Author’s response to reviews

Title: Use of Social Adaptability Index to Explain Self-care and Diabetes Outcomes

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Dear Dr. Tahrani,

Thank you for the opportunity to resubmit our revised manuscript based on comments made by the reviewers. We addressed all concerns raised by the reviewers and have responded to each below. We hope that these changes are acceptable and look forward to the publication of the manuscript.
Maureen Monaghan (Reviewer 1): This manuscript extends the literature on social determinants of health by examining associations among the Social Adaptability Index and diabetes self-care and health outcomes. The Social Adaptability Index is a way to quantify known social risk, resulting in an overall score. However, the overall SAI score was not associated with key self-care behaviors and there were few associations among individual demographic indicators and diabetes outcomes. It is unclear why this sample did not show the expected pattern of relationships, and the authors did not sufficiently explain the lack of findings. Comments are below:

1. The SAI is used to estimate a quantifiable indicator of overall socio-economic status among patients with diabetes. However, a key indicator for health care - insurance status - is not provided. Is this not included in the index? Was this available for the sample?

The reviewer raises a valid concern as health insurance being a key indicator that was not included in the SAI index. This study used the SAI as originally developed by Goldfarb-Rumyantzev et al. 2010 Association between social adaptability index and survival of patients with chronic kidney disease which includes employment status, education level, marital status, substance abuse and income in the calculation for SAI. For this reason, health insurance was not included in the calculation for SAI in this study sample. We added a statement in methods so it is clear there were no changes from the original scale in variables used.

2. In relation to the SAI, it is unclear if the grading of each variable on a 0-3 scale is consistent across all studies or if the grading of categories is personalized to each sample. For example, in an older adult sample with an average age of 60, continuing to work part time or full time may be an indicator of risk rather than being retired, which suggests the ability to voluntarily leave the workforce.

We appreciate the reviewer’s comment regarding the grading of each variable on a 0-3 scale. To date, all the literature citing use of the SAI has used this grading scale across populations. Consideration of part time or full time work in older populations is highly relevant, particularly in an older diabetes population, however, to match previously developed SAI procedures we did not change the grading system based on age. We added a statement in the methods so it is clear no changes were made on scoring.

3. More thorough information about the study sample and study procedures is needed. One of the potential limitations mentioned in the conclusion is that the sample may not be representative; however, little information is given for the reader about sample
representativeness. At a minimum, it is important to present mean scores/values for the variables of interest beyond the demographic characteristics (e.g. mean SDSCA scores; mean A1c; etc.). Additional information about representativeness can be determined from the study enrollment procedures. This sample may be higher functioning in that they were all engaged with a health care provider and presenting for diabetes care. How many patients were approached vs. enrolled in the study? Did they need to be present in person at a diabetes care clinic to complete questionnaires?

Thank you for the points raised regarding the representativeness of this study population. We addressed this reviewer’s concerns by expounding on study procedures on page 4 of the manuscript. We also added mean scores for HbA1c and self-care behaviors in Table 1.

4. There is some question about the selection of dependent variables for this study. The introduction and hypotheses focus on self-care and glycemic control but the methods and results section introduce additional variables, such as quality of life, blood pressure, and lipids. Why were these variables selected and what associations were they hypothesized to have with the SAI?

We appreciate the points raised here concerning the measures of quality of life, blood pressure, and lipids. We included these measures in order to expand the definition of diabetes outcomes beyond a clinical measure. We have modified our introduction on page 4 to specify the use of these variables.

5. There are many analyses conducted to address this hypothesis and it is unclear how the error is adjusted for multiple analyses. Further, as the overall SAI score was not significant with the self-care and health indicators, what is the justification to do further analyses with the subscales when the overall scale as validated is not significant? With so many analyses, it is also difficult to determine the meaning of some of these relationships when so many analyses are conducted, particularly when self-care (e.g. what is done to manage diabetes each day) and outcomes potentially related to this self-care (e.g. A1c) are not examined in the same model.

We understand the concern and have added additional text to page 6 to ensure these points are clarified for the reader and reasoning behind a priori hypotheses are clear. We decided a priori to test a number of self-care and clinical outcomes, and therefore did not adjust our p-value for multiple comparisons. Had we found extensive significance in results, this may be a greater concern, but given the lack of significance while we could adjust the p-value, our results would remain the same. As we were unsure prior to conducting this analysis whether the SAI would perform as an index, we decided a priori to also test the individual subscales, to determine if in
the event of lack of significance at the index level whether any one portion of the index was important. Regarding selections of outcomes, self-care and A1c are regularly considered in separate models when using regression since the influence on each of these may be different. We decided not to adjust our A1c model for self-care behaviors as in the event of a non-significant finding it would be difficult to determine if the index itself lacked an association or if self-care confounded the relationship.

6. The text uses alcohol and tobacco as the indicator of substance use but Table 2 uses language about drugs/alcohol without mentioning tobacco.

We apologize for this oversight and have clarified the language in Table 2 to specify tobacco, alcohol, and drug alcohol use as an indicator for substance use.

Kate Lorig (Reviewer 2): This article shows that the Social Adaptability Index is not useful in determining diabetes outcomes. The authors are to be congratulated for not mixing predictability with associations. Except for warning others not to use this scale, I am not sure about the utility of this article.

1. Given the large data set I think that there are some other things that might have been explored. For example, we usually think of depression as an outcome but in fact it may be associated with poor self-management behaviors and in fact may actually predict these behaviors. It is one of these interesting variables that is probably both predictive of behaviors and affected by behaviors. At least the association could be explored.

While the authors view depression as an important factor in diabetes care and influential in diabetes outcomes, it is generally not considered a diabetes outcome in itself. As such, this study did not consider depression since the primary aim was not to explore other factors that influence outcomes, but to test the utility of the SAI within a diabetes population.

2. Since the paper nicely shows that the social adaptability index is not explanatory for diabetes, the authors could have done a more careful analysis of past publications on this scale to see if the authors has actually shown it to be explanatory in other conditions or merely associated with outcomes. If the data sets used were cross sectional and not longitudinal then it may very well be that this scale has little utility. I think that this paper could be greatly strengthened with an exploration and maybe critique of pass studies. (It should be noted that this reviewer knows nothing about the SAI and is just curious.)
We appreciate the concerns raised by this reviewer. We searched the literature to ensure we accounted for all prior publications of the SAI in our background. All analyses have been conducted in cross-sectional data, and those noting explanatory power of the index were in a population with chronic kidney disease (CKD). We did not find any negative study in the literature regarding the use of the SAI, and as such, the authors see these findings as being relevant because the literature currently suggests that the SAI has utility in explanatory power, whereas we did not find this to be the case. Findings of this study suggest that it may not be adequate for explaining prevalence and incidence for outcomes outside of a CKD population. We have made note of this in the discussion on page 8 to clarify.

Sincerely,

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