Author's response to reviews

Title: Perceived connections between information and communication technology use and mental symptoms among young adults

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

Please find a revised version of our manuscript *Perceived connections between information and communication technology use and mental symptoms among young adults – a qualitative study* submitted for publication in BMC Public Health. We have carefully responded and revised the manuscript according to the valuable comments from Editors and Referees. The modifications are presented point-by-point below. Unless mentioned in the text, the Referee to whose comments we respond to is in brackets.

**Title**

1. The abbreviation ICT has been spelled out: information and communication technology (Editors).

2. “– A qualitative study” has been added to the title (Referee 1).

**Abstract**

1. The specific aim has been replaced by the overall aim, which means that the model now is presented in the aim. (Referee 1)

2. Referee 2 suggested that we add something in terms of implications for theory and practice. To address this, we have modified the conclusions (and added an implication to the “Implication for further studies” section).

**Introduction**

1. Referee 2 points out the need to connect the current study to earlier research. We had deliberately kept the background section limited and chosen to elaborate more in the discussion section by discussing the results in relation to earlier research. Furthermore, a more comprehensive background to the field was done in an earlier article (Thomée et al, 2007), where e.g. technostress (Arnetz & Wiholm, 1997) was referred to. However, we understand the relevance of introducing our study in the light of earlier work, and have added a short summary of earlier research. This summary has been kept rather slim.

**Methods**

1. The ethical approval has been added in the beginning of the Methods section, together with participants’ written consent (which earlier was noted in the Data collection section). (Editors)

2. Clarification of the study group and inclusion process (Referees 1 and 2).
Referee 1 suggested that the section describing the variables that make up the inclusion criteria be deleted. This has not been done, since we find these necessary to define the group. However, we have tried to clarify the inclusion process with the intention that it be easier to follow. Furthermore, the section describing the mental symptom items have been moved to become footnotes to Table: Study demographics.

Both Referee 1 and 2 questioned the relevance of tables 1-3 that describe the cohort exposure in 2004 and prevalence of symptoms in 2005. These were originally added to provide background information about the larger cohort that was the basis for inclusion to the present study. It seems that both Referees find this background information irrelevant. We have addressed this issue by deleting tables 1-3.

Referee 2 specifically questioned why table 1 and 2 presented information about ICT use by gender, while the current study does not present results by gender. The reason for presenting tables 1 and 2 by gender, was that the subjects (16 women and 16 men) were included on basis of having the highest ranking reports of ICT use by gender (while reporting mental symptoms in 2005); and with the tables we could present the values that make up for example an “upper quartile exposure” for a man or a women, respectively. We have now added some of this information instead as footnotes to former table 4, now table 1: Study demographics.

3. Referee 2 questions the lack of information about prevalence of symptoms in 2004. We are not completely sure if we understand the comments, but if we assume that the comments relate to how we present study group demographics, we will try to clarify: The inclusion criteria high exposure prior to reporting mental symptoms, was chosen to mimic an earlier study group in a prospective study where we found associations between ICT use at baseline and mental symptoms at 1-year follow-up (Thomée et al, 2007) done on the cohort in 2002 and 2003. In the current study, however, we did not exclude those with symptoms at baseline, one reason being that we know little about what time span could be relevant connecting ICT exposure and possible effects on mental symptoms. Even though we can see a point in presenting the prevalence of symptoms for the study group in 2004, we find it unnecessary since it would hardly add to the results of the interviews.

4. We have added information to Table 1 Study demographics (former table 4). Besides the above mentioned ICT use upper quartile values, we have also added if the subjects had reported two or three symptoms (which has been deleted from the methods part). We have also added foot notes about the mental symptom items (and hence removed from the text).

5. We have modified the Analysis section, by clarifying the analytical steps and references, and adding one reference concerning qualitative content analysis. We consider the analytic method to be quite straight forward; mainly a summary and description of the manifest content of the subjects’ dictums, using only a low level of interpretation to generate categories on a more abstract level. (Referee 2)

Results

1. Referee 1 suggested that the results could be written more compress, but also commented that the results were well described. Referee 2 referred to the result section as easy to
follow and did not comment the length of the text. We have chosen not to modify the
results text up until the last sentence of the section A model of possible..., (with the
exception of the minor correction of adding “the” to line 4 of Results).

2. Referee 2 pointed out that we discuss possible positive effects of ICT in the results, but that
this is not reflected in the model. This issue had been addressed by adding that possible
positive effects are not reflected in the model to the description of the model (in the last
sentence under “A model of possible...”). The reason for not adding positive effects to the
model was that the main focus of the model concerns the perceived associations between
ICT exposure and mental symptoms (ill health). Questions were not asked explicitly about
possible positive effects, so these comments were mere by-catch and cannot be seen as
fairly reflecting the topic, yet we want to mention that these comments were made.

Discussion

1. The first paragraph of the discussion has been re-written in parts. Referee 2 pointed out that
we should relate our model to general psychosocial stress models. To address this issue we
have related the concept of demands in our model to the models of “demand-control”
(Karasek-Theorell) and “effort-reward imbalance” (Siegrist). Furthermore, the concept of
demands originating from the individual’s own aspirations (intrinsic demands) has been
related to overcommitment (Siegrist) and performance-based self-esteem (Hallsten).

2. We have added a paragraph concerning research on “techno-stress” as suggested by
Referee 2.

Implication for further studies

1. Line 6-7, the reference to tables has been deleted (Referee 1) and a gender difference in
prevalence of symptoms is mentioned (and referenced).

2. An implication for designing interventions and intervention studies is added (last sentence).

Conclusions

1. Referee 2 suggested that we add something in terms of implications for theory and practice.
To address this, we have modified the conclusions, and also added an implication to the
“Implication for further studies” section.

References

1. We have added 6 references necessary to address the comments from the Referees.

2. We have added “[In Swedish]” or “[In Swedish and parts in English]” to references 1, 2, 4 and
55. All have English titles and some have summaries in English.

3. Former reference 7 has been replaced by a reference in English (ref 63). (Referee 1).
All authors have read and approved the revised manuscript. We hope that we have addressed the comments and that the manuscript now is acceptable.

Yours sincerely

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