

PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (<http://bmjopen.bmj.com/site/about/resources/checklist.pdf>) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Descriptive epidemiology of changes in weight and weight-related behaviors of Australian children age 5 years: Two population-based cross-sectional studies in 2010 and 2015
AUTHORS	Hardy, Louise L.; Baur, L; Wen, Li Ming; Garnett, Sarah P.; Mhrshahi, Seema

VERSION 1 – REVIEW

REVIEWER	Sydney G. O'Connor University of Southern California, USA
REVIEW RETURNED	02-Oct-2017

GENERAL COMMENTS	<p>Manuscript ID: bmjopen-2017-019391 Descriptive epidemiology of temporal changes in weight and weight-related behaviors of Australian children age 5 years: 2010-2015</p> <p>In the context of a large New South Wales government investment in promoting young children's healthy development, this study describes cross-sectional changes in child overweight/obesity rates and weight related behaviors in two groups of 5-year-old children, assessed 5 years apart in 2010 and 2015. This appears to be the first manuscript describing the effects of the NSW investment on obesity-related outcomes among 5-year olds. This is a nice study, and findings are interesting, however there are some limitations that should be addressed and several typographical errors throughout the manuscript, as noted below.</p> <p>Title</p> <p>- The title "Descriptive epidemiology of temporal changes in weight and weight-related behaviors of Australian children age 5 years: 2010-2015" is a bit unclear and sounds as though changes within one group of children are being tracked for five years, from 2010 – 2015. Consider rewording to: "Descriptive epidemiology of temporal changes in weight and weight-related behaviors of Australian 5-year old children: 2010-2015"</p> <p>Abstract</p> <p>- Line 28: 'especially in the highest tertile of junk' – does this mean 'membership in the highest tertile...'</p> <p>- Line 44: 'significant positive changes' is ambiguous – to the reader it appears as though you are describing positive linear associations. Consider rewording to significant 'improvements'.</p>
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Introduction

- Given that the examination of differences by sociodemographic group is a large component of the present study, it is surprising that there is no background or rationale given within the Introduction regarding how neighborhood type (urban vs. rural), neighborhood-level SES, language spoken at home. How have these factors been shown to influence children's weight-related behaviors and obesity rates, and how this is expected to influence the current study findings?

Methods

- Why was analysis of sociodemographic differences in weight-related behaviors done only within the 2015 population? Were there limitations that prevented this secondary analysis from being conducted in the 2010 sample? Or was there an a priori reason for only examining sociodemographic differences in 2015?

- Page 6, Lines 52-53: Fix typo in sentence beginning "Additionally, because discretionary foods..."

- Page 7, Lines 48 – 52: Children were classified as inactive or active travelers based on their school-week transport (5 days by driving, or 5 days by other method). Did all children fall into one of these two categories, or did any children have varying methods of transportation throughout the school week (e.g., drive some days, bike other days)? If so, how were they classified for analysis?

Results

- Provide information on participants (e.g. numbers potentially eligible, examined for eligibility, confirmed eligible, included in the study, completing follow-up, and analysed). Additionally, provide information on the number of schools (this is stated in the Abstract but not in the manuscript text).

- Page 9, Line 5: Add a comma between obesity and overweight-obesity to the sentence "overweight, obesity overweight-obesity combined and WtHR..."

- Page 9, Lines 13-20: Please reword this sentence for clarity.

Discussion/Conclusions

- Please provide more comment on the large discrepancy of meeting recommendations for fruit (79%) vs. vegetables (2.3%). Is this expected based on other national surveys?

- Please comment on the response rate (62.0% in 2010 and 69.7% in 2015) as a strength or limitation of the present study. Are participants who completed the survey similar to the overall recruitment pool, or are there suspected differences that limit generalizability?

- Is it appropriate to say that specific sociodemographic sub-groups require additional intervention based on 2015 data alone, and not based on change in specific sociodemographic group information from 2010 – 2015? Perhaps some groups had poorer weight-related behaviors in 2010 but actually made greater improvements over time than the overall sample, which would not be apparent without examining sub-group trends in 2010.

- Page 11, Line 38: fix typo 'improved'

- It seems another limitation of this study is the lack of assessment of children's physical activity behaviors (time spent playing sports, engaging in moderate-to-vigorous physical activity, etc). While parental knowledge of the PA recommendation for children was assessed, actual PA was not, with the exception of transport to school. Please comment on this in the discussion.

	<p>Tables - Include participant sample size from each year as a footnote in tables</p>
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REVIEWER	<p>William Heerman Vanderbilt University Medical Center United States of America</p>
REVIEW RETURNED	<p>05-Dec-2017</p>

GENERAL COMMENTS	<p>Hardy and colleagues have presented a thoughtful manuscript, the purpose of which is to describe temporal changes in weight and weight-related behaviors among 5 year olds in New South Wales, Australia. They have used a repeated cross-sectional design. One important strength is the availability of objectively collected anthropometric measurements. The authors do an excellent job distilling and interpreting large amounts of data, and draw meaningful and appropriate conclusions. What makes this manuscript of most interest is the ability to measure population outcomes in a region that has so heavily invested in population health. This has important public health implications for the regional, national, and potentially international community as decisions are made about how to tailor population-level obesity interventions. The manuscript is very strong, and I would offer only a few minor suggestions to improve clarity.</p> <p>Abstract:</p> <p>1) It would be helpful in the abstract to point out that New South Wales has made substantial community-level investments in obesity prevention. Perhaps under in the objective or setting headings?</p> <p>2) In the conclusions, the authors may want to highlight the population-level implication that is so well articulated in the discussion section. It may be as simple as adding the word "population" or "community" to the final sentence. I might suggest something like: The findings indicate that there is a need to enhance population-level efforts and ensure community programs are targeted...</p> <p>Introduction</p> <p>3) The first sentence is quite long. The authors should consider breaking it into two sentences.</p> <p>4) In the second paragraph of the introduction, the authors should consider removing the clause "as children grow and their mothers return to the workforce." It is not necessary for the logic flow, and may be distracting to readers with different cultural perspectives.</p> <p>5) I was not familiar with the term "up-skilling." While intuitive, if there is another term that is commonly used, the authors should consider using it.</p> <p>6) In the final paragraph of the introduction, the authors should clarify what is meant by "overall investment." Overall investment by whom and for what purpose?</p> <p>Methods</p> <p>7) The biggest concern about this manuscript is the power. The study was only powered to detect a 10% difference in the</p>
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	<p>prevalence of overweight/obesity. This is under-powered, as even a 1% or 2% change would represent a major population level change over a 5 year period. This should be addressed in the discussion.</p> <p>8) It is typical to present weighted estimates when reporting prevalence estimates from surveys with a sampling design. The authors should clarify whether weighted percentages are being reported in the methods section, and also in the tables where the percentages are displayed.</p> <p>Results</p> <p>9) The results are well written and data are clearly displayed in tables.</p> <p>Discussion</p> <p>10) The authors should add as a limitation the ecological fallacy- the idea that the current methodology cannot account for individual level exposure to the population-interventions. This does not detract from the importance of the manuscript, but rather raises the possibility of whether the deficiency in the population-level programming is uptake (in which case dissemination and implementation strategies would be an appropriate next step) or in the type/content of the programming.</p> <p>11) Could the authors speculate as to why the changes in behaviors didn't result in obesity change? Are there other factors that may be contributing to obesity risk?</p>
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VERSION 1 – AUTHOR RESPONSE

WE HAVE INCLUDED THE TEXT BELOW AS A SUPPLEMENTARY FILE IN TABLE FORMAT.

Reviewers' Comments to Author: Authors response

Editorial Request:

- Please revise your title so that it includes your study design. This is the preferred format for the journal. We have revised the title to now read;

Descriptive epidemiology of changes in weight and weight-related behaviors of Australian children age 5 years: Two population-based cross-sectional studies in 2010 and 2015

Reviewer: 1

Reviewer Name: Sydney G. O'Connor

Institution and Country: University of Southern California, USA Competing Interests: None declared

In the context of a large New South Wales government investment in promoting young children's healthy development, this study describes cross-sectional changes in child overweight/obesity rates and weight related behaviors in two groups of 5-year-old children, assessed 5 years apart in 2010 and 2015. This appears to be the first manuscript describing the effects of the NSW investment on obesity-related outcomes among 5-year olds. This is a nice study, and findings are interesting, however there are some limitations that should be addressed and several typographical errors throughout the manuscript, as noted below.

Thank you, and for your comments below.

Title

- The title “Descriptive epidemiology of temporal changes in weight and weight-related behaviors of Australian children age 5 years: 2010-2015” is a bit unclear and sounds as though changes within one group of children are being tracked for five years, from 2010 – 2015. Consider rewording to: “Descriptive epidemiology of temporal changes in weight and weight-related behaviors of Australian 5-year old children: 2010-2015” We have revised the title to now read;

Descriptive epidemiology of changes in weight and weight-related behaviors of Australian children age 5 years: Two population-based cross-sectional studies in 2010 and 2015

Abstract- Line 28: ‘especially in the highest tertile of junk’ – does this mean ‘membership in the highest tertile...’

Line 44: ‘significant positive changes’ is ambiguous – to the reader it appears as though you are describing positive linear associations. Consider rewording to significant ‘improvements’. We have reworded the sentence to now read;

Significant improvements in multiple weight-related behaviors were observed among children in the highest tertile of junk food consumption

Introduction

- Given that the examination of differences by sociodemographic group is a large component of the present study, it is surprising that there is no background or rationale given within the Introduction regarding how neighborhood type (urban vs. rural), neighborhood-level SES, language spoken at home. How have these factors been shown to influence children’s weight-related behaviors and obesity rates, and how this is expected to influence the current study findings? Thank you – this is important background information and we have added the following text.

There is, however, clear evidence that the distribution of child obesity is unequal across population groups. The population distribution of child obesity is higher among children from lower socioeconomic backgrounds status, internationally⁷ and in Australia.⁸ Similarly the prevalence of child obesity can be higher among children from culturally and linguistically diverse (CALD) communities. In 2016, almost half the Australian population were born overseas or had at least one parent born overseas and 21% spoke a language other than English at home.⁹ Language spoken at home is a recognised indicator of CALD background and people who speak a non-English language at home tend to be recent immigrants who may be disadvantaged in health literacy and health care access.^{10 11} These reasons underpin the importance of examining health outcomes by sub-population groups to identify whether there are any apparent or emerging health inequalities among children from disadvantaged backgrounds.

Methods

- Why was analysis of sociodemographic differences in weight-related behaviors done only within the 2015 population? Were there limitations that prevented this secondary analysis from being conducted in the 2010 sample? Or was there an a priori reason for only examining sociodemographic differences in 2015? We have amended this paragraph which now reads;

Policy and decision makers require up-to-date evidence to guide the development of intervention and health promotion activities. Given the established evidence on sociodemographic differences among children’s weight and weight-related behaviours, we report outcomes from the most recent survey (2015) to identify whether sub-groups of children may require greater or more targeted intervention. Page 6, Lines 52-53: Fix typo in sentence beginning “Additionally, because discretionary foods...’

Thank you we have fixed the typo.

Page 7, Lines 48 – 52: Children were classified as inactive or active travelers based on their school-week transport (5 days by driving, or 5 days by other method). Did all children fall into one of these two categories, or did any children have varying methods of transportation throughout the school week (e.g., drive some days, bike other days)? If so, how were they classified for analysis?

We have amended this paragraph which now reads;

For the analysis, children's travel modes were classified as 'inactive travelers' if driven to and from school 5-days/week and 'active travelers' if they walked, cycled, used a skateboard or scooter to travel to and from school 5-days/week. Children who used multiple transport modes to travel to and from school were classified as 'mixed travelers'. Because active travel is considered a healthy behavior and sitting time in car travel is considered less healthy, we only examined children who were active or inactive travelers.

Results

Provide information on participants (e.g. numbers potentially eligible, examined for eligibility, confirmed eligible, included in the study, completing follow-up, and analysed). Additionally, provide information on the number of schools (this is stated in the Abstract but not in the manuscript text). Information on eligibility is in the Methods section, and because the data are cross-sectional there is no follow-up. We also mention in this section that all children were invited to participate.

The following text has however been changed in the Results section;

The 2010 survey comprised 1,141 children in Kindergarten from 44 schools (response rate 62%) and the 2015 survey comprised 1,150 children from in Kindergarten from 41 schools (response rate 70%). Table 1 shows there were no significant difference in the children's socio-demographic characteristics between surveys.

Page 9, Line 5: Add a comma between obesity and overweight-obesity to the sentence "overweight, obesity overweight-obesity combined and WtHR...." Thank you, we have fixed the typo.

Page 9, Lines 13-20: Please reword this sentence for clarity. We have amended this sentence which now reads;

Table 3 shows there were some significant positive changes in behaviours including the lower consumption of junk food, less TVs in children's bedrooms and a higher parental awareness of children's screen-time and physical activity recommendations.

Discussion/Conclusions

Please provide more comment on the large discrepancy of meeting recommendations for fruit (79%) vs. vegetables (2.3%). Is this expected based on other national surveys? Our findings on meeting the recommendations for fruit and vegetable intake are in line with the national health survey data and data from other community samples of children.

We have added a comment below

"This finding is consistent with national surveys and other studies which have shown vegetable intake in Australian children is poor"

(Whitrow et al 2016)

Please comment on the response rate (62.0% in 2010 and 69.7% in 2015) as a strength or limitation of the present study. Are participants who completed the survey similar to the overall recruitment pool, or are there suspected differences that limit generalizability? We have included the following text to this paragraph which now reads;

Our sampling frames were representative of NSW children in terms of type of school, residence and SES, so the findings may not necessarily be generalizable to all Australian children. Survey response rates are often considered an indicator of survey quality yet there is no scientific consensus on a minimal threshold. Response rates >60% are considered acceptable however the representativeness of the sample is potentially of more importance.⁴⁶

Is it appropriate to say that specific sociodemographic sub-groups require additional intervention based on 2015 data alone, and not based on change in specific sociodemographic group information from 2010 – 2015? Perhaps some groups had poorer weight-related behaviors in 2010 but actually made greater improvements over time than the overall sample, which would not be apparent without

examining sub-group trends in 2010. Thank you for this considered comment. While this is definitely one approach to examine our data we felt the reporting of difference by sociodemographic characteristics in 2015, rather than differences in the rate of change between survey years is better information for policy makers. That is, the need to show that there are significant differences now and intervention and investment need to occur among these children, irrespective on any potential improvement in weight-related behaviours

- Page 11, Line 38: fix typo 'improved' Thank you, we have fixed the typo.

- It seems another limitation of this study is the lack of assessment of children's physical activity behaviors (time spent playing sports, engaging in moderate-to-vigorous physical activity, etc). While parental knowledge of the PA recommendation for children was assessed, actual PA was not, with the exception of transport to school. Please comment on this in the discussion. Due to some changes with the physical activity survey questions between surveys we could not compare the changes in physical activity between surveys (as shown in Table 3) however we have included the variables in Table 4. We have also included the question used in the 2015 survey in the methods section and added sentences in the results and discussion reflecting this.

Tables

- Include participant sample size from each year as a footnote in tables We have added the sample sizes to the Tables.

Reviewer: 2

Reviewer Name: William Heerman

Institution and Country: Vanderbilt University Medical Center, United States of America

Competing Interests: None declared

Hardy and colleagues have presented a thoughtful manuscript, the purpose of which is to describe temporal changes in weight and weight-related behaviors among 5 year olds in New South Wales, Australia. They have used a repeated cross-sectional design. One important strength is the availability of objectively collected anthropometric measurements. The authors do an excellent job distilling and interpreting large amounts of data, and draw meaningful and appropriate conclusions.

What makes this manuscript of most interest is the ability to measure population outcomes in a region that has so heavily invested in population health. This has important public health implications for the regional, national, and potentially international community as decisions are made about how to tailor population-level obesity interventions. The manuscript is very strong, and I would offer only a few minor suggestions to improve clarity. Thank you.

Abstract:

1) It would be helpful in the abstract to point out that New South Wales has made substantial community-level investments in obesity prevention. Perhaps under in the objective or setting headings? Thank you, we have included the following sentence in the Objective of the Abstract;

Over the past 10-15 years there has been substantial investment in New South Wales (NSW, Australia) to reduce child obesity through interventions in children age 0-5 years.

In the conclusions, the authors may want to highlight the population-level implication that is so well articulated in the discussion section. It may be as simple as adding the word "population" or "community" to the final sentence. I might suggest something like: The findings indicate that there is a need to enhance population-level efforts and ensure community programs are targeted... Thank you, we added the terms as suggested.

Introduction

The first sentence is quite long. The authors should consider breaking it into two sentences.

Agree! We have partitioned the sentence as follows

Children who are obese during childhood are five times more likely to be obese in adulthood compared with non-obese children.¹ The evidence also shows that obesity-related behaviours

including poor diet quality, decreased physical activity, increased sedentary behaviours and decreased sleep duration are established in, and track from, early childhood.²

In the second paragraph of the introduction, the authors should consider removing the clause “as children grow and their mothers return to the workforce.” It is not necessary for the logic flow, and may be distracting to readers with different cultural perspectives. Agree, and we have removed this text.

I was not familiar with the term “up-skilling.” While intuitive, if there is another term that is commonly used, the authors should consider using it. The term is used in Australia, however for clarity we have replaced the term with the following;

professional development programs

In the final paragraph of the introduction, the authors should clarify what is meant by “overall investment.” Overall investment by whom and for what purpose? We have included the following text to this paragraph;

... the overall investment in early childhood by different stakeholders through multiple programs and in different settings.

Methods

The biggest concern about this manuscript is the power. The study was only powered to detect a 10% difference in the prevalence of overweight/obesity. This is under-powered, as even a 1% or 2% change would represent a major population level change over a 5 year period. This should be addressed in the discussion. We agree with the reviewer and we have now acknowledged that this is one of the limitations in the discussion .

We have added the following to the discussion

“This study was a secondary analysis of two population based surveys. The sample sizes were not large enough to detect a smaller difference in the prevalence of overweight/obesity. For example, to detect 1% or 2% change that is of public health significance at a population level would require a much larger sample size”

It is typical to present weighted estimates when reporting prevalence estimates from surveys with a sampling design. The authors should clarify whether weighted percentages are being reported in the methods section, and also in the tables where the percentages are displayed. The following text has been added to the analysis section and included in the relevant Tables;

‘... and weighted prevalences are presented.

Results

The results are well written and data are clearly displayed in tables. Thank you

Discussion

The authors should add as a limitation the ecological fallacy--the idea that the current methodology cannot account for individual level exposure to the population-interventions. This does not detract from the importance of the manuscript, but rather raises the possibility of whether the deficiency in the population-level programming is uptake (in which case dissemination and implementation strategies would be an appropriate next step) or in the type/content of the programming. We agree and have added a line about the study design ie the study is based on two cross-sectional surveys and hence no causal relationships can be ascertained, so it cannot be determined whether deficiencies in the type/content of the program or in uptake of the program are the reason for the results.

Could the authors speculate as to why the changes in behaviors didn't result in obesity change? Are there other factors that may be contributing to obesity risk? Obesity is a complex problem and there may well be factors that we haven't measured such as genetic susceptibility and certain environmental factors such as the food and physical activity environments which also may play a major part. It is clear that surveillance systems such as SPANS provide an important source of data on obesity prevalence, high risk groups and modifiable determinants to inform preventive action.

We have added the following line to the discussion

“ Understanding the drivers for the changes we observed is difficult because of the complex interacting contexts of obesity prevention. There may well be factors that were not measured such as genetic susceptibility and environmental features, such as the food and physical activity environments, which may also be influencing the prevalence”.

VERSION 2 – REVIEW

REVIEWER	William Heerman Vanderbilt University Medical School United States
REVIEW RETURNED	27-Jan-2018
GENERAL COMMENTS	The authors have responded appropriately to previous comments.