alternatives with the *n*th cost level being selected amongst all choice tasks where the *n*th cost level is present as P_n:
$H0_1 : P_n\ LOW\$ = P_n\ AVERAGE\$ = P_n\ HIGH\$$;
$H1_1 : P_n\ LOW\$ > P_n\ AVERAGE\$ > P_n\ HIGH\$$.

$$H0_1 : P_n\ LOW\$ = P_n\ AVERAGE\$ = P_n\ HIGH\$;$$

$$H1_1 : P_n\ LOW\$ > P_n\ AVERAGE\$ > P_n\ HIGH\$.$$

Hypothesis 2. We would expect no difference in marginal WTP (MWTP) estimates between cost vector treatments, and within income groups between cost vector treatments. We investigate this by testing if equality of MWTP estimates can be rejected.
$H0_2 : MWTP\ LOW\$ = MWTP\ AVERAGE\$ = MWTP\ HIGH\$$;
$H1_2 : MWTP\ LOW\$ \neq MWTP\ AVERAGE\$ \neq MWTP\ HIGH\$$.

$$H0_2 : MWTP\ LOW\$ = MWTP\ AVERAGE\$ = MWTP\ HIGH\$;$$

$$H1_2 : MWTP\ LOW\$ \neq MWTP\ AVERAGE\$ \neq MWTP\ HIGH\$.$$

Hypothesis 3. The different cost vector treatments differ with respect to the total number of status quo choices. Let *P*(SQ) denote the proportion of status quo (SQ) choices made among all choices within a treatment:
$H0_3 : P\ SQ_{LOW\$} = P\ SQ_{AVERAGE\$} = P\ SQ_{HIGH\$}$;
$H1_3 : P\ SQ_{LOW\$} < P\ SQ_{AVERAGE\$} < P\ SQ_{HIGH\$}$.

$$H0_3 : P\ SQ_{LOW\$} = P\ SQ_{AVERAGE\$} = P\ SQ_{HIGH\$} ;$$

$$H1_3 : P\ SQ_{LOW\$} < P\ SQ_{AVERAGE\$} < P\ SQ_{HIGH\$} .$$

Hypothesis 4. The use of different cost vectors does not affect the incidence of non-attendance to the cost attribute across cost vector treatments. We investigate this by testing if equality of incidence of AN-A across cost vector treatments can be rejected. Let $P_{AN\text{-}A}$ indicate either the proportion of respondents who state to not always consider the cost attribute (stated AN-A) or the probability of non-attendance to cost estimated analytically (inferred AN-A):

$H0_4 : P_{AN-A}\ LOW\$ = P_{AN-A}\ AVERAGE\$ = P_{AN-A}\ HIGH\$$;
$H1_4 : P_{AN-A}\ LOW\$ \neq P_{AN-A}\ AVERAGE\$ \neq P_{AN-A}\ HIGH\$$.

$$H0_4 : P_{AN-A}\ LOW\$ = P_{AN-A}\ AVERAGE\$ = P_{AN-A}\ HIGH\$;$$

$$H1_4 : P_{AN-A}\ LOW\$ \neq P_{AN-A}\ AVERAGE\$ \neq P_{AN-A}\ HIGH\$.$$

Hypothesis 5. The use of different cost vectors does not affect the use of the systematic decision strategies. We investigate this by testing if equality of incidence of systematic decision strategies across cost vector treatments can be rejected. Let P_{DS} indicate either the proportion of respondents who employ one of the following systematic decision strategies (DS): i) non-protest serial non-participation; ii) systematic choice of most expensive non-status quo alternative; iii) systematic choice of cheapest non-status quo alternative:

$H0_5 : P_{DS}\ LOW\$ = P_{DS}\ AVERAGE\$ = P_{DS}\ HIGH\$$;
$H1_5 : P_{DS}\ LOW\$ \neq P_{DS}\ AVERAGE\$ \neq P_{DS}\ HIGH\$$.

$$H0_5 : P_{DS}\ LOW\$ = P_{DS}\ AVERAGE\$ = P_{DS}\ HIGH\$;$$

$$H1_5 : P_{DS}\ LOW\$ \neq P_{DS}\ AVERAGE\$ \neq P_{DS}\ HIGH\$.$$

Hypotheses 1, 3, 4 (in relation to stated AN-A) and 5 are tested by inspecting the data non-parametrically. Tests of differences in MWTP (hypothesis 2) and inferred AN-A (hypothesis 4) are based on results of a series of choice models estimated for cost vector and income sub-samples. The econometric approach for their estimation is described in more detail below.

3.3. *Econometric approach*

The modelling approach to analyse the choice data for different treatment groups and combinations of treatments and income groups is based on the random utility theory and uses a random parameter logit model (Train, 2003). We assume the price attribute parameter to follow a log-normal distribution, and the non-price attribute parameters to follow a normal distribution. In all models the simulation of the log-likelihood is performed using 2,000 Halton draws. In the estimation, we allow for correlation of all random parameters (full covariance). Starting values for the model with full covariance are derived from a model with uncorrelated coefficients (Hess and Train, 2017). Confidence intervals for mean MWTP estimates (based on standard errors of coefficients) are calculated using the Krinsky and Robb (1986) bootstrapping procedure. The complete combinatorial test suggested by Poe et al. (2005) is subsequently used to test for statistically significant differences in mean MWTP between sub-samples at the 10% level.

To investigate the use of information processing strategies and specifically attribute non-attendance (AN-A) between treatments, we use the equality constrained latent class model (ECLC) proposed by Scarpa et al. (2009), and following closely the specification reported in Glenk et al. (2015). Rather than using latent classes to represent a discrete mixture of people's preferences, the ECLC captures heterogeneity in attribute processing strategies, in this case AN-A behaviour. Each latent class in the ECLC model represents a different pattern of attribute attendance. The model allows for fully compensatory preferences where all attributes are considered, combinations of ignoring one or more attributes and non-attendance to all attributes in the choice set, reflecting random choice between alternatives (Scarpa et al., 2009). The 'AN-A patterns' are introduced by imposing restrictions on the utility coefficients in each latent class. A zero utility weight is assigned to attribute coefficients that are not considered, while all coefficients to be estimated are constrained to be equal across classes. A key outcome of the ECLC model are the class probabilities for the AN-A patterns, which can be used to infer the probability that an attribute has been ignored. Confidence intervals for AN-A probabilities are calculated using the Krinsky and Robb (1986) procedure, and we use a Poe et al. (2005) test to investigate if differences in estimated AN-A probabilities for the cost attribute are statistically significant at the 10% level.

4. Data

We employ a split sample approach using three different cost vectors to a study in the context of peatland restoration in Scotland. Data comes from an online survey with 1,795 Scottish citizens conducted in February/March 2016. A quota based approach was used to sample from an online panel with age and gender as 'hard' quotas and a 'soft' quota for social grade. Details on the development of the survey instrument and the policy background can be found in Martin-Ortega et al. (2017) and Glenk and Martin-Ortega (2018). Including the cost attribute, each of two peatland restoration alternatives was characterised by five attributes (Table 3). Two attributes described percentage shifts in ecological condition (from poor to good and from intermediate to good condition) relative to the share of peatlands in each ecological condition in a business

Table 3
Description of the choice experiment attributes and levels.

Attributes	Label	Levels[b]
Improvement of peatland share from bad ecological condition to good ecological condition[a]	*poor*	0%, 25%, 50%, 75%
Improvement of peatland share from intermediate ecological condition to good ecological condition[a]	*int*	0%, 25%, 50%, 75%
Focus on peatland restoration in wild land areas	*wild*	Yes, No
Focus on peatland restoration in areas with high or low concentration of peatlands	*conc*	High, Low
Cost (annual tax, GBP per household and year)	*price*	see Table 2

Note:
[a] Shifts (degree of improvement) are relative to the business as usual shares of peatlands for each ecological condition (poor: 40%; Intermediate: 40%; good: 20%).
[b] *poor*, *int* and *price* are scaled by 1/100 and enter the choice models as continuous variables, *wild* and *conc* as effects coded variables taking 1 for Yes (*wild*) and High (*conc*), else -1.

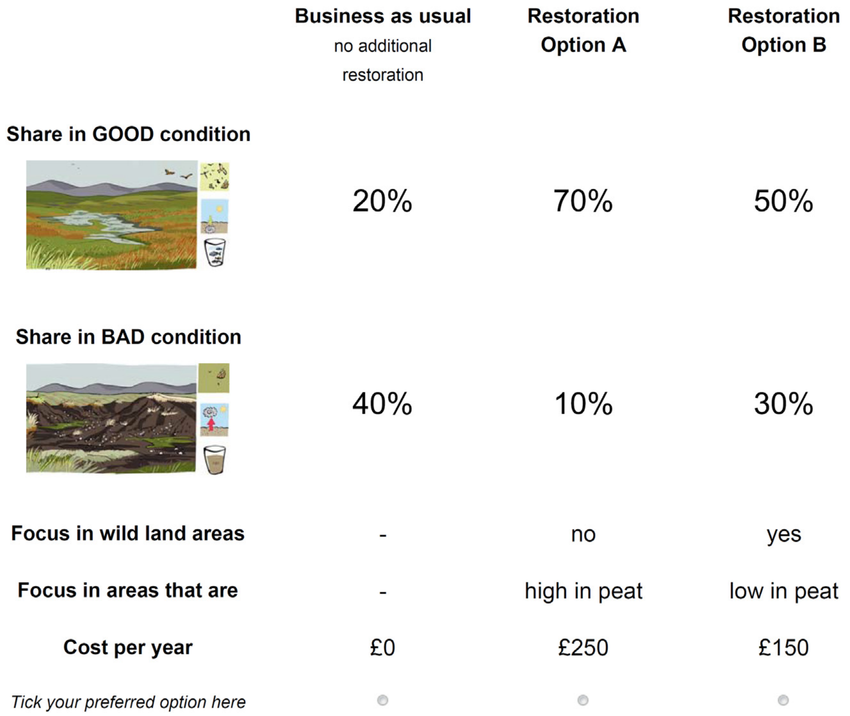

Fig. 1. Example choice set.

as usual (status quo) scenario. The improvements in peatland condition are associated with an increase in ecosystem service provision related to climate change mitigation, water quality improvement and changes to wildlife. Two additional attributes captured preferences for spatial allocation of restoration efforts taking place in i) remote and inaccessible areas ('wild land areas') and ii) in areas where peatland cover more or less than 30% of the land surface ('high/low concentration').

The restoration alternatives include a monetary trade-off in the form of an annual cost to the tax payer, over a period of 15 years, towards a Peatland Trust fund responsible for implementing a restoration programme that would deliver the proposed improvements. We emphasized that results of the survey would be passed on to Scottish Government to inform the current discussions on peatland management. To add credibility to this statement, a web-link to Scotland's Peatland National Plan was added to the survey. Each respondent was presented with 8 choice sets in which they were asked to choose between the 'business as usual scenario' (at no additional cost) and two scenarios of improved peatland condition in exchange of that cost (see Fig. 1 for an example choice set).

The experimental design was generated using Ngene software (ChoiceMetrics 2014). A Bayesian D-efficient design was optimised for an MNL model using prior estimates of parameters based on a pilot study (N = 100) with 40 choice sets blocked into five versions which were randomly assigned so that each respondent faced 8 choice sets, whose order was again randomised across respondents. Respondents were randomly allocated to one of the three cost vector treatments as defined in Table 1. A range of follow-up questions was used to identify reasons for serial non-participation, stated AN-A, stated certainty of choices and stated cut-offs for cost. The usual section on socio-demographics included questions on respondents' income levels. In order to maximise reporting of income, we used a two stage approach. In the first stage,

Table 4
Number of respondents allocated to income groups by cost vector treatment.

Income group	Income range (in £)	Cost vector treatment			
		LOW$	AVERAGE$	HIGH$	Total
INC_{LOW}	<20,800	134	127	148	409
INC_{MED}	20,800–41,599	188	212	203	603
INC_{HIGH}	>41,600	161	140	165	466
Income missing		87	72	89	248
Total		**570**	**551**	**605**	**1,726**

Table 5
Proportions of cost levels chosen when present in choice set.

Cost level (n)	LOW$ Proportion (P) chosen in %	z statistic $P_{LOW\$} - P_{AVERAGE\$}$	AVERAGE$ Proportion (P) chosen in %	z statistic $P_{AVERAGE\$} - P_{HIGH\$}$	HIGH$ Proportion (P) chosen in %
1	47.07 (1,383)	−1.49	49.93 (1,336)	−2.49	54.69 (1,397)
2	54.17 (1,379)	1.16	51.95 (1,357)	−0.4	52.7 (1,442)
3	47.33 (1,612)	1.09	45.4 (1,575)	1.81	42.25 (1,671)
4	47.73 (1,584)	2.33	43.6 (1,587)	1.25	41.43 (1,656)
5	33.98 (1,610)	2.46	29.92 (1,591)	2.50	25.96 (1,610)
6	19.32 (1,584)	2.58	15.84 (1,610)	1.1	14.46 (1,632)

Note: n = 1 is £10 and n = 6 is the maximum cost level.

respondents were asked to directly state their income. For those refusing to do so, a second question offered to indicate which income bracket they fall into.

5. Results

Of the 1,795 respondents, 58 respondents both always chose the status quo *and* stated protest motives identified through follow-up questions (e.g., Dziegielewska and Mendelsohn, 2007). These respondents were distributed almost identically across cost vector treatments (19 respondents for AVERAGE$ and HIGH$, 20 respondents for LOW$). Choice data on 11 respondents was partially missing due to either technical problems or respondents skipping questions by modifying the URL. Both protest responses and those respondents with missing data were excluded resulting in a sample of 1,726 respondents for further analysis. Of these, 33% (N = 566) received the LOW$ cost vector, 34% (N = 588) the AVERAGE$ one and 33% (N = 572) respondents answered choice tasks offered with the HIGH$ cost vector. The split samples do not differ significantly with respect to age, gender, education and income levels.

Income data is available for 85% of the sample (N = 1,478), and tests of sensitivity to changes in cost vector related to income will draw on this reduced sample. Those respondents who did not report their income did not differ from the rest of the sample with respect to age, gender, level of education. Therefore, we do not expect income non-response to affect comparability of cost vector effects across income levels. Since income information was a mix of cardinal and ordinal scaled data, we created three income groups (INC_{LOW}; INC_{MED}; INC_{HIGH}), as shown in Table 4. Because of the discrete allocation of respondents to pre-defined income categories, splitting the sample into three equally sized groups was not possible. We allocated more respondents to the medium income category (INC_{MED}) to emphasise differences between the two relatively low and high income groups. Sizes of all groups are sufficiently large to allow a meaningful comparison.

To test hypothesis 1, we calculated the proportion of alternatives chosen of all choice sets where the nth cost level was present and calculated z statistics for differences between LOW$ and AVERAGE$ and AVERAGE$ and HIGH$ (Table 5). As it would be expected, choice proportions drop *within* each cost vector treatment as cost levels increase. For $n = 1$, choice proportions are between 47%–55%, decreasing to 14%–19% for $n = 6$. A comparison of choice proportions *across* cost vector treatments at the nth cost level shows if an increase in the absolute magnitude of cost levels is associated with a decrease in bid acceptance. At each cost level for $n > 1$, cost is lowest for LOW$, followed by AVERAGE$ and HIGH$, and consequently choice proportions should decrease when moving from LOW$ to HIGH$. As can be seen in Table 5 (third and fifth column), we can reject the null hypothesis of equal proportions for $n > 3$ and a comparison of LOW$ and AVERAGE$, and for $n = 1$ and $n = 5$ for a comparison of AVERAGE$ and HIGH$. This shows that respondents are somewhat sensitive to an increase in the absolute magnitude of cost vectors, especially when moving from LOW$ to AVERAGE$.

However, we find significant differences in choice proportions across cost vector treatments for cost levels that are of the same or similar magnitude. For example, the sixth cost level of LOW$ and the fifth cost level of AVERAGE$ both take a value of £150. However, choice proportions are significantly greater by 10% in the AVERAGE$ treatment. Similarly, the maximum cost level of AVERAGE$ has a value of £250 and the fifth level of HIGH$ is £240. However, choice proportions are significantly greater by 10% in HIGH$. These differences suggest that relative evaluations must play a role; i.e., that respondents also consider how much smaller or larger cost is in *relative* terms at a given cost level compared to other cost levels within each cost vector treatment.

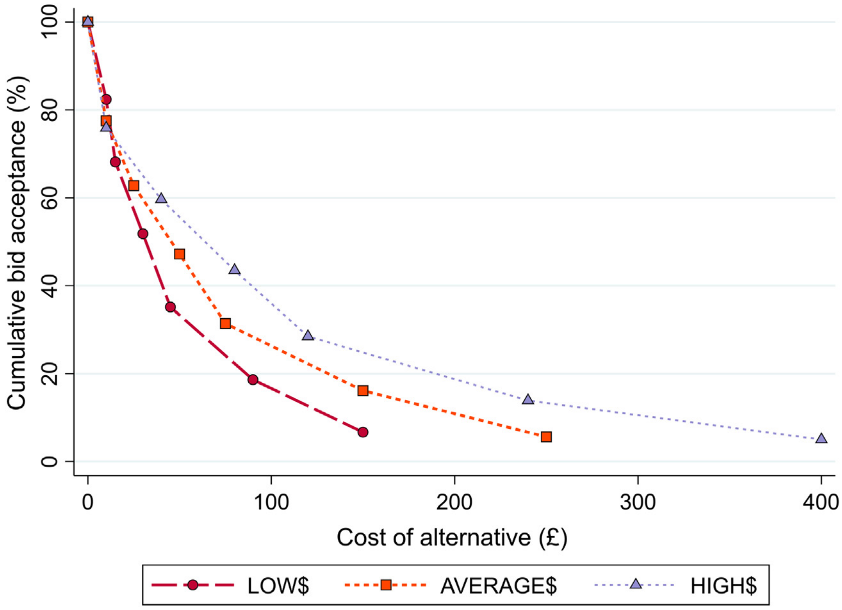

Fig. 2. Bid acceptance (accumulative proportion of choices) for cost vector treatments.

Table 6
RPL model results for cost vector treatments (log-normally distributed price coefficient).

	LOW$		AVERAGE$		HIGH$	
	Coef.	*t*	*Coef.*	*t*	*Coef.*	*t*
Coefficients						
ASC	−0.564	−14.06	−0.499	−16.13	−0.517	−14.41
poor	1.534	4.82	1.734	5.29	1.643	4.8
int	0.936	4.32	1.261	5.67	1.315	5.83
wild	0.240	4.45	0.310	5.94	0.174	3.23
conc	0.245	5.35	0.257	4.71	0.254	4.49
price	1.099	10.98	1.010	11.74	0.652	7.73
Standard deviations						
ASC	0.409	10.78	0.311	10.33	0.457	13.02
poor	2.447	6.90	3.418	8.04	2.895	7.37
int	1.466	6.37	2.223	7.63	1.707	5.86
wild	0.383	6.41	0.412	5.78	0.249	3.74
conc	0.628	11.92	0.770	12.22	0.764	12.39
price	1.224	10.71	1.236	14.05	1.309	20.69
LogL	−2817.5		−2749.7		−2865.0	
Pseudo R2	0.33		0.35		0.37	
N (respondents)	570		551		605	

Note: price is lognormally distributed. The mean estimate m for price can be obtained as $m = (-1) \times \frac{1}{100} \times \exp\left(\mu + \frac{\sigma^2}{2}\right)$, where μ is the coefficient of the mean and σ is the coefficient of the standard deviation, multiplied by −1 since the negative of price enters the utility function because lognormal distribution is limited to the positive domain, and by 1/100 because price was scaled by this factor before entering the analysis. This also applies to results shown in Table 9, Table A1, and Table A7.

Following Mørkbak et al. (2010) and Kragt (2013a), we also plotted ‘bid acceptance curves’ showing the cumulative number of times an alternative was chosen at the *n*th cost level or a lower cost level (Fig. 2). The figure shows that respondents are sensitive to the relative cost levels within each treatment (that is, cumulative acceptance rates decline as cost increases). It can be seen that bid acceptance still decreases considerably between the second highest and the highest level of the cost vectors, although the difference is smaller for HIGH$. This further supports, in line with Kragt (2013a) and Luisetti et al. (2011), that respondents tend to be concerned with relative cost differences between choice alternatives. The analysis of choice proportions and bid acceptance suggests that WTP estimates may be expected to increase as cost vectors increase in magnitude, because relative evaluations play a role especially for the highest cost levels.

We now turn to hypothesis 2, concerned with mean MWTP across cost vector treatments and income groups. Comparing model results across cost vector treatments first (Table 6), all mean attribute coefficients are significantly different from zero, while standard deviations are significant and large in magnitude, suggesting the presence of a considerable degree of unobserved preference heterogeneity in the data.

Table 7
Marginal WTP estimates for cost vector treatments and results of Poe et al. (2005) test for differences between treatments based on Table 6 (RPL with log-normally distributed price coefficient).

	LOW$ mean [95%-CI]	AVERAGE$ Mean [95%-CI]	HIGH$ mean [95%-CI]	AVERAGE$ vs LOW$ p-value	HIGH$ vs LOW$ p-value	HIGH$ vs AVERAGE$ p-value
poor	0.23 [0.13;0.34]	0.29 [0.18;0.41]	0.36 [0.22;0.51]	0.470	0.182	0.486
int	0.14 [0.07;0.21]	0.21 [0.14;0.29]	0.28 [0.19;0.40]	0.208	**0.024**	0.260
wild	7.19 [3.63;11.00]	10.29 [6.73;14.28]	7.47 [3.21;12.27]	0.268	0.926	0.378
conc	7.22 [4.36;10.16]	8.42 [4.95;11.95]	10.84 [6.64;15.85]	0.632	0.206	0.424

Note: significant differences in MWTP estimates at the 10% level (two-sided test) are shown in bold.

Table 8
Mean estimates and confidence intervals of probability of attribute non-attendance (AN-A) to the cost attribute by cost vector treatment inferred from ECLC models (%).

	LOW$	AVERAGE$	HIGH$
Probability of AN-A to *price*	39.2 [34.2–44.5]	28.6 [23.9–33.9]	34.8 [30.2–39.7]

Note: 95% confidence intervals are reported in parentheses. Confidence intervals were calculated using a Krinsky and Robb (1986) procedure with 2,000 draws.

Mean MWTP estimates are reported in Table 7.[6] They appear slightly lower for LOW$ relative to AVERAGE$ and HIGH$, especially for *poor* and *int*. For *int*, this is confirmed by a Poe et al. (2005) test and a comparison of estimates based on LOW$ and HIGH$ treatments, for which significant differences in MWTP estimates are found at the 10% level[7] . Thus, we can reject null hypothesis 2 for comparisons of MWTP between LOW$ and HIGH$ and the attribute *int*. This is broadly in line with findings reported in Hanley et al. (2005); Carlsson and Martinsson (2008) and Kragt (2013a), who all find that a cost vector with higher absolute levels tends to result in higher WTP estimates, although differences were not found to be significant for all (Hanley et al., 2005) and most (Kragt, 2013a) of the attributes.

Within income groups, there are significant differences in MWTP across cost vector treatments only for the medium income group, for which MWTP for *int* is different for HIGH$ compared to LOW$ and different for AVERAGE$ compared to LOW$ (Fig. 3 and Appendix Tables A1, A2 and A3). Several attribute coefficients and corresponding MWTP estimates are not significantly different from zero for INC_{LOW} and INC_{HIGH} sub-samples, preventing a meaningful comparison of MWTP. None of the remaining comparisons of MWTP for the different attributes across cost vector treatments yielded a significant difference at the 10% level for INC_{LOW} and INC_{HIGH} respondents.

In terms of choice patterns and decision and information processing strategies, we first analyse status quo responses (hypothesis 3). Compared to LOW$ (17.61%), the total number of times a status quo alternative was chosen (irrespective of serial non-participation) was significantly higher for AVERAGE$ (22.5%; $\chi^2 = 5.8$, $p = 0.00$) and HIGH$ (24.11%; $\chi^2 = 7.7$, $p = 0.00$). We can thus reject the null hypothesis of equal proportions, confirming a pattern that is in line with theoretical expectations based on standard economic assumptions.

We next investigate whether cost vector treatments resulted in differences in AN-A patterns (hypothesis 4). We distinguish 8 classes in the equality constrained latent class models. One class assumes full compensatory preferences, five classes assume non-attendance to each attribute (including *price*), one class assumes non-attendance to all non-price attributes and a final class assumes that respondents make a choice between the business as usual alternative and the improvement alternatives, but choose one of the improvement alternatives at random. Summary results showing probabilities of AN-A to the cost attribute are reported in Table 8 (results of latent class models can be found in Appendix Table A4, and summary results of AN-A probabilities for all attributes are shown in Appendix Table A5).

Across cost vector treatments, the probability of AN-A to the cost attribute is significantly lower with 29% for the AVERAGE$ cost vector treatment, compared to LOW$ (39%; $\text{AN-A}_{\text{AVERAGE\$}} < \text{AN-A}_{\text{LOW\$}}$: $p = 0.002$) and HIGH$ (35%; $\text{AN-A}_{\text{AVERAGE\$}} < \text{AN-A}_{\text{HIGH\$}}$: $p = 0.042$). The empirical pattern found here are not straightforward to interpret. Lower AN-A in

[6] Exact quantitative results for WTP estimates are sensitive to specification of the distribution of the cost coefficient. In particular, higher marginal WTP estimates are obtained for mixed logit models with a constrained triangular distribution of the cost coefficient, and a greater number of WTP estimates are found to differ significantly across cost vector treatments (see Appendix Table A10 for marginal WTP estimates for the full sample and Appendix Table A11 for estimates for a sample where those systematically choosing the cheapest non-status quo alternative are excluded). However, WTP estimates are not unrealistically low for models using a log-normally distributed cost coefficient, and relative differences in WTP between cost vector treatments are preserved irrespective of the distribution of the cost coefficient. Therefore, primary findings and implications are robust to alternative distributions of the cost coefficient.

[7] A one-sided test of WTP(HIGH$)>WTP(LOW$) is significant at the 10% level for *poor*, but our hypotheses regarding MWTP are non-directional and thus require a two-sided test.

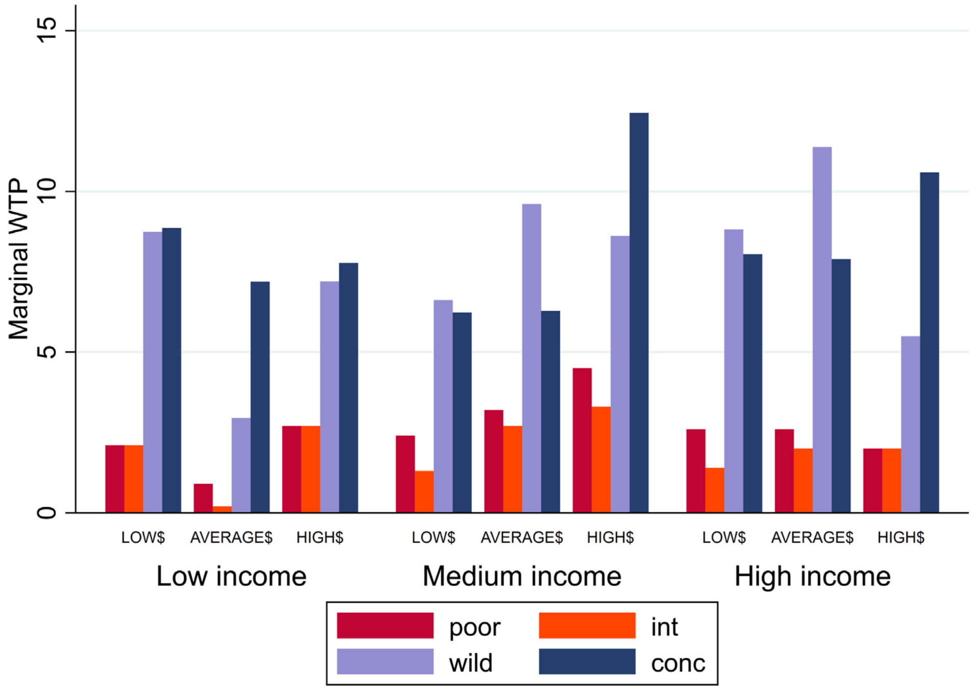

Fig. 3. Estimated marginal WTP estimates by cost vector and income group based results shown in Table A2. Note: Estimates for *poor* and *int* are scaled by × 10 to enhance appearance.

the AVERAGE$ cost vector treatment may suggest that a relatively low magnitude of cost vector can result in insufficient allocation of attention to cost (in line with the reasoning in Section 2.2.2), while a very high magnitude of the cost vector might result in lower attendance to cost by respondents who cannot afford to pay higher cost levels, or perceive payments of such a magnitude as unrealistic.

There is a reasonable level of agreement between the inferred levels of AN-A reported in Table 8 and responses to follow-up questions regarding attribute non-attendance (stated AN-A; see Appendix Table A6), but also some notable differences. Consistent with a hypothesis that allocation of attention increases with cost vector range (Cameron and DeShazo, 2010, see Section 2.2.2), the percentage of respondents stating that they either sometimes or never considered cost when making their choices decreases from 37% in the LOW$ treatment to 34.6% in the AVERAGE$ treatment and 31.8% for HIGH$. The difference between LOW$ and HIGH$ is significant at the 10% level ($\chi^2 = 3.57$, $p = 0.059$). Overall, we therefore can reject equality in the incidence of non-attendance for the cost attribute across cost vector treatments ($H0_4$) for an analysis of both inferred and stated AN-A. However, only findings from stated AN-A support the notion that cost vectors with a greater range (i.e., a higher upper bound) are associated with an increase in respondents' attention allocated to the cost attribute.

Next, we investigate systematic decision strategies, beginning with serial non-participation. Across treatments, and similar to findings of Kragt (2013a), equality of the number of non-protest serial non-participants cannot not be rejected. Also, we find a very low incidence of respondents choosing the most expensive alternative in each choice occasion (<2% in all cost vector treatments; no significant differences). With regard to respondents choosing the cheapest alternative in all choice situations where a non-status quo alternative was chosen, however, we find that 16.3% of respondents employ this strategy in the LOW$ treatment, 22.4% in the AVERAGE$ treatment and 27.2% in the HIGH$ treatment. The differences between the three treatments are statistically significant at the 1% level ($\chi^2 = 12.36$, $p = 0.002$). These results show that differences in the cost vector can affect the degree to which respondents employ decision strategies that are not in line with fully compensatory decision-making. In our case cost vectors of a higher magnitude is associated with a greater incidence of respondents choosing the cheapest alternative in all choice situations where a non-status quo alternative was selected. Such differences in the use of decision strategies may affect WTP estimates.

To explore potential impacts of systematically choosing the cheapest alternative on WTP estimates, we first ran a series of models only including respondents who employed this heuristic. The results showed that non-price attribute coefficients were negative and for several attributes significantly different from zero,[8] suggesting an overall *negative* effect on non-price attribute coefficients in models based on all respondents reported in Table 6. This negative effect is probably driven by small to modest correlation between attributes in the efficient experimental design, which tends to combine lower cost

[8] Full model results are available from the authors upon request.

Table 9
RPL model results for cost vector treatments excluding respondents always choosing the cheapest (non SQ) alternative (log-normally distributed price coefficient).

	LOW$		AVERAGE$		HIGH$	
	Coef.	*t*	*Coef.*	*t*	*Coef.*	*t*
Coefficients						
ASC	−0.521	−12.25	−0.453	−13.63	−0.437	−11.18
poor	2.261	6.56	2.469	6.82	2.196	5.89
int	1.315	5.76	1.630	6.73	1.620	6.57
wild	0.374	6.91	0.417	7.28	0.278	4.97
conc	0.274	5.88	0.313	5.64	0.287	4.96
price	0.943	9.38	0.739	8.42	0.132	1.27
Standard deviations						
ASC	0.405	9.66	0.318	9.01	0.460	10.61
poor	2.658	5.84	3.235	7.36	3.182	5.53
int	1.641	5.33	1.991	6.38	2.024	5.36
wild	0.367	5.60	0.400	5.35	0.259	3.45
conc	0.622	10.96	0.771	11.72	0.791	11.69
price	0.965	15.54	0.868	14.21	0.981	13.78
LogL	−2413.7		−2214.4		−2234.6	
Pseudo R^2	0.32		0.32		0.32	
N (respondents)	494		445		465	

Table 10
Marginal WTP estimates for cost vector treatments excluding respondents always choosing the cheapest (non SQ) alternative (based on Table 9, RPL with log-normally distributed price coefficient).

	LOW$ mean [95%-CI]	AVERAGE$ Mean [95%-CI]	HIGH$ mean [95%-CI]	AVERAGE$ vs LOW$ p-value	HIGH$ vs LOW$ p-value	HIGH$ vs AVERAGE$ p-value
poor	0.53 [0.41;0.66]	0.78 [0.60;0.97]	1.15 [0.83;1.49]	**0.034**	**0.002**	**0.064**
int	0.31 [0.22;0.39]	0.51 [0.38;0.64]	0.85 [0.62;1.08]	**0.010**	**0.000**	**0.016**
wild	17.64 [13.16;21.93]	26.29 [19.68;32.95]	28.96 [18.57;40.00]	**0.038**	**0.068**	0.702
conc	12.88 [8.62;17.15]	19.63 [13.68;26.04]	29.64 [19.71;39.92]	**0.084**	**0.004**	0.114

Note: significant differences in MWTP estimates at the 10% level (two-sided test) are shown in bold.

alternatives with less improvement in non-cost attributes (or higher cost alternatives with greater improvements) in order to avoid dominant alternatives characterised by a low degree of utility balance between alternatives (ChoiceMetrics 2014). We then re-estimated RPL models for split sample treatments.

Tables 9 and 10 report model results and MWTP estimates for cost vector treatments after omitting respondents who systematically chose the cheapest alternative when a non-status quo alternative was selected. Estimates of MWTP are greater in magnitude than those estimated based on our initial analysis reported in Table 6. This is not surprising given that respondents appearing to be extremely cost-sensitive by always choosing the cheapest non-status quo alternative were omitted, and given that the omitted sample, on its own, was found to have a negative effect on non-price attributes as mentioned above. In terms of MWTP differences across treatments, a much more pronounced pattern arises. Across treatments, an increase in mean MWTP estimates for all attributes arises as the levels of the cost vector increase. Poe et al. (2005) tests suggest that mean MWTP is significantly different in magnitude at the 10% level for attributes *poor* and *int* and comparisons across all treatments, and that MWTP is significantly different for AVERAGE$ and HIGH$ compared to LOW$ for the *wild* and *conc* attributes. These findings suggest that differences in the use of a decision strategy across cost vector treatments prevented the full scale of cost vector effects on MWTP to be exposed.

Differentiating by income group (Fig. 4, Tables A7, A8 and A9 in the Appendix) suggests an increasing trend in MWTP as cost vectors increase in magnitude that is more pronounced within the medium and high income groups. For lower income respondents (INC_{LOW}), no significant difference (at the 10% level) is found for all comparisons. For the medium and high income groups, MWTP for the different attributes is between two to three times greater in magnitude for HIGH$ compared to LOW$. This is also reflected by significant differences in MWTP estimates for some of the attributes between LOW$ and HIGH$ for cost vector treatments in the INC_{MED} and INC_{HIGH} groups. For INC_{MED} and INC_{HIGH}, significant differences are

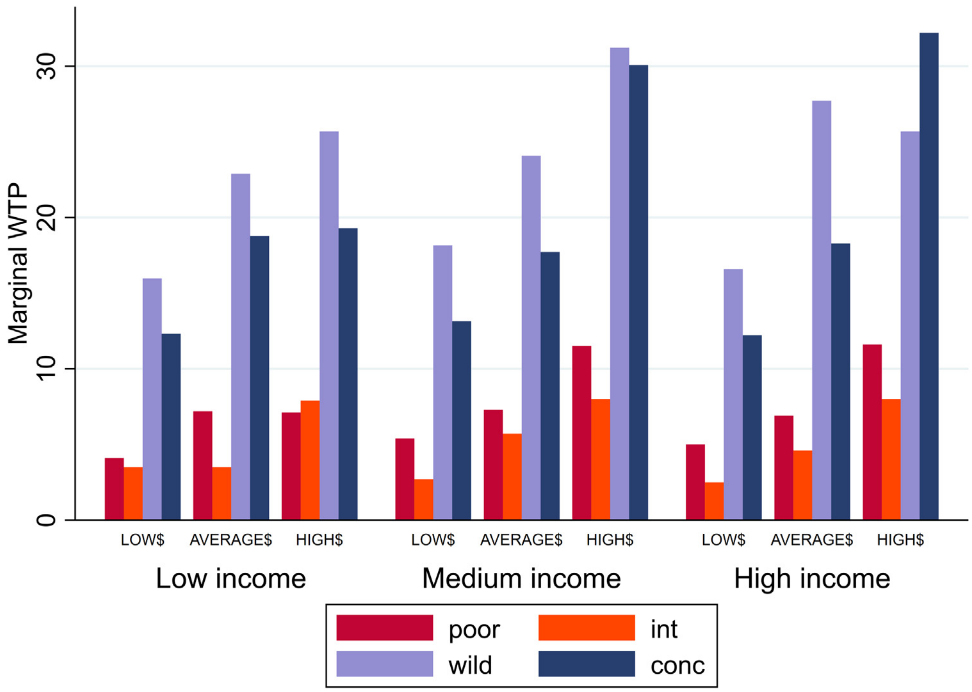

Fig. 4. Estimated marginal willingness to pay by cost vector and income group excluding respondents always choosing the cheapest (non SQ) alternative based on results shown in Table 10. Note: Estimates for *poor* and *int* are scaled by × 10 to enhance appearance.

found for three of the attributes. The results therefore suggest that cost vector differences can affect MWTP particularly for medium and high income groups.[9]

6. Discussion and conclusions

This paper adds to the literature on bid level and cost vector effects in stated preference studies. We compare three split samples with different cost vectors with respect to ‘bid acceptance’, marginal willingness to pay (WTP), status quo choice and the use of decision and information processing strategies. The concept of coherent arbitrariness offers a theoretical perspective through which cost vector effects on welfare estimates can be understood. Overall, we confirm findings of some of the previous studies that suggest that WTP tends to increase as the cost vector offered increases. In line with Kragt (2013a), ‘bid acceptance’ rates do not decrease markedly across cost vector treatments for higher cost vector levels, despite significantly greater absolute differences in costs across treatments. The most likely explanation for the differences in valuations across cost vector treatments is presence of anchoring effects to be expected if respondents behave according to the idea of coherent arbitrariness. As stated by Hanley et al. (2005, 230), it is “undesirable from a methodological point of view if the choice of price vector had a significant effect on resulting preference or willingness-to-pay estimates, since then choice experiments would be subject to a design bias akin to the anchoring effects found in contingent valuation”. We do indeed find such effects of varying cost vectors on WTP estimates.

Our results also suggest that there may be a fat tail equivalent to contingent valuation found in choice experiments. This finding is related to small and statistically insignificant differences in sensitivity to maximum cost vector levels that differed by a factor of more than two across cost vector treatments, indicating the presence of a persistent residual acceptance of alternatives at maximum cost. It is an empirical question if this residual acceptance could be reduced and thus the fat tails ‘pinned down’ if the magnitude of the maximum cost level is further increased. Of course, in the light of our findings, this appears challenging to do without risking a further increase in WTP, for example due to anchoring. In this respect, it may be useful to investigate cost vector effects in the presence of different *ex-ante* devices typically used to mitigate hypothetical bias (e.g. cheap talk, repeated opt-out reminder). While a limited impact on potential anchoring may be expected theoretically, *ex-ante* devices may reduce the incidence of respondents who select alternatives with the maximum cost level, thus helping to pin down fat tails in choice experiments. It would be also be useful to investigate the role of payment and policy consequentiality (e.g., Herriges et al., 2010) in relation to the presence of fat tails in choice experiments. Respondents with strong consequentiality beliefs, especially regarding the prospect of actual payment, can be expected to be less likely

[9] It should be noted that the findings regarding significant differences in marginal WTP estimates across cost vector treatments within income groups may partly be affected by lower sample size and consequently greater standard errors of coefficient estimates in INC_{LOW} and INC_{HIGH} compared to INC_{MED}.

to violate their upper cut-off point (highest acceptable amount they are willing to pay) and consider their actual budget constraint, thus potentially reducing the proportion of acceptance of alternatives at maximum cost.

Perhaps the most significant finding of this study is that different cost vectors can affect decision strategies employed by choice experiment survey respondents. Statistically significant differences in attribute non-attendance (AN-A) for the cost attribute point to differences in choice behaviour in response to different cost vectors. Differences in AN-A could have implications for WTP estimates, but we believe that further investigation of such effects requires a better understanding of reasons that underpin AN-A and that explain differences between alternative approaches to identify AN-A incidence. We also find significant differences across cost vector treatments for a decision strategy that might reflect unwillingness to trade-off cost and other non-cost attributes: always choosing the cheapest alternative when a non-status quo alternative was selected. Our results suggest that varying use of this decision strategy can obscure effects of cost vectors on marginal WTP estimates present for the part of the sample that did not exhibit these decision strategies. Indeed, after omitting respondents who use this decision strategy from the sample, we find a monotonic increase as the magnitude of the cost vector increases, and significant differences between all cost vector treatments for all of the attributes. This suggests that in moving forward, research investigating cost vector effects should take a more detailed look at decision strategies used. This can be considered as non-trivial: many different strategies may be used by respondents, and impacts of these on a naïve model (that does not account for them and might thus suffer from misspecification bias) might counteract systematic differences in preferences and WTP found for respondents who make trade-offs as assumed by random utility maximisation models. As an aside, our results suggest that efficient designs may not always be the ideal experimental design choice in the presence of non-compensatory strategies such as always choosing the cheapest alternative. This aspect deserves further investigation.

In our study, there is only weak support that marginal WTP of lower income respondents is affected differently compared to higher income respondents through the use of different cost vectors, and our findings may be driven by low income respondents being less likely to afford higher costs. Future research could investigate if greater variation in cost vector magnitude especially at the upper bound triggers differences in the response to different cost vectors across income groups. Furthermore, cost vector differences across other socio-economic dimensions or respondent-specific characteristics could be explored, including cognitive skills, engagement with the choice task, or knowledge and experience with a good that have been found to be related to anchoring (Furnham and Boo, 2011; Sugden et al., 2013; Alevy et al., 2015).

Differences in marginal WTP estimates associated with cost vector differences should also be of concern to policy makers who use these estimates. For example, while Hanley et al. (2005) do not find statistically significant differences in WTP estimates, they do find that an increase in benefit estimates of water quality improvement by 45% related to the use of a higher cost vector may tip a cost-benefit analysis from a positive to a negative net present value. Hence, differences in WTP might be judged as politically significant by a policy maker. An illustrative example for our case study shows that the same reasoning applies, even if we draw on WTP estimates from the full sample shown in Table 7 (that do not account for differences in always choosing the cheapest non-status quo alternative). For a peatland policy resulting in a 50% shift from each poor and intermediate to good ecological condition and with a focus on restoration in wild areas and areas with a high coverage of peatlands, WTP estimates for the highest cost vector treatment (HIGH$) are 45% higher compared to the lowest (LOW$) cost vector treatment. If we use results reported in Table 10, this difference more than doubles and becomes 111%. Both of these values can plausibly affect outcomes of a cost-benefit analysis on peatland restoration (Glenk and Martin-Ortega, 2018).

In conclusion, this paper provides further evidence that differences in the cost vector used within discrete choice experiments can have significant effects on WTP estimates, which in turn can impact policy decisions based on cost-benefit analysis. As a result, we recommend that—budget permitting—choice experiment practitioners should routinely use different cost vectors in the design of their studies and report on the sensitivity of WTP estimates to the different cost vectors used. Evidence accumulated in this way over time can then serve as a basis to move beyond relying on an educated guess to guide choice experiment design.

Acknowledgements

This research was partly funded by the Scottish Government Rural Affairs and the Environment Portfolio Strategic Research Programme 2011-2016 and 2016–2021, and the Natural Environment Research Council (NERC) through the project ‘Understanding ecosystem stocks and tipping points in UK peatlands’ (NE/P00783X/1). Authors are grateful to Andrew McBride, Rebekka Artz, Matt Aitkenhead, Willie Towers, Steve Chapman, Kathleen Allen, Anja Byg, Jacqueline Potts, Paula Novo, Carol Kyle, Murat Okumah and Ximena Maier.

Appendix A.

Table A1
RPL model results by cost vector treatment and income group (log-normally distributed price coefficient).

	Low income (INC_{LOW})						Medium income (INC_{MED})						High income (INC_{HIGH})					
	LOW$		AVERAGE$		HIGH$		LOW$		AVERAGE$		HIGH$		LOW$		AVERAGE$		HIGH$	
	Coef.	*t*	*Coef.*	*t*	*Coef.*	*t*	*Coef.*	*t*	*Coef.*	*t*	*Coef.*	*t*	*Coef.*	*t*	*Coef.*	*t*	*Coef.*	*t*
Coefficients																		
ASC	−0.444	−7.52	−0.534	−8.05	−0.557	−6.97	−0.625	−9.41	−0.426	−11.95	−0.515	−8.94	−0.633	−7.74	−0.636	−8.06	−0.531	−8.36
poor	1.148	1.97	0.867	1.21	1.311	2.09	2.005	4.24	2.072	4.17	2.053	3.57	1.511	2.60	1.684	2.56	1.056	1.60
int	1.147	2.89	0.183	0.35	1.317	3.20	1.079	3.24	1.784	5.09	1.533	4.09	0.841	2.24	1.295	2.88	1.058	2.31
wild	0.234	2.49	0.139	1.08	0.174	1.63	0.274	3.18	0.316	3.92	0.198	2.16	0.261	2.80	0.375	3.56	0.145	1.49
conc	0.238	2.88	0.338	3.28	0.187	1.97	0.258	3.47	0.206	2.59	0.286	2.76	0.238	3.01	0.260	2.18	0.280	2.66
price	0.860	3.99	0.794	3.75	0.589	3.56	1.266	8.56	1.071	9.03	0.771	5.86	1.009	5.00	1.011	5.82	0.606	3.80
Standard deviations																		
ASC	0.393	6.78	0.330	3.54	0.520	6.09	0.413	6.40	0.220	5.73	0.443	8.62	0.450	5.99	0.361	5.49	0.415	6.82
poor	3.139	4.78	3.607	4.83	2.248	3.30	1.705	2.45	3.193	5.45	3.167	4.71	3.066	4.96	3.309	4.26	3.320	4.17
int	2.069	3.82	2.415	4.60	1.374	2.89	1.563	3.85	2.146	4.84	1.763	3.67	1.689	4.16	2.375	4.11	2.443	4.87
wild	0.433	3.69	0.631	4.58	0.336	2.71	0.427	3.95	0.393	4.09	0.343	3.30	0.422	3.98	0.298	2.53	0.251	2.17
conc	0.592	5.88	0.686	5.85	0.577	5.55	0.654	7.87	0.740	8.32	0.992	8.20	0.643	6.97	1.019	7.00	0.703	6.46
pric	1.280	9.15	1.702	5.83	1.403	8.87	1.304	12.00	1.274	15.11	1.229	14.43	1.239	8.59	1.324	6.86	1.457	6.95
LogL-RPL	−813.7		−713.2		−815.9		−1055.7		−1236.6		−1121.0		−925.9		−771.6		−907.7	
Pseudo R2	0.30		0.36		0.37		0.36		0.33		0.37		0.34		0.37		0.37	
N (resp.)	134		127		148		188		212		203		161		140		165	

Table A2
Estimated marginal WTP by cost vector treatment and income group (based on model results in Table A1 ; RPL with log-normally distributed price coefficient).

	Low income			Medium income			High income		
	LOW$ mean [95%-CI]	AVERAGE$ mean [95%-CI]	HIGH$ mean [95%-CI]	LOW$ mean [95%-CI]	AVERAGE$ Mean [95%-CI]	HIGH$ mean [95%-CI]	LOW$ mean [95%-CI]	AVERAGE$ mean [95%-CI]	HIGH$ mean [95%-CI]
poor	0.21	0.09[a]	0.27	0.24	0.32	0.45	0.26	0.26	0.20[a]
	[0.01;0.42]	[-0.11;0.29]	[-0.02;0.57]	[0.12;0.36]	[0.18;0.45]	[0.20;0.70]	[0.07;0.44]	[0.04;0.47]	[-0.09;0.49]
int	0.21	0.02[a]	0.27	0.13	0.27	0.33	0.14	0.20	0.20[a]
	[0.07;0.36]	[-0.10;0.14]	[0.06;0.49]	[0.05;0.21]	[0.18;0.37]	[0.16;0.50]	[0.02;0.26]	[0.05;0.34]	[-0.02;0.42]
wild	8.74	2.95[a]	7.20[a]	6.62	9.61	8.61	8.82	11.38	5.49[a]
	[1.78;15.69]	[−4.09;9.99]	[−2.58;16.98]	[2.40;10.83]	[4.79;14.42]	[0.49;16.73]	[2.56;15.09]	[3.92;18.84]	[−2.86;13.84]
conc	8.86	7.19	7.77	6.23	6.28	12.44	8.05	7.89	10.59
	[2.76;14.95]	[0.00;14.37]	[0.13;15.41]	[2.52;9.94]	[1.63;10.94]	[3.79;21.08]	[2.74;13.37]	[0.58;15.21]	[1.35;19.82]

Note:
[a] Not significantly different from zero at the 5% level.

Table A3
Results of the Poe et al. (2005) test (two-sided p-values) for marginal WTP estimates reported in Table A2.

	Low income			Medium income			High income		
	AVERAGE$ vs LOW$	HIGH$ vs LOW$	HIGH$ vs AVERAGE$	AVERAGE$ vs LOW$	HIGH$ vs LOW$	HIGH$ vs AVERAGE$	AVERAGE$ vs LOW$	HIGH$ vs LOW$	HIGH$ vs AVERAGE$
poor	na	0.746	na	0.422	0.134	0.362	0.970	na	na
int	na	0.648	na	**0.027**	**0.026**	0.528	0.604	na	na
wild	na	na	na	0.360	0.658	0.850	0.642	na	na
conc	0.662	0.806	0.882	0.982	0.194	0.222	0.926	0.666	0.642

Note: significant differences in MWTP estimates at the 10% level (two-sided test) are shown in bold; na: comparison includes at least one estimate of marginal WTP that is not significantly different from zero.

Table A4
Model results of equality constrained latent class model with 8 classes used to infer attribute non-attendance.

	LOW$		AVERAGE$		HIGH$	
	Coef.	*t*	*Coef.*	*t*	*Coef.*	*t*
Class1	−0.799	−1.89	−0.074	−0.23	−0.217	−0.94
Class2	−0.002	−0.56	−0.5	−1.21	−0.071	−0.31
Class3	−0.43	−2.21	−0.18	−1.03	−0.552	−2.89
Class4	−1.551	−2.52	−1.044	−2.03	−0.848	−2.48
Class5	1.037	5.35	0.709	3.44	0.518	3.11
Class6	0.048	0.22	−0.567	−2.12	−0.970	−4.07
Class7	0.475	2.2	0.816	3.97	0.9286	7.24
ASC1	−2.810	−4.92	−2.832	−5.98	−3.589	−7.97
ASC2	−11.636	−1.65	−11.079	−1.79	−9.958	−6.9
ASC3	4.386	5.47	2.347	7.71	7.337	2.26
ASC4	−0.495	−0.76	−0.495	−0.68	0.996	2.97
ASC5	−4.217	−9.44	−4.319	−7.88	−6.846	−12.51
ASC6	−1.364	−3.46	−3.166	−1.73	2.524	7.04
ASC7	−1.444	−6.65	−1.947	−8.04	−2.169	−11.92
poor	0.011	2.96	0.02	4.56	0.026	6.11
int	0.007	3.32	0.015	5	0.013	5
wild	0.153	2.48	0.249	3.63	0.194	2.7
conc	0.877	6.73	0.874	6.97	1.011	9.42
price	−0.048	−16.62	−0.033	−18.53	−0.030	−17.82
LogL	−2,935.3		−2,906.1		−3,045.1	
N (resp.)	570		551		605	

Table A5
Mean estimates and confidence intervals of probability of attribute non-attendance (AN-A) for all attributes by cost vector treatment inferred from ECLC models (%).

	poor	*int*	*wild*	*conc*	*price*
LOW$	53.2	49.8	46.3	69	39.2
	[45.9–60.4]	[42.1–57.9]	[38.2-54.5]	[59.5–77.2]	[34.2–44.5]
AVERAGE$	51.9	53.7	49.5	65.7	28.6
	[44.8–58.8]	[45.8–61.4]	[41.8–57.5]	[57–73.2]	[23.9–33.9]
HIGH$	63.6	60.2	59	70.4	34.8
	[57.9–69.2]	[54.7–65.4]	[53.5–64.3]	[64.4–75.7]	[30.2–39.7]

Note: 95% confidence intervals are reported in parentheses. Confidence intervals were calculated using a Krinsky and Robb (1986) procedure with 2,000 draws.

Table A6
Stated attribute non-attendance (AN-A) to the cost attribute in cost vector treatments and across income groups (%).

	1 Sometimes considered	2 Never considered	3 Sum (1 + 2)	4 Inferred A-NA	5 Δ (3-4)
LOW$	28.7	8.2	37.0	39.2	−2.2
AVERAGE$	25.3	9.3	34.6	28.6	6.0
HIGH$	23.4	8.4	31.8	34.8	−3.0

Table A7
RPL model results by cost vector treatment and income group excluding respondents always choosing the cheapest (non SQ) alternative (log-normally distributed price coefficient).

	Low income						Medium income						High income					
	LOW$		AVERAGE$		HIGH$		LOW$		AVERAGE$		HIGH$		LOW$		AVERAGE$		HIGH$	
	Coef.	*t*	*Coef.*	*t*	*Coef.*	*t*	*Coef.*	*t*	*Coef.*	*t*	*Coef.*	*t*	*Coef.*	*t*	*Coef.*	*t*	*Coef.*	*t*
Coefficients																		
ASC	−0.435	−6.05	−0.527	−6.21	−0.433	−5.36	−0.560	−8.01	−0.356	−9.89	−0.444	−6.94	−0.571	−7.02	−0.602	−6.94	−0.455	−6.80
poor	1.633	2.40	2.333	3.31	1.211	1.81	2.489	4.76	2.468	4.87	2.316	3.94	2.223	3.56	2.338	2.97	2.401	3.47
int	1.388	2.97	1.125	2.37	1.330	3.05	1.229	3.47	1.914	5.52	1.608	3.99	1.132	2.82	1.565	3.04	1.651	3.53
wild	0.313	2.96	0.368	3.08	0.213	2.10	0.417	4.63	0.406	5.12	0.314	3.29	0.366	3.74	0.466	3.82	0.267	2.67
conc	0.244	2.75	0.305	2.85	0.169	1.70	0.303	3.87	0.301	3.64	0.305	2.96	0.270	3.20	0.310	2.49	0.333	3.43
price	0.620	2.45	0.545	2.66	−0.189	−0.76	1.107	7.86	0.838	7.02	0.295	2.14	0.920	5.18	0.769	4.51	0.217	1.28
Standard deviations																		
ASC	0.397	6.13	0.464	4.51	0.459	5.77	0.388	5.67	0.216	5.62	0.465	7.59	0.458	5.52	0.383	5.03	0.467	6.23
poor	3.350	4.44	3.559	3.61	2.955	2.94	1.430	2.28	3.105	5.20	2.896	3.92	2.896	4.35	3.398	3.82	3.827	4.49
int	2.454	4.20	2.076	3.25	1.839	2.90	1.265	2.95	2.114	4.83	2.007	3.42	1.878	4.15	2.245	3.65	2.399	3.88
wild	0.455	3.48	0.578	3.54	0.267	2.14	0.329	2.99	0.350	2.90	0.389	2.91	0.448	3.47	0.428	3.25	0.164	1.49
conc	0.627	5.56	0.700	5.81	0.633	5.34	0.656	7.26	0.728	7.80	0.971	7.73	0.678	6.73	0.989	6.50	0.708	6.54
price	1.161	9.68	0.998	7.12	1.024	4.88	0.797	6.05	0.768	7.64	0.837	9.91	0.928	9.97	0.825	6.49	0.892	7.21
LogL-RPL	−708.9		−581.4		−598.7		−856.8		−972.9		−886.7		−826.4		−637.8		−728.9	
Pseudo R2	0.30		0.34		0.31		0.34		0.30		0.33		0.33		0.35		0.33	
N (resp.)	116		102		100		149		158		152		142		113		124	

Table A8
Marginal WTP estimates by cost vector treatment and income group excluding respondents always choosing the cheapest (non SQ) alternative (based on model results in Table A7 ; RPL with log-normally distributed price coefficient).

	Low income (INC_{LOW})			Medium income (INC_{MED})			High income (INC_{HIGH})		
	LOW$ mean [95%-CI]	AVERAGE$ mean [95%-CI]	HIGH$ mean [95%-CI]	LOW$ mean [95%-CI]	AVERAGE$ Mean [95%-CI]	HIGH$ mean [95%-CI]	LOW$ mean [95%-CI]	AVERAGE$ mean [95%-CI]	HIGH$ mean [95%-CI]
poor	0.41 [0.16;0.63]	0.72 [0.41;1.04]	0.71 [0.08;1.36]	0.54 [0.36;0.72]	0.73 [0.51;0.95]	1.15 [0.74;1.56]	0.50 [0.30;0.69]	0.69 [0.33;1.07]	1.16 [0.66;1.66]
int	0.35 [0.19;0.51]	0.35 [0.12;0.58]	0.79 [0.37;1.26]	0.27 [0.14;0.39]	0.57 [0.41;0.73]	0.80 [0.51;1.10]	0.25 [0.12;0.39]	0.46 [0.23;0.70]	0.80 [0.45;1.15]
wild	15.97 [7.91;24.25]	22.87 [10.90;35.24]	25.67 [5.19;47.86]	18.15 [11.43;24.76]	24.07 [16.38;32.06]	31.22 [16.22;45.75]	16.58 [9.46;23.89]	27.70 [16.46;39.24]	25.67 [10.33;40.43]
conc	12.31 [5.67;19.18]	18.77 [8.76;29.43]	19.29 [0.79;37.28]	13.14 [7.75;19.15]	17.71 [10.16;25.62]	30.07 [14.70;45.59]	12.22 [6.06;19.06]	18.28 [6.49;30.73]	32.20 [17.71;47.93]

Table A9
Results of the Poe et al. (2005) test (two-sided p-values) for marginal WTP estimates reported in Table A8.

	Low income			Medium income			High income		
	AVERAGE$ vs LOW$	HIGH$ vs LOW$	HIGH$ vs AVERAGE$	AVERAGE$ vs LOW$	HIGH$ vs LOW$	HIGH$ vs AVERAGE$	AVERAGE$ vs LOW$	HIGH$ vs LOW$	HIGH$ vs AVERAGE$
poor	0.188	0.466	0.998	0.278	**0.024**	0.138	0.452	**0.038**	0.214
int	0.968	0.112	0.132	**0.010**	**0.006**	0.252	0.204	**0.016**	0.190
wild	0.442	0.496	0.866	0.344	0.188	0.484	0.180	0.378	0.870
conc	0.390	0.540	0.952	0.436	**0.092**	0.242	0.466	**0.042**	0.238

Note: significant differences in MWTP estimates at the 10% level (two-sided test) are shown in bold.

Table A10
Marginal WTP estimates for cost vector treatments and results of Poe et al. (2005) test for differences between treatments (RPL with constrained triangular distribution of price coefficient).

	LOW$ mean [95%-CI]	AVERAGE$ Mean [95%-CI]	HIGH$ mean [95%-CI]	AVERAGE$ vs LOW$ p-value	HIGH$ vs LOW$ p-value	HIGH$ vs AVERAGE$ p-value
poor	0.37 [0.23;0.50]	0.56 [0.36;0.77]	0.82 [0.51;1.11]	0.110	**0.010**	0.176
int	0.23 [0.13;0.32]	0.40 [0.26;0.53]	0.66 [0.45;0.86]	**0.042**	**0.000**	**0.036**
wild	15.14 [10.33;19.83]	24.97 [18.39;31.52]	25.54 [15.63;35.26]	**0.018**	**0.062**	0.920
conc	12.21 [7.73;16.67]	11.10 [4.88;17.19]	13.73 [3.72;23.43]	0.776	0.776	0.652

Note: significant differences in MWTP estimates at the 10% level (two-sided test) are shown in bold.

Table A11
Marginal WTP estimates for cost vector treatments excluding respondents always choosing the cheapest (non SQ) alternative (RPL with constrained triangular distribution of price coefficient).

	LOW$ mean [95%-CI]	AVERAGE$ Mean [95%-CI]	HIGH$ mean [95%-CI]	AVERAGE$ vs LOW$ p-value	HIGH$ vs LOW$ p-value	HIGH$ vs AVERAGE$ p-value
poor	0.62 [0.45;0.78]	1.02 [0.77;1.26]	1.45 [1.03;1.87]	**0.010**	**0.000**	**0.082**
int	0.35 [0.23;0.47]	0.66 [0.49;0.83]	1.10 [0.80;1.39]	**0.004**	**0.000**	**0.014**
wild	23.90 [17.62;29.90]	37.65 [28.93;46.22]	44.24 [29.03;58.48]	**0.012**	**0.012**	0.452
conc	16.91 [10.75;23.27]	23.21 [14.43;32.04]	34.84 [18.96;50.88]	0.252	**0.038**	0.208

Note: significant differences in MWTP estimates at the 10% level (two-sided test) are shown in bold.

References

Alemu, M., Mørkbak, M., Olsen, S., Jensen, C., 2013. Attending to the reasons for attribute non-attendance in choice experiments. Environ. Resour. Econ. 54, 333–359.
Alevy, J.E., Landry, C.E., List, J.A., 2015. Field experiments on the anchoring of economic valuations. Econ. Inq. 53 (3), 1522–1538.
Aravena, C., Martinsson, P., Scarpa, R., 2014. Does money talk? — the effect of a monetary attribute on the marginal values in a choice experiment. Energy Econ. 44, 483–491.
Ariely, D., Loewenstein, G., Prelec, D., 2003. Coherent arbitrariness: stable demand curves without stable preferences. Q. J. Econ. 118 (1), 73–105.
Ariely, D., Loewenstein, G., Prelec, D., 2006. Tom Sawyer and the construction of value. J. Econ. Behav. Organ. 60, 1–10.
Bateman, I.J., Burgess, D., Hutchinson, W.G., Matthews, D.I., 2008. Learning design contingent valuation (LDCV): NOAA guidelines, preference learning and coherent arbitrariness. J. Environ. Econ. Manage. 55, 127–141.
Bateman, I.J., Day, B.H., Jones, A.P., Jude, S., 2009. Reducing gain-loss asymmetry: a virtual reality choice experiment valuing land use change. J. Environ. Econ. Manage. 58 (1), 106–118.
Bettman, J.R., Luce, M.F., Payne, J.W., 1998. Constructive consumer choice processes. J. Consum. Res. 25 (3), 187–217.
Blasch, J., Farsi, M., 2013. Context effects and heterogeneity in voluntary carbon offsetting – a choice experiment in Switzerland. J. Environ. Econ. Policy 3, 1–24.
Boyle, K.J., Bishop, R.C., Welsh, M.P., 1985. Starting point bias in contingent valuation bidding games. Land Econ. 61, 188–194.
Boyle, K.J., MacDonald, H.F., Cheng, H., McCollum, D.W., 1998. Bid design and yea saying in single-bounded, dichotomous-choice questions. Land Econ. 74, 49–64.
Braga, J., Starmer, C., 2005. Preference anomalies, preference elicitation and the discovered preference hypothesis. Environ. Resour. Econ. 32 (1), 55–89.
Brown, T.C., Champ, P.A., Bishop, R.C., McCollum, D.W., 1996. Which response format reveals the truth about donations to a public good. Land Econ. 72 (2), 152–166.
Brown, T.C., Kingsley, D., Peterson, G.L., Flores, N.E., Clarke, A., Birjulin, A., 2008. Reliability of individual valuations of public and private goods: choice consistency, response time, and preference refinement. J. Public Econ. 92 (7), 1595–1606.
Cameron, T.A., DeShazo, J.R., 2010. Differential attention to attributes in utility-theoretic choice models. J. Choice Model. 3 (3), 73–115.
Campbell, D., Hutchinson, W.G., Scarpa, R., 2008. Incorporating discontinuous preferences into the analysis of discrete choice experiments. Environ. Resour. Econ. 41, 401–417.
Campbell, D., Hensher, D.A., Scarpa, R., 2011. Non-attendance to attributes in environmental choice analysis: a latent class specification. J. Environ. Plan. Manag. 54, 1061–1076.
Caputo, V., Scarpa, R., Nayga, R.M., 2017. Cue versus independent food attributes: the effect of adding attributes in choice experiments. Eur. Rev. Agric. Econ. 44 (2), 211–230.
Carlsson, F., Martinsson, P., 2008. How much is too Much? An investigation of the effect of the number of choice sets, context dependence and the choice of bid vectors in choice experiments. Environ. Resour. Econ. 40, 165–176.
Carlsson, F., Frykblom, P., Lagerkvist, C.J., 2007. Preferences with and without prices - does the price attribute affect behavior in stated preference surveys? Environ. Resour. Econ. 38, 155–164.
Chien, Y., Huang, C.J., Shaw, D., 2005. A general model of starting point bias in double-bounded dichotomous contingent valuation surveys. J. Environ. Econ. Manage. 50 (2), 363–377.
Colombo, S., Glenk, K., 2013. Social preferences for agricultural policy instruments: joint consideration of non-attendance to attributes and to alternatives in modelling discrete choice data. J. Environ. Plan. Manag. 57 (2), 215–232.
Colombo, S., Glenk, K., Rocamora-Montiel, B., 2015. Analysis of choice inconsistencies in on-line choice experiments: impact on welfare measures. Eur. Rev. Agric. Econ. 43 (2), 271–302.
Cooper, J.C., Loomis, J., 1992. Sensitivity of willingness-to-Pay estimates to bid design in dichotomous choice contingent valuation models. Land Econ. 68, 211–224.
Day, B., Pinto Prades, J.-L., 2010. Ordering anomalies in choice experiments. J. Environ. Econ. Manage. 59, 7675–7697.
Desvousges, W.H., Matthews, K., Train, K., 2015. An adding-up test on contingent valuations of river and lake quality. Land Econ. 91 (3), 556–571.
Dziegielewska, D., Mendelsohn, R., 2007. Does No mean No? A protester methodology. Environ. Resour. Econ. 38, 71–87.
Furnham, A., Boo, H.C., 2011. A literature review of the anchoring effect. J. Socio. 40, 35–42.
Garrod, G.D., Willis, K.G., 1999. Economic Valuation of the Environment—methods and Case Studies. Edward Elgar Publishing, Cheltenham.
Glenk, K., Martin-Ortega, M., 2018. The economics of peatland restoration. J. Environ. Econ. Policy 7 (4), 345–362.
Glenk, K., Martin-Ortega, J., Pulido-Velazquez, M., Potts, J., 2015. Inferring attribute non-attendance from discrete choice experiments: implications for benefit transfer. Environ. Resour. Econ. 60, 497–520.
Gneezy, A., Gneezy, U., Lauga, D.O., 2014. A reference-dependent model of the price-quality heuristic. J. Mark. Res., 153–164.
Hanley, N., Wright, R.E., Koop, G., 2002. Modelling recreation demand using choice experiments: climbing in Scotland. Environ. Resour. Econ. 22, 449–466.
Hanley, N., Adamowicz, W., Wright, R.E., 2005. Price vector effects in choice experiments: an empirical test. Resour. Energy Econ. 27, 227–234.
Hanley, N., Kristrom, B., Shogren, J.F., 2009. Coherent arbitrariness: on value uncertainty for environmental goods. Land Econ. 85 (1), 41–50.
Hensher, D.A., Rose, J.M., Greene, W.H., 2005. The implications on willingness to pay of respondents ignoring specific attributes. Transportation 32 (3), 203–222.
Hensher, D.A., Rose, J., Greene, W., 2012. Inferring attribute non-attendance from stated choice data: implications for willingness to pay estimates and a warning for stated choice experiment design. Transportation 39, 235–245.
Herriges, J.A., Shogren, J.F., 1996. Starting point bias in dichotomous choice valuation with follow-up questioning. J. Environ. Econ. Manage. 30, 112–131.
Herriges, J., Kling, C., Liu, C.-C., Tobias, J., 2010. What are the consequences of consequentiality? J. Environ. Econ. Manage. 59, 67–81.

Hess, S., Beharry-Borg, N., 2012. Accounting for latent attitudes in willingness-to-pay studies: the case of coastal water quality improvements in Tobago. Environ. Resour. Econ. 52, 109–131.
Hess, S., Train, K.E., 2017. Correlation and scale in mixed logit models. J. Choice Model. 23, 1–8.
Hess, S., Stathopoulos, A., Campbell, D., O'Neill, V., Caussade, S., 2013. It's not that I don't care, I just don't care very much: confounding between attribute non-attendance and taste heterogeneity. Transportation 40, 583–607.
Hess, S., Daly, A., Batley, R., 2018. Revisiting consistency with random utility maximisation: theory and implications for practical work. Theory Decis. 84 (2), 181–204.
Hoeffler, S., Ariely, D., 1999. Constructing stable preferences: a look into dimensions of experience and their impact on preference stability. J. Consum. Psychol. 8 (2), 113–139.
Johnston, R.J., Boyle, K.J., Adamowicz, W., Bennett, J., Brouwer, R., Cameron, T.A., Hanemann, W.M., Hanley, N., Scarpa, R., Tourangeau, R., Vossler, C.A., 2017. Contemporary guidance for stated preference studies. J. Assoc. Environ. Resour. Econ. 4, 319–405.
Koetse, M.J., 2017. Effects of payment vehicle non-attendance in choice experiments on value estimates and the WTA–WTP disparity. J. Environ. Econ. Policy 6, 225–245.
Kragt, M.E., 2013a. The effects of changing cost vectors on choices and scale heterogeneity. Environ. Resour. Econ. 54, 201–221.
Kragt, M.E., 2013b. Stated and inferred attribute attendance models: a comparison with environmental choice experiments. J. Agric. Econ. 64, 719–736.
Krinsky, I., Robb, L., 1986. On approximating the statistical properties of elasticities. Rev. Econ. Stat. 68, 715–719.
Ladenburg, J., Olsen, S.B., 2008. Gender-specific starting point bias in choice experiments: evidence from an empirical study. J. Environ. Econ. Manage. 56, 275–285.
Luisetti, T., Bateman, I.J., Turner, R.K., 2011. Testing the fundamental assumption of choice experiments: are values absolute or relative? Land Econ. 87, 284–296.
Martin-Ortega, J., Glenk, K., Byg, A., 2017. How to make complexity look simple? Conveying ecosystems restoration complexity for socio-economic research and public engagement. PLoS One 12 (7), e0181686.
Matthews, Y., Scarpa, R., Marsh, D., 2017. Using virtual environments to improve the realism of choice experiments: a case study about coastal erosion management. J. Environ. Econ. Manage. 81, 193–208.
Meyerhoff, J., Glenk, K., 2015. Learning how to choose - effects of instructional choice sets in discrete choice experiments. Resour. Energy Econ. 41, 122–142.
Mørkbak, R.M., Christensen, T., Gyrd-Hansen, D., 2010. Choke price Bias in choice experiments. Environ. Resour. Econ. 45, 537–551.
Mørkbak, M.R., Olsen, S.B., Campbell, D., 2014. Behavioral implications of providing real incentives in stated choice experiments. J. Econ. Psychol. 45, 102–116.
Oehlmann, M., Meyerhoff, J., Mariel, P., Weller, P., 2017. Uncovering context-induced status quo effects in choice experiments. J. Environ. Econ. Manage. 81, 59–73.
Parsons, G.R., Myers, K., 2016. Fat tails and truncated bids in contingent valuation: an application to an endangered shorebird species. Ecol. Econ. 129, 210–219.
Payne, J.W., Bettman, J.R., Schkade, D.A., 1999. Measuring constructed preferences: towards a building code. J. Risk Uncertain. 19, 243–270.
Poe, G., Severance-Lossin, E., Welsh, M., 2005. Simple computational methods for measuring the difference of empirical distributions. Am. J. Agric. Econ. 87, 353–365.
Puckett, S.M., Hensher, D.A., 2008. The role of attribute processing strategies in estimating the preferences of road freight stakeholders. Transport. Res. Part E 44, 379–395.
Rao, A.R., Monroe, K.B., 1989. The effect of price, brand name, and store name on buyers' perceptions of product quality: an integrative review. J. Mark. Res. 36, 351–357.
Ready, R.C., Hu, D., 1995. Statistical approaches to the fat tail problem for dichotomous choice contingent valuation. Land Econ. 71 (4), 491–499.
Ready, R.C., Whitehead, J.C., Blomquist, G.C., 1995. Contingent valuation when respondents are ambivalent. J. Environ. Econ. Manage. 29, 181–196.
Ryan, M., Wordsworth, S., 2000. Sensitivity of willingness to pay estimates to the level of attributes in discrete choice experiments. Scott. J. Polit. Econ. 47, 504–524.
Scarpa, R., Gillbride, T.J., Campbell, D., Hensher, D.A., 2009. Modelling attribute non-attendance in choice experiments for rural landscape valuation. Eur. Rev. Agric. Econ. 36, 151–174.
Scarpa, R., Zanoli, R., Bruschi, V., Naspetti, S., 2013. Inferred and stated attribute non-attendance in food choice experiments. Am. J. Agric. Econ. 95, 165–180.
Slovic, P., 1995. The construction of preferences. Am. Psychol. 50, 364–371.
Su, L., Adam, B.D., Lusk, J.L., Arthur, F., 2017. Anchoring, information, and fragility of choice experiments: an application to consumer willingness to pay for rice with imporved storage management. J. Agric. Res. Econ. 42, 255–274.
Sugden, R., Zheng, J., Zizzo, D.J., 2013. Not all anchors are created equal. J. Econ. Psychol. 39, 21–31.
Svenningsen, L.S., Jacobsen, J.B., 2018. Testing the effect of changes in elicitation format, payment vehicle and bid range on the hypothetical bias for moral goods. J. Choice Modell. 29, 17–32.
Train, K., 2003. Discrete Choice Methods with Simulation. Cambridge University Press, Cambridge, UK.
Tversky, A., Kahneman, D., 1974. Judgment under uncertainty: heuristics and biases. Science 185, 1124–1131.
van Kooten, G.C., Krcmar, E., Bulte, E.H., 2001. Preference uncertainty in non-market valuation: a fuzzy approach. Am. J. Agric. Econ. 83, 487–500.
van Zanten, B.T., Koetse, M.J., Verburg, P.H., 2016. Economic valuation at all cost? The role of the price attribute in a landscape preference study. Ecosyst. Serv. 22, 289–296.
Veronesi, M., Alberini, A., Cooper, J.C., 2011. Implications of bid design and willingness-to-pay distribution for starting point bias in double-bounded dichotomous choice contingent valuation surveys. Environ. Resour. Econ. 49, 199–215.
Weller, P., Mariel, P., Oehlmann, M., Meyerhoff, J., 2014. Stated and inferred attribute non-attendance in a design of designs approach. J. Choice Model. 14, 43–56.