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Title	Effectiveness of weight maintenance strategies in adult populations treated for obesity: a systematic review with meta-analyses
Authors	Leslea Peirson, Donna Fitzpatrick-Lewis, Donna Ciliska, Muhammad Usman Ali, Parminder Raina, Diana Sherifali
Reviewer 1	**Cintia Curioni**
Institution	Faculdade Arthur Sá Earp Neto, Nutrition
General comments	Overall, it is an interesting paper. Since worldwide, there has been an increase in rates of obesity, it is essential to synthesize the literature to assess the effectiveness of available interventions for the treatment of obesity in the maintenance of weight loss. There are other similar systematic reviews on the topic and it must be clear what are the strengths of the present review. Main problems with the manuscript: Page 4, Lines 18-37 – Introduction 1- The authors commented that other systematic reviews with similar topic were done, but not reinforce why is important to carry out this one. What are the strengths? Page 4, Line 54 – Search Strategy 2- It should be mentioned that the search was limited to English and French. 3- A detail in search strategy: It seems that the strategy designed by United States Preventive Services Task Force included adverse effects… it is not necessary for your purpose, but it will not interfere in the results… Page 5, Line 25 – PICOS Statement 4- I would be interesting if the authors provided a key question that is being addressed, not only list the PICOS components 5- The setting is "Canadian primary-care" – since the existing obesity intervention / strategies could be used in any population, and the search was not focused on Canadian, it is not necessary. Page 5, Line 46 - Study Selection, Quality Assessment and Data Abstraction 6- Was it considered any period of intervention? And for maintenance? Any period was evaluated? Page 6, Line 27 – Data analysis 7- The authors evaluated "the longest available data point in the maintenance phase". It is different to group the weight loss maintained at 6 months with that at 36 months. It would be appropriate to group the same periods, since it is known that as long as the period, more difficult is to maintain the weight loss. The period of intervention could also influence the found results. Page 7, lines 18 / 32 (results) 8- "After full-text screening eight studies (11 papers)" – 3 studies were duplicates? 9- The organization of the paper is not very clear. A systematic review should be as clear as possible. The e-table 1 is a complement of the Table 1, with additional information, very important, and the way that is formatted does not allow an easy comparison between the included studies. There is also repeated information. 10- The risk of bias information is also crucial to be in e-file. 11- Table 2 – there so many footnote. A lot of repeated information. A Table is a complement of the text. The results of risk of bias and GRADE evidence should be summarized and described in the results section. It is a fundamental part of a systematic review. Page 7, line 44 (results: Maintaining Weight) 12- The figure 2 is "polluted". It would be better to put the numbers of mean difference on the other side of the graph. 13- It was mentioned (line 49) "In all studies except one [18], participants in both groups gained weight during the maintenance phase, with less weight gain in the intervention group than in the control". The only study described with weight loss during the maintenance phase is Hill, 1999 and not Hauptman, 2000 (18).

	Page 10, line 46 (comparison with Other Studies) 14- It was mentioned "Another meta-analysis of 11 studies…" – the reference of study should be added. It should also be mentioned in the introduction. Once again, if there are other systematic reviews, the "novelty" should be reinforced.
Reviewer 2	**Jennifer Black PhD RD**
Institution	Registered Dietician
General comments	Thank you for the opportunity to review the paper entitled Effectiveness of Weight Maintenance Strategies in Adult Populations Treated for Obesity: A Systematic Review with Meta-analyses. Note, I do not have specific expertise related to conducting meta-analyses or content expertise specifically related to weight maintenance approaches following obesity treatment. Overall, I found this paper to be well written with clearly described methods, which appear to have been thoughtfully applied. My comments largely focus on areas that could benefit from increased clarification mostly regarding language that may be unclear to some readers. Major Comments (or relatively important questions regarding clarification of terms): 1) Magnitude of differences/clinical significance- It would be helpful if more context and interpretation was provided regarding the magnitude of differences between intervention and control conditions. • For example, little information in the text describes the total average weight re-gained after weight loss treatment (during the maintenance programs). Is a difference of 1.44 kg a lot or a little? What percent of total weight re-gain is that? • The RR of 1.33 is provided for the >5% weight loss outcome, but is never interpreted in terms of magnitude. Is a 33% greater risk of maintaining a 5% weight loss a lot or a little? What is the absolute risk in these studies? Table 3 notes the absolute RR is 12.37% (but that is not described in the text). Is that considered clinically significant? 2) Clarifications: • P8 line 3: how was "marginal significance" defined? • P9 line 15 –says "no effect" was seen – by the difference was -1.39 (and not zero), so perhaps you mean no significant difference was seen. It would help to clarify that the effect was not significant compared to groups that included behavioural intervention (just not orlistat), rather than true controls. 3) Sensitivity analyses- page 6 – it's not clear what is meant here by sensitivity analyses? Are you using this to mean that different weight-related outcomes were examined? 4) Limitations- one of the main limitations of the existing studies seems to be that the orlistat studies include dietary behaviour change in the control group. Except for the two studies with behavioural interventions, few studies compared dietary components to a true control. It therefore seems worth mentioning in the limitations that there were very few studies to draw on to make a conclusion about the role of dietary components alone (although the two behavioural studies are suggestive of benefits). 5) Figures 2/3: The formatting forest plots made it difficult to tell which values fell in which columns. E.g. for the Hiss study, you can't tell where the mean ends and the s.d. begins. (suggest more space or lines between columns). The number of significant digits also seems inconsistent (are 4 digits after the decimal helpful for mean differences and CIs)? A footnote or clearer title would help a reader understand what the mean (and SD) were referring to (i.e. I believe this is mean kg of weight re-gained in Fig. 2) – but if the figure is meant to stand alone a reader couldn't tell that. Minor Comments: 1) Keywords: Your keywords seem to overlook some of the major focus of the study (i.e. no mention of weight maintenance) while focussing on family medicine, general practice and primary care. The studies reviewed don't sound like they were really carried out by general practitioners in family practice – so these terms may be a little misleading 2) Abstract: The search described in the abstract sounds like papers were only reviewed if they were conducted from 2010-2013; but that wasn't the case given the inclusion of the US Preventative Services Task Force references. 3) P5 Line 48: "reviewed in duplicate" is a confusing term – since it could mean that the same reviewer reviewed the abstract twice. 4) Areas in need of clarification: • P 5 Line 39 – how was "minimal component" defined?

	• P 6 Line 4-6: how did you determine if settings were generalizable to Canadian primary care? • Is there room in the text to describe how "risk of bias" was determined and what kinds of biases were identified that downgraded the quality ratings (I realize these are more carefully laid out the appendices but warrant a few words directly in the text). 5) Consistency of terms: Table 1 uses the term "comparator"; but is this what you are calling "control" everywhere else in the paper? 6) Language around direction of differences – I had difficulty understanding/interpreting the negative values of the reported mean differences (e.g. p8 line 46 what does a -1.44 kg change mean here). Instead of reporting the negative mean difference, perhaps you could just say in clearer language that intervention groups regained 1.44 kg less compared to controls (which you in following sentences), since the negative value at fist sounds like you are reporting a weight loss. 7) Wording suggestions in introduction, page 4: Line 7: the "health related problems" of overweight or obesity – would perhaps be more clearly stated as the health related "risks associated with". Line 11: suggest adding the word "greater" as in "obese adults are at greater risk" given that everyone is at some risk of these outcomes. Line 13: I think the word "problem" should be replaced with something like "prevalence" as it is the rates of obesity that are increasing over the past 25 years, not the "problems" per se (or perhaps the magnitude of the problem). 8) Last sentence of paragraph 1 on page 4: Can you clarify what you mean by "weight loss maintenance continues to be a conundrum" and provide supporting references. Who exactly is facing the conundrum (patients, practitioners, weight maintenance programs)? Moreover, it might help readers to have more context about the nature of the problem (i.e. how many people generally keep weight off after weight loss interventions and how much weight is typically regained?)
Author response	Reviewers' Comments (Curioni) 1. The authors commented that other systematic reviews with similar topic were done, but not reinforce why is important to carry out this one. What are the strengths? AR#17 The last sentence of the second paragraph in the introduction section addresses the reviewer's concerns: "To that end, the identification of weight maintenance interventions that sustain and prolong weight loss benefits is needed, as weight-loss maintenance continues to be a conundrum for individuals and health care providers and presents an increasing burden on the health care system." Furthermore, the authors added a Strengths sub-heading and more detail to the conclusion section of the paper to highlight the methodological strengths of the paper. 2. It should be mentioned that the search was limited to English and French. AR#18 See author response #7 3. A detail in search strategy: It seems that the strategy designed by United States Preventive Services Task Force included adverse effects… it is not necessary for your purpose, but it will not interfere in the results… AR#19 It is not clear what revision the reviewer is suggesting, if any. We noted that this review was conducted as part of a larger review considering the benefits and harms of adult overweight/obesity treatment interventions. Harms were not reviewed for this weight loss maintenance review. 4. I would be interesting if the authors provided a key question that is being addressed, not only list the PICOS components AR#20 A research question was added to the end of the PICOS Statement section. 5. The setting is "Canadian primary-care" – since the existing obesity intervention / strategies could be used in any population, and the search was not focused on Canadian, it is not necessary. AR#21 See response #6. 6. Was it considered any period of intervention? And for maintenance? Any period was evaluated? AR#22 No conditions were put on duration of prior intervention or weight maintenance intervention duration. Interventions in the studies that met our criteria ranged from 6 to 36 months and all studies reported immediate post intervention weight outcomes. 7. The authors evaluated "the longest available data point in the maintenance phase". It is different to group the weight loss maintained at 6 months with that at 36 months. It would be appropriate

	to group the same periods, since it is known that as long as the period, more difficult is to maintain the weight loss. The period of intervention could also influence the found results. AR#23 In our analyses studies were combined based on intervention focus, recognizing the heterogeneity in follow up. Considering the limited evidence for weight maintenance, all studies of various durations were considered and we used the longest available data point, which in every study was the immediate post intervention assessment. We appreciate the reviewer's comment. However, due to a very small number of included studies, further subgrouping apart from intervention focus was not possible. 8. "After full-text screening eight studies (11 papers)" – 3 studies were duplicates? AR#24 The search found 8 studies, 2 of which had multiple publications (Champagne had 2 papers; Rickel had 3 papers). The 8 refers to the number of studies, the 11 refers to the number of papers. 9. The organization of the paper is not very clear. A systematic review should be as clear as possible. The e-table 1 is a complement of the Table 1, with additional information, very important, and the way that is formatted does not allow an easy comparison between the included studies. There is also repeated information. AR#25 We arranged the paper to flow more clearly. Additional tables have been added to the manuscript rather than e-files. 10. The risk of bias information is also crucial to be in e-file. AR#26 This has been included in the manuscript. 11. Table 2 – there so many footnote. A lot of repeated information. A Table is a complement of the text. The results of risk of bias and GRADE evidence should be summarized and described in the results section. It is a fundamental part of a systematic review. AR#27 This table was revised. See response #16. 12. The figure 2 is "polluted". It would be better to put the numbers of mean difference on the other side of the graph. AR#28 It is not clear what "polluted" means to inform a revision. We used RevMan to produce the forest plots; this is the format the software generates. As per other reviewer comments we have "cleaned up" the figure by removing extra decimals. 13. It was mentioned (line 49) "In all studies except one [18], participants in both groups gained weight during the maintenance phase, with less weight gain in the intervention group than in the control". The only study described with weight loss during the maintenance phase is Hill, 1999 and not Hauptman, 2000 (18). AR#29 The reviewer is correct. We referenced the wrong reference in the previous version. Thank you for pointing this out. The error has been corrected. 14. It was mentioned "Another meta-analysis of 11 studies…" – the reference of study should be added. It should also be mentioned in the introduction. Once again, if there are other systematic reviews, the "novelty" should be reinforced. AR#30 The reference was added to the end of the statement. The following statement in the beginning of the paragraph speaks to the emerging literature (i.e. novelty): The findings from this review parallel the emerging literature on the effectiveness of weight maintenance programs. Reviewer Comments (Black) 1. Magnitude of differences/clinical significance- It would be helpful if more context and interpretation was provided regarding the magnitude of differences between intervention and control conditions. *For example, little information in the text describes the total average weight re-gained after weight loss treatment (during the maintenance programs). Is a difference of 1.44 kg a lot or a little? What percent of total weight re-gain is that? *The RR of 1.33 is provided for the >5% weight loss outcome, but is never interpreted in terms of magnitude. Is a 33% greater risk of maintaining a 5% weight loss a lot or a little? What is the absolute risk in these studies? Table 3 notes the absolute RR is 12.37% (but that is not described in the text). Is that considered clinically significant? AR#31 We rephrased the results of binary outcomes to quantify the magnitude of effect. The discussion

	section identifies that the maintenance of modest weight loss (5%) is beneficial. The conundrum of weight loss, weight maintenance and what is considered ideal; is still debated in the literature. Text was added to the future implications section to address this issue. Recognizing the conundrum of the increasing overweight/obesity rates in Canada, long-term strategies are needed to address weight loss maintenance and what is considered effective weight loss maintenance (i.e. weight loss amount over what duration). 2. Clarifications * P8 line 3: how was "marginal significance" defined? *P9 line 15 –says "no effect" was seen – by the difference was -1.39 (and not zero), so perhaps you mean no significant difference was seen. It would help to clarify that the effect was not significant compared to groups that included behavioural intervention (just not orlistat), rather than true controls. AR#32 The word 'marginal' was removed to avoid confusion. The authors added the following language "no significant effect". 3. Sensitivity analyses- page 6 – it's not clear what is meant here by sensitivity analyses? Are you using this to mean that different weight-related outcomes were examined? AR#33 As noted in the methods section, by sensitivity analyses we mean subgrouping based on intervention type i.e. behavioral and pharmacological. 4. Limitations- one of the main limitations of the existing studies seems to be that the orlistat studies include dietary behaviour change in the control group. Except for the two studies with behavioural interventions, few studies compared dietary components to a true control. It therefore seems worth mentioning in the limitations that there were very few studies to draw on to make a conclusion about the role of dietary components alone (although the two behavioural studies are suggestive of benefits). AR#34 The reviewer makes a good suggestion here; further text is added to describe the challenge of a true control group without dietary support. Conversely, some control groups received dietary counseling and/or information, above and beyond usual care, which may not reflect usual practice. 5. Figures 2/3: The formatting forest plots made it difficult to tell which values fell in which columns. E.g. for the Hiss study, you can't tell where the mean ends and the s.d. begins. (suggest more space or lines between columns). The number of significant digits also seems inconsistent (are 4 digits after the decimal helpful for mean differences and CIs)? A footnote or clearer title would help a reader understand what the mean (and SD) were referring to (i.e. I believe this is mean kg of weight re-gained in Fig. 2) – but if the figure is meant to stand alone a reader couldn't tell that. AR#35 The other reviewer also mentioned formatting issues for the forest plots. If accepted for publication the editorial formatting process helps with smoothing out some of the reviewer's concerns. The title of Figure 2 was revised to increase clarity. 6. Keywords: Your keywords seem to overlook some of the major focus of the study (i.e. no mention of weight maintenance) while focussing on family medicine, general practice and primary care. The studies reviewed don't sound like they were really carried out by general practitioners in family practice – so these terms may be a little misleading AR#36 Keywords are not listed in the paper. They may be chosen during the submission process. If so, we will take the reviewers suggestions under consideration when re-submitting the manuscript. 7. Abstract: The search described in the abstract sounds like papers were only reviewed if they were conducted from 2010-2013; but that wasn't the case given the inclusion of the US Preventative Services Task Force references. AR#37 A sentence was added to the abstract to clarify. This process is further described in the text of the Search Strategy section. 8. P5 Line 48: "reviewed in duplicate" is a confusing term – since it could mean that the same reviewer reviewed the abstract twice. AR#38 This is clarified by indicating independently by two people. 9. Areas in need of clarification: •P 5 Line 39 – how was "minimal component" defined? •P 6 Line 4-6: how did you determine if settings were generalizable to Canadian primary care? •Is there room in the text to describe how "risk of bias" was determined and what kinds of biases were identified that downgraded the quality ratings (I realize these are more carefully laid out the appendices but warrant a few words directly in the text). Minimal information is defined as receipt

	of information and an example is provided. AR#39 Comment regarding generalizability was addressed in response #6. The ROB table is now included in the main manuscript. A sentence was added to summarize ratings and risk of bias is mentioned in the limitations section. 10. Consistency of terms: Table 1 uses the term "comparator"; but is this what you are calling "control" everywhere else in the paper? AR#40 Table heading revised as suggested. 11. Language around direction of differences – I had difficulty understanding/interpreting the negative values of the reported mean differences (e.g. p8 line 46 what does a -1.44 kg change mean here). Instead of reporting the negative mean difference, perhaps you could just say in clearer language that intervention groups regained 1.44 kg less compared to controls (which you in following sentences), since the negative value at fist sounds like you are reporting a weight loss. AR#41 Language was added to this pooled treatment effect to improve clarity. 12. Wording suggestions in introduction, page 4: Line 7: the "health related problems" of overweight or obesity – would perhaps be more clearly stated as the health related "risks associated with". Line 11: suggest adding the word "greater" as in "obese adults are at greater risk" given that everyone is at some risk of these outcomes. Line 13: I think the word "problem" should be replaced with something like "prevalence" as it is the rates of obesity that are increasing over the past 25 years, not the "problems" per se (or perhaps the magnitude of the problem). AR#42 We appreciate the editorial suggestions. Revisions to wording have been made accordingly. 13. Last sentence of paragraph 1 on page 4: Can you clarify what you mean by "weight loss maintenance continues to be a conundrum" and provide supporting references. Who exactly is facing the conundrum (patients, practitioners, weight maintenance programs)? Moreover, it might help readers to have more context about the nature of the problem (i.e. how many people generally keep weight off after weight loss interventions and how much weight is typically regained?) AR#43 Additional text was added to reflect the reviewers comments: To that end, the identification of weight maintenance interventions that sustain and prolong weight loss benefits is needed, as weight-loss maintenance continues to be a conundrum for individuals, health care providers and an increasing burden on the health care system