Reviewer's report

Title: Diabetes screening with hemoglobin A1c prior to a change in guideline recommendations: prevalence and patient characteristics

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Reviewer: Sue Sue Kirkman

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This is an interesting analysis using real-world data from a small group of community-based family physicians, documenting a significant increase in the utilization of A1C testing to screen higher-risk patients for diabetes. In some ways it left me wishing for additional analyses (such as knowing what happened in 2010, after the ADA guidelines changed), the work stands alone as adding to the literature on screening practices. A few comments:

Major compulsory revisions:

1. Page 3, paragraph 4: The final sentence describing ADA recommendations is awkwardly worded and somewhat inaccurate. The ADA suggested that Hgb A1c is an acceptable test to diagnose diabetes, with a confirmed value of 6.5% or greater being diagnostic.

2. Page 5, paragraph 1: The readers need to know what proportion of patients opt out of the database.

3. Page 6, multiple areas: The authors have done an admirable job of attempting to rule out known diabetes, but the descriptions suggest that there may have been too much “weeding,” unless I am misreading. If anyone with an A1C over 7% was excluded, does this mean that those who were screened and found to have an A1C of 7.1% (or 8%) were removed from the screened numerator? Similarly, both history of GDM and PCOS are indications for screening. Were women with these diagnoses automatically excluded from the screened numerator (or was this only related to use of medications to identify diabetes)? Was there a time difference between GDM and A1C testing, suggesting screening and not concurrent GDM? This needs to be explained more clearly.

4. Page 10, paragraph 1 and also methods: It’s possible the association with lower LDL cholesterol (misnamed LDL in the paper) is related to hypertriglyceridemia, a known association with insulin resistance/hyperglycemia. If LDL-C was calculated using the Friedewald equation, this calculation overestimates VLDL cholesterol at higher TG levels (and therefore may underestimate LDL-C). Also, LDL particle size is smaller with hypertriglyceridemia, so LDL-C may be somewhat low while particle number is not. The authors might comment on potential reasons for this association. Were TG levels examined? More concerning is the seemingly arbitrary decision to exclude LDL-C from the model, despite a p value of 0.04. Typically anything with a p value of 0.1 or lower is included in models.
Minor essential revisions:
1. Page 3, paragraph 4: change plasma sugar level to plasma glucose level
2. Page 6, paragraph 1: As written, the sentence about the sensitivity of the algorithm suggests that this was its sensitivity in this study, which left me wondering what the gold standard was. The reference is to the validation study, so this needs to be clarified.
3. Page 11, paragraph 3: Why can’t the time difference between the two tests suggest whether the A1C was confirmatory or not?

Discretionary revisions:
1. In the US, the use of the term “diabetic” as a noun is considered not politically correct. Consider changing to “diabetic patients” or “patients with diabetes” (or without diabetes).

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I declare that I have no competing financial interests. However, I work for American Diabetes Association, whose guidelines are being (to some extent) validated by this paper, so this might be considered a non-financial competing interest.