Changing with the tide: Semi-parametric estimation of preference dynamics

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Abstract

We test the discovered preference hypothesis against the theory of coherent arbitrariness in a split-sample stated choice experiment on flood risk exposure in the Netherlands. A semi-parametric local multinomial logit model is proposed as an alternative method to the Swait and Louviere (1993) test procedure to control for preference dynamics in stated choice experiments. We find evidence of a declining impact over the choice sequence of an induced starting point bias in the first choice task. The results provide indicative support for convergence in preferences between both samples, which is in line with the discovered preference hypothesis.

Keywords: Preference dynamics; Discovered preference hypothesis; Coherent arbitrariness; Preference uncertainty; Local multinomial logit model

JEL classification numbers – C14, D12, Q51, Q54

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I. Introduction

Do people discover their preferences when they repeatedly make choices? And do these discovered preferences depend on arbitrary value clues? These questions are of key importance for the economic modelling of choices of people, especially for the valuation of non-market goods where researchers strongly rely on estimated preferences obtained from stated choice experiments. These experiments are a popular method to value these goods and services in different research areas, including transport, health and environmental economics (e.g. Louviere et al. 2003). By including cost as an attribute of the non-market good, the researcher can derive willingness to pay (WTP) estimates for a specific change in the non-market good. WTP estimates are obtained through discrete choice models, which have their theoretical foundation in the random utility framework (McFadden 2001). The random utility model (RUM) assumes respondents are rational and have well-defined preferences, i.e. complete and transitive utility functions. Hence, when asked to choose between different alternatives in a stated choice experiment, respondents can identify in each choice task their preferred alternative and they know at which rate they are willing to give up one (policy) attribute for another. However, non-market valuation studies are frequently applied to (new) goods and services with which the respondent has only limited experience. The existence of well-defined preferences has therefore been questioned (e.g. Bateman et al. 2008; Brouwer et al. 2010; Shaikh et al. 2007).

Two contrasting hypotheses exist regarding the extent to which preferences are subject to change as respondents gain experience with the non-market good, the hypothetical market, and its institutional settings. Plott (1996) introduced the discovered preference hypothesis (DPH), which is based on the assumption that through repetition and market experience, individuals discover and learn about their preferences. Learning opportunities are frequently acknowledged to result in reductions in anomalous behaviour, such as the WTA-WTP disparity (List 2003; Loomes et al. 2010). Plott's hypothesis that preferences evolve over a sequence of choices is congruent with the preference construction literature (e.g. Ariely et al. 2003; Ariely et al. 2006; Fischhoff et al. 1999; McFadden 1999; Slovic 1995). For example, Ariely et al. (2003) stipulate in their theory of coherent arbitrariness (CA) that an individual gradually develops a set of stable preferences due to an internal drive for consistency. Since past decisions drive future choices, the convergence process is path dependent and possibly influenced by initial (arbitrary) value cues. As such, the preference construction literature argues that a stable set of preferences is non-existent prior to the stated choice experiment.[2] In contrast, the DPH predicts convergence to a stable set of preferences, but assumes that the convergence level is path independent (Bateman et al. 2008; Braga and Starmer 2005). Closely related is the discussion about the existence and decay of a starting point bias (SPB), which has been extensively discussed in the contingent valuation literature and more recently also in the literature on stated choice experiments (Carlsson and Martinsson 2008; Groeneveld 2010; Ladenburg and Olsen 2008).

The number of studies directly testing the hypotheses embodied in the DPH and CA models is limited. Bateman et al. (2008) and Ladenburg and Olsen (2008) find support for the DPH by observing a rapid decay in the SPB in respectively a repeated dichotomous choice contingent valuation study and a stated choice experiment. Persistent contextual framing effects are also not observed by Carlsson and Martinsson (2008). On the contrary, Groeneveld (2010) does not observe decay in SPB in a stated choice experiment. Related studies show that marginal WTP estimates are sensitive to the maximum level of the cost attribute (Morkbak et al. 2010) and the number of choice cards included in the experiment (Bech et al. 2011). Day and Pinto (2010) find that the order in which choice tasks are presented to the

[2] Even though the DPH assumes stable preferences exist before respondents come to the (hypothetical) market, both the DPH and theory of CA assume preferences are not (yet) fully known *to the respondent* at that stage, which induces choice uncertainty.

respondent may also affect welfare estimates. Dynamics in preferences over the choice sequence are observed in (Carlsson et al. 2010; Holmes and Boyle 2005; Swait and Adamowicz 2001). However, these findings are not supported by Brouwer et al. (2010) and Savage and Waldman (2008). Brown et al. (2008) and Kingsley and Brown (2010) find evidence of preference learning by reductions in decisional variance over a sequence of choices. Indications of fatigue are also found in the literature (Arentze et al. 2003; Bradley and Daly 1994; Caussade et al. 2005; Lundhede et al. 2009).

The stated choice experiment presented in this paper is specifically designed to contrast the DPH and the theory of CA and testing for dynamics in a set of (scale-free) marginal WTP estimates of interest over the choice sequence. Thereby, the main aim of this paper is to empirically identify possible preference dynamics in stated choice experiments. Similar to Ladenburg and Olsen (2008), we induce a SPB and test for dynamics in welfare measures over the choice sequence in a stated choice experiment on flood risk reduction in the Netherlands.[3] The paper offers two main contributions to the literature. First, we use an improved experimental design enabling us to better identify dynamics in welfare measures over the choice sequence. Second, we present a novel econometric approach, called the local multinomial logit (L-MNL) model, as an alternative to the commonly applied Swait and Louviere (1993) test procedure (e.g. Bech, Kjaer and Lauridsen 2011; Brouwer et al. 2010; Carlsson, Morkbak and Olsen 2010; Holmes and Boyle 2005; Ladenburg and Olsen 2008). The L-MNL model provides an intermediate solution to either fully combining or entirely splitting samples of interest. The latter is required when using the Swait and Louviere test.

[3] Inducing a SPB is one approach to contrast the competing hypotheses comprised in the DPH and theory of CA.The presence of a starting point bias is interpreted here as if a stable set of preferences is not known to the respondent at the start of the survey. A decay of the starting point bias supports the DPH, while a persistent starting point bias supports the theory of CA. Support for either hypothesis remains conditional on a stable set of preferences by the end of the choice sequence. Alternative tests contrasting both hypotheses may, for example, alter the order of the choice sequence, thereby inducing path dependence.

Moreover, the L-MNL model is expected to significantly reduce the size of standard errors compared to estimating choice task specific welfare measures (Fröhlich 2006, Koster and Koster, 2011).

The structure of the paper is as follows. Section II describes the context, underlying hypotheses and experimental set-up of the paper. Section III discusses the properties of the local MNL model. Section IV provides a description of the case study and Section V covers the analytical results. Finally, Section V concludes.

II. Empirical approach: contrasting the DPH and theory of CA

Within and between sample preference dynamics

Similar to Ladenburg and Olsen (2008), we induce a SPB between two independent samples through the experimental design. We refer to these samples as the Low Starting Bid (LSB) and High Starting Bid (HSB) sample respectively. The only difference between the two samples is in the first choice task.[4] The policies presented in the first choice task were identical across samples, except for the cost level of the proposed policies. The LSB sample was assigned the lowest levels of the price vector and the HSB sample the highest levels for exactly the same policy alternatives.[5] In the following eight choice tasks all respondents were presented with exactly the same experimental design and the same attribute levels for the subsequent alternatives, including the cost levels. Hence, our study only takes into account starting point effects, but not the effect of showing different attribute levels to different

[4] The words choice task and choice card will be used in the remainder of this paper. The former refers to the position of the choice in the choice sequence. The latter refers to a specific choice situation as included in the experimental design. Respondents were presented with two alternative policies and an opt-out option (more details in Section IV).

[5] Within each sample everybody answered exactly the same first choice task.

respondents (e.g. Carlsson and Martinsson 2008; Hanley et al. 2005; Morkbak et al. 2010; Ohler et al. 2000).[6]

By anchoring respondents on the price of the presented policy alternatives in the first choice task, we expect respondents in the LSB sample to be more sensitive to changes in the cost attribute during the remainder of the choice sequence. They have a lower reference value induced by the experimental design. Therefore, marginal WTP values are expected to be significantly higher in the HSB sample and a potential SPB may be present. As respondents proceed through the remaining eight choice tasks, they encounter different attribute levels and learn about their preferences. Consequently, the impact of the initial choice task on later choices in the sequence is expected to decay. The DPH predicts that marginal WTP estimates stabilize and converge between both samples. More formally, we test for two types of dynamics over the choice sequence. First, we test for *within-sample* preference dynamics, where both the DPH and CA hypothesis predict the emergence of a stable set of preferences due to learning. Second, we test for *between-sample* preference dynamics at the choice task level. The DPH predicts convergence in preferences between samples, whereas CA predicts a set of stabilizing preferences subject to a persistent SPB throughout the choice sequence.

Improving the experimental design

Testing for *within* and *between* sample preference dynamics requires the estimation of choice task specific preference parameters within the LSB and HSB samples.[7] Estimation of a choice model for a specific choice task in each sample requires that all choice cards in the experimental design are answered a sufficient number of times at each moment during the

[6] A common finding in the contingent valuation literature is that using systematically higher bid levels results in higher WTP values (Chien et al. 2005).

[7] We do not include responses of the first choice task in our analysis and only use the remaining eight choice tasks in the empirical section of this paper. Accordingly, we will estimate sixteen choice task specific models, one for each choice task in each sample separately.

choice sequence. If this is not the case, then the parameter estimates will have high standard errors, because only a limited number of trade-offs are considered. Identifying whether preference dynamics are the result of 'true' preference dynamics, limitations of the design, or heterogeneity in preferences across respondents becomes hard under these circumstances. This may have played a role in Ladenburg and Olsen (2008), where each respondent was presented with the same choice task at the same moment in the choice sequence. We work around this issue by applying a rotating procedure, structurally varying the order in which the choice cards are presented to the respondent. More details about the experimental design and rotating procedure can be found in Appendix A.

Due to our careful experimental set-up, we avoid that design elements have an impact on differences in preferences between the two samples, except differences in the choice task at the start of the stated choice experiment and (unobserved) differences in respondent characteristics. To minimize the impact of variations in respondent characteristics on choice task specific parameter estimates, we sampled respondents for both versions independently. The hired survey company launched the LSB and HSB surveys separately. Demographic and socio-economic characteristics were monitored during the survey to guarantee representativeness at the sample level.

III. Econometric Methods to test for preference dynamics over the choice sequence

The Swait and Louviere (1993) test procedure and its drawbacks

The most common test procedure applied to control for preference dynamics over the choice sequence is the Swait and Louviere (1993) test, henceforth the SL-test. The properties of the SL-test are described in Appendix B. In summary, a likelihood ratio test is conducted to test equivalence in preference structure across two datasets.[8] If the datasets (do not) have a similar preference structure, they can be fully combined (analysed separately).[9]

The hard line between either fully combining the datasets of interest or treating them as completely independent, is a major drawback of the SL-test. Treating the datasets as completely independent significantly decreases the efficiency of parameter estimates. Combining the datasets may, however, result in biased welfare estimates due to neglecting subtle differences in preferences. Therefore there is as a trade-off between bias and efficiency. By treating each dataset of interest as independent, the SL-test does not take into account that some datasets are more likely to be comparable than others. In fact, the theory of CA predicts that preferences gradually evolve over the choice sequence before stabilizing at a specific level. More specifically, preferences at a particular stage of the choice sequence are more likely to be similar to preferences revealed in a recent choice task than to those revealed at the other end of the choice sequence. The SL-test therefore has its limitations when aiming to test for within and between sample preference dynamics. In the next section, we propose an alternative econometric model, the local multinomial logit model (L-MNL), which provides an intermediate solution between fully or not combining different datasets.

The local multinomial logit model

In stated choice experiments respondent $i=1,2,...,I$ is presented with a sequence of choice tasks. In each choice task $t=1,2,...,T$ a finite number of alternatives J_{it} is included. Each

[8] The data underlying each of the sixteen models is treated as a separate 'dataset'. We test for within sample preference dynamics by applying the SL-test to two 'datasets' from the same sample at different moments along the choice sequence. Between sample dynamics are analysed in the SL-test by contrasting two 'datasets' at exactly the same moment in the choice sequence, but taken from a different sample.

[9] Alternative applications have contrasted data from revealed and stated preference studies (Adamowicz et al. 1994; Brownstone et al. 2000; Cameron et al. 2002) or compared welfare estimates across different populations in benefits transfer studies (Colombo et al. 2007; Johnston 2007; Lusk et al. 2003). Dynamics in the scale parameter over choice sequences have also been analysed, which is commonly interpreted as a measure of choice accuracy (e.g. Brown et al. 2008; Swait and Adamowicz 2001).

alternative in the choice set is characterized by a set of attributes, which vary in their levels across alternatives and choice sets. The respondent is requested to select the most preferred alternative $j=1,2,...,J_{it}$ from the set of available alternatives. According to micro-economic theory, the respondent is assumed to select the alternative generating the highest level of utility U_{ijt}. The random utility model (RUM) describes this utility function as a combination of deterministic and stochastic components, respectively V_{ijt} and ε_{ijt}. For simplicity, we use a linear-additive utility specification $U_{ijt} = V_{ijt} + \varepsilon_{ijt} = X_{ijt}\beta + \varepsilon_{ijt}$, where β denotes the vector of marginal utility parameters. X_{ijt} represents a row vector of explanatory variables characterizing the chosen alternative j presented to individual i in choice task t. The error term ε_{ijt} captures aspects of the choice process that are either unobserved or not explicitly modelled by the researcher. By imposing an i.i.d. extreme value distribution on ε_{ijt}, the model belongs to the family of logit models. The i.i.d. assumption seems restrictive given that responses are likely to be correlated across observations for the same respondent. We correct for this potential misspecification using robust standard errors. In our sensitivity analysis in Appendix D we show that our main conclusions are not altered if a more sophisticated specification of the choice probability is used. Accordingly, the loglikelihood of observing the vector of choices y in Equation (2) can be described by the sum of logged choice probabilities of the chosen alternatives in each choice task characterized by Equation (1).

$$P(y_{it} = j|\beta, X_{it}) = \frac{exp(X_{ijt}\beta)}{\sum_{k=1}^{J} exp(X_{ikt}\beta)} \tag{1}$$

$$LL(y|\beta, X) = \sum_{i=1}^{I} \sum_{t=1}^{T} ln(P_{ijt}) \tag{2}$$

In the standard multinomial logit (MNL) model marginal utility β is assumed to be constant across respondents and over the choice sequence. We are, however, interested in sample $s=1,2,...S$ and choice task t specific preference parameters β_{st}. This can be achieved by estimating $S \cdot T$ independent models, which potentially suffer from the same efficiency problems underlying the SL-test procedure. The L-MNL model increases efficiency by estimating β_{st} whilst using information from all available data. The L-MNL model discussed in Fan et al. (1995), and for example applied by Frölich (2006) and Fosgerau (2007), is estimated by estimating $S \cdot T$ alternative weighted MNL models. In our case, sixteen weighted MNL models will be estimated, so eight unique models within both the LSB and HSB sample. We label these as the locally estimated models.

Each locally estimated model results in a vector of parameter estimates $\hat{\beta}_{st}$ for the respective local point (choice task t in sample s). Conditional on the local point, Equation (3) assigns a weight K_{ql} to each observation in the dataset, where q represents the sample and l the choice task number of the observation.[10] Let I_q measure the number of respondents in respectively the LSB and HSB sample. The weight is defined by the distance, i.e. degree of similarity, between each observation and the local point. Observations that are considered more similar to the local point, by being in the same sample (q=s) or by being positioned at the same moment in the choice sequence (l=t), receive a higher weight and therefore have more influence on the weighted log-likelihood function.

$$LL_{st}(y|\beta, X) = \sum_{q=1}^{S} \sum_{i=1}^{I_q} \sum_{t=1}^{T} K_{ql} \cdot ln(P_{qijl}) \tag{3}$$

The weights are determined by a kernel density function $g(\cdot)$, which requires as inputs: (i) a vector (or matrix) Z_{st} characterizing the local point; (ii) the value of Z at a specific observation Z_{ql}; and (iii) a set of bandwidth parameters h, such that $K_{ql}=g(Z_{st}, Z_{ql}, h)$. In our estimations, we control for within- and between-sample preference dynamics by means of a two-dimensional kernel density function, modelled as the product of two independent kernel density functions $K_{ql} = K^1_l \cdot K^2_q$. Within-sample preference dynamics are characterized by an ordered categorical variable describing the position of the choice task in the sequence.

[10] Note that the local point varies across models and thereby the weight of each observation in the likelihood function.

Between-sample dynamics are captured by an unordered categorical (dummy) variable, defining to which sample the local point belongs (LSB or HSB). Racine et al. (2006) show that kernel functions for ordered categorical variables and unordered categorical variables need to have the possibility to be an indicator function; and that it must be possible to smooth out a categorical variable.[11] The shape of the two kernel density functions K^1_l and K^2_q, described in respectively Equations (4) and (5), fulfil these requirements when bandwidth parameters h_1 and h_2 are restricted to the interval [0,1].

$$K_l^1 = \begin{matrix} 1 \; if \; l = t \\ h_1^{|l-t|} if \; l \neq t \end{matrix} \tag{4}$$

$$K_q^2 = \begin{matrix} 1 \; if \; q = s \\ h_2 \; if \; q \neq s \end{matrix} \tag{5}$$

The bandwidth parameters smooth the locally estimated preference parameters. Setting both h_1 and h_2 to one will result in the standard MNL model, since every observation gets the same weight. By setting h_1 and h_2 to zero, the L-MNL model is equivalent to estimating sixteen independent models. Any intermediate specification is expected to result in an increase in efficiency relative to this set of independent models, because it draws information from all observations in the dataset. Equation (4) reveals that decisions at the other end of the choice sequence are treated as more dissimilar and receive a lower weight compared to choice tasks closer to the local point. If the bandwidth parameter is too large, then there is risk of over-smoothing. Too much detail disappears and parameter estimates may become biased. If the bandwidth is too small, then there is a risk of under-smoothing, that is, over-fitting to random fluctuations in the data. We use model evaluation criteria, like the *Akaike Information Criterion* (AIC), to select the optimal bandwidth. Hurvich et al. (1998) note that the AIC can lead to under-smoothing, while the *Bayesian Information Criterion* tends to support a high

[11] The former implies that the kernel density can take the value $h=0$ for observations different than the local point. Other observations than the local point are not treated in the estimation of the L-MNL model. The latter ($h=1$) accounts for the fact that within or between sample preference dynamics may not be present.

degree of smoothing. In this paper, we apply the corrected AIC (AICc) as a model selection criterion, which introduces a penalty for additional parameters in the model compared to the AIC. As a rule of thumb, models are considered significantly different if the difference between model criteria is larger than $3/(I \cdot T)$ (Charlton and Fortheringham 2009). The number of parameters in the model can be approximated by evaluating the trace of the hat-matrix (more details are provided in Appendix C). More specifically, we perform a manual grid search to identify the optimal set of bandwidth parameters.

The set of locally estimated preference parameters is used to derive (scale free) welfare measures of interest. These may include marginal WTP for particular attributes or mean WTP for a specific scenario (alternative) or changes in consumer surplus in general. Statistical tests will be performed to test whether these welfare measures reveal any within or between sample preference dynamics. The same set of tests is performed on the results from the sixteen independent models. We hypothesize that the L-MNL increases efficiency of the parameter estimates and thereby increases the power of the test relative to these independently estimated models. The bandwidth parameters of the L-MNL model are informative on the extent to which decisions at various stages of the choice sequence can be treated as similar. This is comparable to the purpose of the SL-test in testing for within and between sample preference dynamics. If the SL-test finds preference dynamics in the database, the researcher still needs to conduct the same tests to find out whether the dynamics in the preference structure also translate into dynamics in the welfare measures of interest. The L-MNL model and the SL-test are comparable in the sense that both methods perform a preference structure test. The SL-test performs a likelihood ratio test to find out whether a variation in preference parameters results in an improvement in model fit, while the L-MNL has a similar purpose by optimizing the selected information criterion through changes in (local) preference and bandwidth parameters. The L-MNL, however, offers a more flexible approach.

IV. Empirical application

Flood risks in the Netherlands

We use data from a stated choice experiment concerning flood risk exposure in a low probability-high impact context in the Netherlands. Large parts (26%) of the Netherlands, especially in the west, are situated below sea level and are threatened by an increase in coastal flood risks due to climate change (PBL 2010). Although most Dutch citizens know they live below sea level, they are not familiar with and have little to no experience making trade-offs regarding flood safety.[12] The central government and local public water authorities are responsible for providing and monitoring flood safety levels (Bouwer and Vellinga 2007). Private flood risk insurance is not available in the Netherlands, and hence the costs of flood safety are all paid by the public sector (Botzen and van den Bergh 2009). However, the Dutch government attempts to shift flood risk responsibilities from the public to the private sector as part of a broader cross-sectoral policy to make the country ‘climate proof’ (Kabat et al. 2005). Since there is currently a lack of incentives at the individual level to reduce exposure and vulnerability to flood risks, preferences are likely to be underdeveloped. Preference uncertainty may furthermore play a role as a result of the small probabilities associated with coastal flooding in the study area and the fact that most people never experienced a flood.[13]

The stated choice experiment is conducted in the densely populated western provinces of North-Holland and South-Holland, where major cities are located such as Amsterdam, The Hague and Rotterdam. The social and economic impacts of a coastal flood in this area are expected to be high as some parts are located up to almost six meters below sea level. The government aims to maintain a flood probability of once every 10,000 years in the study area. Without additional investments in flood control, flood probabilities are expected to increase to

[12] Flood risk considerations in residential location choice are very limited (Brouwer and Schaafsma 2011).
[13] The last catastrophic flood was in 1953 when more than 1,800 people died in the south-western part of the Netherlands.

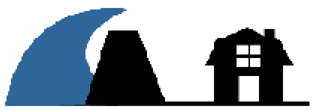
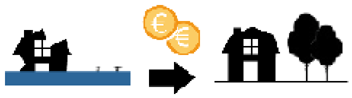
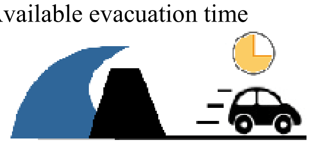
once every 4,000 years by 2040 due to climate change (Maaskant et al. 2009). We are interested in the extent to which people are willing to trade-off an increase in their annual tax payments against a flood risk reduction by preventing the increase of the probability of a coastal flood and its associated socio-economic consequences.

Survey administration

The stated choice experiment was embedded in an online survey conducted in March 2010 targeting a random selection of individual households in the two provinces, measuring their flood risk perception, flood preparedness and degree of risk aversion. Before starting the choice experiment, respondents were first asked for their perception of and subsequently informed about their vulnerability to flood risks. Probabilities were explained with the help of a field-tested risk ladder (Bockarjova et al. 2010; Botzen and van den Bergh 2009; Corso et al. 2001). Elevation levels representing individual household risk exposure were presented by means of a topographic map of the study area. Respondents could access more information about their local elevation levels through an interactive link by typing in their postal code. Pre-tests using computer-assisted personal interviews indicated that the communication methods were considered clear, but that it was difficult for some respondents to imagine and visualize the small probabilities and possible impacts of a flood. The risk ladder helped respondents in comparing flood risks to other more common risks, thereby reducing the cognitive burden of the choice task.

Two alternative (unlabelled) public policy programs and a status quo (SQ) (opt-out) alternative were presented to the respondent. Each policy alternative is described by four attributes: (i) a reduction in flood probability; (ii) compensation of the material damage to each household after a coastal flood has occurred; (iii) available evacuation time; and (iv) an increase in the annual tax to the water authority for all households, including the respondent's household. Table 1 shows the design levels of each attribute and the definition of the SQ option. To assist public understanding of the probability attribute, the relative size of the change in the probability compared to the SQ was also displayed on the choice card.

TABLE 1
Attributes, attribute levels and definition of the status quo option

Attribute	*Possible attribute levels*			
Probability 	1 in 4,000 years	1 in 6,000 years (1.5x smaller)	1 in 8,000 years (2x smaller)	1 in 10,000 years (2.5x smaller)
Compensation 	0%	50%	75%	100%
Available evacuation time 	6 hours	9 hours	12 hours	18 hours
Increase in annual tax 	€40	€80	€120	€160
	Probability	*Compensation*	*Evacuation*	*Tax*
Status Quo	1 in 4,000 years	0%	6 hours	€0

As described in Section II, a potential starting point bias is introduced in the first choice task. More specifically, respondents in the LSB (HSB) were presented with the cost levels €40 and €80 (€120 and €160) for respectively the first and second alternative in the instructional choice card. The policy alternatives depicted in the instructional choice task were identical for all other attribute levels in both samples. The remaining eight choice cards presented to the respondents in both versions come from the same experimental design and are hence directly comparable.

V. Results

The sample consists of 477 respondents, respectively 247 in the HSB and 230 in the LSB sample. Together these respondents made 4,293 choices (477 times 9 choice tasks). Table 2 shows that the independent sampling strategy resulted in two sets of respondents comparable in terms of their main socio-economic characteristics such as income, gender and age (Table 2). That is, statistical tests fail to reject the null-hypothesis of equivalence in the distribution and central tendency of key socio-economic indicators in both samples. Given the comparable samples, we expect that on average respondents in both samples will make their decisions in a similar way. We therefore present a set of attributes-only models to facilitate the illustration of the L-MNL model. If the samples were not comparable, it would be more likely that variations in preferences due to uncontrolled heterogeneity are falsely attributed to within and between sample preference dynamics resulting in biased WTP estimates. Appendix D discusses results for adding gender as an additional control variable to the kernel density function.

TABLE 2

Testing for between sample equivalence in socio-economic sample characteristics

Variable	*Type*	*Description*	*Test*	*d.f.*	*Test-statistic*	*p-value*
Income	Categorical	10 (ordered) income categories	χ^2	9	8.52	0.48
Gender	Dummy	1= male ; 0 = female	χ^2	1	1.14	0.29
Age	Continuous	Respondent age (18-65)	Kolmogorov-Smirnov	-	0.08	0.50

Analysis of preference dynamics based on choice shares

The development in the choice shares across the alternatives over the nine choice tasks are reported in Table 3. Shares for the first choice task highlight that respondents tend to select the cheaper option (alternative 1), and the share of SQ responses in the HSB sample (21%) is higher relative to the LSB sample (13%). The χ^2-test rejects the null hypothesis of an identical distribution of choice shares in the first choice task across the two subsamples at the 10%

level. To test whether the price difference across samples introduced in the first choice task induced a (persistent) starting point bias, we base the rest of the analysis on choice tasks 2-9.

TABLE 3

Share of respondents selecting each alternative in the HSB and LSB subsamples

	Alt 1		*Alt 2*		*SQ*		χ^2-*test* †	*p-value*	χ^2-*test* ‡	*p-value*
Task	*HSB*	*LSB*	*HSB*	*LSB*	*HSB*	*LSB*				
1	54%	61%	25%	26%	21%	13%	5.52	0.06*	5.37	0.02**
2	41%	37%	45%	40%	15%	23%	5.63	0.06*	5.63	0.02**
3	44%	39%	41%	35%	15%	26%	9.07	0.01**	9.07	0.00***
4	38%	36%	43%	35%	19%	29%	6.02	0.05**	5.62	0.02**
5	42%	36%	38%	36%	20%	28%	4.35	0.11	4.20	0.04**
6	44%	35%	38%	38%	19%	27%	6.08	0.05**	4.72	0.03**
7	40%	37%	42%	38%	18%	25%	3.05	0.22	3.05	0.08*
8	42%	33%	43%	42%	15%	25%	8.94	0.01**	7.83	0.01**
9	40%	38%	36%	37%	24%	25%	0.26	0.88	0.11	0.74
Average Tasks 2-9	41%	36%	41%	38%	18%	26%	35.54	0.00***	34.89	0.00***

† Test for between sample differences in choice shares, 2 degrees of freedom

‡ Test for between sample differences in propensity to select the SQ option, 1 degree of freedom

*[**](***) Denotes significance at the 10 [5] (1) % level

Averaged over choice tasks 2-9, alternatives 1 and 2 are both selected 41% of the time in the HSB sample. In the LSB sample this is respectively 36% and 38%. The within sample equivalence in choice shares for the unlabelled alternatives 1 and 2 is a direct consequence of alternating their order of appearance across versions of the design. We limit our discussion in this section to variations in the propensity to select the SQ-option within and between the two samples.[14]

Compared to the first choice task, the share of SQ responses doubles on average in the LSB sample to 26%, whereas this share decreases on average, from 21% to 18% in the HSB sample. Moreover, the share of SQ responses in the LSB sample is consistently higher than the same share in the HSB sample. Statistical support for the presence of a SPB between

[14] Shares for alternatives 1 and 2 are barely informative, because of the alternate ordering and presenting each choice card in the design multiple times at different moments in the choice sequence.

samples is provided by the χ^2-test, which rejects the null hypothesis of an identical propensity to select the SQ option in both samples over choice cards 2-9 at the 1% level. Since the experimental design and respondent key socio-economic characteristics are equivalent in both samples, we attribute this support for a SPB to the initial choice task.

A closer look at the dynamics of the choice shares over the choice sequence reveals that after the first choice task a jump occurs.[15] After this jump, choice shares vary around their mean levels, suggesting stability in preferences. Choice task nine stands out, revealing a relatively higher share of SQ responses in the HSB sample, which comes close to the level in the LSB sample. Indeed, the χ^2-tests in the final columns of Table 3 reveal that only in the final choice task we cannot reject the null-hypothesis of an equivalent propensity to select the SQ option at the 10% level in both samples. A priori there is no reason why choice task nine should stand out. The same choice cards have been answered by similar respondents during the choice sequence. Hence, the choice shares suggest that the observed SQ share in choice task nine is somewhat ad hoc and structural preference dynamics, i.e. convergence, between samples are absent. The choice shares also provide limited support for within sample preference dynamics. The only significant differences in the propensity to select the SQ option are identified in the HSB sample, where the share of the SQ option in choice tasks two, three and eight is significantly lower than the same share in choice task nine.[16] The choice shares are in line with Ariely et al. (2003)'s theory of CA supporting a persistent SPB in the propensity to select the SQ option. Anchoring respondents on a low price in the first choice task seems to increase this propensity.

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[15] This jump is primarily caused by the set-up of the survey. Instead of using the same choice card in subsequent choice tasks, the full design is used and presented at each moment in the choice sequence to the respondents.

[16] Results of the χ^2 –test for within sample differences in choice shares are available upon request.

Analysis of preference dynamics using choice task and sample specific models

Choice shares do not reflect the extent to which alternatives and their attribute levels affect decisions and hence whether WTP estimates are subject to a SPB and dynamics over the choice sequence. To this end we present a set of sixteen choice task and sample specific marginal WTP estimates in Table 4.[17] The high standard errors indicate that estimation at the choice task level is inefficient. Moreover, the 95% confidence intervals reveal that marginal WTP estimates for the probability and evacuation attributes are not significant in most models at the 5% level. Mean marginal WTP levels also reveal that preference dynamics follow a somewhat ad hoc pattern over the choice sequence. The latter is in line with our earlier remark that under-smoothing may result in over-fitting due to random fluctuations in the data. In Table 5, we test whether anchoring on a lower price in the first choice task results in significantly lower marginal WTP estimates in the LSB sample. Although not always significant at the 5% level, Table 5 indicates that at the start of the survey respondents in the LSB sample have a higher tendency to select the SQ option.[18] This effect disappears after choice task three, but then significantly higher WTP estimates in the HSB are found for the probability attribute, and the compensation attribute in respectively choice tasks five and six. In the final two choice tasks, marginal WTP for the evacuation attribute is lower in the LSB sample, but only at the 10% level. In contrast to the choice shares, these estimates only provide limited support for a (persistent) SPB in welfare estimates. The relatively small sample size and complexity of the test contribute to these results. Most remarkable is that differences in marginal WTP estimates between the two samples are not consistently found for the same attributes over the choice sequence. In choice tasks four and seven none of the

[17] Standard deviations of the scale-free marginal WTP estimates are derived by means of the Krinsky and Robb method (Krinsky and Robb 1986; 1990).The test statistic used in all methods to compare simulated marginal WTP distributions is based on the one-sided complete combinatorial approach described in Poe et al. (2005).

[18] An increase in the alternative specific constant and reduction in the cost coefficient both decrease the reported ratio and imply a higher probability to select one of the policy alternatives.

reported WTP estimates of the two samples are significantly different from each other at the 10% level.

TABLE 4

Marginal WTP estimates based on card specific models

Sample	*HSB*															
	ASC				*Prob*				*Comp*				*Evac*			
Task	*Mean*	*St. error*	*2.5%*	*97.5%*	*Mean*	*St. error*	*2.5%*	*97.5%*	*Mean*	*St. error*	*2.5%*	*97.5%*	*Mean*	*St. error*	*2.5%*	*97.5%*
2	118.20	32.80	65.00	194.20	8.50	5.50	-1.20	19.80	1.07	0.36	0.56	1.91	1.60	2.00	-2.30	5.50
3	130.40	35.20	77.40	212.30	4.40	5.00	-5.30	14.50	0.96	0.34	0.46	1.75	2.10	2.10	-1.70	6.20
4	83.60	22.90	39.50	129.40	3.60	3.90	-4.00	11.70	1.00	0.26	0.57	1.60	1.70	1.60	-1.30	5.00
5	55.50	22.90	7.90	99.00	8.80	4.60	0.80	18.60	1.18	0.32	0.72	1.89	1.70	1.50	-1.30	4.80
6	43.50	17.20	8.00	76.10	11.60	3.30	5.80	19.00	0.92	0.18	0.60	1.31	2.90	1.10	0.80	5.20
7	75.90	20.10	38.50	117.50	9.60	3.80	2.60	17.50	0.81	0.20	0.46	1.26	1.60	1.40	-1.10	4.30
8	90.80	14.70	63.30	121.40	2.00	2.50	-2.90	6.80	0.80	0.15	0.54	1.12	2.30	0.90	0.60	4.20
9	40.60	19.20	-1.10	76.00	9.30	3.90	2.40	17.80	0.88	0.21	0.52	1.37	3.90	1.50	1.30	7.20

Sample	*LSB*															
	ASC				*Prob*				*Comp*				*Evac*			
Task	*Mean*	*St. error*	*2.5%*	*97.5%*	*Mean*	*St. error*	*2.5%*	*97.5%*	*Mean*	*St. error*	*2.5%*	*97.5%*	*Mean*	*St. error*	*2.5%*	*97.5%*
2	65.00	25.40	16.90	111.20	8.80	4.30	1.20	17.80	0.70	0.30	0.28	1.27	2.60	2.50	-0.70	6.50
3	76.60	17.90	40.20	111.00	3.80	3.30	-2.80	10.30	0.43	0.18	0.09	0.81	2.80	1.40	0.10	5.60
4	63.70	33.90	-3.20	120.90	6.50	6.10	-3.20	18.80	0.60	0.36	0.08	1.31	1.30	2.40	-3.20	6.10
5	43.60	18.20	5.90	78.90	10.10	3.40	4.10	17.50	0.62	0.18	0.28	1.01	1.40	1.30	-1.10	4.00
6	35.80	17.90	-1.60	70.40	3.30	3.20	-3.00	9.80	1.05	0.20	0.70	1.49	2.10	1.20	-0.30	4.50
7	54.70	13.10	28.20	79.90	4.80	2.40	0.20	9.70	0.61	0.13	0.37	0.87	2.70	1.00	1.00	4.70
8	67.90	16.50	35.30	100.50	4.70	2.90	-0.90	10.60	0.68	0.16	0.37	1.02	0.20	1.10	-2.10	2.40
9	49.20	15.60	18.30	80.10	6.10	2.90	0.40	11.90	0.89	0.17	0.58	1.24	1.20	1.20	-1.00	3.70

Probability – (€ per household per year for an extra 1,000 years in the denominator of the flood probability, from e.g. 1/4,000 → 1/5,000)

Compensation – (€ per household per year for an extra percentage point of compensation)

Evacuation – (€ per household per year for an extra hour of evacuation time)

TABLE 5

Testing if WTP in HSB sample is higher than in the LSB using card specific models (p-values reported)

Task	*ASC*	*Prob*	*Comp*	*Evac*
2	0.08*	0.53	0.17	0.65
3	0.05*	0.46	0.05*	0.62
4	0.30	0.66	0.13	0.44
5	0.33	0.61	0.04**	0.45
6	0.38	0.03**	0.69	0.30
7	0.19	0.13	0.19	0.75
8	0.14	0.76	0.29	0.07*
9	0.63	0.25	0.53	0.07*

*[**](***) indicates significance at the 10[5](1)% level

Regarding within sample preference dynamics in the HSB sample, choice tasks two and three in the HSB sample only differ from most other choice tasks in that sample on the ratio of the Alternative Specific Constant (ASC) over the cost coefficient (see Table 6). The ratio can best be interpreted as the tendency to agree with, or willingness-to-pay for, taking action against increases in flood risk exposure due to climate change. This is irrespective of the actual level of reductions in flood risks provided by the authorities. Encountering lower prices after the first choice task seems to decrease the propensity to select the SQ option as reflected by the level of the ASC-cost ration in choice tasks two and three.[19] This anchoring effect seems to wear out rapidly. The same ratio is significantly different between choice task three and choice tasks five and six in the LSB sample at the 10% level. However, a similar impact and decay of the initial choice task cannot be identified in this sample. Table 6 highlights that more significant within sample preference dynamics are found, but these are likely to be subject to under-smoothing effects. In particular, choice task eight in the HSB sample stands out revealing a low marginal

[19] Such a status quo effect may be induced by what Loomes et al. (2009) label as taste uncertainty. Uncertain respondents may exhibit trade-off resistance. Presenting them with lower prices may alleviate such trade-off resistance. Balcombe and Fraser (2011) also look into the impacts of preference uncertainty on the propensity to select the don't know option.

WTP estimate for the probability attribute relative to choice tasks which are closer by in the choice sequence. Within the LSB sample a consistent pattern also seems to be lacking. Choice task five reveals a higher marginal WTP for the probability attribute at the 10% level compared to choice tasks three, six and seven. Similarly, marginal WTP for the evacuation attribute is a bit lower in choice task eight compared to choice tasks three and seven. Finally, marginal WTP for an additional percentage of compensation in choice task six seems to stand out. As such, the choice task and sample specific model results provide limited support for a (persistent) SPB and the presence of consistent within sample preference dynamics. Only within the HSB sample we are able to identify a change in the propensity to select the SQ option and thereby a decay of the impact of the initial choice task. However, by treating each of the sixteen models as independent, gradual changes in preferences are not captured and under-smoothing may have resulted in over-fitting due to the random fluctuations in WTP parameters as revealed by Table 6.

TABLE 6

Summary of significant within sample preference dynamics using card specific models

		HSB		*LSB*			
Task 1	*Task 2*	*ASC*	*PROB*	*ASC*	*PROB*	*COMP*	*EVAC*
2	3						
2	4						
2	5	+**					
2	6	+**					
2	7						
2	8						
2	9	+***					
3	4						
3	5	+**		+*	-*		
3	6	+***		+*		-***	
3	7	+*					
3	8						+*
3	9	+***				-**	
4	5						
4	6	+*	-*			-*	
4	7						
4	8						
4	9	+*					
5	6				+*	-*	
5	7				+*		
5	8	-*	+*				
5	9						
6	7					+**	
6	8	-*	+***	-*		+*	
6	9						
7	8		+**				+**
7	9	+*				-*	
8	9	+**	-**				

Note: For the HSB sample COMP and EVAC results are not reported because no significant within sample preference dynamics were found.

+ indicates the welfare measure is significantly higher in Task 1 compared to Task 2

- indicates the welfare measure is significantly lower in Task 1 compared to Task 2

*[**](***) indicates significance at the 10[5](1)% level

Analysis of preference dynamics using the L-MNL model

Setting both bandwidth parameters in the L-MNL model to zero results in the same set of parameter estimates as discussed in the previous section. The approximated number of parameters for this L-MNL specification, i.e. Model 1 in Table 7, is close to the number of parameters underlying Table 4. The difference arises due to the use of robust standard errors.[20] The high number of parameters in Model 1 results in an improved model fit, but the *AICc* for this specification is significantly lower than the *AICc* for a standard MNL model neglecting between and within preference dynamics (Model 2). Hence, treating the different samples and choice tasks as independent blocks of observations leads to a reduction in the information criterion and parameter efficiency. In Model 3 we have optimized the *AICc* by controlling for both within ($h_1=0.43$) and between ($h_2=0.20$) sample preferences dynamics, while in Models 4 and 5 the *AICc* is optimized only with respect to either within or between sample preference variations setting the other bandwidth parameter to one. The fit for Model 3 is significantly better compared to Model 4 supporting the notion that within sample preference dynamics are present over the choice sequence. Simultaneously, the bandwidth parameter controlling for between sample preference variation highlights that both samples should not be treated as being the same. Finally, since there is only a marginal increase of 0.01 in h_2 between Model 3 and Model 4, the kernel densities operate in a relatively independent fashion.

[20] Without specifying robust standard errors, there would be five parameters in each MNL model resulting in a total of 80 parameters (16 independent models having 5 parameters each).

TABLE 7

Overview of the AICc criterion for alternative L-MNL specifications

Model	*Description*	*Bandwidth h_1 (within)*	*Bandwidth h_2(between)*	*LL*	*Approx. # of pars*	*AICc*
(1)	Within + between sample variation	0.00	0.00	-3660.78	79.74	1.9616
(2)	MNL	1.00	1.00	-3720.78	10.63	1.9559
(3)	Optimal bandwidth parameter	0.43	0.20	-3677.24	24.01	1.9402
(4)	Optimal between sample variation	1.00	0.19	-3700.79	15.46	1.9480
(5)	Optimal within sample variation	0.46	1.00	-3703.98	16.04	1.9499

Table 8 reports the choice task and sample specific marginal WTP estimates obtained for L-MNL Model 3. The major advantage of the L-MNL model is its increase in efficiency illustrated by the reduction in standard errors compared to Table 4. Improvements up to 72% are found and standard errors of the marginal WTP estimates reduce by 54% on average. The 95% confidence intervals for the probability and evacuation attributes are now strictly positive. The smoothing procedure also has an impact on mean marginal WTP estimates. Table 9 reveals that the HSB sample has a higher marginal WTP for the compensation attribute until choice task six relative to the LSB sample. This effect is only significant at the 5% level in choice task three. Similarly, we find a lower tendency to select the SQ option in choice tasks two and three in the HSB sample compared to the LSB sample. The effect is significant at the 10% level and the L-MNL model thereby provides limited support for a starting point bias due to anchoring on the price attribute of the first choice task. After five choice tasks welfare measures seem to converge between samples, only marginal WTP for the evacuation attribute is significantly higher in the HSB sample in the final choice task, but only at the 10% level. Clearly, smoothing parameter estimates results in a more consistent pattern of between sample preference dynamics compared to Table 5 as most random fluctuations are filtered out.

TABLE 8

Marginal WTP estimates based on L-MNL model 3

Sample	*HSB*															
	ASC				*Prob*				*Comp*				*Evac*			
Task	*Mean*	*St. error*	*2.5%*	*97.5%*	*Mean*	*St. error*	*2.5%*	*97.5%*	*Mean*	*St. error*	*2.5%*	*97.5%*	*Mean*	*St. error*	*2.5%*	*97.5%*
2	99.28	13.93	73.33	128.11	6.85	2.38	2.32	11.68	0.93	0.16	0.65	1.28	1.89	0.98	-0.03	3.87
3	94.13	11.25	73.27	117.69	5.73	1.91	2.14	9.65	0.90	0.14	0.65	1.19	1.98	0.87	0.31	3.71
4	77.65	10.08	58.00	97.89	5.98	1.72	2.80	9.54	0.93	0.12	0.71	1.18	1.91	0.73	0.54	3.39
5	63.58	9.20	45.81	82.05	7.56	1.61	4.53	10.81	0.95	0.11	0.74	1.19	1.99	0.62	0.75	3.23
6	58.59	8.48	41.87	74.91	8.23	1.38	5.65	11.05	0.90	0.10	0.71	1.11	2.28	0.54	1.22	3.38
7	66.17	8.72	49.24	83.32	7.25	1.37	4.62	9.99	0.84	0.10	0.66	1.04	2.19	0.54	1.15	3.27
8	70.95	8.18	55.04	87.31	5.36	1.27	2.94	7.89	0.82	0.09	0.65	1.01	2.29	0.53	1.28	3.37
9	58.52	9.65	39.18	77.55	6.74	1.68	3.61	10.17	0.85	0.11	0.64	1.07	2.77	0.70	1.45	4.19

Sample	*LSB*															
	ASC				*Prob*				*Comp*				*Evac*			
Task	*Mean*	*St. error*	*2.5%*	*97.5%*	*Mean*	*St. error*	*2.5%*	*97.5%*	*Mean*	*St. error*	*2.5%*	*97.5%*	*Mean*	*St. error*	*2.5%*	*97.5%*
2	71.82	12.41	46.90	95.66	6.92	2.08	2.98	11.23	0.67	0.13	0.44	0.94	2.30	0.90	0.61	4.13
3	72.14	10.42	51.40	92.51	5.60	1.71	2.24	9.09	0.62	0.11	0.42	0.84	2.22	0.74	0.82	3.73
4	63.61	10.29	43.60	83.54	6.21	1.66	3.00	9.57	0.68	0.11	0.47	0.90	1.81	0.68	0.51	3.18
5	53.32	8.73	36.24	70.43	7.07	1.48	4.24	10.09	0.74	0.09	0.56	0.93	1.77	0.56	0.68	2.90
6	50.31	8.06	33.74	65.79	5.83	1.30	3.32	8.42	0.81	0.08	0.65	0.99	1.99	0.50	1.04	2.98
7	55.89	7.48	40.98	70.55	5.40	1.23	3.04	7.87	0.73	0.07	0.59	0.88	2.02	0.49	1.09	3.00
8	60.84	8.06	44.85	76.67	5.10	1.36	2.55	7.85	0.74	0.08	0.58	0.91	1.44	0.54	0.41	2.50
9	55.23	8.89	37.76	72.79	5.72	1.53	2.73	8.73	0.80	0.09	0.63	0.99	1.52	0.65	0.29	2.82

Probability – (€ per household per year for an extra 1,000 years in the denominator of the flood probability, from e.g. 1/4,000 → 1/5,000)

Compensation – (€ per household per year for an extra percentage point of compensation)

Evacuation – (€ per household per year for an extra hour of evacuation time)

TABLE 9

Testing if WTP in the HSB sample is higher than in the LSB sample using L-MNL model 3(p-values reported)

	P(WTP_LSB>WTP_HSB)			
Task	*ASC*	*Prob*	*Comp*	*Evac*
2	0.07*	0.51	0.10*	0.62
3	0.07*	0.48	0.05**	0.58
4	0.16	0.54	0.06*	0.46
5	0.21	0.41	0.07*	0.40
6	0.24	0.10	0.25	0.34
7	0.19	0.16	0.20	0.40
8	0.19	0.44	0.25	0.13
9	0.40	0.33	0.39	0.09*

*[**](***) indicates significance at the 10[5](1)% level

The same observation can be made for within sample preference dynamics in Table 10. Within the HSB sample choice tasks two and three reveal a higher ratio of the ASC over the cost coefficient relative to choice tasks five to nine, which implies a lower tendency to select the SQ option at the start of the choice experiment. This effect is significant in all cases at the 5% level. In choice task four, this effect is still present compared to choice tasks six and nine and only at the 10% significance level. The L-MNL model thus supports a gradual decay of the impact of the initial choice task on the tendency to select the SQ option in the HSB sample. Regarding the other policy attributes only a significant difference at the 10% level is found for the probability attribute when comparing choice tasks six and eight in the HSB sample. Within the LSB sample the ratio of the ASC over the cost coefficient in choice task two is significantly higher compared to choice task six, but only at the 10% level. The same ratio is also higher in choice task three relative to choice tasks five and six. This may indicate that respondents in general have a lower tendency to select the SQ option at the start of the survey. This effect can be amplified by anchoring of respondents on high prices in the initial choice task. We believe this is one of the key findings of this paper. However, it is hard to relate this result to other papers, since the cited papers on preference dynamics have paid limited attention to developments in either choice shares or WTP parameters over the choice sequence. Samuelson and Zeckhauser (1988) have introduced the concept of the Status Quo bias, for which many researchers control in their analysis by specifying an error-components logit model (e.g. Meyerhoff and Liebe, 2009). However, we are not aware of other papers looking into the dynamics of this Status Quo bias over the choice sequence. In summary, primarily the HSB sample reveals an impact of the initial choice task on welfare estimates, but this effect gradually wears out after the third choice task. Combined with the lack of overwhelming support for a SPB and the comparable welfare estimates across samples after choice task five, the L-MNL model provides support for Plott's DPH. The extent to which preferences are stable after the fifth choice task remains, however, questionable. First, between sample differences in marginal WTP for the evacuation attribute are found in choice task nine. Second, within the HSB sample dynamics in marginal WTP for the probability attribute are also observed between choice tasks six and eight. Despite the new estimation method, standard errors remain relatively high due to our limited sample size. Not only does this affect the efficiency of our estimates, it also affects the number of times a specific card is answered at each moment in the sequence and thereby possibly the impact of individual respondent characteristics on choice task specific parameter estimates. Keeping this drawback in mind, we interpret our results as providing indicative support for the DPH.[21]

[21] A set of sensitivity tests using respectively gender as an additional variable in the kernel density function, and a random parameter on the cost coefficient is presented in Appendix D. The results do not affect our main conclusions.

TABLE 10

Summary of significant within sample preference dynamics based on L-MNL Model 3

		HSB		*LSB*	
Task1	*Task2*	*ASC*	*PROB*	*ASC*	*COMP*
2	3				
2	4				
2	5	+**			
2	6	+***		+*	
2	7	+**			
2	8	+**			
2	9	+***			
3	4				
3	5	+**		+*	
3	6	+***		+**	-*
3	7	+**			
3	8	+**			
3	9	+***			-*
4	5				
4	6	+*			
4	7				
4	8				
4	9	+*			
5	6				
5	7				
5	8				
5	9				
6	7				
6	8		+*		
6	9				
7	8				
7	9				
8	9				

Note: Results for the other attributes are not reported, because no significant within sample preference dynamics were found.

*[**](***) indicates significance at the 10[5](1)% level

Analysis of preference dynamics based on the Swait-Louviere test procedure

The SL-test for between sample preference dynamics does not find a starting point bias between both samples. The preference structure in the HSB and LSB samples is found to be equivalent in all choice tasks except choice tasks three and seven in Table 11. Indeed, choice task three revealed a significant difference in the ASC over cost ratio and marginal WTP for the compensation attribute between the two samples in the L-MNL model. For choice task seven the L-MNL model did not detect a significant difference in between sample welfare estimates.[22] Also limited within sample preference dynamics are identified by the SL-test. The LSB sample does not reveal any differences in preference structure over the choice sequence, while in the HSB sample only significant differences are found between respectively choice tasks two, three and choice tasks six and nine. This pattern is consistent with our L-MNL estimates revealing that respondents in the HSB sample have a lower tendency to select the SQ option in choice tasks two and three. These results highlight the limitations of the SL test procedure. The independently estimated models used as its inputs are inefficient and the SL test clearly supports smoothing the parameter estimates by fully combining most samples, which prevents us from testing for the presence of preference dynamics in the welfare measures of interest. Even if the preference structure is equivalent, then still differences in welfare measures can be present across samples (see for example choice task two in Tables 5 and 9). By over-smoothing the SL-test is not as flexible as our L-MNL model, and can therefore not provide as much insight into patterns of preference dynamics over the choice sequence.

[22] Analysing the parameters based on the SL-test in preference space reveals that a significant difference can be identified between the estimated parameters for the evacuation and cost attribute in this choice task. The properties of the SL test, however, do not allow us to attribute these differences to variations in preferences or in scale.

TABLE 11

Results for the between sample SL-test

Task	*LL HSB*	*LL LSB*	*LL SUM*	*LL Pooled scale*	*LL pooled*	*LR-test1*	*p-value*	*LR-test2*	*p-value*	*scale ln(HSB / LSB)*
2	-234.33	-232.03	-466.36	-468.94	-470.09	5.17	0.27	2.29	0.13	0.33
3	-238.44	-231.20	-469.64	-475.27	-476.07	11.27	0.02**	-	-	-
4	-238.93	-242.19	-481.11	-482.71	-486.01	3.20	0.53	6.59	0.01**	0.66
5	-236.19	-225.98	-462.17	-466.00	-466.28	7.66	0.10	0.55	0.46	0.16
6	-222.35	-219.96	-442.31	-445.93	-446.89	7.23	0.12	1.93	0.17	0.26
7	-232.57	-209.10	-441.67	-446.91	-447.26	10.48	0.03**	-	-	-
8	-217.26	-221.97	-439.23	-442.88	-444.74	7.30	0.12	3.71	0.05*	0.36
9	-241.60	-216.69	-458.29	-459.98	-460.21	3.38	0.50	0.47	0.49	-0.14

LR-test1 – Test for differences in the preference parameters, 4 degrees of freedom
LR-test2 – Test for differences in the scale parameter, 1 degree of freedom
*(**)[***] indicates significance at the 10(5)[1]% level

The SL-test has the benefit of testing for differences in the scale of the utility function. Significant scale differences between the two samples are found in choice tasks four and eight, where the HSB sample is found to have a higher scale parameter. Our L-MNL model confirms that choice task eight does not display a difference in welfare estimates between the two samples, but in choice task four marginal WTP for the compensation attribute is significantly lower in the LSB sample. Here, the inefficiency of the individual models is thus transferred into the scale parameter. Not surprisingly, significant within sample differences in the scale parameter are identified more often, supporting learning effects over the choice sequence. However, given the limitations of the SL-test with the current sample size, it is likely that the SL-test over-smoothes the within (and between) sample preference dynamics, an effect which may be consequently picked up by the scale parameter. Nevertheless, indications of learning effects are observed.

VI. Conclusions

The existence of a set of well-defined preferences in many environmental economic valuation studies has been questioned due to unfamiliarity and inexperience of respondents with the policy attributes. Plott's (1996) discovered preference hypothesis and Ariely et al.'s (2003) coherent arbitrariness are well known studies in this area, providing contradicting hypotheses on the extent to which respondents cope with this preference uncertainty and how preferences evolve over a sequence of choices. In this paper, we tested for the presence of between and within sample preference dynamics in the face of an arbitrarily induced starting point bias in stated choice experiments. To this end, a uniquely designed choice experiment on flood risk valuation was applied in combination with a new econometric model, which is better suited to test for gradual changes in preferences over the choice sequence. The latter model is contrasted against the Swait and Louviere (1993) test procedure, the most common approach to test for preference dynamics. We argue that the latter test is not designed and suited to test for dynamics in welfare estimates in particular when sample sizes are considered small, which is usually the case for stated choice experiments.

This paper finds limited support for the existence of a (persistent) starting point bias in stated choice experiments. These results are in line with findings by Ladenburg and Olsen (2008) and support the discovered preference hypothesis. The sample provided with a higher bid vector at the start of the choice sequence has a lower tendency to select the status quo option in subsequent choice tasks and thereby reveals lower cost sensitivity. The impact of the initial choice task seems to gradually disappear after the third choice task, resulting in a set of stable marginal WTP estimates in both samples. More specifically, after the fifth choice task welfare estimates are not statistically different across the two samples in our novel L-MNL model at the 5% significance level.

Four implications follow from this paper. First, researchers should be aware of potential dynamics in welfare estimates over the choice sequence and not only focus on inherent differences in preferences across respondents (e.g. Hess and Rose 2009). Absence of stable welfare estimates in stated choice experiments would, however, complicate welfare analysis and policy recommendations as it becomes unclear which choice tasks should be used for welfare analysis and how many choices should be included in the experiment. Ideally, these stated preferences are compared to choices in a revealed preference setting to test if preference dynamics are an experimental artefact or explain the real choice process of an individual. Second, the Swait and Louviere (1993) test procedure has the tendency to over-smooth the data and thereby neglect underlying dynamics in preferences when the underlying models are inefficient. The local MNL logit model is more suited for the purpose of testing for preference dynamics, because it offers improvements in flexibility and efficiency when estimating choice task specific preference parameters. Large reductions in standard errors are observed without the need to bundle observations from various choice cards. As such, the model is able to control for gradual changes in preferences and prevents against over-identification due to random variations in the data by smoothing parameter estimates. It should be noted that applications of the local MNL model are not restricted to variations in preferences over time, but also across, for example, respondents (Koster and Koster, 2011). Third, additional effort needs to be placed in the development of experimental set-ups in which sample sizes and the experimental design enabling researchers to estimate choice task specific choice models. Sample sizes used in this paper are comparable to those used in other studies by Braga and Starmer (2005) and Ladenburg and Olsen

(2008) who also use around 250-300 respondents per sample. Closely related, - and despite our careful study set-up -, individual respondents could have caused the observed dynamics in preferences, since at each moment in the sequence each choice card was answered by ten respondents on average. We reported the results from a set of attributes-only MNL models where the kernel density function was allowed to vary only across samples and choice tasks. We conducted two sensitivity tests to additionally control for heterogeneity in preferences across respondents. The first test follows Ladenburg and Olsen (2008) and identifies whether the observed starting point bias is gender specific. We find that the starting point bias is more apparent for male respondents, but still wears out after a couple of choice tasks. For the second test, we estimated mixed logit models at the optimal bandwidth parameters of our local MNL model. We imposed a discrete distribution on the cost parameter allowing for unobserved heterogeneity in the cost parameter across respondents. The results are reported in Appendix D and confirm our main conclusion. Finally, the sensitivity of marginal WTP estimates to arbitrary initial value clues asks for careful testing of the choice experiment and careful specification of the initial choice task. Looking beyond the scope of the current paper, an alternative approach could be to present respondents with an overview of all possible attribute levels before introducing a specific instructional choice task. In that case, starting point biases (or anchoring effects) may be circumvented by not presenting a single set of arbitrary value cues to the respondent (e.g. Bateman et al. 2004). Since respondents are presented with all attribute levels, their tendency to select, for example, the status quo is more likely to be driven by the choice task at hand. However, the appropriateness of the levels included in the choice experiment needs to be defined in pre-testing stages while taking into account the preference uncertainty of respondents also in those stages.

Appendix A. Experimental Design

The experimental design consists of three blocks of 8 choice cards each. The total set of 24 choice cards was generated by Ngene (NGENE 2010) using a d-efficient design based on a random parameters error components logit model using 100 Halton draws (Rose and Bliemer 2009; Train 2009). The three non-cost attributes were assigned a normal distribution and the error component was used to control for a possible Status Quo effect (Scarpa et al. 2005). Non-zero priors applied in the design generation stage were based on pre-test results. Restrictions were imposed on the design to ensure that (i) the instructional choice tasks in the LSB and HSB sample were not repeated in the subsequent choice sequence; (ii) no dominant alternatives were included in the choice sets; and (iii) the status quo alternative was not repeated as a policy alternative. Both the LSB and HSB sample were presented with the same set of choice cards.

The three blocks of 8 choice cards were used to form 24 versions of the design. In order to optimize the estimation of a choice model at each moment in the choice sequence, we rotated the starting card in each version. That is, Version 1 presented respondents with choice cards 1-8 in ascending order. Version 2 started with choice cards 2-8 and ended with choice card 1. This rotation procedure yielded 24 versions in total. Finally, we alternated the order of appearance of the first and second policy alternatives to prevent effects from reading from left to right. Accordingly, the number of versions was doubled to 48 and respondents were randomly assigned to one version.

Table A.1 shows the number of times each block of the design was applied in each sample and the minimum number of times each block was fully answered, by different respondents, at each moment in the choice task. As such, the rotation procedure resulted that, on average, each choice card in the design was answered ten times at each moment in the design by respondents from a particular sample. A more detailed over view of response frequencies is provided in Tables A.2 and A.3. By evaluating the full design in each choice task, the model can be estimated more accurately at each moment in the choice sequence and results are not influenced by design elements. Our study differs in this respect from Ladenburg and Olsen (2008) who did not apply a similar rotation procedure and let all respondents answer the same choice task at the same moment during the choice sequence.

TABLE A.1

Number of times each block of the design is applied in the HSB and LSB samples

	# of times applied in each sample		*Minimum # of times fully applied in each choice task*	
	HSB	*LSB*	*HSB*	*LSB*
Block 1	86	77	8	5
Block 2	78	70	5	5
Block 3	83	83	7	8

TABLE A.2

Number of times each choice card in the design is applied during the choice sequence in HSB sample

HSB		*Task 2*	*Task 3*	*Task 4*	*Task 5*	*Task 6*	*Task 7*	*Task 8*	*Task 9*
Block 1	Card 1	8	12	12	12	9	11	11	11
	Card 2	11	8	12	12	12	9	11	11
	Card 3	11	11	8	12	12	12	9	11
	Card 4	11	11	11	8	12	12	12	9
	Card 5	9	11	11	11	8	12	12	12
	Card 6	12	9	11	11	11	8	12	12
	Card 7	12	12	9	11	11	11	8	12
	Card 8	12	12	12	9	11	11	11	8
Block 2	Card 9	11	11	9	10	5	12	12	8
	Card 10	8	11	11	9	10	5	12	12
	Card 11	12	8	11	11	9	10	5	12
	Card 12	12	12	8	11	11	9	10	5
	Card 13	5	12	12	8	11	11	9	10
	Card 14	10	5	12	12	8	11	11	9
	Card 15	9	10	5	12	12	8	11	11
	Card 16	11	9	10	5	12	12	8	11
Block 3	Card 17	13	9	9	11	12	11	11	7
	Card 18	7	13	9	9	11	12	11	11
	Card 19	11	7	13	9	9	11	12	11
	Card 20	11	11	7	13	9	9	11	12
	Card 21	12	11	11	7	13	9	9	11
	Card 22	11	12	11	11	7	13	9	9
	Card 23	9	11	12	11	11	7	13	9
	Card 24	9	9	11	12	11	11	7	13

TABLE A.3

Number of times each choice card in the design is applied during the choice sequence in LSB sample

LSB		*Task 2*	*Task 3*	*Task 4*	*Task 5*	*Task 6*	*Task 7*	*Task 8*	*Task 9*
Block 1	Card 1	11	10	10	11	9	11	10	5
	Card 2	5	11	10	10	11	9	11	10
	Card 3	10	5	11	10	10	11	9	11
	Card 4	11	10	5	11	10	10	11	9
	Card 5	9	11	10	5	11	10	10	11
	Card 6	11	9	11	10	5	11	10	10
	Card 7	10	11	9	11	10	5	11	10
	Card 8	10	10	11	9	11	10	5	11
Block 2	Card 9	12	11	9	7	5	9	7	10
	Card 10	10	12	11	9	7	5	9	7
	Card 11	7	10	12	11	9	7	5	9
	Card 12	9	7	10	12	11	9	7	5
	Card 13	5	9	7	10	12	11	9	7
	Card 14	7	5	9	7	10	12	11	9
	Card 15	9	7	5	9	7	10	12	11
	Card 16	11	9	7	5	9	7	10	12
Block 3	Card 17	8	9	12	11	11	9	10	13
	Card 18	13	8	9	12	11	11	9	10
	Card 19	10	13	8	9	12	11	11	9
	Card 20	9	10	13	8	9	12	11	11
	Card 21	11	9	10	13	8	9	12	11
	Card 22	11	11	9	10	13	8	9	12
	Card 23	12	11	11	9	10	13	8	9
	Card 24	9	12	11	11	9	10	13	8

Appendix B – The Swait and Louviere (1993) test procedure

The standard econometric approach to test for dynamics in preference and scale parameters has been to apply the Swait and Louviere procedure (1993). The test consists of three stages. First, the researcher splits the sample into two alternative subsamples, in our case the samples HSB and LSB or specific choice tasks, and then estimates the unrestricted model with a set of unique preference parameters for each subsample. Second, a restricted model is estimated with a common set of preference parameters, but a varying (relative) scale parameter across the two subsamples.[23] A likelihood ratio test is applied to test equivalence of all preference parameters between two samples, with the degrees of freedom being equal to *k-1*, where *k* is the number of imposed parameter restrictions. One degree of freedom is lost by explicitly estimating the relative scale parameter. If the null-hypothesis of equivalent preferences is rejected, the samples cannot be combined and it is unknown whether the observed differences arise due to variation in preferences or also due to variations in scale. The third and final step is only conducted when the former null-hypothesis is not rejected. It tests whether scale is equivalent across both samples. A pooled model with common scale and preference parameters is estimated and its log-likelihood value is contrasted against the second stage model using a likelihood ratio test with one degree of freedom for restricting the relative scale parameter. The null-hypothesis assumes scale is equivalent in both samples.

Appendix C – Optimal bandwidth parameters

Fosgerau (2007) and Fröhlich (2006) argue that the bandwidth parameter generally has a larger impact on model results than the shape of the continuous kernel density itself. They also note that there is not a single bandwidth selection method considered to be the best. A practical approach is to select the smallest possible bandwidth for which all local models converge. This approach seems to work well for large datasets. However, it is unknown in advance if this will result in under-smoothing. Additional criteria are needed in order to have the possibility to test the model against the standard MNL model.

[23] Due to the confounding between preference and scale parameters, variations in scale can only be retrieved after imposing equivalence of preference parameters. For identification normalization of a single scale parameter is required. We normalize the scale parameter to one.

Hurvich et al. (1998) propose a statistic based on the trade-off between model fit and the number of parameters in the model, which can be used to determine the optimal bandwidth and select the appropriate model. The number of parameters in the model can be approximated by evaluating the trace of the hat-matrix H (see below). If the bandwidth h of a categorical variable is low, the fit of the model will be better, but more parameters are needed, so the trace of the hat matrix $tr(H)$ will be higher. Model evaluation criteria like the *Akaike information criterion* (AIC) and *Bayesian information criterion* (BIC) can be used for selecting the optimal bandwidth. Hurvich et al. (1998) note that the AIC can lead to under-smoothing, while the BIC tends to support a high degree of smoothing. In this paper, we apply the corrected AIC (AICc) as model selection criterion $AICc = \frac{-2LL\left(\hat{\beta}\right)}{I \cdot T} + \frac{2 \cdot tr\left(\hat{H}\right)+1}{I \cdot T - tr\left(\hat{H}\right)-2}$, introducing an additional penalty for additional parameters in the model compared to the AIC. As a rule of thumb, models are considered significantly different if the difference between model criteria is larger than 3/$(I \cdot T)$ (Charlton and Fortheringham 2009).

As discussed in Koster and Koster (2011), this L-MNL method has its drawbacks if panel data are used. If one does not correct for the panel nature of the data, the local standard errors will be underestimated. Therefore, the trace of the hat-matrix becomes too low, which will result in an optimal bandwidth that is too low and therefore under-smoothing of the model. We correct for this by estimating robust standard errors clustered over respondents (Freedman 2006).

We follow Nagel and Hatzinger (1992) and Koster and Koster (2011) in deriving the hat-matrix for each of the $I \cdot T$ locally estimated weighted MNL models. Let Ω_l represent the $k \cdot k$ (robust) covariance matrix of parameter estimates belonging to a specific locally estimated weighted MNL model l. Alternatively, Ω_l can be specified as the inverse hessian matrix

$\Omega_l=(X^{*\prime} V_l X^{*})^{-1}$, but using the covariance matrix reduces computation time. X^* is a transformation of the design matrix X, where each observation is multiplied by the square root of its own weight $\sqrt{K_{it}}$.[24] V_l represents the locally estimated covariance matrix of choice probabilities. Due to the IIA property of the (weighted) MNL model, V_l is a block diagonal matrix containing the observation specific covariance matrices of estimated choice probabilities V_{it}^l along the main diagonal:

$$V_{it}^l = \begin{pmatrix} \hat{P}_1\left(1-\hat{P}_1\right) & \cdots & -\hat{P}_{J-1}\hat{P}_1 \\ \vdots & \ddots & \vdots \\ -\hat{P}_1\hat{P}_{J-1} & \cdots & \hat{P}_{J-1}\left(1-\hat{P}_{J-1}\right) \end{pmatrix}$$

$$V^l = \begin{pmatrix} V_{11}^l & & 0 \\ & \ddots & \\ 0 & & V_{nT}^l \end{pmatrix}$$

Nagel and Hatzinger (1992) define the hat-matrix for a standard MNL model by $H=V^{1/2}X(X'VX)^{-1}X'V^{1/2\prime}$. We use this specification to construct the hat-matrix for the locally estimated weighted MNL model l. Rewriting $X^{*\prime}V_lX^* = X^{*\prime}V_l^{1/2\prime}V_l^{1/2}X^*$ and noting the similarity between this and the specification by Nagel and Hatzinger (1992), we can specify the local Hat-matrix in the following way: $H_l=V_l^{1/2}X^*(X^{*\prime}V_lX^*)^{-1}X^{*\prime}V_l^{1/2\prime}$. The specification can be further simplified by replacing the middle statement by the local covariance matrix. $H_l= V_l^{1/2}X^*\Omega_l X^{*\prime} V_l^{1/2\prime}$. Note that for each local point a local Hat-matrix needs to be derived.

Using properties of linear algebra, we can rewrite the trace of the local Hat-matrix by $tr(H_l)=tr(X^*\Omega_l X^{*\prime}V_l)$, which saves substantial computation time. As mentioned in Section IV, the

[24] More formally, X is a $I \cdot T \cdot (J-1)$ by k matrix describing the characteristics of each alternative adjusted for a reference alternative (in our case the status quo option). Additionally, it also includes additional explanatory variables in the model. Hence, each observation is described by (J-1) rows in X.

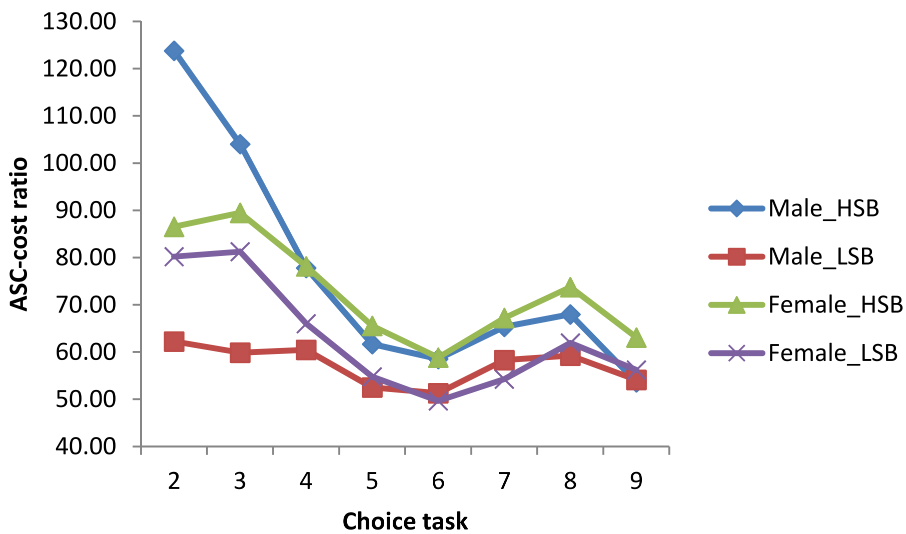
trace of the Hat-matrix approximates the number of parameters in the local model. In the eventual comparison of alternative bandwidth parameters, only the trace elements of the local hat matrix belonging to the local point are used and summed. More specifically, for the first choice card, which contains three alternatives in our case, the first two trace elements of the local hat matrix are stored. For local point two, the elements three and four from its own local hat-matrix. In order to reduce computation time, specific elements c on the trace of the local Hat-matrix can be obtained by calculating $X^*(c,:)\Omega_l X^{*\prime} V_l\,(:,c)$, picking the c-th row of X^* and the c-th column of V_l. The number of parameters related to a specific bandwidth parameter is approximated by summing the stored trace elements over all local models. Clearly, under uniform weights the hat-matrix reduces to the MNL hat-matrix in which the trace sums to the exact number of parameters in the model.

Appendix D – Sensitivity test controlling for preference heterogeneity across respondents

First we test for gender effects by additionally controlling for the binary variable Gender in the kernel density function. The optimal bandwidth parameter is optimized ($h_{gender}=0.26$) while keeping the bandwidth parameters for within and between sample preference dynamics constant (see Model 3 in Table 7). Figure D.1 shows that the starting point bias in the ASC-cost ratio is more apparent for male respondents. This result is in contrast to Ladenburg and Olsen (2008) who find that specifically females are significantly affected by the starting point bias. For the ASC-cost ratio, and also WTP for the compensation attribute, similar convergence patterns are observed across all four depicted subsamples. WTP estimates for the probability attribute are more ad hoc over the choice sequence in this model specification, but again WTP levels seem to converge between the samples over the choice sequence. Last but not least, the evacuation attribute still reveals a divergence of WTP estimates in the final choice task, but this effect turns out not to be gender specific. Hence, our support for the discovered preference hypothesis is not affected by this sensitivity test.[25]

FIGURE D.1

Gender and sample specific ASC-cost ratio over the choice sequence



The second sensitivity test aims to control for unobserved heterogeneity across respondents using a cross-sectional latent class model with two groups. The choice probability of Equation (1) is then modified to:

$$P(y_{it} = j|p, \beta, X_{it}) = p_1 \cdot \frac{exp(X_{ijt}\beta_1)}{\sum_{k=1}^{J_{it}} exp(X_{ikt}\beta_1)} + (1 - p_1) \cdot \frac{exp(X_{ijt}\beta_2)}{\sum_{k=1}^{J_{it}} exp(X_{ikt}\beta_2)}$$

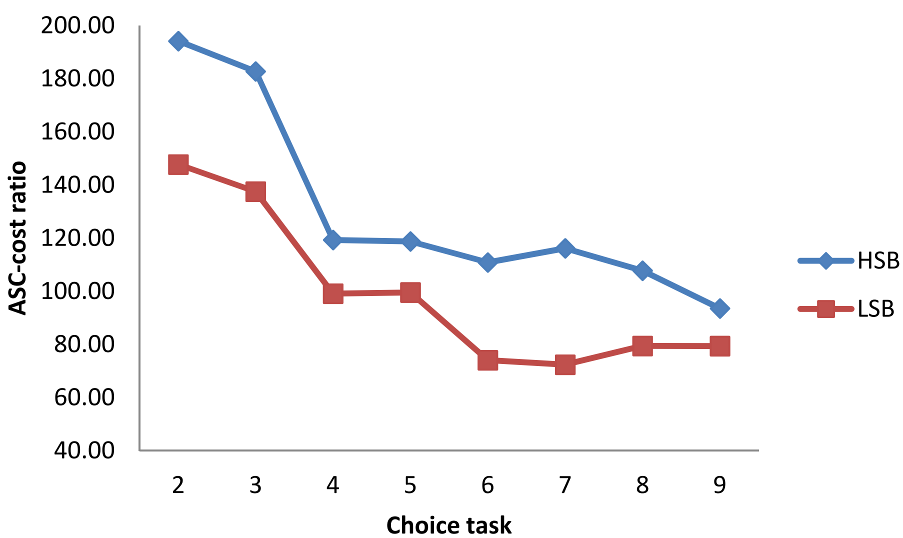
, where $p_1 \in [0,1]$ is the probability to belong to the group with preference parameters $\beta_1$and $1 - p_1$the probability to belong to the group with preference parametersβ_2. For computational tractability we only estimate a mixing distribution on the cost coefficient. Again we estimate the

[25] Detailed results for all policy attributes are available upon request from the corresponding author.

model at the optimal bandwidth parameters as presented in Table 7. Figure D.2 shows again that the mean ASC/cost ratio decreases and that the effect of the starting point bias wears out, implying that the observed patterns of within and between sample preference dynamics are similar to the results obtained from our basic L-MNL model. Not surprisingly, WTP levels are affected as illustrated by the higher level of the ASC-cost ratio over the entire sequence in Figure D.2. Patterns for the other policy attributes and the ASC are available upon request and do not contradict the conclusions from the main text in terms of the patterns over the choice sequence.

FIGURE D.2

Mean ASC-cost ratio controlling for unobserved preference heterogeneity in the cost parameter



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