Reviewer's report

Title: Family joint activities in a cross-national perspective

Version: 1 Date: 13 November 2006

Reviewer: HELEN SWEETING

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Family joint activities in a cross-national perspective

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 family 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? In 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? In the main, 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 the 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, 1st para – perhaps the statements re activities contributing to each member’s well-being, and enhancing communication, should be referenced.
p.3, 2nd para – I think the authors should acknowledge the existence of a possible 3rd variable (e.g. SES or individual personal characteristics) which might contribute, at least in part to the relationship between family time and positive outcomes. A unidirectional model of the ‘positive effect’ of family time on outcomes is probably too simple.
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.
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.

p.6, 4th line – ‘the first two or three categories’ – seems a bit vague.

p.6 – are these the SPSS variable names (‘PARENTS’, ‘INDEX’)? I do not think they need including in this form.

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.

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 – in fact, Ukraine seems to be higher on several measures. The fact that Spain comes out 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.

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

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

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?

p.11 – grandparents were not significant in the univariate analyses, so probably do not need to be included in the multivariate.

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.

Discussion

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

p.12, 5th para – some of the speculation about how much of the country difference is explained by different patterns of family life 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.

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).

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.