Author's response to reviews

Title: Self-rated Health and Factors Influencing Responses among Young Egyptian Type 1 Diabetes Patients

Authors:

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Author's response to reviews: see over
Dear Dr. Du,

I would like to thank the reviewers for their careful review of my manuscript and the constructive critiques received. I am pleased to re-submit my manuscript, Self-rated Health and Factors Influencing Responses among Young Egyptian Type 1 Diabetes Patients. I have included my responses to the reviewers, underlined the changes in the manuscript, and hope that the manuscript is now acceptable for publication in BioMed Central. I am eager for your decision.

Thanks for your time.

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Reviewer 1:
The author has performed a fairly interesting study examining quality of life among Egyptian children and adolescents with T1DM. The paper is simple, clear and well-structured.

We would like to thank the reviewer for the careful review of the manuscript and the wonderful comments.

Reviewer’s comments:

1) Quality of life is a very broad concept, which can be explained in several ways. The author seems to use health-related quality of life and general quality of life interchangeably. In the title he even mentions 'self-rated health AND quality of life'. From the paper I get the feeling that self-rated health is used as a proxy for quality of life but I am not sure whether I agree with that. The paper could benefit from a clear definition of the concepts that are used in the paper.

We thank the reviewer for this comment; clarifications and corrections have been made throughout the manuscript distinguishing health related quality of life from general quality of life.

2) Related to the above-mentioned comment, there are many ways to examine (health-related) quality of life and in my opinion of these SRH does not provide the most valid answers. I would like the author to add some information on why this measure was selected, so that the reader is convinced of the method.

SRH as a measure of HRQoL has been used extensively in diabetes patient populations and thus enables us to compare our population to other T1DM patient populations. To our knowledge, HRQoL among young Egyptians with diabetes has
not been studied before, and SRH is a quick, practical yet reliable and validated measure that, in our opinion, offers valuable information and provides the foundation for future more detailed studies. We have added this to our subjects and methods section as well as our methodological considerations section.

3) The way the paper is written now implies that the subgroup analyses were performed ad-hoc. I would like to see hypotheses in the methods section that can be tested, and I would like to see a related aim (instead of the very general relationship between QoL and 'various variables').

We agree that the way the manuscript was initially written implies that the subgroup analysis was performed ad-hoc. We have made our hypothesis and aim better defined, thank you for pointing this out.

4) A SRH scale with 5 options does not provide a lot of information. However, in the statistical analysis this limited information is even reduced by grouping the variable. Why not use regression techniques so that you can use all the information you have?

In dichotomizing SRH in our analysis, we follow the lead of Manor et al (17) who observed that dichotomization of SRH response groups does not affect results. We have also used logistic regression analysis. In looking at both ends of the spectrum we were aiming at comparing and contrasting the most pronounced differences between the groups using the intensity sampling method (16).

5) Related to point 4), is the fact that there is no statistical difference found between for example absence/presence of complications not just a result of the study design or power? Is the design suitable to detect these differences?
We agree that absence of a statistical difference may be the result of study design or power; however, by employing regression analysis with a 95% confidence interval, we believe that our findings are to a certain degree reliable. This has been added to the discussion section.

6) You mention a relationship between exercise and duration, was this interaction tested?

We have tested for an interaction effect and have not found one; we have overlooked mentioning this in the earlier version of the manuscript. Furthermore, we have controlled for diabetes duration, along with age and sex, and again saw the same results as regards to regular exercise. This has been clarified in the results section.

7) Is it not possible that poorer quality of life results in less exercise? Why is it concluded the other way around?

We agree that this is possible; however, we believe that would have been the case if other diabetes related variables shown to negatively affect HRQoL (such as presence of complications and poor glycemic control) were different between the two groups. That is to say, one would have expected those in the “poorer” group to have had a significantly higher HbA1C value, more acute and chronic complications and poorer compliance in general, but this difference was not seen.
Reviewer 2:

Again we would like to thank this reviewer for the careful review of our manuscript and comments.

Reviewer’s comments:

Major Compulsory Revisions

1. Subjects and methods: Subjects, paragraph 5. There is very little information on how SRH is measured. There is the need to provide clarification on the information collected to measure SRH and quality of life in the study. SRH and quality of life are multi-dimensional concepts, and although the two are associated they are quite distinct. It is important to justify the measure of quality of life used in the study.

We agree with the reviewer that clarifications were needed. We have added clear definitions to HRQoL and SRH instead of the broad terms used previously. SRH is a valid and rapid measure that is frequently used in diabetes patient population surveys, and so provides a point for comparison with other studies. Although it does not provide detailed information, as outlined in the subjects and methods as well as the methodological considerations section, in our opinion it provides the starting point for further more extensive survey studies.

2. Statistical analysis: this section needs to be revised to enhance understanding of the analyses. Currently it is difficult to make any meaning of the analysis described. It will be helpful to indicate the variables included in the analysis and how each of them is measured, and specify the outcome variable at various stages of the analysis.
We agree with the reviewer that more detail was necessary. We have revised the statistical analysis with the faculty at Cairo University Institute of Statistical Studies and Research. Clarifications and corrections have been made in order to better explain the statistical methods performed. We hope that the current version will provide a better understanding.

3. I am not sure how those who reported their health as 'good' have been handled in the analysis (looks like they have been excluded). If they have been excluded then that leaves about 86 patients for analysis. May be a statistician can judge if this number is sufficient for the analysis; but I am confident about the adequacy of the sample size.

The majority of our subjects (n=38, 30.7%) rated their health as “good”. Although the number is sufficient for analysis, our aim was to further study the subset of intense samples at both sides of the scale for more in depth analysis. In doing so, we follow a well established qualitative research method of intensity sampling, in which we collected cases and then selected two subsets of interest or intensity (but not extreme cases) for in depth analysis. In finding out differences between the groups, we can address health behaviors and possibly change management strategies in the “poorer” health group as well as continue to encourage health behaviors in the “better” health group.

Minor Essential Revisions

1. Consider revising the title of the study. In my opinion the current title does not convey the focus of the analysis and write up.
We agree with the reviewer. The title has been changed accordingly and we hope that the current title better reflects the aim and results of the study.

2. Subjects and methods, paragraph 1: “Our patient population is of a low socioeconomic level, being that our services are offered free of charge or for a very minimal cost.” Two issues are mixed up here. First the socioeconomic level of patients; and second the fact that services are offered free of charge or for very minimal cost. The attempt to infer the socioeconomic level of patients from the fact that services are free or cheap is questionable. It is possible for high socioeconomic level patients to access free or cheap medical services. It is better to separate the two issues. The socioeconomic level of patients can be described without linking it to the cost of the services.

We agree with the reviewer and the assumption has been removed.

3. Subjects and methods, paragraph 2, last sentence (“Additional routine screening for diabetes complications, including screening for microalbuminuria, liver and kidney function tests, lipid profile, as well as screening for associated autoimmune thyroid dysfunction which are done annually.”) does not read well. Suggested modification: “Additional routine screening for diabetes complications include screening for microalbuminuria, liver and kidney function tests, lipid profile, as well as annual screening for associated autoimmune thyroid dysfunction.”

We thank the reviewer for the suggested modification which has been made and underlined in the manuscript.
Discretionary Revisions

1. Introduction, paragraph 2, line 5: write ‘lab’ in full

2. Subjects and methods, paragraph 1, line 6: replace ‘(10))’ with ‘(10)’

3. Discussion, paragraph 1, line 8: “……financial stressors, and being they are the primary caregivers” should read “……financial stressors, and being the primary caregivers”

4. Discussion, paragraph 3: Consider joining the last two sentences into one sentence.

All the above discretionary revisions have been made and underlined in the manuscript; we thank the reviewer for this meticulous review.