PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	"Am I really hungry?" A qualitative exploration of patients' experience, adherence, and behaviour change during Hunger Training: A pilot study
AUTHORS	de Bruin, Willemijn; Ward, Aimee; Taylor, Rachael; Jospe, Michelle

VERSION 1 – REVIEW

REVIEWER	Wolfgang Himmel University of Gottingen, Department of General Practice/Family Medicine
REVIEW RETURNED	26-Jul-2019

GENERAL COMMENTS	The paper explores experiences and (self-reported) behaviour changes of obese patients, undergoing a hunger training. The patients were randomised to two groups: a „fingerpricking" group to measure their glucose from a fingerprick sample bevor eating/drinking and a „scanning" group with a sensor inserted under the skin of the arm, so that they could read their glucose level by passing a reader over the arm before they intended to drink or eat something. Although there are some very interesting insights into the patient experiences that may researches, doctors and dieticians better guide to help obese people, I have some serious concerns with the study and the paper: 1. The aim of the paper is not quite clear. The authors mention several objectives at the end of the Introduction, i.e. the experience, adherence and behaviour change after experiencing hunger training, but these aims are located on different levels that require different methods. While open interviews are best suited to talk about experiences, behaviour change will be better captured by observation and / or the analysis of diaries, and "adherence" is first of all a matter of measurement, not of open, unstructured reports. So it should be clear from the very beginning of the paper that we, strictly speaking, only learn about the patient experiences, including their subjective reports on adherence, and some (self-reported) behaviour changes, nothing else. Moreover, the authors suggest at several passages of the paper that the results of their paper will help to inform translation of hunger training from research to practice. Although some of the results are, indeed, interesting enough to improve or adapt hunger training to different settings; however, the authors do not really use this chance. I come back to this point later (see # 12).

	There is another aim of the paper, not mentioned directly, but implicitly running through the study: a comparison between both methods of glucose monitoring (fingerpricking vs. sensor). Again, there are some interesting insights into such differences, as experienced by patients, but the authors should make it either to an explicit aim and then compare both methods far more thoroughly or the comparison should be no longer an issue of the paper. In the first case, the authors should, for example, report much more experiences from the "fingerpricking" group (it seems to me, this group is underrepresented in the paper); in the latter case, the authors could mention this comparison at the end of the paper as a matter of "future research". 2. 'Hunger training', combined with a glucose monitoring, seems to be an interesting approach, given the many frustrating experiences with measures to lose weight over a long time. But hunger training, combined with glucose monitoring is by no means an established method that has proved its efficacy and effectiveness. Apart from an Italian group (refs 13 and 28 in the paper), it is the authors alone who are working with this method (as far as I see). So, the authors should make it absolutely clear that they are applying and investigating a method that still needs rigorous evaluation. However, reading p. 3 (lines 14 ff.) the reader gets the impression that hunger training is a successful and well-studied method with an overwhelming success. This is not the case! And some lines later (25 ff.), the authors themselves have to admit that only about one-third of participants experience a benefit from this method. Again, it is interesting to report about this method, but readers should be adequately informed about the current state of research. 3. I really wonder about the sample of patients. They are obese patients with a BMI of 38, on average. But there are only 3 (!) diabetic patients. On basis of valid epidemiological studies (see, for example https://www.valueinhealthjournal.com/article/S1098-3015(16)31361-4/pdf), we would expect at least 20 percent of diabetic persons. Moreover, reading the study protocol at the end of the paper, a main aim (further aim?) of this study is to avoid that pre-diabetic people develop to diabetic people. And, of course, weight loss is the key. But I wonder again why only a small group of pre-diabetic people (20%) participated in the study. Obviously, the authors did not try to find especially patients from these two groups so that the composition of the sample is not adequate to draw valid conclusions for diabetic and pre-diabetic persons. Even if the authors conclude that participants from these two groups did not report remarkably different experience than the remainder (p. 6, lines 52 ff.), their conclusion is not at all valid on basis of a sample of 3 (!) diabetic persons. There skewed composition of the sample undermines an important aim of the study and should openly discussed. The education level of the sample is no less astonishing. We know from numerous studies that diabetes follows a strong social gradient. Why then so many academics? Perhaps 'hunger training' is a favourite of upper class citizens? This would be no problem, but this should be one of the most important information, and limitations, to know for followers to be successful. 4. As already mentioned, 'adherence' is, first of all, a matter of measurement. But even if it may be interesting to listen to patient reports about adherence, it would be even more interesting to

	know, at the same time, more about their real behaviour and the outcome of this behaviour. So, I wonder why the authors did not tell us how well each participant followed the study protocol and how much the BMI changed after 6 months. Of course, I understand that the authors will publish these results as a paper of its own. Ok, but I am sure it is no problem to disclose this information in the paper under review, too. In this case, we would learn much more about 'adherence'. By the way, quotes in the paper should be followed by more details of the interviewees, not only their group but also age, gender, BMI, education and, once again, adherence to the protocol and changes in BMI. 5. The presentation of the Results section is a bit confusing. The Figure exhibits a different order of themes than the text. Please be more concise and explain in more detail to the reader how you came to your themes and present them then in a transparent way, parallel in text and Figure. 6. I read the participants' quotes with great interest but was a bit disappointed that the authors restricted themselves more or less to a 'labelling' of these quotes instead of trying a more in-depth analysis. Many quotes and many of the analyses remain 'on the surface'. Some of them are typical excuses for eating too much or being obese: social pressure, feeling hungry, lack of flexibility to eat at different times and so on. But is this really the whole truth? Or should we listen more thoroughly to the participants. I wold like to motivate the authors to a second look on the quotes and analyse the participants' thoughts deeper to better understand what their experiences are? 7. Reading the quotes, I was also surprised why, in the end, nobody or only a few showed a critical attitude towards the devices and methods of hunger training. We should, at least, consider that directing and adjusting one´s own behaviour towards an objective measure such as glucose means that one's own feeling and behaviour are sometimes or even often 'wrong'. There is only one passage in the paper (p. 7, lines 44 ff.) where the authors report some patient confusion when glucose levels and own feelings do not correspond. I would recommend the authors to explore these problems in much more detail. To put it in more general terms: The Standards for Reporting Qualitative Research (see p. 20 of your paper) recommend for qualitative research a guiding theory or a research paradigm. I see, besides many others, two such theoretical aspects or approaches: hunger training and glucose monitoring as a form of Bandura's concept of self-efficacy by self-controlling own behaviour and, in sharp contrast, a sort of alienation by being controlled from a medical device. If the authors agree, they should discuss this real and sometimes virtual tension both in the Introduction and the when discussing their results. 8. Since this study analyses patient experiences, the authors should be more reluctant before they draw conclusions from these experiences on the programs' efficiency. For example, on p. 8 (line 32), I have serious doubts that really all participants became aware of non-hungry eating and generally avoided it in the future. If so, I am sure, obesity would be no longer a matter of worldwide concern. By the way, the related quote (lines 33 ff.) is a bit

	confusing; perhaps some smoothing of the quote would be helpful for the reader. 9. Reading some of the quotes, I was not quite sure whether the participants reported a general experience from their former life or whether they wanted to tell us, why—in spite of the hunger training—they had difficulties to follow the program nowadays. See, for example, the quotes on p. 8 (line 58) and p. 9 (lines 3 ff.). Does this happen in spite of the hunger training or are these experiences before the hunger training? Both would be interesting but make a huge difference. The same problem refers on the quotes on p. 9 (lines 7 ff.): Do the participants report experiences as a result of the hunger-training or are these more or less general strategies to cope with hunger and craving in the past? Please clarify. 10. The last aspect mentioned in Table 2 is quite interesting but you should make clear that this is NOT a behaviour change, as suggested by the title of the Table and the subchapter, but a confirmation that a former behaviour, criticized by others, is adequate in the light of the glucose level readings. 11. As already recommended, please be more cautions with your conclusions, especially in the Discussion. Whether or not the participants changed their behaviour, cannot be concluded from their reports. And you should, once again, make clear that hunger-training and your methods are still under examination and evaluation. 12. In contrast, you should much more focus on those aspects of the two methods that may support patients to become more empowered and self-efficient and on those aspects of the two methods that may prevent participants from becoming independent and autonomous. You could also say: a discussion about risks and benefits as well as the characeristics of the sample that may bias the results . And this is exactly what other researchers and practitioners need to be 'informed' as you promised at the end of Introduction. 13. Vice versa, you should focus more on the experiences with the training and not so much comment on the role of family, friends and doctors, issues that are already broadly studied. A minor concern: You mention the interviewers were blinded and an "independent person" was involved. Do you mean the interviewers or the ones who analysed the interviews? In the first case, you would probably avoid a response bias; in the second case you would avoid an interpretation of results according to the study hypothesis. This makes a difference. Please explain this in the Methods section, not only as a strength in the Discussion and give the initials of this independent person, an author, I suppose?

VERSION 1 – AUTHOR RESPONSE

Reviewer comments:

The paper explores experiences and (self-reported) behaviour changes of obese patients, undergoing a hunger training. The patients were randomised to two groups: a "fingerpricking" group to measure their glucose from a fingerprick sample before eating/drinking and a "scanning" group with a sensor

inserted under the skin of the arm, so that they could read their glucose level by passing a reader over the arm before they intended to drink or eat something. Although there are some very interesting insights into the patient experiences that many researchers, doctors and dieticians better guide to help obese people, I have some serious concerns with the study and the paper:

1. The aim of the paper is not quite clear. The authors mention several objectives at the end of the Introduction, i.e. the experience, adherence and behaviour change after experiencing hunger training, but these aims are located on different levels that require different methods. While open interviews are best suited to talk about experiences, behaviour change will be better captured by observation and / or the analysis of diaries, and "adherence" is first a matter of measurement, not of open, unstructured reports. So it should be clear from the very beginning of the paper that we, strictly speaking, only learn about the patient experiences, including their subjective reports on adherence, and some (self-reported) behaviour changes, nothing else. Moreover, the authors suggest at several passages of the paper that the results of their paper will help to inform translation of hunger training from research to practice. Although some of the results are, indeed, interesting enough to improve or adapt hunger training to different settings; however, the authors do not really use this chance. I come back to this point later (see # 12).
There is another aim of the paper, not mentioned directly, but implicitly running through the study: a comparison between both methods of glucose monitoring (fingerpricking vs. sensor). Again, there are some interesting insights into such differences, as experienced by patients, but the authors should make it either to an explicit aim and then compare both methods far more thoroughly or the comparison should be no longer an issue of the paper. In the first case, the authors should, for example, report much more experiences from the "fingerpricking" group (it seems to me, this group is underrepresented in the paper); in the latter case, the authors could mention this comparison at the end of the paper as a matter of "future research".

We acknowledge the reviewer's points regarding our aim and have reworded it to focus on participant experience (lines 82-87). The abstract has also been updated accordingly (lines 26-27). We have indicated in the manuscript where any differences were apparent between participants randomized to the different methods of glucose monitoring – although essentially this was restricted to the "glucose measuring" theme. We now also have a more equitable distribution of quotes from both groups throughout the paper, having added five additional quotes from fingerprickers (lines 237-241; Table 2; lines 320-322).

2. 'Hunger training', combined with a glucose monitoring, seems to be an interesting approach, given the many frustrating experiences with measures to lose weight over a long time. But hunger training, combined with glucose monitoring is by no means an established method that has proved its efficacy and effectiveness. Apart from an Italian group (refs 13 and 28 in the paper), it is the authors alone who are working with this method (as far as I see). So, the authors should make it absolutely clear that they are applying and investigating a method that still needs rigorous evaluation. However, reading p. 3 (lines 14 ff.) the reader gets the impression that hunger training is a successful and well-studied method with an overwhelming success. This is not the case! And some lines later (25 ff.), the authors themselves must admit that only about one-third of participants experience a benefit from this method. Again, it is interesting to report about this method, but readers should be adequately informed about the current state of research.

We acknowledge the reviewer's point and have altered our introduction accordingly (lines 67-70).

3. I really wonder about the sample of patients. They are obese patients with a BMI of 38, on average. But there are only 3 (!) diabetic patients. On basis of valid epidemiological studies (see, for example https://apc01.safelinks.protection.outlook.com/?url=https%3A%2F%2Fwww.valueinhealthjournal.com%2Farticle%2FS1098-3015(16)31361-

4%2Fpdf&data=02%7C01%7Cmichelle.jospe%40otago.ac.nz%7C0d5e26cf09714b47fbf308d748ab715e%7C0225efc578fe4928b1579ef24809e9ba%7C1%7C0%7C637057773559319849&sdata=YT225wM32cHSWXjbtb8nuD6twkHrvJFUzYDSqmnQKcc%3D&reserved=0), we would expect at least 20 percent of diabetic persons. Moreover, reading the study protocol at the end of the paper, a main aim (further aim?) of this study is to avoid that pre-diabetic people develop to diabetic people. And, of course, weight loss is the key. But I wonder again why only a small group of pre-diabetic people (20%) participated in the study. Obviously, the authors did not try to find especially patients from these two groups so that the composition of the sample is not adequate to draw valid conclusions for diabetic and pre-diabetic persons. Even if the authors conclude that participants from these two groups did not report remarkably different experience than the remainder (p. 6, lines 52 ff.), their conclusion is not at all valid on basis of a sample of 3 (!) diabetic persons. There skewed composition of the sample undermines an important aim of the study and should openly discussed. The education level of the sample is no less astonishing. We know from numerous studies that diabetes follows a strong social gradient. Why then so many academics? Perhaps 'hunger training' is a favourite of upper class citizens? This would be no problem, but this should be one of the most important information, and limitations, to know for followers to be successful.

We acknowledge the lack of diversity in our sample and have modified the discussion to make this clearer (lines 365-372) and have included the reference indicated by the reviewer (line 368). We actively tried to recruit those with prediabetes and did manage to recruit a significant number. However, few diabetics were interested in participating. Given the small number of participants involved, we have removed any reference to differences between pre-diabetic and diabetic experiences. We have also clarified the limitation of carrying out this research in a university town in our "Strengths and limitation of study" section after the abstract (line 54).

4. As already mentioned, 'adherence' is, first of all, a matter of measurement. But even if it may be interesting to listen to patient reports about adherence, it would be even more interesting to know, at the same time, more about their real behaviour and the outcome of this behaviour. So, I wonder why the authors did not tell us how well each participant followed the study protocol and how much the BMI changed after 6 months. Of course, I understand that the authors will publish these results as a paper of its own. Ok, but I am sure it is no problem to disclose this information in the paper under review, too. In this case, we would learn much more about 'adherence'. By the way, quotes in the paper should be followed by more details of the interviewees, not only their group but also age, gender, BMI, education and, once again, adherence to the protocol and changes in BMI.

As the quantitative outcomes of our RCT are currently under review it is difficult to include any of these findings in the current paper without creating issues about potential plagiarism. However, for the reviewer's interest we found that scanning glucose with a monitor increased the frequency of glucose measurement compared with self-monitoring blood glucose using fingerpricking. We also found that both the number of glucose measurements and the number of booklet entries predicted clinically relevant weight loss. Both groups lost a mean of approximately 4 kg at 6 months, and we have included this in the results section (lines 157-159). Our findings suggest that either tool is effective for learning to eat according to hunger using the HT program. However, scanning with a monitor achieves better adherence to glucose measurement without sacrificing outcome results.

As stated in our aims, the purpose of this qualitative project was to explore the personal experiences of the participants, with the hope of better advising health professionals who work with patients one-on-one. For purposes of anonymity, we limited the information given after each quote as to not inadvertently identify someone.

5. The presentation of the Results section is a bit confusing. The Figure exhibits a different order of themes than the text. Please be more concise and explain in more detail to the reader how you came to your themes and present them then in a transparent way, parallel in text and Figure.

Figure 3 was designed as a chronological rationale for Hunger Training constructed from the identified qualitative themes. We agree with the reviewer that the figure did not align with the order of presenting our results. For the sake of clarity, we have now excluded Figure 3.

6. I read the participants' quotes with great interest but was a bit disappointed that the authors restricted themselves more or less to a 'labelling' of these quotes instead of trying a more in-depth analysis. Many quotes and many of the analyses remain 'on the surface'. Some of them are typical excuses for eating too much or being obese: social pressure, feeling hungry, lack of flexibility to eat at different times and so on. But is this really the whole truth? Or should we listen more thoroughly to the participants. I would like to motivate the authors to a second look on the quotes and analyse the participants' thoughts deeper to better understand what their experiences are?

As outlined in our methods, we aimed to develop codes and collate themes in order to identify the saturation of ideas put forth by our participants, and this is what we have reported. The analysis did not include mere labelling of quotes, as all coded content was also discussed at considerable length during a two-day meeting. A sentence has been added to clarify this (lines 144-147). Unfortunately, further analysis would require more in-depth follow-up interviews, which we are unable to perform at this time.

7. Reading the quotes, I was also surprised why, in the end, nobody or only a few showed a critical attitude towards the devices and methods of hunger training. We should, at least, consider that directing and adjusting one´s own behaviour towards an objective measure such as glucose means that one's own feeling and behaviour are sometimes or even often 'wrong'. There is only one passage in the paper (p. 7, lines 44 ff.) where the authors report some patient confusion when glucose levels and own feelings do not correspond. I would recommend the authors to explore these problems in much more detail. To put it in more general terms: The Standards for Reporting Qualitative Research (see p. 20 of your paper) recommend for qualitative research a guiding theory or a research paradigm. I see, besides many others, two such theoretical aspects or approaches: hunger training and glucose monitoring as a form of Bandura's concept of self-efficacy by self-controlling own behaviour and, in sharp contrast, a sort of alienation by being controlled from a medical device. If the authors agree, they should discuss this real and sometimes virtual tension both in the Introduction and the when discussing their results.

We reported patient confusion with lack of correspondence between glucose levels and feelings of hunger briefly because it simply did not come up that often in our interviews. This could be due to participants trusting a device over their own feelings of hunger or satiety. We take the reviewers point about describing our guiding theory. We approached this project from the theoretical perspective of phenomenology, seeking commonalities (and differences) of the lived experience of hunger training within this group. We then applied the method of grounded theory to our data analysis. We have now outlined that process more clearly in the abstract (lines 30-31), introduction (lines 80-82), and in the discussion (lines 330-332).

8. Since this study analyses patient experiences, the authors should be more reluctant before they draw conclusions from these experiences on the programs' efficiency. For example, on p. 8 (line 32), I have serious doubts that really all participants became aware of non-hungry eating and generally avoided it in the future. If so, I am sure, obesity would be no longer a matter of worldwide concern. By the way, the related quote (lines 33 ff.) is a bit confusing; perhaps some smoothing of the quote would be helpful for the reader.

We take the reviewer's point that knowledge of hunger does not necessarily lead to sustained action to affect long-term behaviour change, and this has been added to the discussion (lines 347-349). However, based on our analysis of the data collected, indeed almost all participants (meaning between 31 and 37 participants, as described in the methods section) reported that they learned the difference between hungry and non-hungry eating. We have added "reported they" to the sentence in question (line 264), as a qualifier.

Our quotes are all verbatim, and we are unable to adapt them.

9. Reading some of the quotes, I was not quite sure whether the participants reported a general experience from their former life or whether they wanted to tell us, why—in spite of the hunger training—they had difficulties to follow the program nowadays. See, for example, the quotes on p. 8 (line 58) and p. 9 (lines 3 ff.). Does this happen in spite of the hunger training or are these experiences before the hunger training? Both would be interesting but make a huge difference. The same problem refers on the quotes on p. 9 (lines 7 ff.): Do the participants report experiences as a result of the hunger-training or are these more or less general strategies to cope with hunger and craving in the past? Please clarify.

All participant quotes included in the paper were in direct response to our interview questions (see supplementary file for interview guide). The questions posed by this reviewer are interesting points of debate, and we have added to the discussion to that effect (lines 371-375).

10. The last aspect mentioned in Table 2 is quite interesting but you should make clear that this is NOT a behaviour change, as suggested by the title of the Table and the subchapter, but a confirmation that a former behaviour, criticized by others, is adequate in the light of the glucose level readings.

The sentence at the end of Table 1 about being happy to skip breakfast has been deleted.

11. As already recommended, please be more cautions with your conclusions, especially in the Discussion. Whether or not the participants changed their behaviour, cannot be concluded from their reports. And you should, once again, make clear that hunger-training and your methods are still under examination and evaluation.

We have altered our conclusion as requested (lines 347-349).

12. In contrast, you should much more focus on those aspects of the two methods that may support patients to become more empowered and self-efficient and on those aspects of the two methods that may prevent participants from becoming independent and autonomous. You could also say: a discussion about risks and benefits as well as the characteristics of the sample that may bias the results. And this is exactly what other researchers and practitioners need to be 'informed' as you promised at the end of Introduction.

This was a qualitative exploration focused on how participants felt that undergoing hunger training influenced their eating behaviours, and what types of support measures could help to establish healthy eating patterns, for the benefit of participants and practitioners alike. As reported in the results section and laid out in the discussion, both methods of glucose measurement offered some benefit regarding both. Those using scanners described the ease of scanning and how they learned about how different foods affected their glucose, thereby elevating their awareness. Those fingerpricking were more confident that they had established an awareness of hunger and satiety. Finally, both groups had feedback for additional or modified support strategies to enhance hunger training.

13. Vice versa, you should focus more on the experiences with the training and not so much comment on the role of family, friends and doctors, issues that are already broadly studied.

We appreciate that the role of the social environment has been broadly studied for people with diabetes and obese individuals in general. However, in this research we aimed to explore the patient's experiences with specific regard to following the Hunger Training intervention. Whether or not an intervention is successful in real-life is highly dependent on patients' interactions with their social environment and we therefore included this in our semi-structured interviews and this paper, as it will help practitioners with potential implementation by granting enough clinical effectiveness for the intervention.

A minor concern: You mention the interviewers were blinded and an "independent person" was involved. Do you mean the interviewers or the ones who analysed the interviews? In the first case, you would probably avoid a response bias; in the second case you would avoid an interpretation of results according to the study hypothesis. This makes a difference. Please explain this in the Methods section, not only as a strength in the Discussion and give the initials of this independent person, an author, I suppose?

The interviews were conducted by interviewers not known to the participants (WEdB and ALW), to decrease response bias. The analyses were conducted by WEdB, ALW, and MRJ, all of whom were blind to any participant classification. This is now clearly stated (with initials) in the methods section (lines 128-129; 145-147).

VERSION 2 – REVIEW

REVIEWER	Wolfgang Himmel University of Gottingen, Department of General Practice/Family Medicine
REVIEW RETURNED	18-Nov-2019

GENERAL COMMENTS	Thank you for the revision! I still have some minor comments and hope you can consider them for the final version of your paper: 1. The Editor suggested to add "pilot" in the title of the paper. Your proposal sounds somewhat strange (at least, for my non-English ears). What do you think about: "Am I really hungry?" A qualitative exploration of patients' experience, adherence, and behaviour change during a Hunger Training - a pilot study (I cancelled "intervention" since it is not so important in this paper and keeps the title shorter) 2. Although I accept your efforts to ensure anonymity, it should be no problem to add a patient's gender to each quote (I think, gender makes a huge difference, especially in the case of eating and hunger training so that readers will profit from this information). 3. I am afraid your comments on a possible selection bias by your sampling method are not sufficient and underestimate the consequences. Reading lines 366 f., it sounds like the "university town" is the main reason for the skewed distribution of your

	sample to a high degree of education. From all we know, diabetic patients are more often to find among the lower social classes and exactly these folks are more reluctant to undergo active and strong self-responsible trainings. So, not only did you tend to attract the 'false' people, but your training also seems to be less attractive for those who are the biggest problem in diabetes management. In the end, we do not know the definite reasons for the composition of your sample, but you should clearly and strongly discuss these possible reasons and not only refer to the "university town". Readers should be informed that your approach seems to address only a smaller group of the population at risk.

VERSION 2 – AUTHOR RESPONSE

Reviewer 1:

1. *The Editor suggested to add "pilot" in the title of the paper. Your proposal sounds somewhat strange (at least, for my non-English ears). What do you think about:*

   "Am I really hungry?" A qualitative exploration of patients' experience, adherence, and behaviour change during a Hunger Training - a pilot study
   (I cancelled "intervention" since it is not so important in this paper and keeps the title shorter)

The title has been altered as requested.

2. *Although I accept your efforts to ensure anonymity, it should be no problem to add a patient's gender to each quote (I think, gender makes a huge difference, especially in the case of eating and hunger training so that readers will profit from this information).*

Gender has now been added to each quote as requested.

3. *I am afraid your comments on a possible selection bias by your sampling method are not sufficient and underestimate the consequences. Reading lines 366 f., it sounds like the "university town" is the main reason for the skewed distribution of your sample to a high degree of education. From all we know, diabetic patients are more often to find among the lower social classes and exactly these folks are more reluctant to undergo active and strong self-responsible trainings. So, not only did you tend to attract the 'false' people, but your training also seems to be less attractive for those who are the biggest problem in diabetes management. In the end, we do not know the definite reasons for the composition of your sample, but you should clearly and strongly discuss these possible reasons and not only refer to the "university town". Readers should be informed that your approach seems to address only a smaller group of the population at risk.*

We have discussed this limitation in greater detail in our revised manuscript (lines 373-380).

VERSION 3 – REVIEW

REVIEWER	Wolfgang Himmel University of Gottingen, Department of General Practice/Family Medicine
REVIEW RETURNED	28-Nov-2019

GENERAL COMMENTS	Thank you, authors, for the second revision. I look forward to seeing the paper published as it is.