Author's response to reviews

Title: Exposure to and experiences with a computerized decision support intervention in primary care: results from a process evaluation

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Author's response to reviews: see over
Dear editor,

Thank you for considering our manuscript ‘Process evaluation of a computerized decision support intervention to improve quality of primary care’ (revised name: Exposure to and experiences with a computerized decision support intervention in primary care: results from a process evaluation) as having the potential to be published in BMC Family Practice.

We are grateful to both the editor and the reviewers for their time and efforts in assessing our manuscript and appreciate their constructive comments. We have addressed all comments in the revised manuscript and we think it has improved by integrating them. As requested, we have included a list of point-by-point responses to the editor’s and reviewers’ concerns.

We hope you will find our revised manuscript acceptable for publication and look forward to hearing from you.

Yours sincerely,

On behalf of all authors,

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Responses to Editor's comments:

You have received mostly positive reviews. However the reviewers made some specific recommendation which would make your manuscript better. Please address all the points mentioned by the reviewers with point by point response letter and improve your manuscript accordingly. We also ask that you address the following editorial request:

1. Requesting trial registration where clinical trials are described (TRN):
We notice that you are reporting a clinical trial but have not cited a trial registration number. This must be obtained before we can begin peer review of your manuscript. We would like you to confirm that your clinical trial is in a publicly accessible registry before we begin peer review. The trial registration number should be included as the last line of the abstract of the manuscript.

   Response: Our clinical trial is indeed available in a publicly accessible registry with the following registration number: NCT01773057. We have included this information in the last line of the abstract of the manuscript (p.3; line 61).

2. Please provide an English language version of the questionnaire as an additional file.

   Response: We have translated the relevant parts of the Dutch questionnaire into English and have attached this version as an additional file (see Additional File 1). We have also referred to it in the manuscript as Additional File 1 (see p.9; line 209).

3. Please remove the ethics approval as an additional file.

   Response: We have removed the ethics approval as requested.
Response to reviewers’ comments

Reviewer 1: Kathrin Schlößler

Reviewer's report:
Dear editors and authors,

I recommend publication in general (positive remarks 1+2). However I have one major issue (discussion of study sample;3) and some minor or discretionary comments (4-12).

1. Research question, study design and relevant conclusion
The research question is of interest and relevant to the readers of BMC Family Medicine. The authors analyze exposure to and experiences with a computerized decision support system (CDSS). To analyze the “active ingredients” of a complex intervention, process evaluation is essential1. The results can contribute to explain the somewhat disappointing results of the negative effect on main outcome variables in the evaluation study (C-RCT)2,3. Furthermore this study adds a nice example of a triangulated approach4, using log-data combined with an empirical and theory-based self-developed questionnaire, to the body of implementation research. The authors draw clear conclusions which are relevant for the implementation of CDSS in study-settings as well as in daily practice.

2. Intervention and control group, statistical analysis
The intervention- and control-group are well defined according to the main study (C-RCT). However the difference in the intervention group (one additional module, “Heart failure”) compared to the 9 basic modules in the control group does not seem relevant considering exposure to and experiences with the CDSS. Consequently the authors do not compare these variables between groups, but focus on process evaluation within the whole sample of a representative subgroup of the main study and conduct only descriptive statistics (positive remark).

Major Compulsory Revisions:
3. Discussion of study sample against the background of both data sources
The authors discuss their findings on exposure to and experience (attitude /barriers) with a CDSS against the background of previous qualitative results and actual literature. I agree with their view of the strengths of the study and the timing of the process evaluation as a possible limitation. However a thorough discussion of the study population itself is missing [Major concern]. As the sample group of the two approaches is not identical selection bias may have occurred in the self-selected questionnaire-sample.

Response: We agree with the reviewer that a thorough discussion of the study population is missing and have therefore included it in the Discussion section of the manuscript by adding the following text: “However, whereas the samples of the two approaches used (questionnaire, log-data from NHGDoc-server) were quite comparable in terms of background characteristics, selection bias may have occurred with respect to use and attitudes in the self-selected questionnaire. It may well be that responding PCPs used NHGDoc more often and had more positive attitudes towards NHGDoc compared to non-responders. The difference between reported use of NHGDoc in the questionnaire and data from the NHGDoc server point in that direction as well. This should be taken into account in interpreting our findings” (p.21; line 499-506).

In this context I consider one additional limitation. As the authors mention in the discussion section, (paragraph 8) one strength of the study is its use of multiple methods. Such a triangulation can in fact improve the validity of the results mentioned. But therefore both approaches should be related to each other: In the result section (Paragraph 5 “NHGDoc server”) the authors cite the proportion of practices that used personalized functions of NHGDoc. According to this aspect other variables should be available, e.g. estimated frequency of use of basic function. I suggest “completing the picture” by reporting and discussing the log-data related to self-reported variables mentioned in table 2.
Response: With respect to the additional limitation we agree with the reviewer that, in order to improve the validity of the results of our study using multiple methods, it is important to relate both approaches to each other. We have now elaborated on this issue in the Discussion section (p.21; line 499-506). However, we do not have additional NHGDoc log-data available. Whereas the available log-data on alerts are already presented in the manuscript, data regarding the use of the feedback function is not available other than from the survey. Moreover, in the log data we were not able to distinguish between general practitioners and practice nurses (only between individual users) as we were in the survey.

**Minor Essential Revisions**

4. Table 3: In the method section on data collection (Paragraph 7 “perceived barriers”) 6 knowledge-related barriers are mentioned. However in Table 3 only five are displayed while the table is otherwise complete. So this might cause confusion -Which item was left out and why?

Response: We had indeed left one item on knowledge out of Table 3 as this item referred to being aware of the existence of the system NHGDoc as a whole. However, to complete the picture we now have added this item to Table 3 (p.31) and also referred to it in the Results section (p. 16; line 379-380).

5. Figure 1: As I understand, figure 1 shows the number of requests (absolute number) per week. Contradictory, the text on the y-axis states numbers of request per week. Which approach is correct?

6. Figure 1: In addition some explaining text (eg. Q=quartal; X-Axis: Time) could be added for a better understanding.

Response to 5/6: We agree with the reviewer that this may be confusing and have changed Figure 1 to include Time (weeks) on the X-Axis and Number of requests on the Y-axis. (see Figure 1_revised). Furthermore, we have added Q = quarter at the bottom of Figure 1 (p.28).

7. Figure 1: The reported number of requests per week (about 300) seems relatively low keeping in mind that 537 GPs and 225 PN use the program within their consultation of 9 (10 respectively) common concerns. According to the text I assumed that an automatic request is sent each time data of patients with one of the complaints is entered into the electronic health record system. Maybe I misunderstood this part - Please clarify!

Response: We agree with the reviewer that the number of 300 requests send per week was indeed relatively low. However, in interpreting this number it is important to realize that the different domains/disease areas do not result in different alerts each. Rather, the advices are combined in one alert, consisting of an alert window with different tab pages for all relevant domains/disease areas.

8. Figure 2: The headline of the figure states "Figure 1" while it seems to be Figure 2 according to the text.

Response: We do not see a headline stating Figure 1 where Figure 2 was stated in the text. We therefore assume this mistake has arisen during converting the original manuscript and figures into a reviewer version by the editorial office.

9. Figure 2: In the method-section on data collection (Paragraph 6 “Experience”) seven statements were mentioned to assess the general attitude towards CDSS. However in figure 1 eight statements are included while I would only name the first 4 and number 6 "statements of general attitude" while the other three statements seem to refer rather to perceived barriers. Please clarify!

Response: We indeed used 8 (rather than 7) statements to measure the general attitude towards CDSSs. We have corrected this in the Methods section (p.10; line 238). Furthermore, we agree with the reviewer that some of the statements may also refer to barriers to using
CDSSs in general but we believe these may be part of the general attitude towards working with CDSSs. The 8 statements were derived from literature combined with results from our previously conducted focus group study (p.10; line 238-240).

Discretionary Revision
10. The authors conducted their study on a sample of 231 practices (of 1100 Practices in the NL in total). However I could not fully understand how the "comprehensive recruitment plan" was actually carried out. Additionally a study-flow chart would be helpful e.g. as an appendix with supplementary material.

Response: We agree with the reviewer that readers are unable to fully understand how the comprehensive recruitment plan was carried out. We have therefore added some explaining in the text (p.7; line 153-157), while also referring to our protocol paper (Lugtenberg et al., 2014, Implementation Science) in which the recruitment plan is described in more detail.

11. The potential of log-data can be used more extensive. Depending on the kind of log-data information on time spend by reading alerts or actually working with the program could be investigated. It would also be interesting, which alerts have been read by GPs or NP respectively (e.g. alerts on data registration and drug prescription respectively). A nice example of broad use of log data might be the paper by Hirsch et al. (2012).5

Response: We agree with the reviewer that it would be interesting to report on more log-data such as time spend by reading (different types of) alerts. However, we only have data on whether alerts were opened and not on whether they were actually read and the time spent on doing that. Also, our data do not distinguish in types of users (GPs, PNs). We therefore believe we have reported on all available and relevant data in the manuscript.

12. As the CDSS had the possibility to directly provide feedback to NHGDoc (the organization that has done the development) further information about these comments would be interesting-Although this option was rarely used as reported by the authors. This feedback could be appraised with content analysis and reveal valuable suggestions to address implementation and intervention failure from a broader range of users. Maybe no new aspects compared to the earlier qualitative analysis were found here. However, the authors could mention and discuss this third data source.

Response: We agree with the reviewer on the value of the feedback data to be used as an additional source. Unfortunately we do not have these comments included in our database.

Literature
4) O’Cathain, Alicia; Murphy, Elizabeth; Nicholl, Jon (2010): Three techniques for integrating data in mixed methods studies. In: BMJ 341, S. c4587. DOI:10.1136/bmj.c4587.

Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests: I declare that I have no competing interests.
Reviewer 2: Carlo Piccinni

Reviewer’s report:
Lugtenberg et al. have performed a study on exposure to and experience with a Dutch CDDS among general practices (GPs and NPs). The study was aimed to evaluate the impact of an intervention (alerts within CDSS concerning heart failure) by analysing differences between an exposed group (general practices using CDSS with these alerts) and a control group (general practices using CDSS without these alerts). Moreover the study collected information on experience with this CDSS in order to describe its usage and possible barriers. This study derives from a cluster randomised trial on the same CDSS. Although data presented in this manuscript are not “exciting”, the study appears interesting and the discussed topic need to be study in deep to better design the future CDSSs. Therefore, I have some major and minors suggestions to improve the manuscript.

Major Compulsory Revisions
1. Title: the study was not designed to improve the “quality of primary care” (as reported in the presented title). I suggest to focus the title on the level of use and barriers in the use of CDSS in primary care.

Response: Not this study but the computerized decision support intervention was designed to improve the quality of primary care. We agree with the reviewer, however, that the title may be confusing and have therefore changed it into: ‘Exposure to and experiences with a computerized decision support intervention in primary care: results from a process evaluation’. (p. 1; line 5-7).

2. Results (general): in this section there are few results on the effect of the intervention on the two compared groups.

Response: Indeed, we have not presented results on the effect of the intervention on the two groups. Reason for this is that this process evaluation was conducted alongside the trial to gain insight into the factors responsible for the intervention’s impact rather than focusing on the impact of the intervention as such. Preliminary results on the (disappointing) effects of the interventions have been described elsewhere as stated in the Background section of our manuscript (p.5; line 101-102). In describing the results on exposure to and experiences with the interventions we did not compare the two groups either, as the difference in the intervention group (one additional module on 'Heart failure’) compared to the basic modules in the control group, does not seem relevant for the process evaluation. Consequently, we focus on a process evaluation within the whole sample and only distinguish between GPs and PNs wherever possible.

3. Discussion (general): Whereas a detailed discussion on the low level of use and possible barriers in the use of CDSS is presented, little importance is given to the findings on the effect of the intervention.

Response: We agree with the reviewer that in this study we have given limited importance to the findings on the effect of the intervention for reasons stated above (see comment no 2). However, we agree with the reviewer that it is important to report on the lack of changes in outcomes in the Discussion section, in order to adequately interpret the findings of our process evaluation. We have therefore reported and reflected on the lack of changes in outcomes in the Discussion section (p.18; line 420-421/p. 22; line 519-528).

4. Discussion (general): In this section the authors should better emphasize the importance of their study on the developing of the future CDSSs on the basis of their results. Moreover, they should propose strategies to improve the use of CDSS among general practices.

5. Discussion (general): The authors should clarify the importance of their study on other CDSS (operating in other countries) different from that used in the analyse.
Response 4 and 5: We agree with the reviewer on the importance of our study for the development of future CDSSs and have therefore emphasized this and proposed several strategies to improve the use of CDSSs in general practices in the Discussion section (p.20; line 466-469 + line 480-483 + p. 21; 491-494 + p.22; line 508-528). In doing this, we have also reflected on the importance of our study with respect to other types of CDSSs (p.20; line 463-466 + p.22; line 511-517).

Minor Essential Revisions
1. Abstract (page 2 lines 32-34): the study aim appears vague; the authors should rephrase the sentence in the abstract, specifying their purpose (as reported at page 5 lines 111-112).

Response: We have rephrased the aim of our study in the abstract (p.2; line 32-34) to be in line with the study aim as reported at the end of the Background section.

2. Abstract (page 2 lines 45): please include the crude figures beside the percentages

Response: we have added the crude figures besides the percentages in the abstract (p.2; line 45).

3. In different point of the manuscript the authors cite their unpublished (only submitted) article (see ref n. 15). Since, at this time, this work is not available, authors should provide more information on the analysed CDSS, and the used methodology.

Response: We agree with the reviewer that, since our focus group article has not yet been published, we should provide some more information on this study. We have therefore added relevant information on the used methods and main results of our focus group study to the Methods section of our manuscript (p.10; line 224-234).

4. Methods: the sentence at page 8 lines 178-179 appears in contrast with the sentence at page 7 lines 153-154. Please rephrase to better explain what general practices did not know about the intervention.

Response: We understand that the reviewer believes that the two sentences are in contrast with one another. However, general practices were indeed randomly assigned to either the control arm or the intervention arm (p.7), while at the same time being blinded to group allocation and unaware of participating in an intervention trial with concurrent control group (p.8). The small difference between the intervention group (one additional module on 'Heart failure') compared to the basic modules in the control group, made this possible. General practices were only aware of participating in a NHGDoc evaluation study and were aware of the fact that data were collected from their electronic health record systems for the course of a year, but did not know they had been randomly allocated in a trial.

5. Results (Table 1): In tab 1 are reported 3 different groups of responders (Practices, GPs and PNs). It is not clear if the first group is the sum of the other two. The authors should rearrange the content of this table to better describe their results and consequently they should change the title of this table.

Response: We have now rearranged the content of Table 1 and changed the title accordingly into: Table 1: Characteristics of the responding GPs and PNs and their practices compared to the total study population (p.28).

Level of interest: An article whose findings are important to those with closely related research interests.
Quality of written English: Acceptable.
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests: I declare that I have no competing interests.
Reviewer 3: Yinsheng Zhang

Reviewer's report

This paper introduced a process evaluation carried out for a specific CDSS. The paper used strict evaluation method and analyzed the factors affecting CDSS.

Minor Essential Revisions
# Line 100: Why "admission" can be used to assess the effectiveness of CDSS.

Do the authors mean "readmission"?

Response: We do in fact mean 'hospital admissions’ rather than readmissions as outcome measures of the effectiveness of the CDSS, as our patient group is being treated by primary care practitioners and most of them have not (yet) been admitted to a hospital.

# Following lines have some grammar mistakes.

Line 128: change to "NHGDoc Basic functions" or "Basic functions of NHGDoc"

Line 137: change to "NHGDoc Personalization functions" or "Personalization functions of NHGDoc"

Response: We have changed the titles of the paragraph into "Basic functions of NHGDoc" (p.6; line 128) as well as into "Personalization functions of NHGDoc" (p.6; line 137).

Discretionary Revisions
# In this paper, most collected data are from questionnaires. More actual usage data from system logs could be more preferred.

Response: Although we do agree with the reviewer that most data are derived from a questionnaire, we believe that one of the strengths of our study is that we actually combined questionnaire data with data from the NHGDoc server (log-data). We have presented all available and relevant log-data in the manuscript.

#In keyword part, "implementation" may be too vague as a keyword.

Response: We have deleted implementation as a key word as we agree with the reviewer that it is quite vague (p.3; line 63).

Level of interest: An article whose findings are important to those with closely related research interests
Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests: I declare that I have no competing interests.