Author's response to reviews

Title: Socio-cultural determinants of adiposity and physical activity in preschool children: a cross-sectional study

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Author's response to reviews: see over
Ms.
Melissa Norton
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Resubmission of an original article

Dear Editor-in-Chief, dear Editorial Board

We would like to thank you for the thorough and helpful review of our paper “Socio-cultural determinants of adiposity and physical activity in preschool children: a cross-sectional study” (MS: 1618094019410888). Enclosed you find the revised version according to the reviewer’s comments with changes marked in yellow. The specific point-by-point responses to the reviewer’s comments are mentioned below. As outlined, we have carefully considered each suggestion.

Hoping that this revised version will now be acceptable for publication in BMC Public Health, we thank you in advance for considering it.

With kind regards on behalf of all the co-authors,

Flavia Bürgi
Reviewer 1 Comments to the authors

General Comments and Major Compulsory Revisions:

1. This paper reports data on adiposity, physical activity, sedentary behaviour and ‘motor skills’ in young children differing by socio-cultural factors. Significant differences are found.
2. The paper is interesting and novel in reporting on children from the same country but differing by region and language.
3. More needs to be said on why such differences are occurring, beyond ‘socio-cultural’ differences. What specifically might explain such differences?

We thank the reviewer for his positive comments. Concerning point 3, the discussion about why such differences are occurring has been extended and added in the discussion section. In general, it can be said that differences in health between the German and French speaking part of Switzerland (with the same policy and legal system) have been shown to reflect broader European patterns (Faeh, J Epidemiol Community Health, 2009). Outside of the health sector, differences exist in many areas such as educational and employment levels, political attitudes, school system and (public) transport as well as a different migrant population (see below for more details). These factors may be linked to the key outcome measures of our study.

Specific Comments (P6)

• apart from language, how do these samples differ?
• end of para 1: give percentage (for n=485)

There were no differences in age and sex, but there were some differences in the prevalence of migrant status and low educational level of the parents (see table 1). The migrant population also differs and is now mentioned in the table 1 (most frequent migrant countries per region) which may influence the local population. Duration of stay in Switzerland of migrants in both parts of Switzerland was not different. The other variables that we have in the study are already related to health or lifestyle, which would be the outcome. Apart from known differences in educational level between the French and German speaking part (Faeh, BMC Public Health, 2010), the two parts of Switzerland also differ in their school (website http://www.edk.ch/dyn/11553.php) and public transport system, but we did not measure these outcomes in our samples.

The percentage of the 485 children (=74%) is now mentioned.

Specific Comments (P7)

• para 2, L (line) 1: repetition

The sentence “The accelerometers were worn around the hip.” has been deleted.

Specific Comments (P7)

• para 3, L2: best to avoid use of the word ‘inactivity’ when referring to ‘sedentary behaviour’.

Initial we used “inactivity” as term for our accelerometer-measured activities below 25 counts per 15 s and “sedentary behaviour” as an umbrella-term for our low intensity activities (such as accelerometer-measured inactivity and TV-viewing time). But the reviewer is right that this use of terminology could be confusing.
Therefore, we have agreed on the following use of terminology: The term “inactivity” has been removed in the whole document. The accelerometer-measured activities have now been named “sedentary activities”, which is in concordance with the literature (Evenson, J Sports Sci, 2008). The few times it is mentioned, the term “sedentary behaviour” still refers to both, measured “sedentary activities” and “TV-viewing time”.

Specific Comments (P7)
- para 3: how do your counts for sedentary behaviour compare with those recommended by Reilly with preschool children?

Reilly et al. (Obes Res 2003) defined their cut-off point for sedentary behaviour at <1100 counts/min. But they saved data in 1-minute epochs. More recent publications have recommended to save data in 15-second epochs, as this corresponds more to the behaviour of preschool children (Rowland, Pediatr Exerc Sci 2007; Ward, Med Sci Sports Exerc 2005; Corder, Curr Opin clin Nutr 2007). As we saved data in 15-seconds epochs, these counts cannot just be compared to those of Reilly (as being below a count for 1 min cannot be just extrapolated from 4 x 15 second counts). We chose the published cut-off points for sedentary activities by Evenson et al. (J Sports Sci 2008). Compared to a published study in preschoolers, analysed with the same epoch size and a similar cut-off point (<37.5 counts/15s), our children had a similar percentage of sedentary activities (52%) compared to those children (55%) (Williams, Obesity, 2008).

Specific Comments (P7)
- I don’t think this is really a test of ‘motor skills’; it is more a test of ‘agility’.
- This raises the issue of why you are assessing this. What is the justification?

The reviewer is right that the obstacle course assessed agility. We follow the classification of Molnar et al. (Eur J Pediatr, 2000) that considers agility to be a part of skill-related fitness. To keep it simple and understandable for non-physical fitness specialists, we decided to use the term motor skills. We now specified in the methods section that the obstacle course tests agility. If the reviewer wishes, we could replace the term motor skills throughout the paper, but it may make it less clear. We chose the obstacle course, as it reflects the physical fitness abilities that young children use and need in their daily life activities (like running, turning, jumping above and crawling beneath an obstacle) and therefore also has a high relevance for their daily life. In addition, the interobserver validity was excellent and it was thus an ideal test to be used in a relatively large population.

Specific Comments (P8)
- para 2: why are you assessing migrant status? All aspects of assessment require justification in the Introduction. This also applies to time outside and TV viewing.

Different parental characteristics (such as migrant status, educational level or workload) as well as TV viewing are known risk factors for obesity and inactivity in children and were therefore assessed in our study. We now mention all these aspects in the introduction. We now also mentioned the “time outside” in the introduction.
Specific Comments (P10)
• para 2, L3: avoid the word ‘inactivity’. You are assessing ‘total sedentary time’ and TV viewing – two different constructs.

The word “inactivity” has been removed. See comment above concerning the terminology of sedentary behaviour.

Specific Comments (P15)
• para 2, L4: why and how might social networks play a role?

Concerning the social networks, the discussion section has been extended on page 15. Since diverse phenomena in adults can spread within social networks, it seems also possible that obesity or PA might spread from person to person. Having obese or sedentary social contacts influences the adoption of such specific behaviours (Christakis, N Engl Med, 2007). It is possible that this peer networks also plays an important role in children (Crawford, Int J Obes, 2010).

Specific Comments (P16)
• para 2, L1: I would like to see more on how the ‘broader social environment’ influences the key variables of interest. This is central to your study.

In the discussion section on page 15/16, we tried to describe more factors belonged to the broader social environment. For example, it is evident that differences in the school and public transport system, as well as in the educational level of the large population (Fäh, BMC Public Health, 2010) influence our outcome variables. For example, preschool in the French part of Switzerland resembles primary school more than it does in the German part of Switzerland (choice of academic objectives, classrooms).

Specific Comments (P23)
• ref 38 has detail missing.

The reference 38 has been completed.
Reviewer 2 Comments to the authors

Please provide up-to-date references to support the claim you make in the first sentence of the Introduction. You have cited a paper published in the year 2000 to suggest that “the high prevalence remains a great public health concern” (page 5). A much more recent paper is needed to support this claim.

A more recent reference has been provided (Ogden, JAMA, 2010).

It would be useful to know years of living in Switzerland for migrants within the French speaking and German speaking parts – the effect of regional environment may reflect greater assimilation by migrants living in German speaking parts as opposed to just migrant status.

In our study, years living in Switzerland for migrants did not differ between the German speaking and French speaking parts. But it is right that this information could be useful for the readers and therefore we added this information in the results section. We now also added the most frequent migrant countries in both parts of Switzerland into Table 1.

Also, why were parenting practices not measured? Given that we now know the preschool years are a critical period in childhood for the development of childhood obesity because many of the eating and physical activity habits that contribute to later obesity become established during these years, and the primary social force influencing preschool children is the family, it is surprising that parental practices and belief (parental instrumental behaviours, parental modelling, knowledge of PA) were not included. Your methodology is also limited by the fact that child eating and parent feeding styles/practices were not considered across the French speaking and German speaking parts of Switzerland.

The reviewer is complete right that parenting practices are essential for childhood behaviour and obesity, and both are influenced by individual and regional socio-cultural characteristics. We did not measure parenting practices because our focus was the influence of the different socio-cultural characteristics. We were not able to include an assessment of parental attitudes and beliefs in a serious way in addition to the comprehensive questionnaire which included questions about general health, quality of life, physical activity patterns and nutritional habits in a population-based sample where most parents were migrants. As the reviewer is right that this fact is a limitation of our study, we added this point to the general part and the limitations of the discussion section.

However we know from our questionnaire, that parents in the German speaking part were more frequently physically active with their children compared to those in the French speaking part. But we think that more results would overcharge the frame of this paper and we therefore did not add it to the discussion (that has already increased in size due to revisions in response to the comments of reviewer 1). However, if the reviewer wishes we will integrate this finding in our paper.

Reference 38 is not complete in Reference List

The reference 38 has been completed.