

## PEER REVIEW HISTORY

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### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Determining barriers and facilitators to engagement for families in a family-based multicomponent healthy lifestyles intervention for children and adolescents: a qualitative study
<b>AUTHORS</b>	Wild, Cervantée; Rawiri, Ngauru; Willing, Esther; Hofman, Paul; Anderson, Yvonne

### VERSION 1 – REVIEW

<b>REVIEWER</b>	Elissa Jelalian The Miriam Hospital Weight Control & Diabetes Research Center; Alpert Medical School of Brown University, Providence, RI, USA
<b>REVIEW RETURNED</b>	18-Mar-2020

<b>GENERAL COMMENTS</b>	<p>This manuscript reports information from a qualitative interview-based study of past users of a multi-component healthy lifestyle intervention for families. The paper is well-written and addresses the important question of understanding barriers and facilitators to initial and sustained program engagement. Another strength is the purposeful sampling procedure which allowed for perspectives from families with varying levels of engagement. There are, however, some concerns with the manuscript as detailed in the following.</p> <p>(1) The authors reference a previous publication that focused on surveys conducted with the same study population focused on how previous experiences of healthcare influence engagement with health services. Further clarification is needed regarding how the information included in the present study differs from these published findings.</p> <p>(2) Additional information is needed regarding some of the study procedures. In particular, it would be helpful to know the specific interview questions that were asked as well as any guidelines regarding follow-up prompts as well as the length of interviews.</p> <p>(3) The authors used a stratified random sampling procedure to solicit input from a broad range of participants. It is not clear whether the analysis of interviews was undertaken separately for these groups. It would seem that identifying themes within these groups could be a unique contribution of this research.</p> <p>(4) I appreciate the information reported in the COREQ checklist. It would also be helpful to include a brief statement regarding some of the information included in the checklist within the body of the manuscript. In particular, information regarding the number of participants solicited from each of the four recruitment categories would be helpful. This could potentially be integrated into Table 1.</p> <p>(5) As noted above, it would be informative to better understand unique responses from participants in each of the four recruitment categories. The authors note at the start of the Conclusion that differences were found between participants who were engaged</p>
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	highly and those who did not engage. However, the basis for this conclusion is not clear.
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<b>REVIEWER</b>	Natalie Tyldesley-Marshall Institute of Applied Health Research, College of Medical and Dental Sciences, University of Birmingham, Birmingham, B15 2TT, UK
<b>REVIEW RETURNED</b>	31-Mar-2020

<b>GENERAL COMMENTS</b>	<p>I really enjoyed reading this. It's a well-written paper and I found it very easy to follow – from clear rationale to methods, the findings and where they came from.</p> <p>My revisions are only minor:</p> <p>Abstract “was identified as a mitigator these three themes” is missing a word.</p> <p>P8 Capitalising “table 1” consistently. (Sometimes named “Table One”) Reference needed for thematic analysis. (Though this sounds more like a version of framework analysis, and if so, provide reference and update abstract and methods.)</p> <p>P9 Results starts with “sixty-four”, then 74. Be consistent throughout. (And P29 “eleven” and “13”.) “Program” misspelt.</p> <p>P19 I was unclear what was meant by sentence with “growth trajectories”.</p> <p>P21 CEKW should be CW</p> <p>P29 You mention purposive sampling here, then randomising. Earlier only randomising mentioned. Should mention purposive sampling earlier to clarify.</p> <p>P30 Derivation of themes could be elaborated by a line or so.</p>
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<b>REVIEWER</b>	Kerstin Hein Center for Pediatric Palliative Care University Children's Hospital Munich Germany
<b>REVIEW RETURNED</b>	02-Apr-2020

<b>GENERAL COMMENTS</b>	<p>Dear authors,</p> <p>I very much liked the introduction of your work. It is very clear and well written. The programme, the context, the research question and the relevance of your work are easy to understand. I also liked the respect you show along the whole manuscript towards the Maori culture.</p> <p>However, the manuscript has some weaknesses regarding the description of the methods and the presentation of results.</p> <p>Design and methods seem to be appropriate to the research question, but need to be better described. I have to admit that I am not acquainted with Kaupapa Maori theory and therefore I miss an</p>
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	<p>explanation about the connection between the Kaupapa Maori theory and qualitative research. What are the reasons for choosing qualitative methods? Is your work grounded on a certain methodological orientation like participatory research or phenomenology? Can Kaupapa Maori be understood as a methodological theory or is it more like a paradigm?</p> <p>At page 11 (patient and public involvement statement) you provide information about the development of research questions, which could lead to the interpretation that this study has a participatory approach. Is this true? Then, it would be helpful to acknowledge it in the design of the study.</p> <p>The structure of the chapter on methods should be revised: Page 9, paragraph 2: The conduction of interviews should be described under "data collection". Page 10, paragraph 1: it makes more sense to locate this information under "design".</p> <p>Data collection should provide more extended information about the interviews. Did you use a certain kind of interview? Can you provide the interview guideline? How long were the interviews? How many persons were present during the interviews? This information appears later on in a table, but it would be more useful in the description of data collection as it makes a difference, if you are interviewing a whole family or just one parent or a child.</p> <p>Page 10, Analysis: I miss the name and description of the specific method used for data analysis. Thematic Analysis according to whom? Or is it content analysis? It would be also helpful to specificate the version of MAXQDA.</p> <p>Participation of children: What are the reasons to set the age limit of participation of children and adolescents at 11 years? Where there any children or adolescents in the sample or was there a complete lack of their voices? If children participated, were they interviewed in the presence of their parents or did they have a separate conversation?</p> <p>In the results, I would suggest the revision of domain 2. What are the real barriers of participation: Age, gender or genetics? Or rather the fear of stigmatisation or trivialisation of obesity on the part of parents? Or making the problem visible due to the invitation to participate in the programme? Is it how professionals communicate the programme?</p> <p>I would be also interested in knowing if there are differences between the various groups of participants (for example, according to the level of engagement). And who exactly is responsible for the disengagement: Is it the child or the adolescent or the parents? Are there any differences between the answers of mothers and fathers?</p> <p>Considering the importance of multiple stressors (domain 1), it would be helpful to include in the demographics some information of the socio-economic background of participants. It would also be important to include the age range of participants.</p> <p>Finally, I noticed that the table reveals a high participation of mothers, which certainly affects the results. But gender only seems to be discussed in terms of children and adolescents but not in terms of the actual participants. It would improve the quality of the manuscript if you also include a discussion on the gender of participants and its effect on the results.</p>
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## VERSION 1 – AUTHOR RESPONSE

### Reviewer: 1

Reviewer Name: Elissa Jelalian

Institution and Country: The Miriam Hospital Weight Control & Diabetes Research Center; Alpert Medical School of Brown University, Providence, RI, USA

Please state any competing interests or state 'None declared': None declared

This manuscript reports information from a qualitative interview-based study of past users of a multi-component healthy lifestyle intervention for families. The paper is well-written and addresses the important question of understanding barriers and facilitators to initial and sustained program engagement. Another strength is the purposeful sampling procedure which allowed for perspectives from families with varying levels of engagement. There are, however, some concerns with the manuscript as detailed in the following.

We thank the reviewer for the time invested in providing feedback, which has strengthened the quality of this manuscript.

(1) The authors reference a previous publication that focused on surveys conducted with the same study population focused on how previous experiences of healthcare influence engagement with health services. Further clarification is needed regarding how the information included in the present study differs from these published findings.

Thank you for highlighting this. The survey was limited by its size and the lack of participants who declined involvement with the intervention. However, it did give an initial indication that previous experiences of healthcare influenced subsequent engagement with health services. Participants did not elaborate on this further in the open-text portion of the survey, so it was deemed important to explore further in the in-depth interviews. The information included in the present study further extrapolates these findings and includes participants who declined intervention.

We have amended the last paragraph of the introduction for clarity, shown below:

“Previous evaluation of the experiences of Whānau Pakari participants and their caregivers has shown the programme to be a positive and beneficial experience for those involved, emphasising the importance of connectedness, knowledge-sharing and self-determination, the collective journey alongside other families and programme deliverers, and the importance of a non-judgemental, respectful environment.<sup>15</sup> These findings were limited by the survey’s relatively small sample size and the lack of representation from participants who declined intervention. Therefore, the objective of the present study was to understand barriers and facilitators to initial attendance and ongoing retention in the Whānau Pakari programme.”

(2) Additional information is needed regarding some of the study procedures. In particular, it would be helpful to know the specific interview questions that were asked as well as any guidelines regarding follow-up prompts as well as the length of interviews.

Thank you for this suggestion. We have included an interview schedule guide as a supplementary file, along with guidelines regarding follow-up prompts. We have also amended the methods section to include more specific details around the interview procedure:

“The semi-structured interviews were approximately 30-60 minutes in duration and conducted by CW and NR together where possible (see supplementary file for interview schedule).”

(3) The authors used a stratified random sampling procedure to solicit input from a broad range of participants. It is not clear whether the analysis of interviews was undertaken separately for these groups. It would seem that identifying themes within these groups could be a unique contribution of this research.

Thank you for raising this point. The analysis was undertaken for all participants together. The recruitment of four groups of participants with varying levels of attendance were to ensure variation within the sample, to increase the diversity of participants and the range of experiences. It was clear from our initial appraisal of the data that the degree to which participants engaged with the programme was on a continuum rather than fitting neatly into discrete categories. We have included a paragraph in the methods section to acknowledge this approach:

“It became clear from our initial appraisal of the data that the degree to which participants engaged with the programme was on a continuum rather than fitting neatly into discrete categories. Therefore, the groups have been analysed together, noting where there may be key differences according to the degree of engagement.”

(4) I appreciate the information reported in the COREQ checklist. It would also be helpful to include a brief statement regarding some of the information included in the checklist within the body of the manuscript. In particular, information regarding the number of participants solicited from each of the four recruitment categories would be helpful. This could potentially be integrated into Table 1.

Thank you for this suggestion. We have included more information regarding the number of participants solicited from each category in the table notes in Table 1, and have updated the COREQ checklist to reflect this, providing more detail as to the reasons for non-participation.

(5) As noted above, it would be informative to better understand unique responses from participants in each of the four recruitment categories. The authors note at the start of the Conclusion that differences were found between participants who were engaged highly and those who did not engage. However, the basis for this conclusion is not clear.

Thank you for raising this point. As mentioned in (3) above, unique themes were not developed from the analysis for each group; rather, the extent to which participants were affected by each domain determined their engagement level. We recognise that a unique contribution of this research is the inclusion of participants with a wide range of levels of engagement. The analysis instead generated

unifying domains with differential effects which lead to varying levels of engagement. In addition to the methods section, we have amended the beginning of the results section for clarity, which now states:

“Three major interacting domains and subthemes affecting participant engagement are described in panel 1 in participants’ own words. A fourth domain of respectful, compassionate care was identified as a mediator, which was able to mitigate the effect of the first three themes. Unique themes according to level of engagement with the programme were not generated. While each domain was prevalent in participant accounts across all recruitment categories, the extent to which a domain affected each group determined engagement.”

We have also inserted clarifying sentences throughout the results section to distinguish where the extent of each domain was great enough to prevent engagement: for example, in Domain 1:

“Both initial attendance and ongoing retention were affected by a lack of participant resources, even if participants expressed a desire to attend. Participants who engaged with Whānau Pakari and other services despite the impact of adverse stressors appeared to have more resources, and thus were less affected by this domain.”

In Domain 3:

“Past experiences of healthcare influenced participants’ opinions, perceptions and behaviour in relation to seeking care again. This was a multidimensional phenomenon, acting across both weight and ethnicity. If participants had had negative experiences in the health system in relation to their weight or ethnicity, then they were less willing to engage with Whānau Pakari and other health services. This mostly affected participants who declined further input after their referral or who discontinued after one assessment.”

## **Reviewer: 2**

Reviewer Name: Natalie Tyldesley-Marshall

Institution and Country: Institute of Applied Health Research, College of Medical and Dental Sciences, University of Birmingham, Birmingham, B15 2TT, UK

Please state any competing interests or state ‘None declared’: None declared

I really enjoyed reading this. It’s a well-written paper and I found it very easy to follow – from clear rationale to methods, the findings and where they came from.

We thank the reviewer for their comments, which have improved the quality of the manuscript.

My revisions are only minor:

Abstract

“was identified as a mitigator these three themes” is missing a word.

Thank you for identifying this. We have amended to:

“A fourth domain, respectful and compassionate healthcare, was identified as a mitigator of these three themes, facilitating participant engagement despite previous negative experiences.”

P8

Capitalising “table 1” consistently. (Sometimes named “Table One”)

Thank you; we have amended this on page 8 to “Table 1”.

Reference needed for thematic analysis. (Though this sounds more like a version of framework analysis, and if so, provide reference and update abstract and methods.)

P9

Thank you for identifying this. We agree this method is closely related to framework analysis as described by Gale and colleagues (2013), a useful and interesting approach to undertaking qualitative analysis. In this case we did not undertake the charting stage, instead following the general method set out by Braun and Clarke for thematic analysis. We have provided the reference for this method of thematic analysis as Braun & Clarke (2006, 2019) on page 9:

“Interview transcripts were coded and analysed inductively in MAXQDA (2018),<sup>22</sup> according to Braun and Clarke’s method for reflexive thematic analysis, which aligned well with the reflexivity and awareness of researcher theoretical positioning required of research informed by Kaupapa Māori Theory.”

Results starts with “sixty-four”, then 74. Be consistent throughout. (And P29 “eleven” and “13”.)

Thank you for identifying this. We have amended the numbers to numerical format throughout the manuscript and COREQ checklist to ensure consistency.

“Program” misspelt.

We have ensured that “programme” is spelt according to UK and NZ English throughout.

P19

I was unclear what was meant by sentence with “growth trajectories”.

Thank you for raising this point. Growth trajectories refers to the expected pattern of growth without intervention. We have clarified this on page 19, which now states:

“There appears to be a disconnect between the focus on early life intervention due to the expected growth trajectories of young children with overweight or obesity into obesity in adolescence and adulthood,<sup>31 32</sup> and the concerns and priorities of parents with young children.

P21

CEKW should be CW

Thank you; this has been corrected.

P29

You mention purposive sampling here, then randomising. Earlier only randomising mentioned. Should mention purposive sampling earlier to clarify.

Thank you for raising this point. The reference to purposive sampling was made in error. We have amended sections 10 and 11 of the COREQ checklist to more clearly reflect the sampling method, which was stratified random sampling from the pool of eligible potential participants. This now states:

“We identified eligible potential participants who fit the criteria, and then stratified these participants into groups by engagement level and ethnicity. We then randomised the list of potential participants in each group, and then contacted each participant one by one (stratified random sampling). Participants were recruited by telephone and interviewed in person over a six-month period from June to December 2018.”

P30

Derivation of themes could be elaborated by a line or so.

We have amended the COREQ checklist item 26 to state:

“Themes were derived from the data according to Braun and Clarke’s 2006 and 2019 method for reflexive thematic analysis.”

**Reviewer: 3**

Reviewer Name: Kerstin Hein

Institution and Country: Center for Pediatric Palliative Care, University Children's Hospital Munich, Germany

Please state any competing interests or state ‘None declared’: None declared

Dear authors,

I very much liked the introduction of your work. It is very clear and well written. The programme, the context, the research question and the relevance of your work are easy to understand. I also liked the respect you show along the whole manuscript towards the Māori culture.



However, the manuscript has some weaknesses regarding the description of the methods and the presentation of results.

We thank the author for their appraisal of our manuscript and thoughtful comments, which have improved the quality of the manuscript.

Design and methods seem to be appropriate to the research question, but need to be better described. I have to admit that I am not acquainted with Kaupapa Māori theory and therefore I miss an explanation about the connection between the Kaupapa Māori theory and qualitative research. What are the reasons for choosing qualitative methods? Is your work grounded on a certain methodological orientation like participatory research or phenomenology? Can Kaupapa Māori be understood as a methodological theory or is it more like a paradigm?

Thank you for raising this point. We have clarified this section to provide more explanation about Kaupapa Māori Theory, which is a methodological approach to research which makes space for a Māori worldview and centres Māori voice and experience. It challenges the dominant research paradigms and methodologies which have historically harmed and marginalised Indigenous communities, including Māori. While it has similarities with participatory action research, Kaupapa Māori research focuses on Māori values, people, history and contemporary realities. Some understand Kaupapa Māori as a methodological theory; others understand it as more of a paradigm. Our study was an application of the methodological principles as an approach to research. We have amended this section to provide more clarity and further references for those unfamiliar with this approach.

“In NZ, health research is required to be responsive to the needs and diversity of Māori.<sup>16</sup> The study design and research approach was informed by Kaupapa Māori methodological principles. Kaupapa Māori Theory is a methodology which resists persistent power imbalances and the continued use of cultural deficit theory (attributing poor health to something inherent to a ‘culture’) to explain inequities between Māori and non-Māori,<sup>16 17</sup> and is aligned with a social and structural determinants of health framework.<sup>18</sup> As a methodological approach, Kaupapa Māori research centres Māori voice and experience, and prioritises understanding people within their contexts and whānau (families).<sup>18</sup> It was hoped that this approach would reduce many of the known barriers to research participation for Indigenous peoples, and enable participants to engage positively in the research process.<sup>19</sup> While Kaupapa Māori research can use both quantitative and qualitative methods, in this study, a qualitative research design was chosen in order to ensure that priority was given to ensuring the voices and experiences of Māori participants were understood in this study.”

At page 11 (patient and public involvement statement) you provide information about the development of research questions, which could lead to the interpretation that this study has a participatory approach. Is this true? Then, it would be helpful to acknowledge it in the design of the study.

Thank you for highlighting this point. Although the study design was indeed informed by previously voiced experiences of participants, the participants themselves were not actively involved in the study design or development of research questions, so we do not feel it is appropriate to claim this as a form of participatory research. We have amended this statement to provide clarity as how research questions were developed:

“The research questions were informed by the experiences of participants voiced, unsolicited, during clinical assessment during the previous trial and in previous focus group research.<sup>14</sup>”

The structure of the chapter on methods should be revised: Page 9, paragraph 2: The conduction of interviews should be described under "data collection". Page 10, paragraph 1: it makes more sense to locate this information under "design".

Thank you for this suggestion. We have moved the paragraph on the conduction of interviews to the section described under "data collection" and have moved the paragraph "patient and public involvement statement" to the "design" section.

Data collection should provide more extended information about the interviews. Did you use a certain kind of interview? Can you provide the interview guideline? How long were the interviews? How many persons were present during the interviews? This information appears later on in a table, but it would be more useful in the description of data collection as it makes a difference, if you are interviewing a whole family or just one parent or a child.

Thank you for this comment. We have now included an interview schedule as a supplementary file and have amended this section to include more information about the interviews. This now states:

"The semi-structured interviews were approximately 30-60 minutes in duration and conducted by CW and NR together where possible. NR led the interviews with Māori families when appropriate. Interviews took place in the participant home or alternative locations chosen by the participant (including a hospital, participant workplaces, and a community library) in order to minimise inconvenience and travel barriers. Most interviews were undertaken with one participant (the parent or caregiver) but a portion included two or more family members, including children (Table 1). A *koha* (gift, donation or contribution) was offered to participants in acknowledgement of their time and as a sign of reciprocity for the information shared."

Page 10, Analysis: I miss the name and description of the specific method used for data analysis. Thematic Analysis according to whom? Or is it content analysis? It would be also helpful to specificate the version of MAXQDA.

Thank you for raising this point. We have amended the section to refer to reflexive thematic analysis according to Braun and Clarke (2006) and elaborated on further in their more recent 2019 work. We have also specified the version of MAXQDA. This section now states:

Interview transcripts were coded and analysed inductively in MAXQDA (2018),<sup>22</sup> according to Braun and Clarke's method for reflexive thematic analysis, which aligned well with the reflexivity and awareness of researcher theoretical positioning required of research informed by Kaupapa Māori Theory. CW developed the coding matrix with peer review from EW, coded the interview data, and identified the initial themes. The authors collaborated to finalise the themes and develop the framework. The acknowledgement of different researcher standpoints allowed the authors to debate, challenge, and refine interpretations of the data, thereby developing a more nuanced interpretation of the data.<sup>23</sup> Specifically, the researchers agreed to apply the 'Give-Way' rule if there was disagreement over the interpretation of the data concerning Māori participants, with the final decision involving cultural interpretation of Māori participants' experiences passing to a Māori researcher.<sup>18 25 26</sup>

Participation of children: What are the reasons to set the age limit of participation of children and adolescents at 11 years? Were there any children or adolescents in the sample or was there a complete lack of their voices? If children participated, were they interviewed in the presence of their parents or did they have a separate conversation?

Thank you for raising this point. We made the decision to limit participation of children and adolescents to 11 years and older as this was deemed an appropriate age for understanding the concepts of barriers and facilitators of engagement. Although we initially intended to interview families, with children alongside their parents/caregivers, most parents/caregivers decided not to include their children or were unable to as the interview was held during school hours (at the request of the parent/caregiver). This has been included as a limitation.

Five interviews included a child or adolescent participant, who were interviewed in the presence of their parents. We have updated the Table 1 notes to include this information, and have also amended the limitations section to state:

“The main limitation of this study was the lack of child and adolescent voice with regards to their experiences with Whānau Pakari, as only five interviews included the child or adolescent as a participant. While it was intended to conduct interviews with families, many parents at recruitment were reluctant to involve their children due to the sensitivity of material discussed or were unable to involve them due to timing issues. This meant that children’s experiences have mainly been explored through their parents’ accounts, rather than through their own voice.”

In the results, I would suggest the revision of domain 2. What are the real barriers of participation: Age, gender or genetics? Or rather the fear of stigmatisation or trivialisation of obesity on the part of parents? Or making the problem visible due to the invitation to participate in the programme? Is it how professionals communicate the programme?

Thank you for raising this point and identifying these relevant barriers. The inclusion of age, gender and genetics as sub-categories were to demonstrate the specific ways that societal beliefs around weight and body size manifested differently for participants, according to these characteristics. These barriers, especially the fear of stigmatisation, are also relevant to domain 3, which further demonstrates the interconnectedness of domains. We have removed the sub-headings of age, gender and genetics from the body of the manuscript in order to more clearly report domain 2 as intended and avoid the reduction of the barriers into discrete characteristics. We have also included an opening paragraph under the subheading ‘Domain 2’ to signify this:

“Societal norms and beliefs around weight and body size led to the minimisation of obesity and the fear of stigmatisation for participants (see also Domain 3). These manifested differently according to the age, gender and according to the perceived role of genetics in obesity, and resulted in lower engagement. An exception was participant beliefs around perceived genetic propensity towards obesity, which in some cases led to higher participant engagement.”

We have also clarified the section of the results section about genetic propensity to obesity, to emphasise that lower or higher engagement in this case was a result of the perceived role of genetics in obesity (we agree, genetics themselves are not the barrier to engagement).

“Overweight and obesity was often associated with perceived genetic propensity to obesity by participants. This was sometimes specifically linked to ethnicity, and specifically that Māori and Pacific Island peoples are ‘naturally big’. A perceived familial propensity towards overweight resulted in participants reportedly acting in two ways: either they did not want to engage because they felt that there was no point, given they perceived their weight to be genetic (panel 1), or they were compelled to engage more in order to counteract their genetics.”

I would be also interested in knowing if there are differences between the various groups of participants (for example, according to the level of engagement). And who exactly is responsible for the disengagement: Is it the child or the adolescent or the parents? Are there any differences between the answers of mothers and fathers?

Thank you for raising this point, also raised by reviewer 1 and addressed in points 3 and 5.

Participant data pointed to the effect of the domains on the family unit, rather than specifically being barriers for either the parent or the child; hence, we have not delineated who is responsible for the disengagement. This is in alignment with Kaupapa Māori approaches to research which is interested in participants within their contexts and whānau (family).

For the small number of male parents who participated, answers did not appear to differ from female parents’ answers. This has been noted in the manuscript in the discussion section (as per your final point of feedback).

Considering the importance of multiple stressors (domain 1), it would be helpful to include in the demographics some information of the socio-economic background of participants. It would also be important to include the age range of participants.

Thank you for this suggestion. Unfortunately, we do not have the age range of participants, but have included in the demographics section a note that the sample included parents, grandparents, caregivers, as well as a small number of children/teens. We have also identified the range of socioeconomic backgrounds in the sample:

“74 interviews were conducted (out of a potential cohort of 74) with families who had varying levels of engagement, across a six-month period from June to November 2018 (76 participants in total) (Table 1). Half of the interviews were with Māori families (families with a Māori child who had been referred to the service), including interviews with non-Māori parents of a Māori child. Participants included parents, grandparents, other caregivers, and the children/adolescents themselves (n=5), and were from a range of socioeconomic backgrounds (deciles 1-10 of the 2013 NZ Index of Deprivation).<sup>25</sup> Full details of interview recruitment rate and reasons for non-participation are included in the COREQ checklist (Appendix One).”

Finally, I noticed that the table reveals a high participation of mothers, which certainly affects the results. But gender only seems to be discussed in terms of children and adolescents but not in terms of the actual participants. It would improve the quality of the manuscript if you also include a discussion on the gender of participants and its effect on the results.

Thank you for this observation. We agree this is a limitation of the study. We attempted to overcome some of this limitation by asking participants if their partner's views differed from their own but acknowledge this still may have affected the results. Much of the literature also only discusses the effect of child/adolescent gender, rather than the gender of the parent, in terms of reasons for engagement or disengagement. We have added this into the limitations section of the discussion, which now states:

“The main limitation of this study was the lack of child and adolescent voice with regards to their experiences with Whānau Pakari, as only five interviews included the child or adolescent as a participant. While it was intended to conduct interviews with families, many parents at recruitment were reluctant to involve their children due to the sensitivity of material discussed or were unable to involve them due to timing issues. This meant that children’s experiences have mainly been explored through their parents’ accounts, rather than through their own voice. In addition, previous literature has largely focused on the effect of child/adolescent gender rather than parent gender on perceived barriers to engagement.<sup>10</sup> In our study, the majority of participants were mothers or female caregivers, which may have affected the results. Lastly, it is possible that participants were discretionary in what they chose to share; however, the disclosure of extremely personal and sensitive experiences suggests that any researcher-participant power dynamics were overcome by steps the interviewers took to mitigate this difference (see COREQ checklist).”

#### VERSION 2 – REVIEW

<b>REVIEWER</b>	Elissa Jelalian The Miriam Hospital Weight Control & Diabetes Research Center, Warren Alpert Medical School of Brown University, USA
<b>REVIEW RETURNED</b>	26-Jun-2020

<b>GENERAL COMMENTS</b>	This is a revised version of a previously submitted paper that includes qualitative data from users of a multi-component healthy lifestyle intervention for families in New Zealand. The authors have been very responsive to the initial reviews and the resulting manuscript is strengthened. I have the following minor comments. (1) Lack of information regarding demographics of the participants should be identified as a study limitation. (2) On this read of the manuscript, it struck me that the authors report that written informed consent was obtained from all study participants. However, this would not have been relevant to the small number of children and teens who were enrolled in the study. Please clarify the process for obtaining assent from these participants as relevant. (3) I appreciate the authors’ rationale for not evaluating relevant themes across participants of differing levels of engagement. However, understanding which factors are at play at different levels of engagement/ points of contact could be quite valuable in informing specific strategies used by health care providers to facilitate engagement. Perhaps this could be noted as an area for future research.
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<b>REVIEWER</b>	Kerstin Hein Center for Pediatric Palliative Care, University Children's Hospital Munich, Germany
<b>REVIEW RETURNED</b>	29-Jun-2020

<b>GENERAL COMMENTS</b>	All previous comments were properly addressed. Design and methods are now clear and transparent and the presentation of results more comprehensive and easier to understand. The quality of the manuscript has clearly improved. From my point of view there is no need for further revisions.
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## VERSION 2 – AUTHOR RESPONSE

### Reviewer: 1

Reviewer Name: Elissa Jelalian

Institution and Country: The Miriam Hospital Weight Control & Diabetes Research Center, Warren Alpert Medical School of Brown University, USA

Please state any competing interests or state 'None declared': None declared

This is a revised version of a previously submitted paper that includes qualitative data from users of a multi-component healthy lifestyle intervention for families in New Zealand. The authors have been very responsive to the initial reviews and the resulting manuscript is strengthened. I have the following minor comments.

We thank the reviewer for the additional feedback provided, which has further strengthened the quality of this manuscript.

(1) Lack of information regarding demographics of the participants should be identified as a study limitation.

Thank you for this suggestion. We have amended the limitation section to include this, which now states:

The main limitation of this study was the lack of child and adolescent voice with regards to their experiences with Whānau Pakari, as only five interviews included the child or adolescent as a participant. While it was intended to conduct interviews with families, many parents at recruitment were reluctant to involve their children due to the sensitivity of material discussed or were unable to involve them due to timing issues. This meant that children's experiences have mainly been explored through their parents' accounts, rather than through their own voice. In addition, previous literature has largely focused on the effect of child/adolescent gender rather than parent gender on perceived barriers to engagement.<sup>10</sup> In our study, the majority of participants were mothers or female caregivers, which may have affected the results. While this study included a range of participants from a variety of different backgrounds (table 1), it lacks specific participant demographic information such as age, socioeconomic status and education level. Lastly, it is possible that participants were discretionary in what they chose to share; however, the disclosure of extremely personal and sensitive experiences suggests that any researcher-participant power dynamics were overcome by steps the interviewers took to mitigate this difference (see COREQ checklist).

(2) On this read of the manuscript, it struck me that the authors report that written informed consent was obtained from all study participants. However, this would not have been relevant to the small

number of children and teens who were enrolled in the study. Please clarify the process for obtaining assent from these participants as relevant.

Thank you for identifying this. We have amended the manuscript in several places to state that assent was obtained along with proxy parental consent on their child's behalf. This has been amended in the following sections:

Study design, last paragraph:

Ethical approval for the Whānau Pakari Barriers and Facilitators study was granted by Central Health and Disability Ethics Committee (NZ) (17/CEN/158/AM01). Written informed consent was obtained from all study participants, or informed assent with proxy parental consent in the case of the child and adolescent participants aged under 16 years.

Data collection, last paragraph:

Informed consent (or assent with proxy parental consent in the case of the child and adolescent participants aged under 16 years) was obtained to record, transcribe and analyse participant data. All participant information was anonymised. Participant ethnicity for both the parent/caregiver and child was confirmed at the time of the interview by using the NZ Census 2006 ethnicity question.<sup>23</sup> All interviews were audio-recorded and independently transcribed. Participants were offered their transcripts to review for accuracy and acceptability.

Ethics committee approval statement:

Ethical approval for the Whānau Pakari Barriers and Facilitators study was granted by Central Health and Disability Ethics Committee (NZ) (17/CEN/158/AM01). Written informed consent was obtained from all study participants, or informed assent with proxy parental consent in the case of the child and adolescent participants aged under 16 years.

(3) I appreciate the authors' rationale for not evaluating relevant themes across participants of differing levels of engagement. However, understanding which factors are at play at different levels of engagement/ points of contact could be quite valuable in informing specific strategies used by health care providers to facilitate engagement. Perhaps this could be noted as an area for future research.

Thank you for this feedback. We have amended the last paragraph of the conclusion to signify this as a future area of research:

In conclusion, this study found that much of the difference between Whānau Pakari participants who engaged highly and those who did not engage appeared to be due to the degree to which participants were affected by the impact of factors at the system and societal levels. Focusing purely on weight in multicomponent interventions does not acknowledge the complexity of contemporary family life. However, family-based multidisciplinary intervention programmes such as Whānau Pakari are an opportunity to acknowledge the wider societal challenges affecting achievement of healthy lifestyle change. Health professionals and providers can engage in respectful and compassionate care to help counteract past negative experiences of healthcare. Referral pathways for healthy lifestyle change programmes need to be as flexible as possible to remove any barriers to engagement, and referrers need to develop a deeper understanding of the importance of the referral conversation in relation to weight. Future research should focus on specific strategies to facilitate engagement at different points of contact with family-based multidisciplinary healthy lifestyle services. Respectful, compassionate care is critical to enhanced retention in programmes, and ongoing engagement in healthcare services overall.

Reviewer: 3

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Please state any competing interests or state 'None declared': None declared

All previous comments were properly addressed. Design and methods are now clear and transparent and the presentation of results more comprehensive and easier to understand. The quality of the manuscript has clearly improved. From my point of view there is no need for further revisions.

Thank you for your feedback and for the time invested in peer reviewing this manuscript.