Reviewer's report

Title: Diabetes in Sub Saharan Africa 1999-2010: Epidemiology and Public Health Implications

Version: 2 Date: 31 January 2011

Reviewer: Dermot Maher

Reviewer's report:

General

This is a very timely and useful review and will provide a valuable resource for anyone needing to obtain readily the available information on diabetes in the region. The literature search has been done methodically and thoroughly.

Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

1. Some restructuring of content would be useful. Some information presented in the Results is not directly relevant to the issue of the epidemiology of diabetes in sub-Saharan Africa and would better belong either as “scene setting” in the Background or as explanation for the usefulness of the findings in the Discussion. The first paragraph of “Important infections and diabetes” belongs in the Background. Most of the text in the section “Important infections and diabetes” derives from referenced papers describing the relationship between diabetes and infections from parts of the world other than sub-Saharan Africa, so doesn’t represent part of the results of the search for relevant literature from the region specifically. The only relevant papers I can see are those references 62 and 77.

Background

2. It would be useful to refer to the latest editions from 2010 of the reports which are the referenced source of the statement about the region being home to the highest prevalence of HIV (refer to UNAIDS report 2010 rather than 2008), TB (refer to WHO Stop TB Report 2010 rather than Dye et al Lancet 2006)) and malaria (refer to WHO report from 2010 rather than 2005).

3. The statement that diabetes and other NCDs have yet to become a political priority is referenced to a 2001 publication, so either the statement should say that they hadn’t become a priority by 2001 or a more up to date reference should be provided.

4. The comment that the review expands on other recent views refers to a number of publications including reference 9 from 2001, which doesn’t really count as recent.

5. It would be useful to refer to the UN special session on NCDs coming up in
September 2011 which is a very important marker of global recognition of importance of NCDs.

Methods

6. The authors should explain why they’ve chosen the time period specifically starting January 1999 as opposed to another start date.

7. Since there are very few population-based prevalence studies in the region the authors should consider expanding the end-point of the time period a little, e.g. to the end of 2010, which would enable the inclusion of the diabetes prevalence study from rural Uganda published in IJE on 5 October 2010 (Maher D, Waswa L, Baisley K, Karabarinde A, Unwin N, Grosskurth H. Distribution of hyperglycaemia and related cardiovascular disease risk factors in low-income countries: a cross-sectional population-based survey in rural Uganda. International Journal of Epidemiology 2010; 1-12 doi: 10.1093/ije/dyq156)

8. It is important to define the geographical basis of the review. Studies from small Indian Ocean islands such as Mauritius and the Seychelles where the majority population is not African are not really of direct relevance to the countries on the African continent. The WHO African region is probably not a suitable basis for defining the geographic scope because it includes some countries north of the Sahara as well as some Indian ocean islands. A pragmatic definition of sub-Saharan Africa is the region of the continent (i.e. African mainland) south of the Sahara.

Discussion

9. The comparisons of results in different countries in the region are interesting and valuable. It would also be of great interest and value if the authors could indicate some of the key differences in the findings in sub-Saharan Africa and in other regions.

Conclusions and recommendations


11. The authors would considerably strengthen their recommendations by indicating who might have responsibility for undertaking a particular action – at the moment the list of recommendations is a bit vague because there’s no indication of who could or should do what.

Abstract

12. The statement under Results that “HIV and its antiviral treatment increase the risk of obesity and insulin resistance” needs re-writing to clarify that it’s the antiretroviral treatment and not HIV that can cause metabolic problems including
obesity and insulin resistance.

13. The statement under Conclusions that “Associations between diabetes and important communicable diseases…need to be acknowledged” is weak and needs replacing with a stronger recommendation for action by health providers to address the interaction between communicable and non-communicable diseases (see Maher D, Smeeth L, Sekajugo J. Health transition in Africa: practical policy proposals for primary care. Policy and practice. Bulletin of the World Health Organization 2010; 88: 943-948).

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

14. The authors refer to “case fatality” where it may be better to refer to survival or mortality (depending on which way round you want to view it) after a specified time period. Case fatality often refers to the overall proportion of people with a condition who will die from it, rather than the proportion likely to die in a given time period.

Discretionary Revisions

Discussion

15. The authors may want to consider the overall context of the statement in the 3rd paragraph under “outcomes of diabetes”, that “An increased prevalence of diabetes in the region may fuel a surge in tuberculosis”. Increased obesity is the main factor amenable to behavioural change that is driving increases in diabetes prevalence around the world. Since undernutrition is a risk factor for tuberculosis, increased obesity with a shift in the population distribution of overweight may have population-level impacts in opposite directions: decreased tuberculosis (though less malnutrition) and increasing diabetes with increased tuberculosis.

Level of interest: An article of importance in its field

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 interests.

I have referred to two of my publications in my review because I think they're relevant (rather than specifically because they're mine).