Reviewer's report

Title: Maternal attitudes and child-feeding practices: relationships with the BMI of Chilean children.

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Reviewer: Karen Lock

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Thankyou for asking me to review this study from Chile. It is an interesting area of research exploring pathways by which parents influence their childrens eating behaviour and thus risk of becoming overweight. As the authors state, and as far as I am aware, this is one of the few studies on this subject from Latin America and in itself it is important. Although there has been some analyses of how ethnicity impacts on parental influence, how parental attitudes to feeding their children varies in different countries is not known and this study adds to this body of knowledge. The study design, sampling, study population and statistical analytical approach are clearly stated and, in general, appropriate to the research question. I have some specific comments and points below.

Study design: The authors try to explore this question using existing longitudinal data on weight in children and new self reported data from mothers. The research uses both cross-sectional and retrospective approaches. As the authors themselves state, to actually assign causality, a prospective longitudinal design would be preferable, however this is a pragmatic research design. One problem with the retrospective design is that the children have been subject to an intervention and this could have important influence on parental attitudes and beliefs to weight and eating behaviours which is not able to be assessed as the authors had not assessed attitudes and beliefs of parents before the intervention. The authors discuss this in the conclusion (p16, p18) but clearly this limits the generalisibility of this study to a general population and the fact the children are post-intervention should be made more explicit in the introduction, conclusion and abstract.

The process of selection of children was explicit and appropriate. However, I was surprised that there is no discussion of why socio-economic status (SES) of the children or their families was not included in the study. In the statistical analyses (p10) the authors state that maternal child feeding practices may differ between boys and girls, but another key influence on child feeding and BMI in several countries has been found to be SES. This was discussed as a limitation of the study in the conclusion (p18) and one study is cited to defend this. The authors state in the conclusion that mothers were ‘mostly low income women and more likely to give socially desirable answers’(p17), however, nothing in the methods suggests that the SES of mothers was explicitly assessed. If it was, then why not include this in the analysis? If it was not assessed then how can the authors make a statement about the income statues of the families? As the authors are
clearly aware, SES will influence more than the maternal responses but can influence the actual feeding/eating behaviors (central to this research question). The authors should discuss how SES is likely to affect the results considering the wider body of research on links between SES and BMI in children.

Measurement tool: The CHQ was used to assess self reported maternal beliefs, attitudes and behaviours. In the conclusion, p19, the authors state that the CHQ can be used as a valid measure in Latin America. I am not sure that this paper has shown this. I think it is important to have more information about how the CHQ and the likert scales were validated in this study. The authors merely state on p7 that the CHQ was translated and tested for comprehension. For example, it is well known that the labels assigned to the scores on a Likert scale are important, and such label terms can have different meanings from person to person, and in different cultures (not to add the issue of translating these terms). As these scales are central to the research findings it is essential to know how the authors assessed comprehension of the terms used, such as ‘overweight’ and ‘low/high pressure’ etc, and how they validated the tool. Also they need to explain whether it is a reasonable assumption to pilot it on mothers in Santiago when the research is held in a different area (which they state on p18 could be different culturally), if no explicit comparison is made between the two groups of mothers (ie in key issues such as education level, SES etc?

I was also concerned that the issue of incentives were not discussed explicitly. The paper states that mothers who filled out a questionnaire received a present supported by Chilean food company (stated on p7). The authors do not declare any conflicts of interest, or clearly state that funding was received from a particular food company. I think it is essential in any public health research that any food industry involvement in research needs to be explicit. Obviously sponsorship of research could have a significant impact on mothers responses and I think the results cant be considered until this potential bias is discussed.

It is very good to see Latin American research being presented to a wider audience. Obviously the authors are not native english speakers and the manuscript would benefit from being proof read by an english speaker as their are a few typos.

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, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I have no conflicts of interest