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The confounding of effects in rank-based conjoint-analysis

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Abstract

Basic confounding patterns for full-profile conjoint analyses based upon ranking are examined. It is shown that commonly used orthogonal main-effect designs can lead to biased part-worth estimates, especially to an underestimation of less important variables. An alternative design procedure is developed to overcome this flaw. The model is tested by means of simulation analyses and is applied to a marketing research study. Some guidelines for applications are provided.

A. Introduction

Conjoint analysis enjoys large popularity among marketing researchers, as it combines easy-to-handle data collection with sophisticated evaluation methods. Rank-based conjoint analysis is often used to estimate the respondent's metric utility function (see overview in: Green, Srinivasan,1978 and 1990). However, recent studies show that there are some inherent limitations concerning the accuracy of the part-worth estimates (Currim et al., 1981; Müller-Hagedorn et al., 1993, Steenkamp, Wittink, 1994).

To be specific, Darmon and Rouziès (1994) observe in a simulation analysis that less important variables tend to be underestimated at the benefit of more important variables, especially if the data possess a low level of noise. There are indications that this distortion can be caused by the interference with highly important variables (Teichert, 1994).

This study investigates the systematic patterns of such biases in more detail. It focuses on the most severe case of distortion; when a variable of less importance is not at all distinguishable from the other variables. This variable is then known as "confounded" (Box, Hunter, Hunter, 1978). Confounding patterns are analyzed based on the mathematical capabilities of rank-ordered conjoint-analyses. Simple statistics and estimation models are used to provide a clear focus. To further simplify, the explanations are restricted to two-level designs. The results, however, are transferrable to conjoint-analyses that are more complex.

In the following, basic confounding patterns of ranking data are first analyzed. The results of the simple model are subsequently transferred to orthogonal main-effect designs with more than three variables and are tested in a simulation analysis. Possible solutions to the confounding patterns revealed are compared. The preferred alternative design model is then applied using large-scale empirical data. Finally, implications and guidelines for marketing research applications are discussed and presented.

B. Confounding patterns in ranking data

a) Basic confounding patterns

Part-worth values of variables are not directly observed in conjoint analyses. The data base consists of holistic preference judgments regarding a set of stimuli, being pre-specified combinations of variables and their levels. Estimates for the individual variables are derived from statistical methods of decomposition.

Rank-based conjoint analyses use the ranking of stimuli as the dependent variable and the stimuli-defining variable levels as the independent variables. Table 1 offers an example. The preferability of four different air-freight-services is surveyed. The stimuli are defined by three variables with two levels each. Dummies are used to code the variable levels.

No statistical method is able to retrieve information not provided in the ranking, as this is the only dependent variable in the analyses. A variable has no measurable effect if it exerts no influence on the ranking. This is ceteris paribus the case, if the average ranks are equal at the different variable levels.

Table 1: Example of a rank-based conjoint problem for air-freight-services (simplified example from Mengen, 1993): variables, levels and codings.

service alternative	characteristics of service			preference rank (1 = best case)
	independent variable A: „transport time"	independent variable B: „guarantee"	independent variable C: „tracking"	dependent variable
# 1	A = +1 „overnight"	B = +1 „money back"	C = +1 „real-time"	?
# 2	A = +1 „overnight"	B = -1 „no guarantee"	C = -1 „no tracking"	?
# 3	A = -1 „two days"	B = +1 „money back"	C = -1 „no tracking"	?
# 4	A = -1 „two days"	B = -1 „no guarantee"	C = +1 „real-time"	?

One thus obtains a simple estimate regarding the size of an effect by contrasting the average ranking values at the different variable levels. Applying the robust ANOVA technique (see Wittink, Cattin, 1981), the size of the effect E(A) of variable A with two levels (+A,-A) can be defined as the difference between the average ranks $\overline{R}$:

$$E(A) = \overline{R}(+A) - \overline{R}(-A) \qquad (1)$$

The estimated part-worth value PW(A) of variable A is a normalized transformation of the size of the effect, and can be defined as:

$$PW(A) = \frac{E(A)}{\sum_{X} |E(X)|} \qquad (2)$$

with X being the set of all independent variables and $A \in X$.

When dealing with metric data, effect estimates are obtained without ambiguity from an orthogonal design: Such a design is a subset of stimuli of the completely enumerated, full-factorial design which fully correlates the levels of relevant effects and irrelevant interaction effects (Kuhfeld, Tobias, Garatt, 1994). These identities are known as design-generating "aliases". Orthogonal designs avoid any other correlation of the levels of the relevant independent variables. The effects of the other variables are balanced out and do not influence the individual estimations. Therefore there is no ambiguity in the interpretation of the outcomes of the estimating contrasts (Box, Hunter, Hunter, 1978).

Orthogonality of ranking data is a necessary, but not a solely sufficient condition to prevent ambiguity of effect estimates. Information concerning effect size is lost in the recoding of metric preference values to ranking judgments. Adding one and the same metric part-worth value to a different stimulus may or may not influence its ranking, depending on the total utility of the stimulus in comparison with the utility of the neighbouring stimuli. Thus, the estimating equation (1) can be influenced by other variables, leading to an ambiguous effect estimate.

Orthogonal main-effect designs may even lead to the confounding of a less important variable. This variable may be an alias of the (non-existent) interaction of two highly important variables. If this is the case, then a change of the level of the less important variable always coincides with a change of the level of exactly one of the highly important variables. Thus the effect of the less important variable is always offset by the simultaneously occuring effect of one of the larger variables. Ceteris paribus, the smaller variable will not be able to influence the ranking structure. It will not be distinguishable, i.e. it will be confounded.

This effect can be illustrated using the example introduced in table (1): This is the most simple case of a 2^{3-1} orthogonal main-effect design. The design is generated by choosing a subset of stimuli with the design-generating alias C = A * B. A set of four stimuli results, as shown in table 1 and visualized in figure 1. It can be seen, that the stimuli distinguish themselves simultaneously in the levels of exactly two variables. Therefore the effect of the least important variable is always offset, this variable is not able to influence the preference structure.

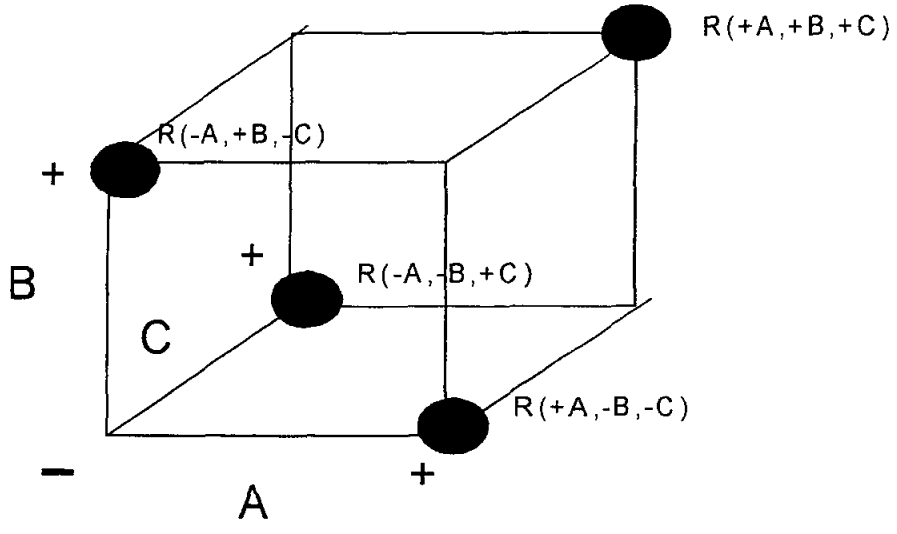


Figure 1: Observations of a 23-1 orthogonal main-effect design with • showing C = A * B

This effect can be shown in mathematical terms as following: Let the "true" part-worth values $\tilde{PW}$ of the upper levels of variables A, B, and C, each having the dichotomous levels (+1,-1), be described by the following inequalities:

$$\tilde{PW}(A) > \tilde{PW}(B) > \tilde{PW}(C) > 0 \qquad (3)$$

Let R(+A,+B,+C) be the rank of a stimulus consisting of variables A, B, and C with dummy level A=+1, B=+1, C=+1. The rank order of the four stimuli selected (see figure 1) can then be deduced from the inequalities (3) as:

$$R(+A,+B,+C) > R(+A,-B,-C) > R(-A,+B,-C) > R(-A,-B,+C) \qquad (4)$$

The effect of variable C is estimated to be the contrast according to equation (1). According to the ranking structure of (4) it is calculated by adding the first and fourth ranks and subtracting the second and third ranks. The resulting estimate must consequently

be zero. This implies that the smallest variable is confounded in the design which was chosen.

If the study on air-freight services would have been conducted as outlined in table (1), the deterministic model had been incapable of evaluating the respondent's preference for providing a tracking service (variable C).

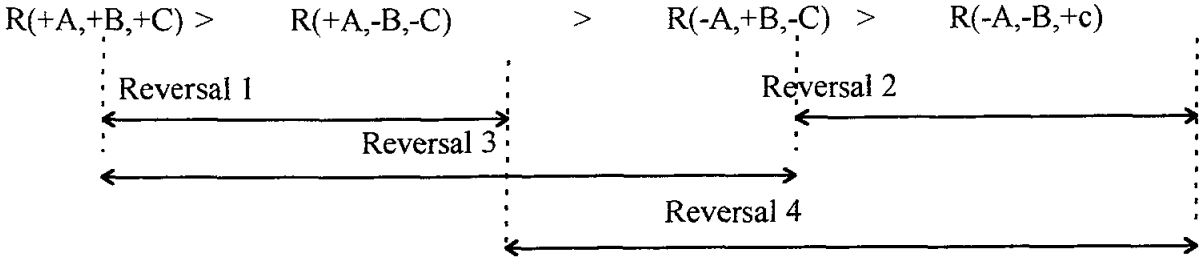
b) Confounding in two-level main-effect designs

Real-life orthogonal main-effect designs are extensions of the simple 2^{3-1} orthogonal main-effect design as outlined above. The basic pattern remains the same, however, if a less important variable C is confounded by the interaction term of two more important variables A and B.

For any design with confounding C = AB, one obtains only replications within the three-dimensional space of A, B and C at four levels as shown in figure 1. These replications differ in the values of the added variables.

The added variables do not improve the estimability of effect C, as long as the basic ranking structure as outlined above remains unchanged. Reversals within the replications are irrelevant for the estimation of effect C since they occur at the same level of C.

The confounding patterns of C are overcome if the basic ranking order (see inequalities 4) changes in respect to the relative positions of those observations with different levels of C (+C;-C). Thus there are four possibly relevant reversals within the three-dimensional space of A, B, C:



Variable C remains confounded if the added variables do not even cause a change in two neighbouring ranks (reversal 1 or 2).This is the case if the effect of variable B is larger than the sum of effects of the added variables and of C.

To illustrate, two more variables D and E are added to the example used above and a 2^{5-2} orthogonal main-effect design with eight stimuli is chosen. A set of six possible designs fulfills these design characteristics (with generating aliases DE = +/- AB; +/- AC; +/- BC). These designs consist of four stimuli in the three-dimensional space of ABC, with two occupants each. The replications distinguish themselves simultaneously in the levels of D and of E.

The requirements for the occurrence of reversals 1 to 4 are calculated for each design. The design alternatives exhibit differences in their ability to overcome the basic confounding pattern (see table 2). Best results are obtained by the designs with confounding DE = +/- AB. Reversals require the smallest relative size of the added variables in this case.

Table 2: Requirements for reversal of ranks

	Alternative main-effect designs (design generating aliases)					
	DE = AB	DE = - AB	DE = AC	DE = - AC	DE = BC	DE = - BC
reversal 1	B < D-C	B < D-C	B < D-C	B < D-C	B < D+E-C	B < D-E-C
reversal 2	B < D+C	B < D+C	B < D+C	B < D+C	B < C+D-E	B < C+D+E
reversal 3	A < D-C	A < D-C	A < D+E-C	A < D-E-C	A < D-C	A < D-C
reversal 4	A < D+C	A < D+C	A < C+D-E	A < C+D+E	A < C+D	A < C+D

The additional variables are not able to offset the confounding of C= AB in any design if:

$$|E(A)| \cap |E(B)| > \sum_{X \neq A,B} |E(X)| \qquad (6)$$

The basic confounding patterns are revealed to be of relevance in an orthogonal main-effect design if two variables dominate the entire preference structure. A variable is confounded if it is an alias of the interaction term of the dominating variables.

c) Simulation example

A simulation analysis is performed to indicate the relevance of the hypothesized confounding patterns. A 2^{5-2} orthogonal main-effect design is chosen with confounding of

the main effects C=AB and E=-BD (see table 3). Preference functions are evaluated on an aggregate level using the software package SPSS.

Table 3: 2^{5-2} orthogonal main-effect design

Stimulus	A	B	C=AB	D	E=-AD
I	-1	-1	1	-1	-1
II	1	-1	-1	1	-1
III	-1	1	-1	-1	-1
IV	1	1	1	1	-1
V	-1	-1	1	1	1
VI	1	-1	-1	-1	1
VII	-1	1	-1	1	1
VIII	1	1	1	-1	1

A deterministic model and stochastic models are tested for a preference function with given "true" preference values, with PW(A) > PW(B) > PW(C) > 0 and PW(B) > PW(D) > PW(E) >0. To create the stochastic models, the Carmone and Green (1981) method is applied. A normal distributed error term is added to each preference judgment before transforming the metric values into ranking judgments. The size of the error term is varied as a percentage of the variation coefficient (see Carmone, Green, 1981) in three levels.

Table 4: Simulation outcomes of the main-effect design

	A	B	C	D	E
"true" part-worth	40%	31%	12%	10%	7%
deterministic model	47%	33%	0%	20%	0%
stochastic models:					
5% error term	49%	30%	1%	18%	2%
10% error term	46%	31%	1%	17%	4%
20% error term	45%	28%	3%	16%	8%

As seen in table 4, the effects of both variables C and E are utterly confounded in the deterministic model. This can be explained in terms of the confounding patterns chosen,

since both variables C and E are confounded with the interaction term of two larger variables.

The estimate of variable E approaches its „true" value with increasing error term. This improvement of aggregate estimation accuracy stems from the fact that, due to error, some reversals of rank are likely to occur between stimuli having similiar underlying utilities. Such stochastic mis-ranking provides additional information and leads to an improved estimate of variable E (Teichert, 1994). The deterministic confounding pattern is overcome by means of stochastic data.

However, limitations to the stochastic improvement are evident. The effect of variable C remains clearly underestimated. This can also be explained by the confounding structure chosen. Since variable C is confounded with the interaction of the dominating variables A and B, reversals due to error are less likely to exert a significant influence on the estimating contrast of C. The stochastic effect is not sufficient to overcome the confounding patterns.

The simulation analysis shows the existence of confounding patterns that were hypothesized based on theoretical assumptions. It demonstrates that some ambiguities can be overcome on an aggregate level by means of the stochastic error. The analysis also indicates that the effect of a variable which is confounded with the interaction term of two dominating variables remains underestimated.

d) Extrapolation to higher-level designs

The underestimation of less important variables is not restricted to two-level designs. Darmon and Rouziès (1994) observed this bias for three-level designs. A detailed analysis of the mathematics behind that would be out of scope of this study, because higher-level designs are much more complex and interaction effects are not as easy to code.

However, the above outlined confounding patterns are directly transferable to subsets of higher-level designs consisting of variables with each two levels. A non-representative review of conjoint-analyses by Schubert (1991) shows that dichotomous variables are included in around half of the observed studies (see table 5). Therefore the findings are applicable for a broad area of conjoint-applications.

Table 5: Frequency of study designs (n=123 conjoint studies, source: Schubert, 1991)

Design type	all variables with 2 levels	majority of variables with 2 levels	minority of variables with 2 levels	majority of variables with 3 levels	majority of variables with more than 3 levels
frequency	17%	16%	11%	32%	24%

Furthermore, higher-level designs can ex-post be treated as two-level designs, if all but two variable levels are equally preferred. Then confounding patterns can occur as if dichotomous levels were used. Such an effect can be supposed in a study on futures contracts (Pocsi et al., 1994): Within this 3^{3-1} study design two variables possessed only two levels with different part-worth values. These variables obtained neglectible importance weights (2.4% and 5.1%). In addition, the conjoint analysis produced even a different rank order than a simultaneously tested self-explicated model. Thus it can be supposed that confounding did occur.

Finally, confounding in higher-level designs may also occur on a case-by-case basis with different variables involved. Those confounding patterns are not easily revealed. However, there are indications for their existence. For example, a complex hybrid conjoint study on the design of country clubs (Toy et al., 1989) revealed an importance weigth of 1% for the variable „recreational facilities", while this variable got 13% in a self-explicated model. Since both models achieved identical results for the remaining six variables, it can be supposed that the highly reduced fractional factorial design confounded this variable.

e) Implications of the confounding patterns revealed

Some implications for marketing research applications may now be drawn. The confounding patterns are indeed relevant for conjoint applications that try to estimate the relative part-worth of effects. They can be harmful, if reliable information on less important effects is desired: this may well be the case in many marketing research applications.

- When brands are compared within a conjoint analysis, the market potential of niche products may be underestimated. This particular bias can be especially relevant for smaller companies trying to enter a market from a niche position.

- When conjoint analysis is used to design new products, a set of possible product features can be evaluated in order to derive R&D targets. From a cost-benefit perspective, it may be beneficial to develop attractive combinations of less expansive design factors. The underestimation of smaller effects may conceal this possibility.

In general, the overestimation of major effect's values may lead to a simplification of responses. This, in turn, may result in the stereotyping of desired products, whereas in reality, USPs could well be gained from variations in features rated second.

The biases are of less relevance for marketing applications focusing on the estimation of major aspects. However, even those estimates are affected, as the importance weigths of the confounded effects are not equally distributed among the larger variables.

C. Possible solutions in order to overcome confounding

a) Usage of orthogonal designs with higher resolution

Confounding of main effects with two-factor interactions does not occur if orthogonal designs with higher resolution are utilized. The confounding patterns outlined avove are thus avoided, and unambiguous estimates of the smaller effects can be attained.

To illustrate this hypothesis, a replication of the simulation was performed with an extended 2^{5-1} design requiring 16 stimuli. This design does not confound main effects with two-factor interactions. The results are presented in table 6. As seen in the table, the degree of distortion of variables C and E diminishes within the extended design.

Table 6: Simulation outcomes of the extended design

	A	B	C	D	E
„true" part-worth	40%	31%	12%	10%	7%
deterministic model	44%	29%	15%	9%	3%
stochastic models:					
5% error term	43%	29%	14%	10%	5%
10% error term	42%	29%	13%	10%	6%
20% error term	41%	30%	12%	10%	7%

It can be concluded that extended designs make possible an unambiguous estimate of the effects of smaller variables. According to the results of the simulation analyses (see D), it seems that it should be sufficient to use compromise designs, which avoid a confounding with the interaction term of the largest expected effects. Other two-factor interactions may remain confounded with main effects, as their confounding effect could be corrected by the stochastic error term.

A significant increase in the number of stimuli, however, is often required to maintain orthogonality. For the design shown, it was necessary to double the number of stimuli.

b) An alternative design scheme

Complex, higher resolution designs are in many cases inapplicable. They may demand the inclusion of unrealistic combinations of variable levels, or they may overload the processing capacity of interviewees with too many stimuli. Therefore, an alternative design scheme is offered, providing good approximations of the effect estimates while avoiding an "explosion" of the design in terms of number of stimuli.

The basic idea is to include additional stimuli in order to resolve the ambiguities. In dealing with metric data, one would add a second experiment, consisting of stimuli that leave the known effects constant and vary only the ambiguous effects (Box, Hunter, Hunter, 1978). A combined estimate is derivable if both the original and the added stimuli are based on orthogonal designs.

Ranking data require a different procedure. Any additional 2^{3-1} design would lead to analogous confounding patterns as outlined in (B). An alternative procedure is thus suggested.

The proposed *alternative design scheme* uses the original main-effect design as basis and combines two evaluation steps: Selected stimuli are added and integrated into the design to overcome the confounding outlined above. In an initial evaluation step, the resulting *extended design* is used to segregate the effect of the originally confounded variable. Second, the *main-effect design* is applied in order to achieve unbiased estimates for the remaining variables. Finally, the outcomes of the extended design and main-effect design are integrated and an estimate is made regarding the entire preference structure.

The following explanations are based on the example used above (see C). A deterministic model is applied to demonstrate the generic calculus.

Preparation of the conjoint-experiment: generation of an extended, non-orthogonal design

The orthogonal main-effect design consists only of a subset of stimuli within the three-dimensional space A,B,C. This is the reason for the confounding pattern of variable C. It is therefore suggested that selected stimuli be added to overcome this deficit. Table 7 shows the set of four possible stimuli.

The additional runs are applied to resolve the ambiguity with respect to A,B, and C. The other variables are, in this respect, of no relevance: their levels are thus arbitrarily balanced. The extended design is used to estimate variable C alone, therefore orthogonality must only be ensured with respect to variable C.

Table 7: Possible stimuli of the alternative design scheme

Stimulus	A	B	C=AB
I, V	-1	-1	1
II, VI	1	-1	-1
III, VII	-1	1	-1
IV, VIII	1	1	1
Additional	stimuli		C = -AB
Add 1	1	-1	1
Add 2	-1	1	1
Add 3	-1	-1	-1
Add 4	1	1	-1

To illustrate the efficiency of this procedure, only two stimuli are added to the orthogonal main-effect design, making a total of ten stimuli. The added ranking stimuli are: (+A,-B,+C,-D,+E); (-A,+B,+C,+D,-E).This subset is selected as variable C is here confronted with the adversary effects of A and B, and is therefore more likely to influence the ranking.

Evaluation of the results

Step 1: Estimate the absolute effect of variable C

The extended design is used to obtain an isolated estimate for the absolute effect of variable C. This is the only information which can be gained from table 8, as C does not correlate with any variable. Estimates for the other effects are biased, since the non-orthogonal design does not balance them. Those estimates are meaningless and as such are not interpreted.

Table 8: Estimation steps of the alternative design scheme

	A	B	C	D	E
"true" part-worth in %	40%	31%	12%	10%	7%
Step 1: Estimation of effect C *					
size of effects	-3,8	-2,6	-1,7	-1,0	-0,2
part-worth in %	41%	28%	18%	11%	2%
Step 2: Estimation of the other effects *					
size of effects	-4,5	-3,5	given	-1,5	0
part-worth in %	n.a.	n.a.	n.a.	n.a.	n.a.
Step 3: Integration of outcomes					
size of effects	-4,5	-3,5	-1,7	-1,5	0
part-worth in %	41%	31%	15%	13%	0%

*) The shadowed fields are biased and are not used for evaluation.

Step 2: Estimate the absolute effects of the other variables

The underlying orthogonal main-effect design is used in a second estimation step to obtain unbiased estimates for the remaining variables. Since the effect of variable C has already been calculated, it can be excluded here. The effect E(C) = - 1,7 implies that any stimulus loses an equivalent of 0,85 ranking points when variable C is added with a positive level, and gains 0,85 ranking points when variable C is added with a negative level.

Accordingly, the single ranks R_i of the stimuli i are adjusted by subtracting the specific effect E(C) of variable C. The resulting ranks R_i^C represent a hypothetical, real-numbered ranking structure of a conjoint-analysis without variable C:

$$R_i^C = R_i - \frac{E(C)}{2} \qquad (7)$$

The effects of the remaining variables are estimated based on this adjusted ranking structure. The results are found in table 8. Still, it is only possible to estimate the absolute size of the effects, since variable C is excluded from the calculation.

Step 3: Integrate outcomes of steps 1 and 2

Finally, the outcomes of the two estimation steps - using both the extended design and the orthogonal main-effect design - are combined. Thus the alternative design scheme derives an estimate for the entire preference function. The absolute effects of all variables are

added, and relative values are calculated according to equation (2). The outcomes are the estimates of the relative part-worths.

c) Comparison of solution schemes

As can be seen in the table 9, the alternative design scheme leads to markly improved estimates for variables A through D than the orthogonal main-effect design. The only flaw in comparison with the extended design is that variable E remains confounded. This, however, was expected, as the additional stimuli did not concern the estimability of variable E. The feasibility of the alternative design scheme has thus been demonstrated.

Table 9: Comparison of simulation outcomes using deterministic models

	A	B	C	D	E	Total absolute deviation *
„true" part-worth	40%	31%	12%	10%	7%	
orthog. main-effect model (8 stimuli)	47%	33%	0%	20%	0%	38%
extended design (16 stimuli)	44%	29%	15%	9%	3%	14%
alternative design proc. (10 stimuli)	41%	31%	15%	13%	0%	14%

*) Total absolute deviation = Sum of absolute deviations of the estimated values from the "true" values.

In summation, a significant improvement in estimation accuracy can be achieved by adding only a few stimuli to the standard orthogonal main-effect design. The rank-specific confounding patterns can be well overcome in a directed manner. Thus, the alternative design scheme should be an useful tool in marketing research applications, which must consider the trade-off between methodological accuracy and practicability.

d) Implementation of the alternative design scheme

Confounding patterns are, in actual applications, neither positively identifiable in advance nor do they necessarily occur in each case, as individual preference functions differ. The first estimation step of the alternative design scheme, however, is only applicable for ex-ante specified and de-facto confounded variables: Usage of the extended design would lead

to biasedness, if the variable expected to be confounded turns out *not* to be so. In this case, the extended, non-orthogonal design may lead to an overestimation of the separately estimated variable.

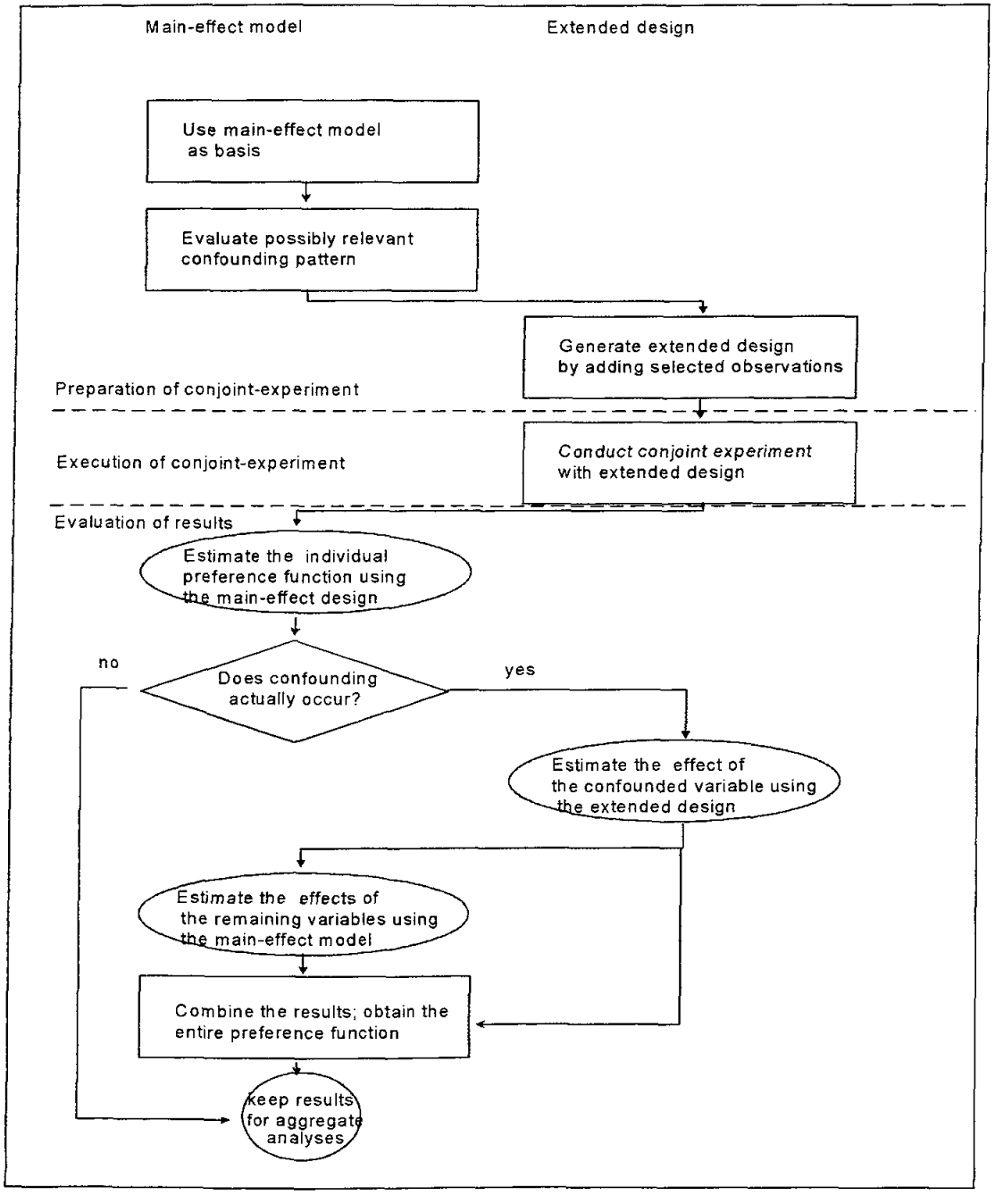


Figure 2: Steps of the alternative design scheme

It is therefore advisable to follow a case-by-case procedure (see figure 2). A simple main-effect model is chosen as the basis. This design is analyzed with regard to the confounding of main-effects with two-factor interactions. Of the confounding patterns revealed, that pattern which includes the interaction term of the variables expected to be largest is the one deemed potentially relevant. A few selected stimuli are added to the main-effect design in order to correct the possible confounding. The resulting extended design is used to conduct the conjoint experiment. Evaluation is performed on an individual level.

For each observation, the necessity of applying the extended design is examined. The individual preference data are evaluated as if they were obtained solely from the underlying main-effect model. Results are then used to determine whether confounding actually did occur. This is the case, if the two predicted dominating variables actually do dominate the entire estimated preference function (see equation (6)).

If examination reveals no confounding, the results of the orthogonal main-effect design are then considered and retained as unbiased effect estimates. The evaluation scheme outlined above (see b) is applied to those observations possessing the confounding that was predicted: the extended, non-orthogonal design is used to estimate the effect of the confounded variable. The adjusted main-effect design is used to estimate the remaining variables. Finally, results are combined to obtain the entire preference function.

If heterogeneous preference functions with different dominating variables are expected, one may add different sets of stimuli simultaneously. If, however, the design approaches the complexity of an orthogonal design with higher resolution, one should forego the alternative approach. In such a case, the advantage of a less complex design is diminished, whereas the information efficiency of orthogonal designs remains superior.

D. Empirical example

In order to examine the efficiency of the design scheme developed here, a large-scale marketing research application is used to apply the calculus. An international study on product preannouncements (Schirm, 1995) serves as the empirical basis. Within this full-profile conjoint study, four variables with two levels each (see table 10) build a full-factorial 2^4 design with 16 stimuli. Potential customers were asked to rank the stimuli according to respective credibility of the preannouncements. The outcomes are used to

assess consumer's attitudes towards non-existent products, and to derive efficient preannouncement strategies.

Table 10: The variables and levels used in a marketing research application

	Technology (variable A)	Announcement (variable B)	Time horizon (variable C)	Manufacturer (variable D)
Level one	minor improvement	brief	1 year	niche player
Level two	major improvement	detailed	4 months	market leader

This study has been selected because it meets optimally the requirements for a meaningful test application. That is, first, the sample is likely to provide a solid basis both in terms of size and of complexity, as it consists of 739 observations leading to 5 distinct clusters of interviewee's responses. Second, its design makes a comparison of models possible, since the full-factorial design can easily be partitioned into orthogonal main-effect designs. Finally, confounding patterns are expected, as the two variables „Technology" and „Announcement" dominate the entire preference function in about one-fourth of all the observations.

The data are used to calculate effect estimates based on three different designs:

1. Full factorial design
2. Main-effect design
3. Alternative design

The outcomes of the full-factorial design serve as substitutes for the unknown "true" preference values. This is reasonable for the purpose of this study, because the focus lies on the internal validity. The outcomes of the full-factorial design can serve as a benchmark for the other designs, since the input data of those designs are subsets of the full-factorial stimuli.

In order to calculate estimates for the other two design schemes, a main-effect model with confounding C = A * B is arbitrarily chosen as a basis. Two stimuli, having complementary confounding, are added for the alternative design. The rankings observed

in the full-factorial design are transformed to rankings of the subdesigns. The evaluation steps are performed as if the study woud be conducted anew (see figure 2).

A summary of the outcomes is provided in table 11. Examination of the whole sample indicates that a slight improvement of estimation accuracy can be gained by applying the alternative design instead of the orthogonal main-effect design: variable C reaches a close approximation of its "true" value. The inferior estimation accuracy of variable A is not to be interpreted in favor of the main-effect design, since it stems from an overestimation of variable A within the confounded subset.

Table 11: Comparison of design schemes

	Entire sample					observations with confounding of C				
	n= 739					n= 211				
Design Type (D)	A Techno -logy	B Announce -ment	C Time horizo n	D Manu- facture r	** Total absolute deviatio n	A Techno -logy	B Announc e-ment	C Time horizo n	D Manu- facturer	** Total absolute deviatio n
AVG. PART-WORTH VALUES in %*										
Full Factorial D.	-48,5%	12,3%	-17,4%	21,9%		-60,7%	13,8%	-13,6%	11,9%	
Main-Effect D.	-48,2%	12,6%	-14,2%	25,0%	6,9%	-65,8%	16,1%	-0,1%	17,7%	26,7%
Alternative D.	-46,0%	12,8%	-16,6%	24,5%	6,4%	-58,5%	17,0%	-8,5%	15,9%	14,5%
AVG. IMPORTANCE WEIGHTS in %										
Full Factorial D.	37,8%	26,2%	20,5%	15,5%		45,6%	33,9%	12,2%	8,3%	
Main-Effect D.	37,3%	24,5%	19,8%	18,4%	5,8%	48,9%	38,3%	0,5%	12,3%	23,3%
Alternative D.	35,7%	24,5%	21,8%	17,9%	7,5%	43,3%	38,6%	7,4%	10,7%	14,1%

*) A "+" indicates that level two is preferred (see table 10 for coding of variable levels).

**) Total absolute deviation = Sum of absolute deviations of the estimated values from the "true" values.

Confounding of the variable C was detected in 211 out of the 739 observations. Focusing on this confounded subset, the effect of the alternative design approach becomes more evident. As expected, the main-effect design is utterly incapable of estimating the confounded variable C (deviations from zero stem from inconsistencies in the ranking structure). At the same time, this leads to an overestimation of the other variables.

The alternative design is able to offset the confounding patterns to a large degree. Identity with the "true" values of the full-factorial design was not to be expected, as the additional

stimuli were not concerned with the variable D, and because the ranking subsets entail inconsistencies.

The picture is less clear when making a comparison of the weights of importance. A more detailed analysis is therefore performed with the help of a frequency table. Its results clearly demonstrate the improvement of estimation accuracy achieved by using the alternative design (see table 12). The main-effect design *incorrectly* classifies the effect „Time horizon" as unimportant (<5% importance value) in 23% of all observations. Applying the alternative design, 18% of the incorrectly classified observations can be regrouped: 8% turn out to possess an average importance of 5-10%, and a further 9% have importance weights of 10-15%. This information is an especially useful outcome of the conjoint analysis, since it is not fairly evident otherwise, and because other survey techniques might conceal this hidden aspect.

The results of the main-effect design would have been misleading in this study if they were used as the basis for a product preannouncement strategy. They would suggest that consumer's attitudes towards the „Time horizon" of a product preannouncement is split almost evenly between those who regard this aspect as highly important and those who neglect this aspect in building their perception of credibility. This would be especially harmful if the subgroup of confounded observations was the targeted market segment: the recommendation for a preannouncement strategy would have been to pay little or no attention to timing aspects.

Table 12: Comparison of the distributions of importance weights of „Time horizon"

n = 739	Frequency of the importance of variable „Time horizon" in % of total observations			
	<5%	<10%	<15%	>15%
Full Factorial Design	9%	17%	21%	53%
Main-Effect design	32%	7%	8%	53%
Alternative design	14%	15%	17%	53%

In sum, a good approximation of the „true" preference values was achieved by using the alternative design scheme. This shows its attractiveness, as only 10, rather than 16, stimuli were used. Reduced complexity of the ranking task should also enhance estimation accuracy, since response quality tends to be better with fewer stimuli (Cattin, Weinberger, 1980).

The main-effect design performed quite well in terms of the estimation of average values of part-worth values. However, detailed analyses - such as clustering techniques - would have led to misclassifications, because nearly one-fourth of all observations exhibited significant deviations from their „true" preference structures. The potential pitfalls of using a main-effect design are thus evident.

E. Conclusions

- Rank-based conjoint analysis is more exposed to confounding than metric data. Confounding of metric data arises only due to the unforeseen existence of interaction effects not taken into consideration in the design's creation (Carmone, Green, 1981). Confounding of ranking data, however, occurs as well in the absense of interaction effects.
- Orthogonality does not guarantee unbiased effect estimates in the case of ranking data. Conjoint analyses based on orthogonal main-effect designs may yield questionable effect estimates on the individual level. Detailed analyses of individual preference structures will then be biased. Smaller effects in particular run the risk of being underestimated.
- Careful choice of a design can aid in avoiding the rank-specific confounding patterns. If it is predicted that two variables dominate the preference model, a design should then be chosen which avoids confounding its interaction term with a main effect. In accordance with the findings of recent research (Kuhfeld, Tobias, Garatt, 1994; Perrey, 1996) it appears advisable to put considerable emphasis on the design choice and not rely uncritically on computerized design generators.
- Marked improvements in estimation accuracy can be achieved using the proposed alternative design scheme. The procedure is recommended when two variables dominate the preference model and its interaction term is confounded

with another main effect. Complex designs are not required to overcome this confounding pattern.

- Ambiguities result from the nature of ranking data itself and can be explained by the underlying confounding patterns. Accordingly, other biases such as the number-of-levels effect may be reexamined in this light (Wittink et al., 1989; Steenkamp, Wittink, 1994). One may suppose that it is not the absolute number of levels which causes the biases, but the underlying confounding patterns. Eventually, the number-of-levels effect may be overcome in a similar way; by adding stimuli to the orthogonal design. This area is clearly in need of further study.

F. Guide to marketing research applications

Experimenters should try to limit the complexity of the ranking task as much as possible, in order to ensure a high degree of response reliability. If at all applicable, the standard main-effect model ought to be used. If this model cannot be applied, the complexity should be restricted to the greatest degree possible, and the lowest acceptable number of stimuli needed should be added.

A sequential model for choosing a design is thus suggested as outlined in figure (3): the experimenter must first of all ask whether accurate part-worth estimates are relevant to the purpose of his study. If he is concerned solely with the estimation of market shares, relevance is rather limited (Darmon, Rouziés, 1994). The standard main-effect model may be used instead, since it has proven its robustness in this regard (Carmone, Green, Jain, 1978).

However, the knowledge of part-worth values that constitute the choice can be relevant for a broad range of questions in product positioning. The experimenter then needs to make a sound decision regarding the design which is most adequate. This choice requires some ex-ante knowledge of the expected preference functions. If no information is available from previous studies, a pretest should be performed.

The experimenter ought to ask whether there are two dominating variables present. If not, and if a simple preference model is expected, the standard main-effect model should be sufficient. If, on the other hand, two dominating variables are indeed expected, a more complex design should then be applied in order to obtain unbiased part-worth estimates.

To reach a final decision of design choice, the experimenter should then assess whether or not interaction effects are relevant. If no relevant interaction effects are predicted, the alternative design should be used. This guarantees a good approximation of effect estimates and avoids unnecessary complicating of the ranking task.

However, interaction effects may occur in particular between the two dominating variables (Louviere, 1988). In such a case, the estimability of the confounded variable deteriorates, since it would be an alias not only of the combination of two effects but also of a separate relevant effect. An orthogonal design with higher resolution would then be needed to balance the interaction effect.

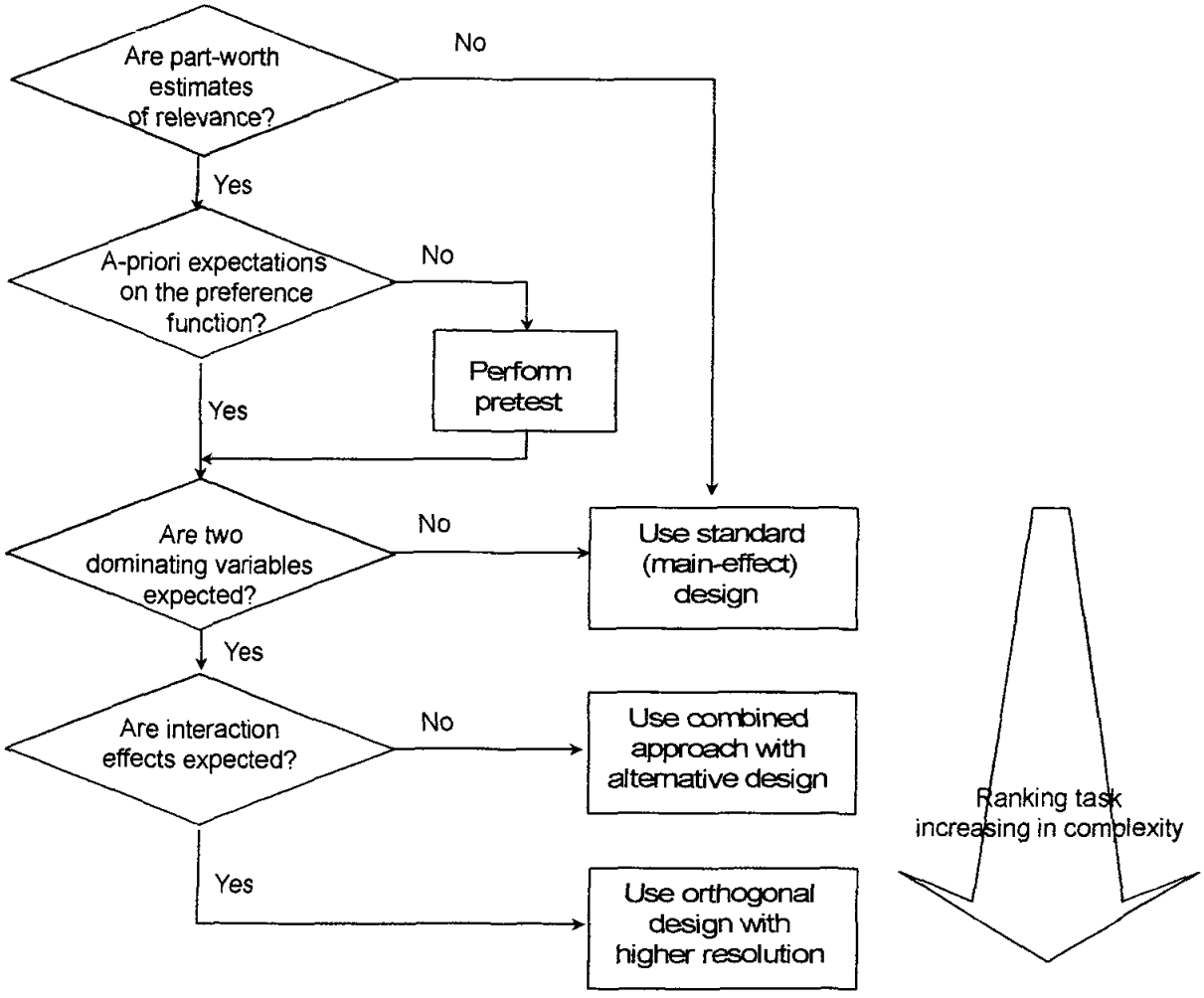


Figure 3: Guide to marketing research applications

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