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Optimal sequential strategy to improve the precision of the estimators in a discrete choice experiment: A simulation study

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A B S T R A C T

Introduction: In order to solve the problems related to prior parameter misspecification in DCEs, Bliemer and Rose (2010) proposed a sequential approach where the design is updated after each respondent. This paper tries to find a more efficient alternative sequential method since the original proposal could be very time-consuming and expensive under some circumstances.

Methods: 11 different strategies were simulated using 8 to 16 choice sets following a Monte Carlo approach. The accuracy and bias of the estimates of each strategy were studied using the relative error and mean value of their estimates.

Results: The DCE performs similarly to the original strategy by updating the design after five respondents. Among the other strategies, we discovered that, under certain circumstances, updating the design after 20 or 10 respondents led to accurate and not significantly biased estimates.

Conclusions: For a strategy to be efficient it might not be necessary to update the DCE after each respondent, but we found that updating the prior information relatively often and regularly can be almost as efficient as the original sequential proposal (for example, updating after five respondents might be a good choice). In addition, our findings suggest that each DCE has different efficient strategies depending on the number of attributes, levels, sets, and alternatives, so it can be concluded that a universal "optimal sequential strategy" does not exist.

1. Introduction

Discrete choice experiments (DCEs) allow researchers to gain insights into public preferences for hypothetical products or services. Their origins date back to 1927, when Thurstone described the *Law of Comparative Judgement.* In Thurstone's framework, choices between two specimens can be explained in terms of their characteristics (Thurstone, 1927). However, it was not until 1974 that Daniel McFadden applied the same logic to Economics, coming up with the Random Utility Theory (RUT) (McFadden, 1974). Random Utility Theory posits that the utility derived by one individual from a product or service depends on some observable variables (known as attributes) and some unobservable variables (such as idiosyncratic individual characteristics) (McFadden, 1994; Train, 2009). This definition enabled McFadden to approximate the probabilities of choosing one alternative over another by estimating an underlying utility function through the conditional logit (CL) model, a form of logit similar to the multinomial logit (MNL) model (McFadden, 1974).

Since the advent of the RUT, the number of published DCE studies has increased almost exponentially, mainly in areas such as economics, transportation research, environmental studies, civil engineering, and health (Haghani et al., 2021). A vast methodological literature has also been generated, leading to changes in applied studies (Johnson et al., 2013) and competing paradigms (Louviere et al., 2010; Rose and Bliemer, 2014).

In practice, DCE surveys are administered to individuals from a target population. These surveys comprise several questions known as choice sets. Each choice set contains two or more alternatives from which the respondent must choose one. Each alternative

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represents a version of the same hypothetical product or service, differing in one or more characteristics (attributes). Respondents' choices can be easily used to estimate the utility function of the population in almost any statistical software. Whereas the collection and analysis of data is a simple task, the greatest difficulties are usually found in the design stage of the experiment.

One of the first tasks of a DCE project is to identify and select several attributes and levels (Bridges et al., 2011). In a DCE with 4 attributes of 3 levels each, the total number of profiles (different alternatives) would be $3^4 = 81$. If these alternatives were combined into pairs to create choice sets, the total number of combinations would be 3 240 choice sets. Those DCEs that use all possible choice sets are known as full-factorial designs (FFD) (Traets et al., 2020). Full-factorial designs can be applied by randomly selecting, for example, ten choice sets to give to each respondent. Alternatively, the 3 240 choice sets could be split into, for example, 270 blocks and allocated to a group of respondents. However, FFDs are rarely used because researchers frequently want to avoid a large number of versions of the survey (for example, when doing a pen and paper survey or surveying different segments of the population). Since respondents can answer a maximum of 18 choice sets (Mangham et al., 2009), fractional designs are typically used, i.e., DCEs that include only some of the potential choice sets. At this point, a fundamental question arises: which choice sets should the researcher use?

In the past, researchers answered this question by borrowing construction techniques from experimental design theory focused on linear regression models (Rose and Bliemer, 2014). In this context, design methods based on orthogonal arrays became very popular. However, it did not take long for some authors to note that orthogonal designs were not optimal for non-linear models (Fowkes, 1998), and many new methodological contributions were made in the departure of DCEs from orthogonality. In the past, there has been a great deal of confusion surrounding the names and assumptions of the different methods proposed in the literature (Louviere et al., 2010). Still, most non-orthogonal approaches are based on reducing the determinant of the asymptotic variance-covariance (AVC) matrix (D-error) and thus decreasing the standard errors of the parameter estimates (some recent applied examples can be found at Doherty et al., 2021; Dong et al., 2020; Foreman et al., 2021; Hynes et al., 2021; Lyu and Hwang, 2021; Pérez-Troncoso et al., 2021; Tait et al., 2019). Nevertheless, the determinant of the AVC can only be computed if the parameter values are known, which means that the parameters need to be known beforehand to make a precise estimation of the parameters. This paradox has been overcome by using prior parameters.

Prior parameters, or simply priors, are *a priori* information about the parameter estimates which may come from any reliable source (literature, pilot experiments, expert consultations) (Bliemer and Rose, 2010). Many assumptions have been made about prior parameters over the years. For example, Fowkes and Wardman (1988) suggested constructing experimental designs tailored to fixed (known) non-zero priors (Rose and Bliemer, 2014). Alternatively, Bunch et al. (1996) assumed zero priors and selected the design with the lowest D-error among a set of candidate orthogonal designs. Although Bunch's approach was criticised for not posing any difference to the optimisation problem in the linear framework (Ferrini and Scarpa, 2007), they contributed to popularising the use of the D-error statistic (Rose and Bliemer, 2014). On the other hand, Huber and Zwerina (1996) proposed using a set of non-zero priors to select the design with the lowest D-error abandoning the orthogonality requirement.

In all the above cases, the misspecification of priors implies that the efficiency of the design will be lower. However, all priors are somewhat misspecified because researchers do not know the result of their experiments beforehand. Several strategies have been proposed to deal with this problem. For instance, Sándor and Wedel (2001) relaxed the assumption of perfectly specified priors by assuming that priors are random variables that follow a distribution (Rose and Bliemer, 2014). A different proposal, known as sequential designs, can be found in Bliemer and Rose (Bliemer and Rose, 2010), where the authors suggest updating the priors, and the design, each time a new response is collected (by "a response" we refer to all choices made by one respondent). The latter proposal can be summarised in the following steps:

a. Set the priors to zero
b. Compute a design that minimises the D-error.
c. Present the DCE to a new respondent.
d. Pool the new response with previous results (if any).
e. Estimate a conditional logit model.
f. Replace the priors with their respective coefficients if they are significant.
g. Go back to point b or finish if there are no more respondents.

However, the latter approach could be too expensive and time-consuming even if the DCE is conducted online because there will always be two respondents that complete the experiment simultaneously. This problem has already been mentioned in Bliemer and Rose (2010), where the authors propose an automatic implementation that updates the experimental design after a specific number of responses or period of time.

Based on the proposal of Bliemer and Rose, we hypothesise that sequential designs might maintain their performance when updated less frequently (for instance, after 5, 10, 20, or 100 new respondents). In that case, the efficiency of the approach would increase as a result of obtaining the same performance while consuming less time and resources. This article aims to test the performance of sequential strategies that use more than one response to update the priors and compare them to the standard sequential approach.

2. Methods

The package idefix (Traets et al., 2020) in R (R Core Team, 2020) was used to simulate the results of 11 different sequential

Table 1
Iterations and respondents used in each simulation strategy.

Simulation id	Iterations (I)	Respondents per iteration (n)
1	1	200
2	2	100
3	3	50 (x2) + 100
4	4	50
5	6	20 (x5) + 100
6	10	20
7	11	10 (x10) + 100
8	20	10
9	21	5 (x20) + 100
10	40	5
11	200	1

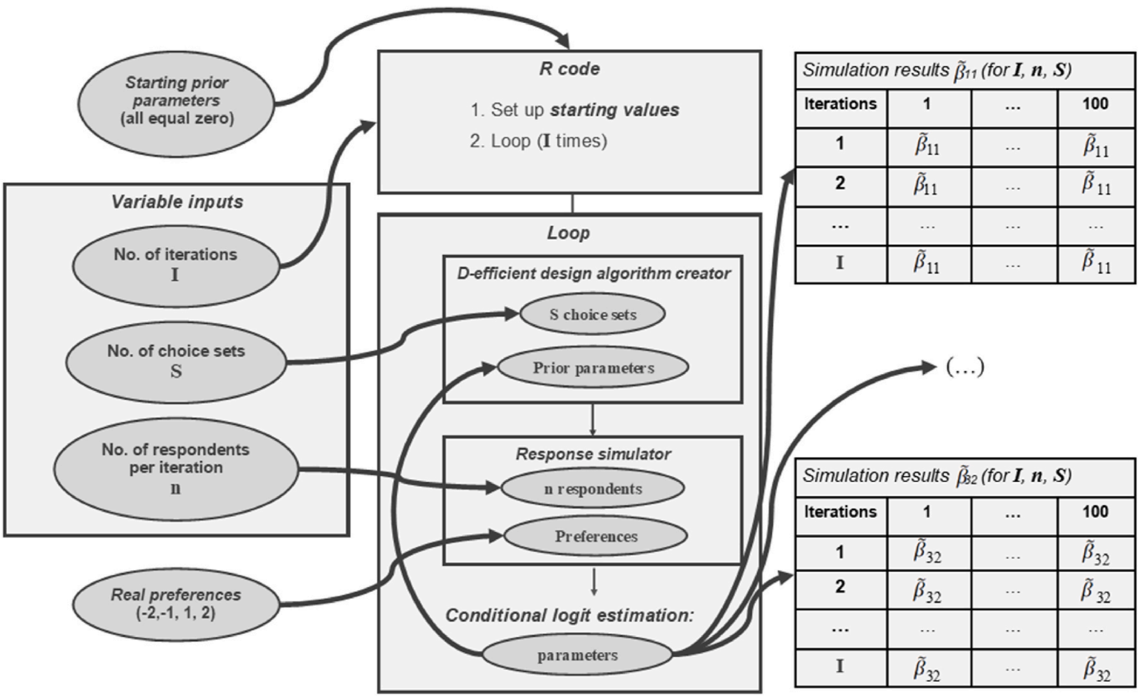

Fig. 1. Representation of the R code used to perform the simulations.

strategies. A Monte Carlo approach was followed, so each simulation was repeated 100 times. The strategies differed in the number of respondents used to calculate the next iteration of the design, and each strategy was tested using a range of 8–16 choice sets with 2 alternatives. All DCEs simulated used the same attributes and levels specification, $2^2\ x\ 3$ (two two-level attributes and one three-level attribute) with the same simulated parameter values ($\beta_{11} = -2, \beta_{21} = -1, \beta_{31} = 1, \beta_{32} = 2$, using effects coding). These coefficients used to simulate preferences will be known as "target coefficients."

A sizable number of tests were carried out before deciding the final specification, which was chosen as a balance between design size and computational time (each strategy took from 30 min up to 26 h to perform the 100 runs [using a computer with an AMD Ryzen 536 00X 6-Core Processor 3.79 GHz and 16 GBs of RAM]). The details of the simulations can be seen in Table 1.

Fig. 1 is a graphical presentation of the R code used. This code consists of a loop that creates a *D-efficient* design based on the prior parameters from the last conditional logit model. The code represented in Fig. 1 executes the following procedure:

1. Code is initialised setting n (respondents per iteration), I (no. iterations), and S (no. of choice sets). Starting prior parameters are set to zero, and target coefficients are fixed.
2. A D-efficient design is created using the idefix package (Traets et al., 2020) in R (R Core Team, 2020).
3. n responses are simulated for the D-efficient design created in the last step.
4. The response data is pooled in a database with previous data (if any).
5. A conditional logit model is estimated with the data from the whole database.

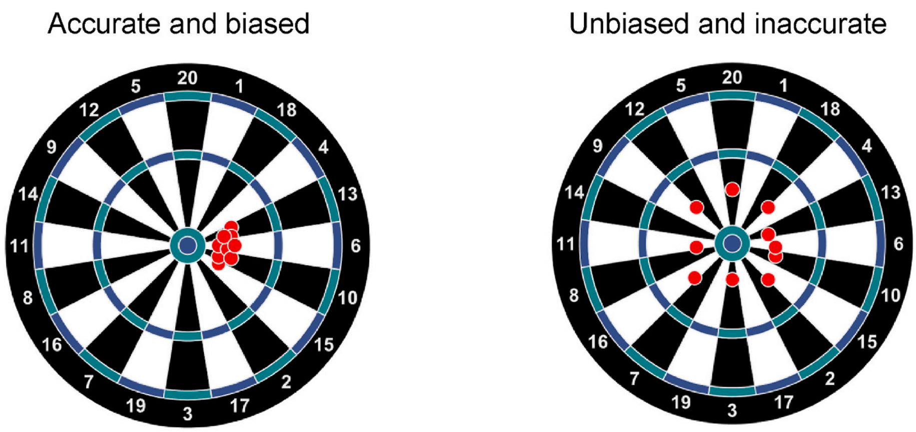

Fig. 2. Illustrations of bias and accuracy.

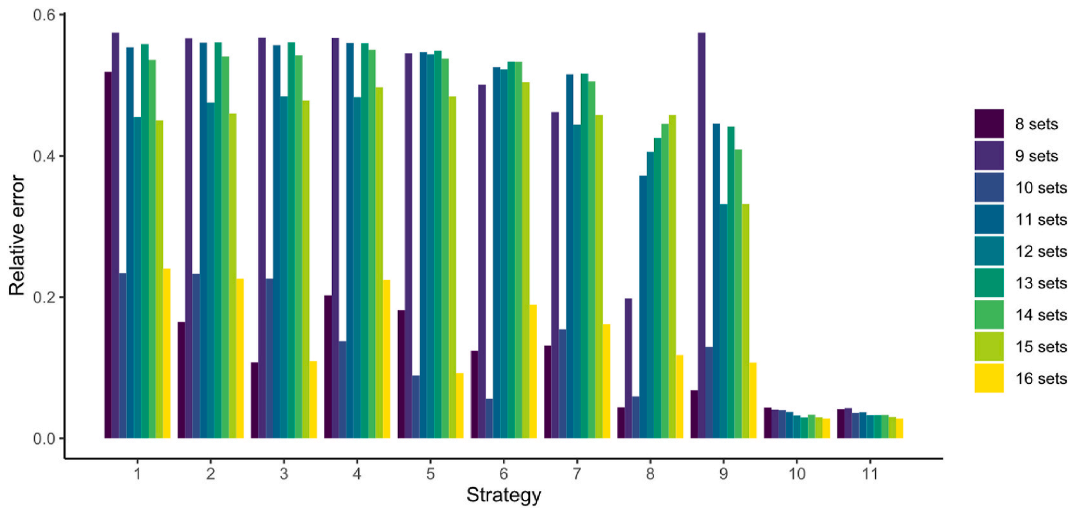

Fig. 3. Relative error of the estimates.

6. The parameters of the conditional logit model replace previous prior parameters only if they are significantly different from zero ($\alpha = 0.05$).
7. Back to point 2 (when this loop is repeated I times, go to point 8).
8. Store the significant coefficients from each iteration.
9. Go to point 1 (repeat this procedure 100 times to complete the Monte Carlo approach).

Thus, the R code used resulted in four excel files per strategy (one per coefficient) with I rows (where it can be seen how the coefficient varies when n responses are added in a new iteration) and 100 columns (where the same procedure is repeated 100 times to follow the Monte Carlo approach).

Each strategy yielded nine different results (8–16 choice sets) with 100 estimates (due to the Monte Carlo approach) for each of the four coefficients. In the baseline analysis, the bias and accuracy of the estimates of all strategies were studied. Bias, which was considered significant or non-significant according to the results of a *t*-test, was defined as the deviation of the mean of the 100 estimates from the target coefficient (illustrated in Fig. 2, right panel). However, accuracy, measured by the mean relative error, was defined as the size of the differences between the target coefficient and the 100 estimates (Fig. 2, left panel). In a secondary analysis, the results obtained in the baseline analysis were tested by modifying some design features in a sensitivity analysis.

Strategy 11 represents the exact Bliemer and Rose's (2010) proposal. Conversely, strategy 1 represents the procedure followed in many studies (where the D-efficient design is built based on zero priors and presented to all respondents). The rest of the strategies use either a constant or non-constant number of respondents per iteration (for example, strategies 2, 4, 6, 8, 10, and 11 are constant because they use the same number of respondents per iteration. Conversely, strategies 3, 6, 7 and 9 can be considered non-constant).

In this paper we used the coordinate exchange algorithm (CEA) to maximise the determinant of the information matrix of the experimental design. The CEA algorithm uses Bayesian optimality, i.e., the D-error is computed assuming that priors are not fixed but

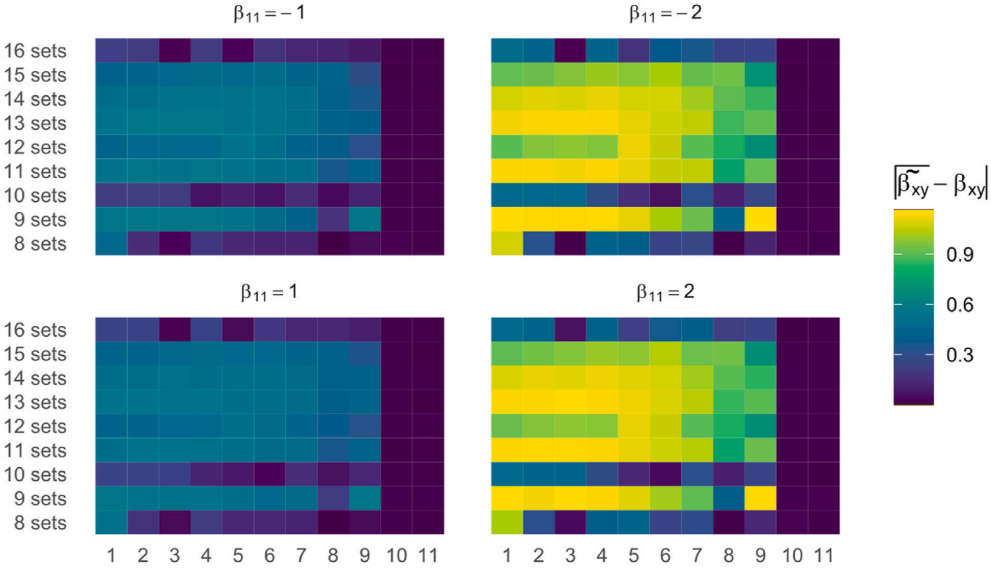

Fig. 4. Illustration of the bias depending on the strategy and sets. Notes to the figure: each panel represents the estimates of one of the four coefficients. Each sector (square area) illustrates a design approach: a combination between a design strategy (x-axis) and a number of choice sets (y-axis). The colour of the squares illustrates the close an estimate is to the target coefficient in absolute value. $\overline{\widetilde{\beta_{xy}}}$ represents the mean of the estimates (i.e. each number in Supplementary Material), and β_{xy} the target coefficient. . (For interpretation of the references to colour in this figure legend, the reader is referred to the Web version of this article.)

follow a prior preference distribution (Traets et al., 2020).

3. Results

$11 \times 9 \times 100 = 9900$ simulations were carried out using the R code disclosed in the Supplementary Materialhttps://doi.org/10.17632/xw88pgzybn.1. As a result, we obtained the coefficients reached in each iteration of each Monte Carlo repetition for each strategy. We focused on the estimates yielded in the last iteration. These final estimates represent the coefficients obtained at the end of the DCE (and can be observed in the Excel file attached as supplementary material). Thus, the precision and the bias significance of each strategy were based on the 100 final coefficients obtained for each target coefficient (−2, −1, 1, 2) in each strategy using from 8 to 16 choice sets.

3.1. Accuracy

The accuracy of the estimates was measured using the mean of the relative error, computed as $r_e = \left|\frac{\widetilde{\beta_{xy}} - \beta_{xy}}{\beta_{xy}}\right|$. Each estimation was compared with its corresponding target coefficient: $\beta_{11} = -2$, $\beta_{21} = -1$, $\beta_{31} = 1$, or $\beta_{32} = 2$. Fig. 3 shows the relative error (averaged over all four parameters) depending on the strategy and number of choice sets used (an alternative presentation of the results can be found in Supplementary Material Table S1).

As shown in Fig. 3, estimates from strategies 10 (I = 40, n = 5) and 11 (I = 200, n = 1) show a low relative error regardless of the number of sets in the DCE, which implies that the estimates are very close to the target coefficient. However, with only a few notable exceptions, the relative error of the rest of the strategies reflects a weak accuracy. The exceptions mentioned above include strategies 6 and 8. Strategy 6 (I = 10, n = 20) has a low relative error only when 10 choice sets are used, while strategy 8 (I = 20, n = 10) exhibits a high degree of accuracy when 8 and 10 choice sets are used.

The approaches that yield the most accurate estimates, strategies 10 and 11, reduce their relative error when a higher number of choice sets is used. Nevertheless, this trend cannot be extended to the rest of the strategies. As we mentioned before, all other strategies, especially estimates from strategies 6 and 8, are more accurate when a specific number of choice sets is used. In general, accuracy improves when 8, 10, or 16 choice sets are used. This result suggests the existence of optimal points that might or might not depend on other design characteristics such as the number of attributes and levels or the number of alternatives.

3.2. Bias

The heatmap in Fig. 4 gives us a quick overview of the absolute distance between the estimation mean and the target coefficient. The significance of the bias was checked using a *t*-test and presented in Table S4 in Supplementary Material. In Fig. 4, the closer an

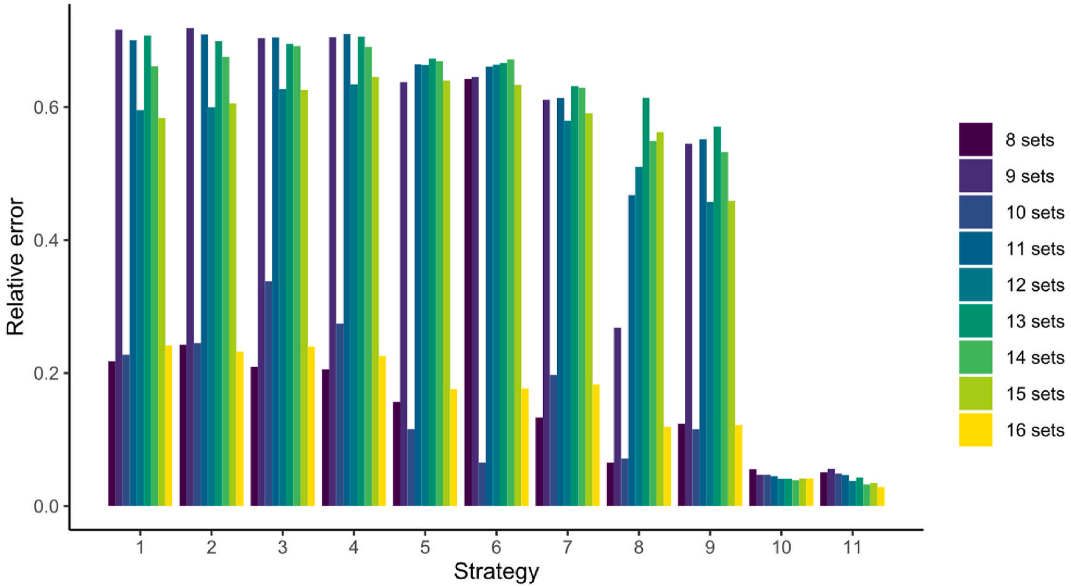

Fig. 5. Relative error of the estimates (for a $2^2 \cdot 3 \cdot 4$ experimental design).

estimate is to zero (purple), the more likely the estimate is not significantly biased.

Most of the estimates obtained with strategies 10 (I = 50, n = 5) and 11 (I = 200, n = 1) are not significantly biased. All other strategies, with few exceptions, are biased either upwards or downwards. Exceptions include strategies 3 (I = 3, n = 50, 50, 100), 6 (I = 10, n = 20), and 8 (I = 20, n = 10), which are not significantly biased (or are very close to the target value) when a specific number of choice sets are used.

When the analysis is carried out based on the number of sets, the pattern in Fig. 4 indicates that estimates based on DCEs with 8, 10 and 16 choice sets were more likely to be unbiased.

Under the circumstances in which the simulations were conducted (3 attributes of $2^2 \cdot 3$ levels and 200 respondents) only estimates from a few strategies can be considered accurate and not significantly biased. This is the case of strategies 10 and 11, whose performance improves the more sets are used. Since strategy 11 is the original proposal by Bliemer and Rose (2010), it was expected to yield unbiased and accurate estimates. However, to the best of our knowledge, it is the first time that the rest of the strategies were tested empirically. We discovered that strategy 10 (I = 40, n = 5) produced accurate and unbiased estimates, but the same cannot be said of the other strategies (1, 2, 4, 5, 6, 7, 9). Nevertheless, some interesting patterns emerged when certain strategies (3, 6, 8) and certain number of sets (8, 10, 16) were used. We, therefore, carried out a sensitivity analysis to test whether these patterns change when some design features vary.

3.3. Sensitivity analysis

In this section, the consistency and reliability of the results obtained previously were tested under different circumstances. First, the impact of the number of attributes and levels on the strategy performance was tested by adding a new four-level attribute, thus increasing the number of parameters to be estimated. Next, the impact of the sample size was tested using a different number of simulated responses.

a. Number of parameters

The ability of strategies to yield accurate and unbiased estimates was studied by adding a new four-level attribute and repeating the simulations. However, due to hardware and time limitations (computational time increased substantially when three new parameters were added to the model), the Monte Carlo approach was reduced to 20 repetitions in strategies 7, 8, 9, 10, and 11 (from 8 to 11 sets) and to 10 repetitions in strategy 11 from 12 to 16 sets.

The magnitude of the relative errors of each strategy can be observed in Fig. 5 and Table S2 in Supplementary Material. As shown, the addition of a new four-level attribute did not change the results obtained previously. Estimates from strategies 10 and 11 remain among the most accurate estimates regardless of the number of choice sets used. In addition, strategies 6 and 8 still show high accuracy when used, respectively, with 10 and 8 choice sets.

Again, the significance of the bias of each estimate was evaluated using a one-sample *t-test*. The results, which can be observed in detail in Table S5 from Supplementary Material, are closely aligned with the findings in the baseline analysis. Strategies 10 and 11 yield unbiased (or very close to the target coefficient) estimates regardless of the number of choice sets used. Among the other strategies, strategy 8 is not significantly biased in most parameters when 8 choice sets are used, while strategy 6 is not significantly

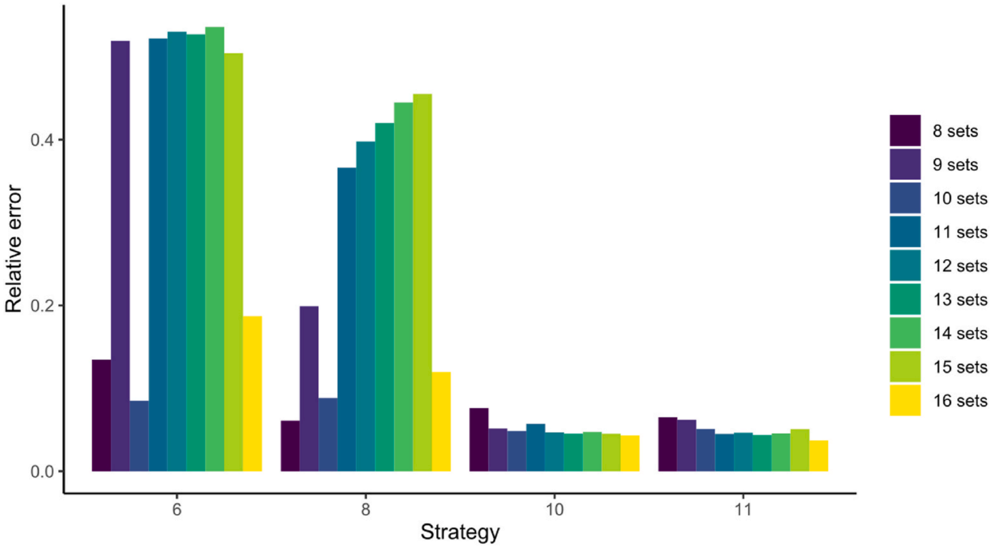

Fig. 6. Relative error of the estimates using 100 responses.

biased in some parameters when 10 choice sets are employed. Regardless of the *t-test*, all the strategies mentioned reached estimates very close to the target coefficient, so any of them could be used interchangeably. Unlike in the baseline analysis, strategy 3 turned significantly biased when the model size increased.

b. Sample size

To study the effects of decreasing the sample size the simulations were carried out using only 100 respondents, $N = 100$ (a higher number of responses was not considered since we expected that it would only improve the estimations). The Monte Carlo approach was restricted to 20 repetitions as in the previous case. Besides, only strategies 6, 8, 10, and 11 were simulated since the rest were expected to lose efficiency due to the sample size reduction. Due to the decrease in the number of respondents, the number of iterations required to complete the survey was reduced to half: strategy 6 (I = 5, n = 20), strategy 8 (I = 10, n = 10), strategy 10 (I = 20, n = 5), strategy 11 (I = 100, n = 1).

As shown in Fig. 6 and Table S3 in Supplementary Material, strategies 10 and 11 show no uniform decreasing pattern when the number of choice sets increases. On the other hand, strategies 6 and 8 exhibit lower relative error when 8, 10, and 16 sets are used. With respect to the significance of the bias of the estimates, the conclusions are analogous to those obtained in the baseline analysis (see Table S6 in Supplementary Material).

4. Discussion and conclusions

In this article, an investigation has been carried out to find efficient alternatives to Bliemer and Rose's (2010) sequential discrete choice experiments. Ten alternative serial strategies have been tested and compared to the original proposal through a simulation study. By evaluating the accuracy and bias of the estimates reached by each strategy, we discovered that for a strategy to perform well, it might not be necessary to update the DCE after each respondent. However, the results suggest that updating the prior information relatively often and regularly can be almost as efficient as the original sequential proposal. A good example of this is strategy 10, which updates prior information after five new respondents.

We found that some strategies had a low relative error and a non-significant bias when a specific number of sets was used. However, we cannot claim that the findings are independent of the characteristics of the design, so they should not be extended to DCEs that use more or fewer attributes, alternatives, or sets. On the other hand, the reasons why strategies 6 and 8 yield accurate and unbiased estimates only when a certain number of choice sets are used remain unknown. Our findings suggest that each DCE has a different optimal sequential strategy depending on its number of attributes, levels, sets, and alternatives. For example, for a $2^2\ x\ 3$ design with 8 choice sets, strategy 6 would be optimal because it obtains non-biased and accurate estimates while minimising the number of iterations needed. Other optimal strategies could be found by increasing the number of simulations performed with different experimental designs. However, new simulations based on more complex experimental designs require more powerful hardware than the one used in this research. The code and results are available for other researchers to analyse, replicate, and modify.

Because a universal "optimal sequential strategy" might not exist, we cannot make universal practical recommendations. However, two hypotheses can be put forward. First, approaches like strategy 10 are likely to produce accurate and non-biased estimates as the original proposal for most DCE settings (and thus, more efficient). Secondly, probably, each DCE setting (defined by its attributes, levels, sets, alternatives) has a different optimal sequential strategy that should be found in future research.

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Declaration of competing interest

No conflict of interests.

Data availability

Code available in Supplementary Material and GitHub.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jocm.2022.100357.

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