

Peer Review File

Article information: <http://dx.doi.org/10.21037/mhealth-20-154>

Reviewer A

Comment 1: Thank you for the opportunity to review this manuscript that presents findings from a piloted RCT of a physical activity tracker + kinesiologist-led PA intervention vs. kinesiologist-only PA intervention. The authors presented findings concluding that physical activity trackers were feasible to implement among adult family medicine patients with type 2 diabetes, and led to improved cardiometabolic risks.

Reply 1: We would like to thank the reviewer for his/her kind comments. We are grateful to this reviewer for her/his constructive suggestions. We are most appreciative for the detailed and insightful reviews received in response to this submission and we think that our manuscript has been substantially improved throughout this process.

Comment 2: Notable strengths of this study include reaching a priority population of individuals with type 2 diabetes, random assignment, good measurement of cardiometabolic risks, and conducting research in the important setting of family medicine. This work comes across as a very thoughtful effort.

Reply 2: We would like to thank the reviewer for his/her kind comments.

Comment 3: Overall, nearly 6000 words is considerably long, especially for a pilot study manuscript, and substantial revisions throughout for conciseness and organizational clarity would be helpful. Minor but frequently occurring examples include the repetitiveness of stating that adults ages 18-90 are included and that individuals <18 are excluded, and describing the consent process and measures in the intervention section.

Reply 3: We agree with the reviewer and have made changes accordingly. Thank you for your editorial assistance. Consequently, we cut 756 words in our manuscript (originally 5910 words to 5154 words). All the minor changes can be seen in track changes. As suggested, here are the major modifications we made during the revision:

In the Methods section:

- 1) the repetitions in the intervention section (p.8-9 lines 180-198) have been trimmed.
- 2) the detailed explanations on the self-determination theory and one source on the external regulation (p.11 lines 256-258) has been removed. We found that it was not necessary since we are referring to two sources that are sufficiently detailed.

In the Results section:

- 1) steps regression coefficient per day was removed (p.15 line 384) since the average step number weekly is already presented in Table 3.
- 2) cardiometabolic risk variables results (p.16-17 lines 424-440) were cut down since they were a secondary outcome and are reported in Table 1.

In the Discussion section:

- 1) as seen in track changes, we deleted repetitions. The major modifications are in line 450, line 486, line 489, line 511, line 518 and line 531.

Comment 4: The comparator groups are a bit hard to understand at present. It sounds as though the main goal was to test benefits of physical activity trackers, yet a few issues (without sufficient justification/explanation) appear to detract from this: a) a kinesiologist-led PA intervention seems far more than typical “standard care” would include in a family medicine practice, and b) the comparator group also wore pedometers for the first week of intervention, which seems confounding. Of note, it may also help to control for other provider visits during the intervention.

Reply 4: We apologize for the lack of clarity and/or confusion around the comparator groups. This is an important issue.

a) We agree with this reviewer that a kinesiologist-led PA intervention is far more than typical “standard care” in family medicine practice. The intervention of the kinesiologist was necessary in the intervention group to optimize the activity tracker use with a PA promotion intervention and for the questionnaires completion and interpretation. Indeed, it was necessary to be able to attribute changes between the control group and the intervention group to the activity tracker so the kinesiologist intervention was implemented in both groups. This justification was added in the study design (p.7 lines 141-145).

b) We agree with this reviewer when he/she says that the comparator group wore pedometers for the first week can seem confounding. The pedometers were only used to measure the step number

in the control group during the first week to make sure they were sedentary. This clarification was added in the revised manuscript in the physical activity measurements section (p.10 lines 235-239). Also, we removed the pedometer from the material section since it was only used to document the steps of the control group in the first week (baseline) to confirm sedentarity, and it was not part of the comparison (p.8 line 175). Accordingly, we revised Table 3 and decided to include only data from the activity tracker as the inclusion of the data from the pedometer could be confusing (p.29). We also removed the steps of the control group from the Results section (p.15 line 377) and the Discussion section (p.17 line 450 and p.19 line 511) in order to clarify the message.

Comment 5: The discussion section focuses heavily on restating the results (e.g., “no significant difference at baseline in the Godin Leisure-Time Exercise Questionnaire”) rather than presenting the authors’ broader interpretation of the findings, which would be more appropriate and useful.

Reply 5: Thank you for your comment, which is well taken. We fully agree with the reviewer and we deleted the repetitions in track changes directly in the Discussion section. The PA promotion intervention supported by a kinesiologist and the personalized training plan in both groups could explain these increases. Indeed, the participants in both groups were more aware of the PA intensity to reach and had a PA plan to follow. We believe that the higher increment in the intervention group, although not significant, may be partly explained by an increased motivation for PA due to the activity tracker. We added our broader interpretation of the findings in the Discussion section (p.18 lines 489-494).

Comment 6: The conclusion that physical trackers increase motivation and physical activity also does not appear well-supported by the actual data presented.

Reply 6: The reviewer is right and we would like to thank this reviewer for this useful comment. We based our interpretation that activity tracker increases motivation and physical activity by the higher increments in the Godin Leisure-Time Exercise Questionnaire in the intervention group and by 86% of the participants of the intervention group being satisfied of their activity tracker use. Following your comment, we revised our conclusion as: “Our pilot study shows that an activity tracker could be a potential motivation tool to increase physical activity in patients with T2D. Also, the use of an activity tracker improves some cardiometabolic risk variables.” (p.22 lines 623-625).

Reviewer B

Comment 1: The manuscript describes a pilot RCT looking at the use of an activity tracker to improve physical activity and associated health outcomes among individuals with diabetes. The topic seems important, since there is a need to find relatively low-cost interventions that can be scaled to improve the health of those with diabetes. My specific comments are below:

Reply 1: We would like to thank this reviewer for his/her careful review of the manuscript and thinking that our topic is important. We agree with the reviewer that there is a need to find relatively low-cost interventions that can be scaled to improve the health of patients with diabetes. In fact, it is why we supported our patient partner when he came with the idea to add a monitoring

device to document more objectively physical activity that we know can improve the health of patients with diabetes.

Comment 2: My main concern is that given that meta-analyses have already been done on the topic of portable monitoring devices (e.g., Baskerville et al., Qiu et al.), what can be added by a study with endpoints for only 22 participants? Is the main difference only that of a Fitbit versus pedometers or other older monitoring technology? I understand this is a pilot study, but given the small sample size I feel that the authors should do more work to clarify what new information is provided by this study.

Reply 2: We understand the issue raised by the reviewer. The reviewer's main concern was to know what our pilot study adds to the literature. Our study included data on more outcomes than the meta-analyses presented by Baskerville et al. and Qiu et al. First, the meta-analysis of Qiu et al. documented exclusively the use of pedometers while the meta-analysis of Baskerville et al. documented mostly pedometers and a few accelerometers on cardiometabolic variables only and PA while our randomized pilot trial (RCT) combined many outcomes. Few studies have been conducted on activity trackers like the *Fitbit* since it is a newer technology. Second, our study was conducted in a primary care setting and is a RCT pilot study of feasibility in order to conduct a larger scale study in the future. The outcome was oriented on the satisfaction of the activity tracker and the motivation to practice PA as it was assessed by the motivation questionnaire (reasons to do PA) and the Godin Leisure-Time Exercise Questionnaire (PA). The kinesiologist-led PA intervention, which provides a more comprehensive intervention, is also different and a strength. Our study documents for the first time the motivation as assessed by the BREQ questionnaire

version 2 in an activity tracker intervention for patients with T2D. These important elements were added in the Discussion section (p.18 lines 478-484). The pedometer was only used to measure the step number in the control group during the first week to make sure participants were sedentary and we made it clearer in the revised manuscript (p.10 lines 235-239).

Comment 3: In the Methods of both the Abstract (p.3) and the main body of text (p.7), it would be important to make it clearer that the two groups get the same intervention with the exception of the activity tracker. I wasn't sure that was the case until I reached page 9.

Reply 3: We apologized for this lack of clarity as we agree with this reviewer that it is important to make it clearer that the two groups get the same intervention with the exception of the activity tracker. We made changes accordingly by reformulating the Abstract (p.3 lines 54-56) and the Study design section (p.7 lines 139-141).

Comment 4: Although the authors acknowledge and discuss it, the different methods of baseline step measurement (pedometer vs. Fitbit) seems to be a serious limitation for being able to compare whether one arm performed better than the other over time. I agree with the authors that the most likely explanation for the baseline difference in steps is the measuring device, but the differences make it hard for me to conclude that the intervention arm did better over time because of the Fitbit. It may be true, but I'm not confident we can conclude it from the data.

Reply 4: Thank you for your comment, which emphasizes an important issue. We agree with the reviewer that the different methods of baseline step measurements seem to be a serious limitation and that the differences make it hard to conclude that the intervention arm did better over time because of the *Fitbit*. We are glad that the reviewer agrees with us that the most likely explanation for the baseline difference in steps is the measuring device. After revision, we revised Table 3 and removed data of the control group (p.29) since the pedometer was only used to assess the baseline step number and to confirm sedentarity in the control group. We also removed the steps of the control group from the Results section (p.15 line 377) and the Discussion section (p.17 line 450 and p.19 line 511) to avoid confusion regarding this issue. In order to compare both groups at baseline, it would have been better if all participants have had a pedometer for one week, but this could also bring confusion for participants in the intervention group who would have to use 2 different devices. The increment of PA in the Godin Leisure-Time Exercise Questionnaire, although not statistically significant, was higher in the intervention group. We believe that this could be likely explained by an increased motivation for PA due to the activity tracker. Accordingly, we changed our conclusion and it reads: “Our pilot study shows that an activity tracker could be a potential motivation tool to increase physical activity in patients with T2D. Also, the use of an activity tracker improves some cardiometabolic risk variables.” (p.22 lines 623-625).

Other point: While doing the revision, it appeared that there was a notice of retraction (<https://jamanetwork.com/journals/jamanetworkopen/fullarticle/2774324>) on December 18th 2020 on the meta-analysis of Hodkinson et al. cited in the introduction (p.6 line 110). We removed this source and our literature review is still supported by 4 reliable sources.