Reviewer's report

Title: A prospective study of weight development and behavior problems in toddlers: the Norwegian Mother and Child Cohort Study

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Reviewer: Robert Bradley

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The need to know how weight is implicated in the development of behavior problems and vice versa is important given the long-term negative consequences of chronic overweight and psychiatric disorders. Thus, efforts to examine relations as such problems emerge have potential to further delineate what may well be complex relations. That is the primary goal of this very large-scale study being conducted in Norway. Even so, despite the fact that there is limited research on relations between weight and behavior problems during infancy and early childhood, it is not clear that this study offers useful new information on the emergence of critical relations between weight development and behavior problems. The difficulties with this study are both conceptual and methodologic – both of which are partially acknowledged by the authors.

The central problem with this study is the lack of convincing conceptual framing. There is no current theoretical support for the idea that being overweight in infancy starts a process that leads to behavior problems or vice versa. Neither is there any convincing empirical support for such a conjecture (the authors essentially admit the latter). The fact that there is both theoretical and empirical support for a causal connection beginning about the time children enter middle childhood in no way argues for its earlier emergence. Indeed, a penetrating analysis of the findings during middle childhood and later suggest the opposite. As children’s cognitive and social competences develop, then the processes that link overweight to internalizing problems begin to shape the course of development. The situation pertaining to externalizing problems is even more complicated, with inconsistent findings. Most of the research cited pertains to children of at least school age and in some cases to adults. Nothing, including earlier findings by this team of scientists, offers a convincing reason to undertake the study described in this manuscript. Indeed, in the absence of a persuasive theoretical argument (that would likely apply to a biologically based clinical condition), there is good reason not to undertake this study in light of the inconsistent findings pertaining to young children. Specifically, if a small (but significant due to large sample size) association had emerged, it could very well indicate a relation that only applies to a small clinical subgroup or limitations of measures – that is, it would not indicate the more general relation envisioned by the authors. In a very real sense, it would just further cloud the true general relation that is at issue here. Perhaps not surprisingly, there was no evidence of a relation in children under three in this study and the only relation that emerged was contrary to what was originally expected. Again, if one understands the
findings on older children and adults and one respects the cognitive and social limitations of children under age three, there is no reason to suspect the emergence of a relation this early in life (except perhaps for some rare biologically based clinical conditions).

The matter is made worse by the measures employed – again, to the authors credit they acknowledge the potential limitations of the key measure of behavior problems. There is good reason to worry about the ability of parents to report accurately on children’s behavior problems when children are very young – despite the widespread use of measures like the CBCL. The patterns of association one sees from these measures typically does not comport with what the developers of CBCL envisioned. Again, there is evidence of this in the results reported in this manuscript. Most specifically, short of behavior that is clearly in an extreme range, parents frequently cannot discriminate between behaviors indicative of one type of problem and another. This problem is exacerbated in the current study when the number of indicators was reduced - better that it had been expanded. As appropriate and wise as it was to enlist the assistance of clinical and developmental psychologists in picking items that would represent internalizing and externalizing behaviors, it was insufficient. More critically, the authors needed to establish the discriminant validity of each indicator. In effect, it is not surprising that the researchers found a “very high correlation between subscales” that supposedly represented different latent constructs. In effect, the scales do not work as intended: what psychologists may well be able to distinguish, parents often cannot.

The authors properly review many of the limitations of their study – somewhat underselling the actual limitations. Despite their candid admissions pertaining to many of the problems (including ones pertaining to external validity), they assert that the “study fills an important gap in the literature by demonstrating that behavior problems and weight are still unrelated in toddlers”. Such an assertion seems strange given all the limitations and the fact that there is no theoretical reason to believe that an association exists in the first place. Providing evidence for the null case in this instance does not advance science. The existence of a “gap” does not per se signal a need for research designed to fill the gap. If the authors have substantive theory and appropriate data that perhaps pertains to a clinical subgroup, then they may well be able to advance understanding of relations between weight status and behavior problems for this subgroup, rather than targeting such a relation in the general population.

**Level of interest:** An article of limited interest

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**
I have no competing interests