Author's response to reviews

Title: Configural frequency analysis as a method of determining patients' preferred decision-making roles in dialysis

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Author's response to reviews: see over
Cover letter

In the name of all authors, I would like to thank the reviewers for their very constructive critical remarks as well as for the valuable advices. The recommendations were incorporated in the manuscript, documented in the following revision note and marked in the manuscript by using the track changes mode in MS Word.

We hope that we accurately considered the recommendations. We are available at any time for further inquiry or incorporation of additional changes to the manuscript.

Referee 4

Comment:
1. On page 7 the authors present a number of advantages of CFA. They state that CFA “.. requires only variables on a nominal level …”. I doubt whether this is an advantage. You could formulate in terms of … CFA is suitable for nominal data. Furthermore, the authors state that: Another problem with typal analyses based on correlations (like factor analysis) is that they take into consideration only first-order interactions of the variables”. This statement seems confusing. For example, factor analysis is usually performed on zero-order, bivariate correlations.

Answer: The term “requires only variables on a nominal level” was changed to “is suitable for data…”, as was suggested. It is true that factor analysis is performed in the beginning by means of bivariate and therefore zero-order correlations. But this is not the end result of the approach. We’d add the factor “factor” to the model and receive in the end interactions of the first-order. Please see “Krauth J: Typological Personality Research by Configural Frequency Analysis. Person individ Diff 1985, 6:161-168.”

Comment: 2. On page 9 the authors basically describe the study design. As 187 dialysis centres participate to contact the patients, the type of design is multilevel (level 1: patients; level 2: dialysis centres). In the manuscript the effect of level 2 was not taken into account. I would appreciate if the authors discuss this aspect of the design and possible threatening of valid statistical inferences.

Answer: The discussion now includes a paragraph “limitations”, which addresses the aspects of a multilevel design.

Comment: 3. On page 9 is reported that dialysis units (=centres?) received 14972 questionnaires. As the individual centres did not keep count of how many survey questionnaires had actually distributed to their patients, the response rate could not be calculated. Of the questionnaires distributed 6318 were returned for statistical analysis. But, it might be of relevance to present the “response rates” for each individual centres, based upon the number of questionnaires the 187 dialysis centres received. Do the “response rates” differ for the different centres? What is the median value
(interquartile range, minimum, maximum) of these “response rates”? Possible effects of different centres have to be discussed.

**Answer:**
We agree that it might be of relevance to know the response rates for the individual centres as well as for the overall study population. Unfortunately, we do not know them and there is no possibility to retrieve them. Also the numbers of how many questionnaires each dialysis center received are not helpful, since we do not know, how many surveys had been handed out to the patients and how many had been cast off at the end of December.

**Comment:**
4. On page 10 five predictor variables are mentioned, one of them is age. Age as predictor variable was not discussed in the introduction. I would suggest to describe the rationale that age was considered as predictor variable.

**Answer:**
Age has been introduced in the introduction, please see page 6: “The results of these studies are not quite clear, yet they indicate several tendencies. For instance, various studies were able to confirm that the older the patients were, the less likely they were to prefer an active role [9, 10, 21-24]. In contrast, yet other studies demonstrated that the age of patients had no influence on the patients’ preferred role [12, 25].”

**Comment:**
5. On pages 10-12, the (semi-quantitative) measures were discretized into dichotomisations or trichotomisations. The question arises whether discretising variables induced loss of information. And why they have dichotomized three variables and two variables trichotomised? Furthermore, I don’t understand why the authors have used different ways of discretizing: sometimes the mean, sometimes median, sometimes otherwise. It would be helpful if the authors describes the rationale of different ways of discretising the variables. The (semi-quantitative) measures are clearly non-nominal, while the authors state that one of the advantages of CFA is that the variables are on a nominal level (see Page 7).

**Answer:**
We added some explanations von page 10-11 about the rationale of the categorization. It is true that being able to use data on a nominal level is an advantage of CFA. However, it is also possible to use a composition of different scale levels, which we showed in the present study.

**Comment:**
6. On pages 12-13 CFA is described. As most readers are not statisticians, I would appreciate if the authors give a clear description of CFA in only nontechnical terms (i.e. without statistical terms and without symbols), while a technical description, \ including a clear explanation) is presented in the ”Appendix”. The technical part of the description on Pages 12-13 should be moved to the already presented ”Appendix”.

**Answer:**
The description of the CFA on pages 12-13 was altered, while the more technical description was moved to the appendix, as was suggested.
Comment:
The description of the CFA on pages 12-13 “Analyses” was altered and the descriptions of table 2 were changed likewise.

Comment:
8. On page 14, “Sample characteristics” the authors try to show that the study sample is representative of the overall population. The study sample was compared with the overall population with regard to gender, age, educational level, length of dialysis and Karnofsky index. The question is whether these characteristics are of real relevance. It could be that the relevant factors are psychological of nature. The motives to participate or not might be of great importance, which may introduce selection bias. I think that this aspect should be elaborated in the “Discussion”.

Answer:
We addressed this under the new paragraph “limitations” in the discussion.

Comment:
9. The phrase on Page 15 “There were also no differences observed between the exploratory sample and the confirmatory sample due to the random division of the sample” needs reformulation. I would suggest something like: “As the patients were randomly assigned to either exploratory or confirmatory sample and as the two subsamples were large, it was expected that only differences due to random error would be found. These results were in line with this expectation.”

Answer:
The paragraph was altered, according to the suggestion.

Comment:
10. In Table 2, on Page 31, I believe that the codes j1 to j6 don’t have any additional value. Verbal explanation of the codes of the six variables is sufficient. In addition, the authors may consider to present the real percentages (instead of the p-values or add to the p-values).

Answer:
This was changed according to the suggestions, please see answer to comment 7.

Comment:
11. On page 18 the algebraic notation of the predictor-criterion configurations do not “read” easily. I prefer a statement in nontechnical terms. Regarding the “Discussion” section, the length of it may be shortened.

Answer:
We considered changing the algebraic notations in nontechnical terms. However, since we are already explaining the meaning of the algebraic notations subsequent to their statement, we decided that a second explanation would not be necessary. The discussion was shortened.
Referee 2

Comment:
1. Background, page 7-8: The authors provide a rather long description of the statistical method Configural frequency analysis. The section could be shortened a bit, and some of the content could be part of the discussion section instead. Further, it ought to be described that the method identifies both higher frequencies than expected (types) as well as lower frequencies (antitypes), even though only types are in focus in this study.

Answer:
The description of the CFA on pages 7-8 was shortened and a description of antitypes was added to the manuscript.

Comment:
2. The study aim could be stated more clearly. Make a precise statement of the aim. The use of the term “attempt” makes it quite vague. “The present study will attempt to predict the preferred decision-making roles of German patients with end-stage renal disease (ESRD) through the comprehensive application of sociodemographic and psychosocial factors”. The fact that only types are investigated and not antitypes could perhaps be stressed.

Answer:
The statement of the study aim was altered.

Comment:
3. The methods section could be structured more clearly and perhaps more logically. Description of the study design should precede sample and setting, followed by instrument/measures, data collection and data analyses. Now, the headings are used in a bit confusing order, and sometimes blended, e.g. sampling and data collection.

Answer:
The order of the paragraphs in the method section was changed.

Comment:
4. Study design: the section describes more about data analyses and how the data material was handled than about the design of the study. As I see it, a cross sectional design was used. It could be discussed whether a cross sectional design could be used to identify predictors and predict a specific outcome. However, I realize that the attempt to divide the material in two parts is a way to try to overcome that barrier. The method discussion section lacks a discussion concerning this.

Answer:
A discussion of the cross sectional design was added to the “limitations” of this study.

Comment:
5. Study design: Figure 1 does not show any textual information, just squares and arrows, at least in the version that I have been reviewing.
Answer:
Figure 1 was uploaded again.

Comment:
6. The sample selection is somewhat unclear to me and the description in the manuscript makes me confused. To me, it is unclear how “the patients of the exploratory and the confirmatory samples (n=1969; n=1914)” were selected from the group of 6318 responses?

Answer:
For CFA we could only use those questionnaires for the analyses with absolutely no missing values. Therefore we had to disregard the responses with missing values and could only include 3883 patients (n=1969; n=1914) without missing values, as we described in the sample characteristics.

Comment:
7. The text under the heading “Sampling and data collection” comprises information regarding other things, e.g. the instrument and ethical issues. Example: “In the fourth quarter of 2005, questions concerning participation in treatment decision-making were included in the QiN health-related quality of life survey. Ethical approval for this survey was obtained by the Ethical Committee Board of the University Hospital of Cologne.”

Answer:
The information under “Sampling and data collection” was altered.

Comment:
8. Predictor variables, page 11: motivation for the use of median for dichotomization of values for desire of information, and mean values for trust? Why not the same method? Distribution of the variable?

Answer:
We extended the explanation of the data handling for the predictor variables.

Comment:
9. Predictor variable, page 11: Unclear description of how the third predictor variable, perceived involvement by the physician, was dichotomized. Two, out of three items, measured on a 5 point Likert scale should be answered in the same direction to identify either positive or negative. How did you decide whether it was positive or negative direction? Where did the mid alternative fall on the dichotomized scale?

Answer:
We extended the explanation of the data handling for the predictor variables. Since we were interested in clear statements, we decided, that only categories 1 and 2, which were clearly in favour of patient involvement had to be regarded as a positive answer. Category 3, which was either “perhaps” or “sometimes”, was to be regarded as a negative statement along with category 4 and 5.
Comment:
10. Configural frequency analysis, page 12: A very good description of the Configure frequency analysis. However, I think the section could be shortened a bit. I understand the authors wish to give incentives for their choice of statistical method, but the reader do not need all these detailed information.

Answer:
The description of the CFA on pages 12-13 was altered, while the more technical description was moved to the appendix (as was suggested by reviewer 1).

Comment:
11. Results page 14: Lack of statistical measurements to back up the statements that the patients in the exploratory and the confirmatory groups differed from the overall sample regarding age, educational level, need for information, and regarding gender? Further, it is unclear to me how the exploratory and the confirmatory groups were selected and I do not understand the comparison of them with the overall study sample. They must be a part of the overall sample, but apparently not half of the sample (1969 + 1914 = 3383).

Answer:
For CFA we could only use those questionnaires for the analyses with absolutely no missing values. Therefore we had to disregard the responses with missing values and could only include 3883 patients (n=1969; n=1914) without missing values, as we described in the sample characteristics.
In a next step, we divided the sample (of 3883) randomly into two subsamples. By chance, we then obtained one subsample with 1969 patients and one subsample with 1914 patients (please see “Study Design”).
To make sure, that these subsamples (with no missing values) were not biased, we compared them with the overall sample.

Comment:
12. Results, page 14: How come there is a difference between groups of 7 % regarding need for information if the division of the sample were done randomly?

Answer:
There is no difference in regard to the need for information between the subsamples, exploratory and confirmatory sample (please see table 1). However, there is a higher need for information (7%) within the subsamples (n=1969; n=1914; altogether n=3883) than within the overall sample of 6318. This might be due to the fact that the subsamples only include questionnaires with no missing values.

Comment:
13. Results, page 15: You could give a description in the text, apart from in table 1, of the patients’ participation preferences.

Answer:
We are describing the patients’ participation preferences at the end of the results section.
Comment:
14. Results, page 16: It would be of interest to know how many patients constituting type I, II, III, and IV. You could see it in table 4 in the appendix, but it should deserve to be visible in the article.

Answer:
We added those numbers into table 3 so that they are now visible in the article.

Comment:
15. Results, page 16: “Furthermore, it is predicted that patients with a high desire for information prefer to play an active role……..”. The result is that information, in combination with other attributes, is a predictor, but not in itself. The section is in need of re-wording.

Answer:
The section was altered.

Comment:
16. Discussion, page 17: The conclusion stated at the end of page 17, that a physician can have a direct influence on the treatment results of his patients by making simple changes in his behavior is not supported by the result of this study.

Answer:
The conclusion was removed from the manuscript.

Comment:
17. Discussion, page 18: how come only one variable was described here as “marginally higher” in the subsamples? In the beginning of the result several variables were mentioned. However, without measures to support the statement of a significant difference. The text on the middle section of page 18 is more like a result, than a discussion of the study findings in the light of previous knowledge in the field.

Answer:
As we pointed out in “Sample characteristics”, we did not regard e.g. an age difference of 1 year within a sample of altogether 3883 patients as restrictive to the validity of the sample. Therefore we only paid special attention to the difference of need for information (7%).

Comment:
18. Discussion: It would be good to have a clearer discussion section about the method, e.g. regarding the sampling procedure, choice of cross sectional design in relation to prediction. Some of the content under the heading Conclusion is more about methodological issues.

Answer:
Some of the content of the conclusion section was moved to the discussion section.
Comment:
19. Conclusion: the section lacks a clear conclusion. The text is more a discussion about methodological issues, and future studies.

Answer:
The content of the conclusion section was altered.

Comment:
20. Table 1, page 30: unclear to me how the groups low and high information seeking preferences can be about 39% and 61% respectively - As I understood it, a median dichotomization was used. How come such differences? (this goes for the variable trust in the physician as well). This question is connected with the problems of understanding how the subgroups exploratory and confirmatory samples.

Answer:
In a first step, the scores of every single patient were categorized by the median, mean or by the content of the items (please see section method). Therefore, every patient was now categorized as belonging to the group of patients with e.g. low or high information seeking preference. In a second step, we calculated a statistic of the variable information seeking preference, with the result that then e.g. 39% of the patients have been categorized into low information seeking preference.

Comment:
21. Background page 7-8: The rather long list of previous areas of studies using CFA could be summarized and shortened.

Answer:
We considered this point. However, we found it of particular importance to demonstrate the broad variety of possible applications of this method.

Comment:
22. The last section on page 9 describing types of decision making seems to be misplaced.

Answer:
We agree that the types of decision making are not a perfect match for the section “Sampling and data collection”. However, we did not want to produce a new section for the types of decision making and in doing so prolong unnecessarily this article. Therefore we chose the section we felt would fit the best for demonstrating types of decision making.

Comment:
23. Predictor variables, page 11: in this section the terms “variables” and “attributes” are used. Should benefit from a unified use of terms.

Answer:
The term “attribute” was changed against “variable” in this section.
Comment:
24. page 14 at the bottom: “However, because of their minimal occurrence, these characteristics are not regarded as restrictive to the validity of the samples”. Do you mean representativity?

Answer:
By comparing the subsamples to the overall samples, we tried to exclude selection bias. During the selection step of a research study, if an unequal number of subjects have similar subject-related variables there is a threat to the (internal) validity. Therefore, since the occurrence of such similar subject-related variables has been minimal, we did not regard this as restrictive to the (internal) validity of the samples.

Comment:
25. The use of the term confirmatory sample is changed to inferential sample in table 2 and 4.

Answer:
The term “inferential sample” was changed into “confirmatory sample” in the tables and in the manuscript.

Comment:
26. Discussion section, page 16: the description of “passive types” and “active types” could be better described as patients preferring a passive role or an active role. It sounds better than “passive types”.

Answer:
We agree that passive types might not sound very appealing to all readers. However, with CFA we intend to introduce a new typal analysis to the field of SDM research. For this reason, we decided to hold on to the “active- and passive type” terms.

Comment:
27. Why be so imprecise in the text regarding figures of the sample of 6318 patients, e.g. page 18 line 3: 6000; page 14 line 15: 6000. That counts also for the figure 15,000 patients described on page 8 which is

Answer:
The numbers have been changed.

Comment:
28. Why is not table 4 part of the result section? Isn’t it of interest to know the proportions of the subgroups, at least for the groups that constitutes the types. The frequencies of the types could be added in the head of table 3.

Answer:
We prepared a more elaborate description of the method including a calculation example for which the numbers of table 4 are needed for the additional file, as was suggested by other reviewers. The frequencies of the types have been added in the head of table 3.
Referee 3

Comment:
1. Perhaps my chief concern was that although the abstract spoke of four patient types, I really understood there to be two types dependent on preference for active roles, and two types dependent on trust of the physicians. I recognize that adds up to four types, but I came away thinking that there are two essential variables: preference for active/passive and trust/distrust.

Answer:
It is true that there are two types preferring an active role and two types preferring a passive role. However, only all predictors in combination with each other are able to predict a specific type (active, passive or SDM). Therefore we cannot mingle the predictors and the outcome of the prediction analysis (prediction types). All we could do is, to discuss the importance of a single predictor to the model, which we did by discussing the importance of informational desire of the patients.

Comment:
2. I am not a “figure-type of person” or a statistician. My manuscript may have been faulty, but figure 1. was largely blank, and figure two seemed more like a table.

Answer:
Figure 1 has been uploaded again.

Comment:
3. Many dialysis patients are blind or have cognitive incapacity. I wasn’t clear from the manuscript how this was managed.

Answer:
The patients which took part in this survey showed a Karnofsky Index of 74 (mean). This means that they are on the brink of a status where they are able to carry on normal activity and to work and need no special care. Unfortunately, we have no information about the handling of the patients with lower scores, which might be more impaired, since the dialysis units did not keep records.

Comment:
4. The authors acknowledged that they were limited by not being able to determine the nonparticipants. Despite the acknowledgement of the limitation, this is a big problem for the study design.

Answer:
We agree that it might be of relevance to know the response rates for the individual centres as well as for the overall study population. Unfortunately, we do not know them and there is no possibility to retrieve them. Also the numbers of how many questionnaires each dialysis center received are not helpful, since we do not know, how many surveys had been handed out to the patients and how many had been cast off at the end of December.
Comment:
5. I was pleased to see the review of the few somewhat similar studies that have been performed in the UK and USA. The literature on trust was placed later in the manuscript, and my preference would be that all of the literature review appear together more in the beginning.

Answer:
We started the “Background” section with a paragraph addressing “Patients’ preferences for involvement in treatment decisions”. In this paragraph we first explained the concept of “Shared decision-making” and then addressed the latest literature. When we described numerous studies, which have examined the desired involvement of patients in making treatment decisions using univariate or multivariate analyses in an attempt to identify determinants of the preference for active participation in treatment decision making, we listed the literature for all predictor variables we were going to use in our study (e.g. age, educational level, need for information, and trust). Therefore we find the literature reviews on trust as being placed at the best position in this section.

Comment:
6. The writing overall was extremely clear and lucid. I had no difficulty following the logic, method, and findings.
I was struck in the list of decisions that the authors chose to cite that hemodialysis patients face they did not include many of the most important: whether to continue or discontinue dialysis, whether to shift to palliative care, whether to repeatedly agree to ICU and other admissions, whether to repeatedly agree (especially if they have diabetes) to amputations, etc.

Answer:
It is true that there are usually several decisions that have to be made during hemodialysis. However, our patients showed a Karnofsky Index of 74 (mean). Therefore, they are on the brink of a status where they are able to carry on normal activity and to work and need no special care. Decisions, like repeatedly ICU admissions or palliative care are of no concern to our patients.

Comment:
7. The discussion of trust seemed somewhat murky to me, as this is a questionnaire that is given at a single time in the course of treatment and trust is a factor that I suspect varies with time and according to the clinical experiences and outcomes.

Answer:
We added the following sentences to the limitations: “The questionnaire was given at a single time in the course of treatment while trust is a factor that is assumed to vary with time and according to the clinical experiences and outcomes. However, the assessment of trust in our study is based on long-term experiences of patients. All patients that took part in the survey have been under long-term treatment in the participating facilities. Most of them were able to establish a stable relationship with their physician over time which is based more or less on trust depending on the character of the relationship.
Referee 1

Comment:
1. It is stated in the discussion that "a physician maybe in a position to have a positive influence on the trust of the patient if the physician was to undergo a simple change of behaviour". Whilst there is a simplicity in what is being suggested, I feel the reality is far from simple in terms of influencing physicians to make such changes and clearly identifying what the nature of such changes should be.

Answer:
The conclusion was removed from the manuscript.

Comment:
2. The discussion pays little attention to the potential study weaknesses and issues of generalisability. Whilst there is some coverage of these issues in the "conclusions", I feel it would benefit the paper if these could be moved to the discussion and perhaps be expanded on.

Answer:
We addressed the potential study weaknesses in a new paragraph “Limitations” in the Discussion section.