Author's response to reviews

Title: Striking variations in consultation rates with general practice reveal family influence

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Author's response to reviews: see over
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Dr. Chrissie Kouremenou
Assistant Editor
BMC - Family Practice

Ref: MS 2261674121062106 – Low and high attendance at general practice reveal family influence

Dear Chrissie Kouremenou,
Thank you very much for considering our paper for publication in BMC-Family Practice. The comments of the reviewers helped us to improve our paper. We now have revised the paper related to the items raised by the reviewers; all changes can be found in the new PDF of our paper. Please find below a detailed reaction on all comments.

**Reviewer: Kevin Bennett**

1. The reviewer would like a clearer description of the factors that can help general practitioners to identify a patient as being in one of the families mentioned in the paper. We now described those factors more clearly in the results and conclusions of the abstract, and in the discussion and conclusion of the paper. See e.g. in the abstract the result section that has been edited: “These factors include circumstances such as their economic status and number of children, as well as socialisation conditions such as specific health knowledge and family beliefs. The chance of significant low frequencies of contact due to family influences increases significantly with factors such as, paid employment of parents in the health care sector, low expectations of general practitioners’ care for minor ailments and a western cultural background.” And the conclusion of the paper has been changed into: “Next to the family health status, shared circumstances and socialisation play an important role. Such factors as a high income, larger family size, specific health knowledge, health beliefs, and expectations of care, explain part of the differences in contact frequencies between families with low and families with high consultation rates, due to a family effect. Specific knowledge and/or networks in the health care sector seem more influential than the educational level of the parents.” (Page 10, top). Furthermore, we added three references related to how general practitioners can get informed about the family context in the consultation room: “To do so, easy and feasible questions have been developed for use in consultations to enable the general practitioner to be informed about the family context of individual patients (2;3;27).” (Page 9, middle).

2. In fact, the paper had been edited by a native English speaker before submission. To increase its appeal, again, we asked a native English speaker to edit the paper. Hopefully, the second editing has substantially improved the readability of the paper.

3. Page 3: The reviewer suggests resetting the paper as a discussion of social factors that are of influence on consultation rates. We followed his suggestion and organised the background of the paper more around family factors that are of influence on consultation rates (page 3). Also in the discussion section we organised the text more around family factors of importance (Page 8-9).

4. Page 3: We added the reference in which the hypothesis about parents of larger families having less time to monitor the health of the family members was studied: (15).
5. Page 4: The reviewer asks for more detailed information related to the dataset. We understand that this part of the method section is confusing, since the methods and population size of the various parts of the Dutch National Survey differ. In the method section the reader is now informed about the final study sample of this study on page 4 (Methods, Dependent variables): “This resulted in a study population of 42,397 families.” Also on page 4 (Methods, data) the reader is now informed about the total population of the Dutch survey before selecting the families with children, the percentages of response, the representativeness of the responders, and the way the additional survey was conducted. In addition, we added in the method section that the 96 clinics represent 2% of all Dutch general practices (page 4, near the top). For clarity purposes we inserted a Table in our paper in which the dataset and the final study sample are included in more detail (Table 1, page 13). In Table 1, for example, we specify the total N, for both families and individuals, and that the random sample of 5,313 individuals that responded to the health interview has been aggregated to families. In the Table we also describe the number of missing values (incomplete information) for each variable. For example, it is made clear that related to the educational level of the parents more than 12,000 families were missing. The missings are also discussed in the discussion section of the paper (page 9): “Information about the type of insurance, the educational level of the parents and the cultural background of the parents was missing in about 20% of the families. Such incomplete information about the educational level of the parents and cultural background, occurred significantly more often in the families with strikingly low consultation rates.”

6. Page 4: The reviewer asks about possible bias introduced by defining the families. We made a selection of families with children up to the age of 21. The results of the study are valid for these families. We do not see what potential bias the definition of our study sample may have introduced. In fact, we excluded children of families (not families!) below the age of 2 just to prevent bias caused by babies attending a baby clinic in stead of general practice. This has been described clearer in the text on page 4: “Families with children below the age of 2 or beyond the age of 21 were not excluded. Only the children in the specific age categories were excluded.” In addition, the remark about elderly parents living with adult children may have caused the confusion, and has therefore been removed.

7. Page 4: The term ‘generation’ is explained on page 5 (middle): “being a child or parent” and the meaning of ‘practice context’ is also explained on page 5: “The latter not only captures the influence of general practitioners, but also the geographical contexts of families, because almost all patients live in close proximity to the practice.” Furthermore, the analysis plan was not clear enough. We rewrote the part about the multilevel analysis; we now describe more extensively the unit of analysis and what variables were used. The graph has been omitted (see in reply to reviewer 2). In spite of the suggestion of the reviewer we decided to keep the part with regard to defining the families at its current place (page 4). The reason for this is that the realisation of the three groups of families is not considered to be a central part of the analyses presented in the result section. Rather, it was a necessary step to calculate the dependent variables before the analyses could be performed. In the plan of analyses (page 6-7) we present the analyses that lead to the results as described extensively in the result section.

8. Page 5: Initially, we referred to health needs in accordance with the wording of Andersen, since his article was referred to in the introduction. We agree with the
reviewer that health needs and health status are not interchangeable. Since we measure health status, we now use “health status” throughout the article.

9. Page 5: Insurance type has two levels that proved to be a reliable proxy for social economic status. In addition, also the educational level of the parents has been used as a proxy for social status and income. This is described more clearly (page 6, near the top).

10. Page 5: References have been added linking mothers’ employment and time constraints (page 6, near the top), and relating the number of children to time constraints and experience (page 6, near the top). Two references have been added linking cultural background with health beliefs and life-style: 22 and 23 (page 6, middle).

11. Page 5: The sentence “…. were used as proxies for the outcome of socialization.” has been changed into: “The study design was cross-sectional, which makes it impossible to focus on the process of socialisation. Rather, we evaluated socialisation conditions including: the health knowledge of the parents; family health beliefs, such as expectations of general practice care in case of minor ailments; trust in health care; and performance of self-care activities (Table 1).” (now page 6)

12. Page 6: The meaning of a high score on the Nijmegen Expectation Questionnaire is explained more extensively on page 6: “A higher score denotes less belief in the benefits of consulting general practice for common ailments, which means that a person will probably not attend general practice for those complaints.”

13. Page 6: The reviewer is right in saying that it is better not to omit variables from the analyses just because they are not significant. Indeed we had to restrict the number of variables due to sample size. To be more sure we didn’t introduce bias, we performed separate analyses with the omitted variables corrected for the family health status to evaluate whether this would result in different outcomes. We repeated the analyses with the variables on educational level of both parents and paid employment of the mother. Only the educational level of the father was significant. However, when introducing this variable in the final analyses, it leads to similar results as presented in Table 5. This issue is discussed in the discussion section now (limitations of the study, page 9).

14. Page 16-17: The sample size is given above the tables. The samples in Table 4 and 5 indicate the number of families that were included in all steps of the analyses.

15. Page 7: The analyses were performed in three steps “…. to differentiate between the influence of health status, family circumstances and socialisation conditions.” The educational level of the father looses its significance when introducing socialisation conditions (table 4), and we wanted to show this.

16. Page 7: On page 4 we now describe how many families are listed in the same practice. The fact that children up to the age of 2 also attend baby clinics doesn’t have a negative impact on our results because we excluded them from the analyses to prevent bias (see also under 6.).

17. Page 7-8: As stated before (see also under 1 and 3), the discussion section has been reorganised around social factors and in the conclusion the main message of the paper is reworded.

18. We agree with the reviewer that the limitations of the study need to be described more extensively. We followed this suggestion and added limitations related to the use of proxy measures, the questioning of the health interview, the elimination of non-significant variables in table 5 and the issue of missing data (page 9, limitations of the study).

19. The last paragraph of the conclusion has been deleted.
Reviewer Heidi-Ingrid Maaroos
To a large extent, the comments of this reviewer are in accordance with those of the other reviewer. If applicable, we mention this in the text and refer to the explanations related to the comments of the other reviewer.

1. The title has been changed as suggested by this reviewer.
2. The abstract has been redesigned; especially the background, results and conclusions, after the changes in the article were made.
3. Background: references have been added in the text (page 3), the text is restructured around family factors (see also under 3 other reviewer) and the study question has been reformulated: “To what extent can the family context explain differences between both strikingly high, and low, consultation rates among families with children aged up to 21?” (Page 3-4).
4. Methods: also to this reviewer the study sample and methods were not clear. Hopefully, by adding Table 1 and the changes in the text as described before (reviewer 1, point 5) we have clarified this part of the paper. The inappropriate title “definition of families” has been changed by “Dependant variables: realisation of three groups of families”.
5. Page 4-5: we now described more extensively how the expected frequencies were calculated in the method section, e.g.: “In the present study we used a multi level poisson regression model with three levels, individual, family and practice, to define three groups of families based on their family effects(17). This family effect cannot be measured directly; therefore we extracted it from the frequency of contacts of individual family members.” (page 5) […] “Individual frequencies were predicted on the basis of a parent or child’s age and sex, and controlled for clustering of individuals in families and families in practices.” (page 5)
6. Discussion: The sentences that strictly speaken do not belong to a discussion initially were used to refresh the design of the study to the reader. As suggested by the reviewer, they now have been removed. Also the last paragraph of the discussion section has been deleted. Further, references have been added to compare the results of our study with other studies. Reference to Tables 2 and 4 was also thought to add to the readability of the paper, but are now deleted as suggested.
7. The conclusion has been changed and now stresses the central findings of the paper.
8. Figure 1 has been deleted since it added little extra information.
9. The headings of the columns for the second part of table 1 have been added.
10. Related to the references that regard thesis (now 21 and 24): unfortunately, it is impossible to change this with published papers, because there are not any.

We hope that with the changes made all questions have been answered.
Yours sincerely, also on behalf of the co-authors,

Mieke Cardol,
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