Author's response to reviews

Title: The socio-demographic patterning of sexual risk behaviour: a survey of young men in Finland and Estonia

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Version: 2 Date: 27 May 2009

Author's response to reviews:

Andrea Bucceri PhD
Scientific Editor

On behalf of my co-authors, I submit the enclosed revised manuscript for consideration by BMC Public Health. The reviewer comments have been addressed below with numbered point-by-point response to the concerns.

As suggested, a native English speaking colleague has copyedited the paper.

Sincerely
Minna Nikula

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Title: The socio-demographic patterning of sexual risk behaviour: a survey of young men in Finland and Estonia
Version: 1 Date: 20 April 2009
Reviewer: Bozicevic I

Reviewer’s report:
ABSTRACT
Background: please add the age range of men included in the surveys
1. (18-25 years) - has been added in the text; page 2, para 1.

INTRODUCTION
• Page 2. Last paragraph. Please add the reference to the statement “In general, sexual risk behaviour has been studied… than men”.

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INTRODUCTION
• Page 2. Last paragraph. Please add the reference to the statement “In general, sexual risk behaviour has been studied… than men”.
2. The sentence has been clarified and the reference added, page 3, last para.

METHODS

• Page 6. 1st paragraph. Please clarify the statement that 15% of the cohort does not enter training. Could this bias the results, and if so, in what way?

3. This has been modified in the methods section to make it clearer, page 4, par 3. In the discussion, we provide available information/characteristics of the 15% exempted from the training (as a part of the limitations) and discuss the possible implications to the study, page 9, para1.

Please state separately what the response rate in Estonia was, and how many data were missing (or had other problems that made them unsuitable for the analysis)

4. The sentence with Estonian response rate has been modified and stated separately, page 5. In addition the number of unsuitable data for the analysis is now indicated, page 5, para 3.

• Page 6, 1st paragraph. Please specify how sexually active was defined. The sentence starting with “Only sexually active respondent were included... STIs” is not clear.

5. The definition of sexually active respondents (=ever engaged in sexual intercourse) has been added in the text where it is mentioned for the first time (methods section, page 5, para 3) and additionally included in the "Additional file 1" where definitions of all the other variables are presented.

RESULTS

• Table 1. It is not necessary to show two samples for Finland. If the aim of the paper is to compare the Finnish and the Estonian sample, then the Finnish sample of 18-25 years old should be excluded.

6. Since men participate in the military training only once in their lifetime, the age group 18-25 is representative of the entire group (85%) of young men (born between 1980-1987) who join the military training in Finland. If we leave this age group out the Finnish sample looses it's representatively. Thus we wanted to show the two age groups in the table 1. See also second reviewer, point 2.

Table 1. For STIs, please add “self-reported STIs, ever”

7. Self-reported STIs, ever, has been added to the Table 1.

• Page 7. Para 3. Please describe what is meant by “partner was an unknown person”. Is this a casual partner?

8. Clarification of the question related to this variable is provided in the "Additional file 1" page 1-2. "Unknown" was used as an indicator assuming that it is more difficult to negotiate condom use when the sex partner is an "unknown"
person. We think that casual partner does not necessarily need to be an "unknown person" to the respondent, thus "unknown" refers more rigorously to someone that was not known to the respondent previously.

• Page 7, para 4: are all differences non-significant?
9. Yes they are. A word "all" has been added to the sentence, page 7.

• Table 2, title: is the age range 18-25, or 19-25? For the purpose of comparison, age 18 should be excluded from the Finnish sample.
10. The title in tables 2-4 has been modified. See also point 6, para 5.

• Table 2: Is the table showing one logistic regression model, or separate adjustment as described in the footnotes? If the later, the presentation of the table is wrong and it does not fit into what is described in the text. The same is for the Tables 3 and 4.
11. This has been clarified in the footnotes of tables 2-4

• Table 2: In the Finnish sample, only being in the age group 20-25 increased the odds of reporting >6 partners in life. From the table, it is not clear that age as a variable is statistically significantly associated with the outcome.
12. We used confidence intervals to indicate statistically significant findings rather than p-values.

• Table 2. Does the relationship status refer to the current status?
13. Yes it does. This has been clarified in the footnote of tables 2-4.

• Table 2 and 3: background factors should be changed to selected socio-demographic and sexual behavior variables
14. This change has been made in all tables.

• P-values for all the variables should be shown in the table, in case it is claimed that the variables (on not the categories of the variables) are significantly associated with the outcomes.
15. We used confidence intervals instead of p-values to indicate the level of significance in the associations.

• For the sample of the Russian ethnic group, age is not significantly associated, and should not be interpreted as positive association
16. We have clarified this in the text, page 7, para 1. See also next point 17.

• In the Results, describe findings that are relevant i.e. statistically significant as a reader can see all the results in the tables
17. Since the study is focusing on comparing trends of associations in two
countries (context analysis rather than unit analysis), we think that also non-significant findings with similar direction (increasing or decreasing) of odds ratios between the two countries/ethnic groups, thus confirmative, can be important. We have checked that all statistically significant findings are clearly mentioned (as being statistically significant) and presented separately from findings with similar trends but which are non-significant.

• Does the model in the Table 4 include as the outcome variable reporting ever having STIs? If so, the variable “unprotected last intercourse” does not fit into the model

18. Although, we are aware that ever STI may have occurred earlier than unprotected last intercourse, we considered that condom use behaviour in the recent past can be a proxy of an attitude towards condom use in general, since our study cohort is relatively young (most been sexually active less than ten years).

DISCUSSION

• Page 9, 3rd sentence of the Discussion: please re-write it as it is unclear. Point out here which factors were significantly associated with the outcome

19. This part of discussion has been re-written, page 8, para 3.

Page 9, 3 para. Is there any evidence in Finland that young men are not willing to participate in such surveys? In other countries, young men were successfully recruited in surveys on sexual behaviour.

20. This was said in the context of comparing young men to young women. This is now been clarified in page 8, para 4. The reference for this information has been also added.

The authors should discuss the bias related to self-reporting of STIs.

21. This has been added to the discussion section, page 9, para 2.

One of the recommendations should be to consider testing on Chlamydia from non-invasive specimens (urine) as part of such surveys. In some other countries (UK, Slovenia), it was found that chlamydia prevalence was higher in young men than in young women.

22. A suggestion for further research related to this has been added to the discussion section, page 12, last para.

The association of early sexual intercourse (<15) came out as a strong correlate of unprotected sex intercourse in Finland. As it is a factor amenable to change, it should be discussed from the point of STI prevention and sexual health promotion. It also came out as predictor of multiple partnership in both countries. The authors should discuss the relevance of these findings for sexual health
promotion in Finland and Estonia, and describe briefly in the Introductory part of
the manuscript the current advantages and disadvantages of programmes on
reproductive and sexual health in Estonia and Finland

23. We do consider that this is a valid and important point, when this indicator is
used to describe the exposure time for possible negative sexual health
outcomes. However, we choose this indicator together with the
socio-demographic indicators because it is known to correlate with for example,
early drop out of school, family disruption and disadvantage, and further on with
later sexual risk behaviour and adverse sexual health outcomes. In this context,
because age, education and relationship status differed mostly between the
countries, they were given a higher priority in the discussion.

Reviewer's report
Title: The socio-demographic patterning of sexual risk behaviour: a survey of
young men in Finland and Estonia
Version: 1 Date: 11 May 2009
Reviewer: Kristi Rüütel

Reviewer's report:
Authors refer in the methods section that a more detailed description of the
surveys has been presented previously. Yet I'd suggest elaborating the methods
section more because currently the description of sampling methods does not
provide good enough understanding of the sample. The authors state that in
Finland approximately 85% of all men in each age cohort enter into military
service, while less than 10% opt for non-military service and they consider the
high response rate in Finland as one of the strengths of the study. At the same
time they state that the majority of those entering into military service are 18–19
years old and from the additional file 2 it can be further seen that 60% of the
participants in Finland were 18–19 years old. Therefore the differences in the
age-groups between the samples in Finland and Estonia are major – in Estonia
only 16% were 18–19 years old and the age ranges are more evenly distributed.
Because of these reasons it remains unclear why the age adjustment was
performed for the Estonian data using Finnish men as a reference population? In
this type of study I would use age adjustment for Finnish and Estonian men
separately – for example adjusting the age of the Estonian participants to the to
the total sample.

1. The two age adjustment could have been done equally well by using either
Finnish men or both (Finnish and Estonian men) as a reference population,
however, we chose to conducted the analysis by using the Finnish data as a
reference population. See also next point 2.

The main results are very reasonable and affirm our current knowledge (unprotected intercourse in a steady relationship is logical – people in steady relationships may use other means to prevent unwanted pregnancies). The likelihood of self-reported STIs increased statistically significantly in the older age – older people have been exposed longer, and life-time prevalence can be higher in older age groups). How about data from Finland where only 10% of participants were aged 21 years and older? How representative do the authors consider the young men who enter military service later than others to be compared to the rest of young men in their age-group? The limitations for Estonian sample are somewhat better described compared to the Finnish sample.

2. Men aged 21 years and older are not representative of their own age cohort (21-25 years), however, since men participate in the military training only once in their lifetime, the age group 18-25 is representative of the entire group (85%) of young men (born between 1980-1987) who join the military training in Finland. Education is the mayor determinant for the timing (age) when men (18-29 years) enter the military training. Since we have adjusted for education, the age comparing inside the group is valid. We have added clarification of this to the methods section, page 4, para3.

The article needs some language corrections before being published. Some of the terms used by authors may not be universally understood (for example „study brigade”).

3. The word "brigade" has been clarified in the text where is mentioned for the first time, page 4, para 4. Additionally, the language has been checked by our organization’s (THL) English language editor, who is a native English language speaker.

Table 1 is confusing. If all Estonians aged 19–25 years report 3% lifetime STI prevalence (crude) then how Estonian E and Estonian R separately report respectively 7% and 8%? The same for “sexually active”, and “condom use, last intercourse”.

4. The age-adjusted STI prevalence in Estonia was 5% (not 3%, which was the Finnish figure) for the entire Estonian sample. The 7% and 8% correspond to the crude estimates in Estonia. The age-adjustment by the Finnish age distribution reduced the figure down to 5%.

Table 2 – would the results be different if multiple life-time partners were checked for all four factors simultaneously – age, education, relationship status and first
sex<15 (in case all these were included in the same regression model)

5. This has been clarified in the footnotes, tables 2-4

Table 4 – some data seems to be missing.

6. Data was not missing, but some numbers were placed in the wrong row. This has been corrected in table 4.

From supplementary file it seems as if in Estonia people who had not started sexual life yet were categorized under “other type of partnership”. Was it so?

7. See clarification for the categorization below. The analysis itself was conducted for those sexually active and in some cases for all respondents as indicated in the methods section.

In Estonia the question was:

"What best describes your partnership status during the past 4 weeks?"

a) I live with my steady partner,
b) I have a steady partner, who does not live with me,
c) I have a non-steady partner/s,
d) I do not have a steady, or a non-steady partner and
e) I have not started sexual life yet.

Categorized;

i) steady partner = lives or does not live with a steady partner and - (refers to a and b)

ii) non-steady partner = others. -(refers to c,d,e)

In the current wording the following two statements seem somewhat contradictory with regards to the “sexual relationship outside of steady dating”:

1) However, surprisingly, in Estonia, relationship status did not correlate with the reporting of multiple lifetime-partners. One explanation for our finding could be the higher tolerance for casual sexual contacts outside a long-term partnership, which has also been previously observed in Estonia

2) Estonians were less likely to approve of a sexual relationship outside of steady dating, that their average age of sexual debut was 1–1.5 years older than in Finland, that the average number of lifetime partners was lower.

8. The first refers to reported behaviour and the second to reported attitudes; according to Haavio-Mannila and Kontula, the first one refers to their finding on respondents reporting casual sex/parallel relationships more often in Estonia than in Finland. The second one refers to their finding on differences in attitudes. However, this may confuse the reader and we have changed the wording, page 10, para2.
Discretionary Revisions

In the introduction authors say that “Estonian HIV prevalence is estimated to be 1.3%, which is over ten times the Finnish rate of 0.1%. The youth aged under 25 have increasingly been affect by Chlamydia in Finland and by HIV in Estonia”. One could argue that the HIV-epidemic in Estonia is driven by injection drug use and even though the first wave of the bridging population – sexual partners of injecting drug users – are increasingly affected, I would not compare HIV-prevalence rates in Estonia and in Finland in the context of general population sexual behavior. This is a subject authors could further elaborate on in discussion section focusing on possible risks for wider HIV-spread from risk groups to general heterosexual population.

9. We have clarified the difference in the nature of the HIV epidemics in the two countries (background, page 3, para 2) Additionally the possible interaction of the epidemic in the two countries, besides the differences in their evolvement, has been clarified in the discussion, page 12, para 2.

Even though it is not possible to analyze it for Estonian data it would be interesting to know about Finland whether the condom use at last intercourse was different based on the type of the partner. The fact that a person is in a steady relationship does not mean they had last sex with steady partner and therefore knowing whether the condom use patterns were different not only based on relationship status but also based on the type of last partner would help us to understand the possible risks. Finland also had a question about prostitute as a last partner. As authors discuss about potential links in the epidemiological development of STIs because of the geographical closeness and increasing cross-border traffic between the two countries it would be good to provide data to support these arguments.

10. We conducted this analysis (by partner type) and it showed a very similar pattern to what was found by type of relationship, thus we used only relationship status as a determinant. In relation to the question on prostitute as a last partner, the number of respondents in this age cohort who reported having had sex with a prostitute were a few, thus the data does not provide enough evidence to elaborate on this important point further. However, we have added this as a specific point to be considered in future research, page 12, para 3.

The question of sex between men should not be left out. Nowadays many young men are engaging in sex with men which increases their risk for HIV and sexually transmitted infections and therefore I’d recommend analyzing data also based on
sexual orientation.

11. This is a very important point; however, sex between men was not asked in the Finnish questionnaire, so we were unable to use this variable.

The issue on ethnicity as a possible factor for risk behaviors is an interesting one. Yet most of the Russian speaking men aged 19–25 in Estonia have been born in Estonia and therefore it is not very surprising that there are no major differences between two ethnic groups. Ethnicity can even be a confounder because considering Estonia’s historical background it can be very significantly linked to educational and employment related issues.

12. This is an interesting point; however, including ethnicity in a regression model would have complicated the cross-national approach and thus we chose the current presentation.