PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Socioeconomic status, remoteness, and tracking of nutritional status from childhood to adulthood in an Australian Aboriginal Birth Cohort - the ABC study.
AUTHORS	Sjöholm, Pauline; Pahkala, Katja; Davison, Belinda; Juonala, Markus; Singh, Gurmeet

VERSION 1 – REVIEW

REVIEWER	bert little School of Public Health University of Louisville
REVIEW RETURNED	07-Oct-2019

GENERAL COMMENTS	These are paired observations. The analyses look like contingency table analyses, and should probably consider using McNemar's test because the data are observations on the same person over time. The techniques used are not for paired data. Notably, paired data are more powerful statistically than unpaired data, and this is not highlighted.

REVIEWER	Katherine Thurber NCEPH, RSPH, ANU, Australia
REVIEW RETURNED	31-Oct-2019

GENERAL COMMENTS	Thank you for the opportunity to review this manuscript. It presents analysis of data from a unique cohort study. I have outlined below some substantive comments and some minor comments for consideration. Analytical approach The analytical approach chosen included complete cases only (n=315/686), which resulted in losing more than half the sample. The authors acknowledge that this does potentially introduce bias, particularly given remoteness and disadvantage differences in included vs. excluded participants. The authors may wish to provide an indication of the potential magnitude and direction of bias resulting from these differences. Did the authors explore differences in baseline BMI between those included vs. excluded from the analysis? I did not see this result reported in the manuscript, and consider that it would be an important form of bias to consider.

	Did the authors consider using an approach that did not require complete cases? For example, use of growth curve models would enable inclusion of all (plausible) data points. This approach would also enable use of BMI as a continuous measure, rather than losing information by only focusing on BMI categories. I would recommend that the authors explore other potential statistical approaches to improve the robustness of findings. The authors have examined the relationship between child BMI and adult underweight (underweight vs. normal weight + overweight/obese) and the relationship BMI and adult overweight/obese (overweight/obese vs. underweight + normal weight). What are the implications of the lack of independence of these two outcomes? Were any measures taken to account for this? Did the authors consider adjusting for other potential confounders? Implications I consider that the manuscript could be improved through strengthening description of the practical implications of the findings. The implications are described in a non-specific manner e.g. 'Socioeconomic factors, remoteness and gender must be addressed when assessing nutrition-related issues…' (Abstract) or 'differences … should be addressed when developing new strategies to reduce the immense health inequalities in Australia' (Summary). The prevalence of underweight is very high in this sample, and the prevalence of overweight/obesity is relatively low, compared to the national Aboriginal and Torres Strait Islander population. More attention could be given to this, and how 'nutritional status' interventions may look different in this setting if the goal is to shift the BMI curve to the right for those currently experiencing underweight, rather than only shifting the BMI curve to the left. Consideration in the context of related literature There are published data on associations (cross-sectional and longitudinal) between weight status and factors including birth weight, age, sex, remoteness and BMI in other Aboriginal and Torres Strait Islander cohorts. I consider that it would be valuable to explore how the findings from this cohort study align with previously published studies, and to identify the novel elements that are gained through this study. For example, in the introduction, the authors state: 'Little is known about the association between socioeconomic status and weight status within indigenous populations' – there is some published evidence on this specific to the Aboriginal and Torres Strait Islander population; perhaps this should be referred to here? The authors refer to indigenous populations more broadly (i.e. internationally) – if this international literature review was conducted, could findings be incorporated into the manuscript where relevant? Ethical considerations

	The patient and public involvement statements is valuable. However, I would be interested to see reporting of the engagement processes that have occurred specific to this paper. E.g. how were Aboriginal and Torres Strait Islander peoples involved in conceptualising, designing, analysing, or interpreting findings from this specific study? How are results from this specific study being disseminated? Minor points: Article Summary: I would be mindful of stating that 'cultural issues' posed a 'logistical challenge' – could you be more explicit about what posed a challenge? Introduction: If comparing the prevalence of underweight in the Aboriginal and Torres Strait Islander population to the prevalence in the non-Indigenous population, please provide the confidence intervals around these estimates so that they can be compared. I would consider reducing the emphasis on comparison to the non-Indigenous population, and increase focus on the Aboriginal and Torres Strait Islander population, including referring to relevant literature from this population (as described above). I would recommend revising this sentence to be specific to 'birth cohorts', given the existence of several large scale Indigenous cohort studies within Australia alone: 'To date, the study is one of the longest running and largest Indigenous [birth] cohorts in the world'. Alternatively the word 'largest' could be removed. If including a statement that it is not clear what cut-off points are 'most appropriate' for this population, I would consider providing more justification around why a different cut-off point might be 'appropriate'; the need for different cut-off points might be interpreted as implying a biological difference between Aboriginal and Torres Strait Islander and non-Indigenous peoples. Methods: How were implausible measurements (implausible measurements at one time point, or implausible changes over time) assessed and handled in the analysis? How many participants were <18 years old? If this is a small group, did the authors consider running a sensitivity analysis excluding these participants given the different BMI cut-offs used? Defining area-level disadvantage and remoteness: were the cut-offs for IRISEO used established cut-offs? If not, how were these chosen? How was remoteness defined? Did these measures change over time? How was this taken into account in analysis? E.g. were these treated as time-varying exposures? Defining birth weight categories based on z-scores: what cut-offs were used? What was the rationale for selection of these cut-offs? Suggest including more information about the age categories used and the justification for this. Results:

	Line 27-50: are these results statistically significant? Did the authors explore the interaction between remoteness and SES? When reporting the results on tracking, I would also be interested to see: of those who were overweight/obese at time X, what % were overweight/obese at time Y? (and the same for underweight.) This would give a sense of the extent of later overweight/obesity (or underweight) that is predicted by child weight status – i.e. what proportion of the total burden of these outcomes is explained by early life weight status. Discussion: Line 32-40, where referring to other studies, it would be valuable to refer to the age group of the cohort in that study (and if it is similar to the age group of the cohort under study). There is evidence of different relationships between socioeconomic measures and weight status for Aboriginal and Torres Strait Islander youth versus older adults. Lines53-60: where referring to findings from other studies, it is important to clarify whether these are specific to the Aboriginal population or not. If they are not, it would be important to be explicit about this, and to state whether or not the findings are likely to be applicable to the population of interest. Line 22: 'Nutrition education has been reported to have some positive effect on obesity' – can you please provide more information about what was found, what population was studied (e.g. local or national, age group, remoteness – to help understand the likely relevance to the population under study), and the quality of the evidence? Conclusions about physical activity do not seem to be informed by the findings of this study.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Bert Little
Institution and Country: School of Public Health
University of Louisville
Please state any competing interests or state 'None declared': None
Please leave your comments for the authors below
These are paired observations.
The analyses look like contingency table analyses, and should probably consider using McNemar's test because the data are observations on the same person over time.
The techniques used are not for paired data.
Notably, paired data are more powerful statistically than unpaired data, and this is not highlighted.

RESPONSE:
The authors would like to thank the reviewer for this valuable comment. We have now conducted Cochran's Q tests (an extension of the McNemar's test) for the differences in nutritional status over the course of the three follow-ups by sex and McNemar's tests for comparing the changes between the individual follow-ups by sex. We believe that this has significantly improved the statistical robustness of the results and strengthens the longitudinal aspect of the analyses.

Reviewer: 2
Reviewer Name: Katherine Thurber
Institution and Country: NCEPH, RSPH, ANU, Australia
Please state any competing interests or state 'None declared': None declared.
Please leave your comments for the authors below
Thank you for the opportunity to review this manuscript. It presents analysis of data from a unique cohort study. I have outlined below some substantive comments and some minor comments for consideration.

RESPONSE: The authors are grateful for this thorough review and the expert comments. Many of the comments have led us to make some changes to the manuscript that we believe have significantly improved the manuscript as a whole.

1) Analytical approach
The analytical approach chosen included complete cases only (n=315/686), which resulted in losing more than half the sample. The authors acknowledge that this does potentially introduce bias, particularly given remoteness and disadvantage differences in included vs. excluded participants. The authors may wish to provide an indication of the potential magnitude and direction of bias resulting from these differences.

RESPONSE: This comment is very relevant. As the people excluded participants were more often from urban and less disadvantaged areas, the underweight rates might be exaggerated. We have included this issue in the discussion:

"There were significant geographical differences between participants and non-participants, with participants being more often from non-urban and more disadvantaged areas. This potential bias may exaggerate the prevalences for underweight in the cohort as underweight was more prevalent in the remote and disadvantaged regions."

2) Did the authors explore differences in baseline BMI between those included vs. excluded from the analysis? I did not see this result reported in the manuscript, and consider that it would be an important form of bias to consider.

RESPONSE: BMI and WHtR values between excluded and included participants were analysed at all follow-ups and no significant differences were found. This information has now been added in the methods and results sections.

"Differences in BMI and WHtR values between included and excluded participants at all follow-ups were analysed using t-tests."

"There were no significant differences between the BMI or the WHtR values at any follow-up between participants included and those not included (for BMI: P=0.48 for Wave-2 and Wave-3, P=0.47 for Wave-4; for WHtR P=0.5, 0.46 and 0.52 respectively)."

3) Did the authors consider using an approach that did not require complete cases? For example, use of growth curve models would enable inclusion of all (plausible) data points. This approach would also enable use of BMI as a continuous measure, rather than losing information by only focusing on BMI categories. I would recommend that the authors explore other potential statistical approaches to improve the robustness of findings.

RESPONSE: As we specifically wanted to analyse the proportions of children who remained in the same weight and WHtR categories in adolescence and adulthood, this approach seemed to be the

most suitable one. To account for the possible bias, sensitivity analyses utilizing all data points have now been conducted and the results added to the methods section. As stated before, there were no significant differences between the included and excluded participants regarding the anthropometric measures used. In a previous publication from the same cohort, BMI was used as a continuous measure and a linear mixed effects model was used with all plausible data points. A significant association was found between remoteness and IRSEO and the longitudinal development of BMI. This approach was well suited for that analysis but was not applicable to our research question. We have added this literature reference (Juonala et al MJA 2019) and related text in the discussion part.

”Sensitivity analyses
To test for bias due to the large amount of people not included, sensitivity analyses were performed for tracking analyses for all plausible values for the whole cohort. Logistic regression analyses adjusted for sex, age at follow-up and time between follow-ups determined that tracking of overweight/obesity was significant from Wave-2 to Wave-3 (P<0.0001, OR=17.3) and Wave-4 (P<0.0001, OR=16.1) as well as from Wave-3 to Wave-4 (P<0.0001, OR=18.7). Tracking was also significant for underweight from Wave-2 to Wave-3 (P<0.0001, OR=15.8) and to Wave-4 (P=0.003, OR=4.1) and from Wave-3 to Wave-4 (P<0.0001, OR=11.2). High WHtR tracked from Wave-2 to Wave-3 (P<0.0001, OR=9.9) and to Wave-4 (P<0.0001, OR=21.8) and from Wave-3 to Wave-4 (P<0.0001, OR=7.9). Low WHtR tracked from Wave-2 to Wave-3 (P=0.002, OR=2.6) and to Wave-4 (P=0.005, OR=2.4) and from Wave-3 to Wave-4 (P<0.0001, OR=7.8).”

“In a previous study from the ABC, a significant association was found between remoteness and areal disadvantage at birth and longitudinal development of BMI measured at the same follow-ups as the present study. [33]”

4) The authors have examined the relationship between child BMI and adult underweight (underweight vs. normal weight + overweight/obese) and the relationship BMI and adult overweight/obese (overweight/obese vs. underweight + normal weight). What are the implications of the lack of independence of these two outcomes? Were any measures taken to account for this? Did the authors consider adjusting for other potential confounders?

RESPONSE: The variables in the tracking analyses were binary. For overweight/obesity, the categories were ”overweight/obese” or ”not overweight/obese” and these same categories were applied for all follow-ups and respective binary categories were used for underweight and the two WHtR categories. We thus calculated odds ratios for staying in the same category from childhood through adolescence and to adulthood. These categories exclude each other and lack of independence was not found to be a risk in this cohort.

5) Implications
I consider that the manuscript could be improved through strengthening description of the practical implications of the findings. The implications are described in a non-specific manner e.g. ‘Socioeconomic factors, remoteness and gender must be addressed when assessing nutrition-related issues…’ (Abstract) or ‘differences … should be addressed when developing new strategies to reduce the immense health inequalities in Australia’ (Summary).
The prevalence of underweight is very high in this sample, and the prevalence of overweight/obesity is relatively low, compared to the national Aboriginal and Torres Strait Islander population. More attention could be given to this, and how ‘nutritional status’ interventions may look different in this setting if the goal is to shift the BMI curve to the right for those currently experiencing underweight, rather than only shifting the BMI curve to the left.

RESPONSE:
This is a very relevant comment, as we had not emphasized the high rates of underweight and its

implications. We have now added discussion about this with thoughts about possible intervention strategies:

“The reasons behind the dual burden of malnutrition, particularly the high rates of underweight in the remote and more disadvantaged communities, are multifactorial and include high food prices, low incomes, overcrowded households and rudimentary cooking facilities [38-39]. Approaches that have been suggested to improve diet in the remote communities include eliminating socioeconomic constraints by reducing prices on fruit and vegetables in the community stores and enhancing nutrition-related consumer education and thus improving food security and self-efficacy to cook. [40] Nutrition education including cooking skills workshops, group education sessions and store interventions have been reported to have some positive effect on obesity in Indigenous communities according to a review study that included both remote and urban communities in Australia [41]. Multi-sector participatory approaches to strengthen food systems in remote Indigenous communities are needed [42] with a special focus on nutrition in the early life.”

6) Consideration in the context of related literature
There are published data on associations (cross-sectional and longitudinal) between weight status and factors including birth weight, age, sex, remoteness and BMI in other Aboriginal and Torres Strait Islander cohorts. I consider that it would be valuable to explore how the findings from this cohort study align with previously published studies, and to identify the novel elements that are gained through this study.
For example, in the introduction, the authors state: ‘Little is known about the association between socioeconomic status and weight status within indigenous populations’ – there is some published evidence on this specific to the Aboriginal and Torres Strait Islander population; perhaps this should be referred to here? The authors refer to indigenous populations more broadly (i.e. internationally) – if this international literature review was conducted, could findings be incorporated into the manuscript where relevant?

RESPONSE: We have now conducted a more extensive literature search and revised the introduction with relevant literature references.

“The effects of socioeconomic factors and remoteness on nutritional status have generally been similar for the Indigenous and the non-Indigenous Australians with obesity being concentrated in urban and less disadvantaged areas. [4,16] To our knowledge, there have been no studies examining the longitudinal development of nutritional status and its associations with socioeconomic factors in very remote regions of Australia, where food insecurity is high [17] and malnutrition and underweight are more common. [18]”

7) Ethical considerations
The patient and public involvement statement is valuable. However, I would be interested to see reporting of the engagement processes that have occurred specific to this paper. E.g. how were Aboriginal and Torres Strait Islander peoples involved in conceptualising, designing, analysing, or interpreting findings from this specific study? How are results from this specific study being disseminated?

RESPONSE:
For this small analysis from the larger dataset of the study, no Indigenous researchers were directly involved. However, Indigenous researchers have been involved in all aspects of the study at each of the follow-ups including investigators, data collection team and local community members employed as research assistants, encouraging and facilitating formal research training. Extensive consultation with expert, Indigenous and cohort reference groups was conducted prior to each follow-up to obtain advice and guidance on contact methods, acceptability of planned procedures and methods of

feedback to individuals and communities. Due to the difficulty in providing individual feedback after the initial visit, feedback is aimed at the community level in remote areas. Updates are published in community newsletters and in the national Aboriginal and Islander Health Worker Journal and provided to local community groups. Presentations are made regularly at local (eg hospital grand rounds, Chronic disease network) and national meetings to disseminate results to policy makers. The study has clear commitment to engaging with Indigenous communities and building Indigenous capacity.

8) Minor points:
Article Summary:
I would be mindful of stating that 'cultural issues' posed a 'logistical challenge' – could you be more explicit about what posed a challenge?

RESPONSE: We have removed the mention about cultural issues, as they did not pose a logistical challenge as opposed to the geographical issues.

9) Introduction:
If comparing the prevalence of underweight in the Aboriginal and Torres Strait Islander population to the prevalence in the non-Indigenous population, please provide the confidence intervals around these estimates so that they can be compared. I would consider reducing the emphasis on comparison to the non-Indigenous population, and increase focus on the Aboriginal and Torres Strait Islander population, including referring to relevant literature from this population (as described above).

RESPONSE: Confidence intervals were not available in the report from the Australian Bureau of Statistics but the difference was statistically significant.

10) I would recommend revising this sentence to be specific to 'birth cohorts', given the existence of several large scale Indigenous cohort studies within Australia alone: 'To date, the study is one of the longest running and largest Indigenous [birth] cohorts in the world'. Alternatively the word 'largest' could be removed.

RESPONSE: This sentence has been modified according to the suggestion.

11) If including a statement that it is not clear what cut-off points are 'most appropriate' for this population, I would consider providing more justification around why a different cut-off point might be 'appropriate'; the need for different cut-off points might be interpreted as implying a biological difference between Aboriginal and Torres Strait Islander and non-Indigenous peoples.

RESPONSE: This is a relevant comment and we have removed that sentence, as we have no reason to believe that a biological difference would exist.

12) Methods:
How were implausible measurements (implausible measurements at one time point, or implausible changes over time) assessed and handled in the analysis?

RESPONSE: The study prides itself on its accurate data. At each follow-up of the study, a small group (3-4) of trained researchers directly measured height and weight using standardized methods despite multiple sites. Weight and height values for participants who had BMI ±4 SD from the mean were manually checked for all follow-ups and no biologically implausible values were found.

13) How many participants were <18 years old? If this is a small group, did the authors consider running a sensitivity analysis excluding these participants given the different BMI cut-offs used?

RESPONSE: In wave 2 all participants were < 18 years old. I wave 3, 123 participants were < 18 years old and 192 were 18 years or older. In wave 4, all participants were > 18 years old. As the group of those individuals aged < 18 years at wave 3 was not small, we believe there is no need to run an additional sensitivity analysis in this setting.

14) Defining area-level disadvantage and remoteness: were the cut-offs for IRISEO used established cut-offs? If not, how were these chosen?

RESPONSE: No well-established cut-offs for IRSEO exist and using eg. tertiles would have resulted in underrepresentation of the participants from less disadvantaged areas as these were much fewer compared to the numbers from more disadvantaged areas. In a previous publication (Juonala et al MJA 2019), cutoffs of 0-40 for least disadvantage, 41-80 for moderate disadvantage, 81-90 for high disadvantage and 91-100 for highest disadvantage have been used. We used the same cut-offs but combined the second and the third group because of small numbers in the moderate group. We have now replaced the actual cut-off ranges with the limits used for clarification

15) How was remoteness defined? Did these measures change over time? How was this taken into account in analysis? E.g. were these treated as time-varying exposures?

RESPONSE: “Remote” was defined as living in a rural community with an Aboriginal council) and all “non-remote” locations were classified as urban. The same definition was used for all the waves. This has been added in the methods.
The main analyses focused on examining the effect of remoteness at birth with later nutritional status and was therefore not treated as a time-varying exposure. We have now added statistics about the changes over time regarding living location in the methods section.

16) Defining birth weight categories based on z-scores: what cut-offs were used? What was the rationale for selection of these cut-offs?

RESPONSE: The categories were <-2, -2 to -1, -1 to +1, +1 to +2 and >+2 according to the cut-offs used in WHO growth charts. We have now added this information in the methods section.

17) Suggest including more information about the age categories used and the justification for this.

RESPONSE: The age categories are a result of the mean ages of the participants during each of the conducted follow-ups, and are reported as such.

18) Results:
Line 27-50: are these results statistically significant? Did the authors explore the interaction between remoteness and SES?

RESPONSE: The results were statistically significant and this has now been clarified in the manuscript.

19) When reporting the results on tracking, I would also be interested to see: of those who were overweight/obese at time X, what % were overweight/obese at time Y? (and the same for underweight.) This would give a sense of the extent of later overweight/obesity (or underweight) that is predicted by child weight status – i.e. what proportion of the total burden of these outcomes is explained by early life weight status.

RESPONSE: This is reported in the results section:

”Of the participants who were affected by overweight/obesity in childhood, 67.6% remained in the same weight status category in adolescence (OR 16.0, P < 0.0001) and 83.8% in adulthood (OR 9.8, P < 0.0001). Of the adolescents affected by overweight/obesity, 86.2% were affected by overweight/obesity as adults (OR 22.5, P < 0.0001). Underweight status was also significantly stable throughout the follow-ups. Of the participants who were underweight in childhood, 76.7% were underweight in adolescence (OR 22.6, P < 0.0001) and 46.7% remained underweight in adulthood (OR 9.8, P < 0.0001). Of underweight adolescents, 83.8% were underweight in adulthood (OR 17.3, P < 0.0001).”

20) Discussion:
Line 32-40, where referring to other studies, it would be valuable to refer to the age group of the cohort in that study (and if it is similar to the age group of the cohort under study). There is evidence of different relationships between socioeconomic measures and weight status for Aboriginal and Torres Strait Islander youth versus older adults.
Lines53-60: where referring to findings from other studies, it is important to clarify whether these are specific to the Aboriginal population or not. If they are not, it would be important to be explicit about this, and to state whether or not the findings are likely to be applicable to the population of interest.
Line 22: ‘Nutrition education has been reported to have some positive effect on obesity’ – can you please provide more information about what was found, what population was studied (e.g. local or national, age group, remoteness – to help understand the likely relevance to the population under study), and the quality of the evidence?
Conclusions about physical activity do not seem to be informed by the findings of this study.

RESPONSE: The age groups are have now been referenced. We have clarified, which studies were conducted in non-Indigenous populations. Information about nutritional education was has been clarified. The remark about physical activity was has been removed.

VERSION 2 – REVIEW

REVIEWER	bert little School of Public health and Information Sciences University of Louisville Louisville, KY USA
REVIEW RETURNED	03-Jan-2020

GENERAL COMMENTS	Dual burden is "Double burden of malnutrition. The double burden of malnutrition is characterised by the coexistence of undernutrition along with overweight and obesity, or diet-related noncommunicable diseases, within individuals, households and populations, and across the lifecourse." The authors use this phrase in different contexts. It should be defined and used that one way. p.8, line 17: expiration should be clarified to mean breathing expiration. Methods should mention the sampling bias of higher participation in disadvantaged areas. It is unclear why SES indicators are not used as covariates in logistic regression to adjust for differences.

	P.11, line 22: when differences are discussed, the "direction" of the significant difference should be defined. Several other places, not just this place in the manuscript. P.12, line 48: what statistic measured "significantly stable"? P.17, line 8: dual burden takes on yet another meaning here.... P.17, line 55: fruit SHOULD BE fruits. P.18, line 25: is this sentence trying to say that :those from disadvantaged areas were more likely to participate than those from better-off areas?" UNCLEAR P.19, lines 5-10: Are you implying interventions for females only? Sounds like it.

REVIEWER	Katherine Thurber NCEPH, RSPH, ANU, Australia
REVIEW RETURNED	15-Dec-2019

GENERAL COMMENTS	The authors have comprehensively responded to the comments received. I have two remaining points for consideration: 7) Thank you for clarifying the engagement processes embedded in this study. It would be valuable to include in the manuscript any of the dissemination activities planned for this work (i.e. presentations and meetings to local organisations, stakeholders, or governance groups). 19) I realise my original comment may have been unclear. The information that is presented in the manuscript is valuable. In addition, I was interested to see what % of overweight/obesity at the adult age point was predicted by overweight/obesity at the child age point, and at the adolescent age points. This gives an indication of the proportion of overweight/obese adults that have been overweight/obese from these early ages, and the proportion of overweight/obese adults that were not overweight/obese at the earlier ages (i.e. who developed overweight/obesity later). Currently the results only show prospective tracking.

VERSION 2 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: bert little
Institution and Country: School of Public health and Information Sciences, University of Louisville, Louisville, KY, USA
Please state any competing interests or state ‘None declared’: None

Please leave your comments for the authors below
Dual burden is "Double burden of malnutrition. The double burden of malnutrition is characterised by the coexistence of undernutrition along with overweight and obesity, or diet-related noncommunicable diseases, within individuals, households and populations, and across the lifecourse."

The authors use this phrase in different contexts. It should be defined and used that one way.

Response: Thank you for the comment. We have specified the definition of the term in the introduction
”The dual burden of malnutrition defined as the coexistence of obesity and underweight within individuals, households or populations is a phenomenon commonly seen in low and middle-income countries but less so in high-income countries.[1]”

p.8, line 17: expiration should be clarified to mean breathing expiration.
Response: We have changed the term ’expiration’ to ’exhalation’, which is less ambiguous.

Methods should mention the sampling bias of higher participation in disadvantaged areas.
Response: This is detailed in the results and discussion sections.

It is unclear why SES indicators are not used as covariates in logistic regression to adjust for differences.

Response: Thank you for this comment. There is no good reason not to adjust for SES in the main analyses. We have therefore rerun the analyses with IRSEO category as a confounder. This changed the results only slightly and tables 2 and 3 as well as the corresponding numbers in the results section have been edited accordingly.
“Regression analyses were adjusted for age at follow-up, sex and time between compared follow-ups as well as IRSEO category that was used as a proxy for socioeconomic status.”

P.11, line 22: when differences are discussed, the "direction" of the significant difference should be defined. Several other places, not just this place in the manuscript.

Response: We have now clarified the direction of the differences in the following sentences in the results section:
“The differences in weight status over the course of the three follow-ups were significant with rates of underweight decreasing and rates of overweight/obesity rising (P<0.0001 for underweight and overweight/obesity for both sexes).”
“The changes in WHtR over the course of the three follow-ups were significant with rates of low WHtR decreasing and rates of high WHtR rising (P=0.002 for males and P=0.03 for females for low WHtR and P<0.0001 for both sexes for high WHtR).”

P.12, line 48: what statistic measured "significantly stable"?
Response: This sentence has been modified:
”Underweight status also showed significant tracking throughout the follow-ups.”

P.17, line 8: dual burden takes on yet another meaning here....
Response: The sentence has been specified to refer to the dual burden within the population.
“The dual burden of malnutrition within the population and the urban-remote differential in nutritional status has been previously described in the cohort at an average age of 25 years.[34] “

P.17, line 55: fruit SHOULD BE fruits.
Response: This has been corrected.

P.18, line 25: is this sentence trying to say that :those from disadvantaged areas were more likely to participate than those from better-off areas?" UNCLEAR
Response: Yes, this is what the sentence was trying to say and has now been clarified.

“There were significant geographical differences between participants and non-participants, with non-

participants being more often from urban and less disadvantaged areas."

P.19, lines 5-10: Are you implying interventions for females only? Sounds like it.
Response: Thank you for this remark. We are implying interventions for the whole population but wish to emphasize the gender perspective here. The last two sentences were modified to better reflect to the need of interventions for both females and males.
"The differences in central adiposity between males and females that seem to arise after childhood indicate a need for targeted and successfully timed approaches in dietary interventions. The high prevalence of underweight across all age groups requires special attention in the process of improving nutritional health overall in the remote Indigenous communities. "

Reviewer: 2
Reviewer Name: Katherine Thurber
Institution and Country: NCEPH, RSPH, ANU, Australia
Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below
The authors have comprehensively responded to the comments received. I have two remaining points for consideration:

7) Thank you for clarifying the engagement processes embedded in this study. It would be valuable to include in the manuscript any of the dissemination activities planned for this work (i.e. presentations and meetings to local organisations, stakeholders, or governance groups).
Response: Findings reported in this publication will form part of the dissemination process detailed in the methods section.
As detailed
'Updates are published in community newsletters and in the national Aboriginal and Islander Health Worker Journal and provided to local community groups, stakeholders and governance groups.'

19) I realise my original comment may have been unclear. The information that is presented in the manuscript is valuable. In addition, I was interested to see what % of overweight/obesity at the adult age point was predicted by overweight/obesity at the child age point, and at the adolescent age points. This gives an indication of the proportion of overweight/obese adults that have been overweight/obese from these early ages, and the proportion of overweight/obese adults that were not overweight/obese at the earlier ages (i.e. who developed overweight/obesity later). Currently the results only show prospective tracking.

Response: Thank you for clarifying this. We indeed misunderstood the comment in the first review. This is a great remark and we have now added these numbers in the results section:
"Conversely, of the participants who were overweight/obese at Wave-4, only 28.4% had been overweight/obese already at Wave-2 and 45.9% at Wave-3."
"Of the participants who had a high WHtR at Wave-4, 24.2% had a high WHtR already at Wave-2 and 49.7% at Wave-3."