Reviewer’s report

**Title:** An economic evaluation for prevention of diabetes in developing country: A modelling study

**Version:** 2  **Date:** 20 March 2013

**Reviewer:** Kathryn Backholer

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BMC Public Health

This paper models the benefits (in regards to diabetes prevalence, QALYs and diabetes comorbidities) and cost-effectiveness of diabetes screening and intervention programs, initiated at three different ages. The input data to the model is derived from a large population based cross-sectional Chinese cohort and intervention effectiveness is derived from the reputable Da Qing clinical trial of diabetes prevention. However, I have several questions regarding the assumptions and methodologies, which are outlined below. I have the following comments that I hope will be of assistance to the authors:

**Major Compulsory Revisions**

- The assumption of 100% capture and uptake of screening, and subsequent 100% uptake of high risk individuals into prevention programs with 100% compliance is, as the authors acknowledge in their discussion, is very unrealistic, questioning the generalisability of results. It is unclear why more realistic assumptions were not made regarding capture, uptake and compliance.
- Methods: The authors state that the model is run for a time horizon of forty years, but it is not clear how long the interventions were applied for. The intervention sessions are stated to occur an average of 9 times a year, but for how long?
- The authors derive their effectiveness for lifestyle intervention from the 6 year follow-up of the Da Qing Prevention Program. Why not use the results from the 20 year follow-up (Lancet, 2008)?

**Minor Essential Revisions**

- The authors repeatedly refer to ‘screening for diabetes, undiagnosed diabetes and IGT’. If an individual already knows they have diabetes then screening is not needed. Thus screening should only be used to detect undiagnosed and pre diabetes.
- Abstract, conclusions, last sentence: The authors refer to ‘lowest cost-effectiveness’, which may be interpreted as a negative result. I assume the authors mean ‘most cost-effective’?
- Background: please provide references for the mathematical models referred to
in paragraph 3.

- **Methods:** Please clarify what is meant by 'eligibility for annual incidence of IGT were....'

- Sensitivity and statistical analysis: Please provide more information for sensitivity analyses. How were sensitivity tests derived? what was prevalence changed to? prevalence of what? How was the robustness of the model evaluated?

- Consistence with terminology – DM or diabetes throughout.

- How was the statistical difference in the effectiveness between the modelled strategies examined?

- The specific information regarding input parameters (and references) for the model belongs in the methods section of the manuscript.

- Please add units to all tables and figures.

**Discretionary Revisions**

- The authors may find the manuscript by Kahn et al, 2010 in the Lancet relevant.

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

**Quality of written English:** Not suitable for publication unless extensively edited

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests