Author's response to reviews

Title: Family joint activities in a cross-national perspective

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Version: 2 Date: 15 February 2007

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

First of all we would like to express our thanks to the reviewers for the first evaluation of our article. We have revised the manuscript once again in accordance with the provided comments very seriously and carefully. Please find our responses to the reviewers’ comments below.

Respectfully,

Authors

To reviewer HELEN SWEETING (Version: 1 Date: 13 November 2006)

Dear Dr. Helen Sweeting,

We are grateful to you for deep analysis and useful suggestions on how to improve the article. Most of your comments were taken into account. Below are our responses to all your comments.

Reviewer’s report and authors’ responses (AR):

General

This paper uses data from 13 and 15 year olds in 6 different countries, collected as part of the 2001-2 WHO HBSC study. It examines between-country differences in levels of joint activities, and also differences according to age, gender, SES and various aspects of family structure.

BMC Public Health asks referees to consider the following general points:

1. Is the question posed by the authors new and well defined? I think the question of cross-national differences may have been considered before, although this kind of comparative data is unusual. The question is clear, but I think the rationale could be stronger (see further comments below).

2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work? I general I would say yes, in fact some of the description may be too detailed. However, I wonder if some of the authors’ further speculations could also be answered by the data, by including further interactions in the models or looking separately by country (again, see below).

3. Are the data sound and well controlled? Yes, I think so.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition? Again, yes.
5. Are the discussion and conclusions well balanced and adequately supported by the data? Yes.  
6. Do the title and abstract accurately convey what has been found? Yes.  
7. Is the writing acceptable? The writing is generally clear, but, since English is not the authors’ first 
language, some editorial input would be useful.

Referees are also asked to recommend:  
Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached). I think the authors should definitely improve the rationale for comparisons and discuss the effects of an index created via factor analysis rather than simply adding the frequencies. I think the paper would also be strengthened by removing the histograms but conducting separate analyses for each activity, and from country-by-country analyses – but this is somewhere between major and minor revisions.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct). I think most of my other comments fall under this heading.

Comments

Please note that the manuscript does not have numbered pages, but to make my comments easier to follow I have numbered my copy, starting with the title page (so Abstract = p.2, Introduction begins top of p.3, Methods begin bottom of p.4, Results begin p.8, Discussion begins p.11, References begin top of p.16).

Introduction

p.3, 1\textsuperscript{st} para – perhaps the statements re activities contributing to each member’s well-being, and enhancing communication, should be referenced.

\textit{AR: Done.}

p.3, 2\textsuperscript{nd} para – I think the authors should acknowledge the existence of a possible 3\textsuperscript{rd} variable (e.g. SES or individual personal characteristics) which might contribute, at least in part to the relationship between family time and possible outcomes. A unidirectional model of the ‘positive effect’ of family time on outcomes is probably too simple.

\textit{AR: We agree with you that relation between the family time and its outcome is complex rather than unidirectional. The statements included in this para were taken as conclusions of other studies. In regard to your remark this para was re-written.}

p.4 – I don’t think a strong enough case is made for the joint-national comparison. It might be helped if there was a link to literature on cross-national / cultural differences in family life and teenage behaviour. For example, the UK Institute of Public Policy Research has recently produced comparative figures (‘Freedom’s Orphans’ – http://www.ippr.org/). Similarly, there is no real rationale for the examination of relationships between family time and SES or aspects of family structure.

\textit{AR: Thank you for the good reference. The rationale of our study was re-written.}

Methods

p.5 – I think it would be worth including response rates in each country – although I assume that since the study was schools-based, these would all be high.

\textit{AR: In ‘Sample’ subchapter we have added the sentence ‘For all participating countries response rates were over 90%’.}

p.6. 4\textsuperscript{th} line – ‘the first two or three categories’ – seems a bit vague.

\textit{AR: This phrase was deleted.}

p.6 – are these the SPSS variable names (‘PARENTS’, ‘INDEX’)? I do not think they need including in this form.
AR: in the paper these names were used as abbreviations, e.g. in Table 3-5.

p.7 – some of the very detailed descriptions are probably not needed. Also, I wasn’t clear about the construction of the family time index – presumably this was simply the result of including all 8 variables into the ‘factor’ procedure and requesting a single factor, and then saving that single factor? Was only one factor extracted without this constraint? It is also possible that different factor structures might have been obtained if each country had been analysed separately.

AR: Yes, you are right. The simple ‘factor’ procedure for a single factor was applied. Description of this statistical analysis method was shortened. Of course, the ‘factor’ procedure might have been applied for each country and, moreover, for separate groups ‘country’ X ‘gender’ X ‘age’ in order to analyse and/or compare factor structures in these groups. It might be quite interesting analysis but the present paper was not aimed for that analysis. In the present study ‘factor’ analysis was carried out in the total data set in order to compare factor values between countries as well as between gender and age subgroups

Results

p.8 / Figures – I wonder if the histograms are needed? This is very detailed. An alternative would have been to examine mean scores for each item by gender, age and country, so allowing the identification of significant differences. As it is, the authors are ‘eyeballing’ the histograms and have to make statements such as ‘it seems that…’. I wonder if the reporting of these results was done with a knowledge of the results in respect of the factor index, in which Spain is highest on the overall index presumably reflects that fact that different individual activities have different weightings on this index, since it is a factor rather than a simple sum of the 8 frequencies. I think this methodological point is worth some discussion.

AR: We have retained the figures. The second reviewer considered these figures as positive value of the article. also was not against figures. Table(s) of mean scores for each item by gender, age and country as an alternative solution appeared too cumbersome and did not save a space. The data of this subchapter serve to overview response rates and to generate hypotheses for further analysis. For this purposes ‘eyeballing’ method is quite appropriate. Assessment of statistical significance always is good but presentation of p values on graphs (or in tables) might overcrowd them if different kinds of p values are calculated. The subchapter that describes the figures and the corresponding discussion were corrected slightly.

p.9 – I don’t think we need the mean and sd of the index values – these are, by definition, 0 and 1.

AR: This information was removed into Methods.

p.10, 2nd para – sentence beginning ‘Because positive values …’ does not make sense.

AR: The para, were this sentence was, was re-written totally.

p.10-11 / Tables 3-4 – these tables are very detailed – do they really need to include SE, 95% CIs and df? Given that these are univariate analyses, it might have been interesting to use the oneway procedure in order to identify not simply overall significance levels, but groups which were significantly different from each other. For example, in respect of parents – are all the means different from each other, or are both parent and single parent not different from each other, but, perhaps, each different from stepfamily?

AR: Table 3 was replaced with new one. Some not actual information (F-values, df) was deleted from the Table 4. Homologenuous groups were identified by Post Hoc tests in univariate analysis, therefore in the paper (Table 3) there are presented only results of identification of countries that were characterized with high or low mean of the Family Time Index. The subchapter of Statistical analysis was appended with a message about Post Hoc testing.

p.11 – grandparents were not significant in the univariate analysis, so probably do not need to be included in the multivariate.
AR: ‘Grandparents’ variable was excluded from the multivariate analysis. Table 5 (Models B and C) was recalculated.

p.11 – interactions with country were included, but I wonder if all 2nd order interactions should have been included, to check for different patterns in respect of gender, age and SES?
P.11 – since Model C shows significant interactions between country and home and children, we cannot be sure of the overall effects of these variables. Perhaps it would be worth using multivariate models to look at differences on a country-by-country basis. This would allow for the identification of where the different patterns in respect of home and children occurred.

AR: Considering this and other comments that are addressed to modeling (p12, p.12-13) we have performed a lot of multivariate models on total data set and country-by-country basis. Therefore any new exclusive findings were obtained. Presentation of these results will expand the article nevertheless the present version of the article is overcrowded.

Discussion

p.12, 4th para – the findings in respect of boys and physically active family time are not mentioned in the Results.

AR: Both Results and Discussion were corrected in regard to this note.

p.12, 5th para – some of the speculation about how much of the country difference is explained by different patterns of family might be helped by the country-by-country analyses suggested above.

p.12-13 – similarly, the speculation about whether poverty might explain the findings in respect of single parents could be checked in analyses which look at the effects of these variables before and after controlling for each other, as could the question about whether different parental structures ‘explain’ the country differences.

AR: see AR to comment for p.11

Conclusions

The authors state that the cross-national differences are small compared with those in respect of age and gender – but actually, the differences in respects of age (mean index value for 13 yrs = 0.19, for 15 yrs = -0.19 are slightly smaller than those in respect of the two most diverse countries ( mean for Spain = 0.19, for Greenland = -0.21).

AR: This statement is not based on comparison of the ranges of variable variance as you give arguments, but rather is based on variance analysis (see, for instance, Table 5). In regard to this comment, an indication to variance analysis was included in the mentioned statement.

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions.

Level of interest: An article whose findings are important to those with closely related research interests.

Quality of written English: Needs some language corrections before being published.

Statistical review: No.

Declaration of competing interests: I declare that I have no competing interests.
Dear Dr. Anja Marjatta Taanila,

We are grateful to you for deep analysis and useful suggestions on how to improve the article. Most of your comments were taken into account. Below are our responses to all your comments.

Reviewer’s report and authors’ responses (AR):

General

The article is a very interesting description from the joint family activities in the different European countries. As the authors write, it is important to pay more attention to the role of the parents and common family life in the children’s growth and development.

The article is well written. Statistical analyses have been described well and even though there are many figures and large tables they clarify the results well.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

In the discussion it would be important to discuss the differences and similarities between countries more accurately. Furthermore, for instance, the result that boys in every country spend more time with their families than girls was unexpected result.

AR: We have made several attempts to improve Discussion, including discussion of results that boys spend more time within their families than girls.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

The language of the article needs revision and there are some spelling mistakes.

AR: We have made several attempts to improve the language and to avoid spelling mistakes.

In my opinion the children aged 13 to 15 are pupils not students.

AR: The schoolchildren, or pupils, from middle-school and secondary schools in scientific literature often are named as ‘students’. For example, see recent publication in BMC Public Health: Athina Kyrlesi, Elpidoforos S Soteriades, Charles W Warren, Jeni Kremastinou, Panagiotis Papastergiou, Nathan R Jones, Christos Hadjichristodoulou: Tobacco use among students aged 13–15 years in Greece: the GYTS project BMC Public Health 2007, 7:3 (8 January 2007) http://www.biomedcentral.com/content/pdf/1471-2458-7-3.pdf

Discretionary Revisions (which the author can choose to ignore).

What next?: Accept after minor essential revisions.

Level of interest: An article of importance in its field.

Quality of written English: Needs some language corrections before being published.

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.