Reviewer's report

Title: Effect evaluation of the introduction of a Chronic Care Model-based program for type 2 diabetes in Belgium.

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Reviewer: David Whitford

Reviewer's report:

This is a quality improvement report in the management of type 2 diabetes. Its main interest lies in its claim that it is the first development of this nature in Belgium. Data from a region that implemented a new chronic disease model in primary care were compared with data from a comparable control region. The two research questions are clearly stated at the end of the Background section. The methods are well described.

It is not clear from the description of the chronic care model as to what degree care has shifted from hospital based to primary care based or the model of shared care (are patients discharged from hospital care, or sharing care, or sharing a system of care etc). It is also not clear what proportion of the study population would be part of this chronic care model and how many are still attending full hospital based care. This has implications for any conclusions that may be drawn as quality improvements could be related to improvements in hospital care as opposed to the chronic care model.

There is no description of what data cleaning process was used. I would assume that there was some error in the data with possible outliers.

A particular problem with this study is the way in which patients with type 2 diabetes are defined. There is no independent disease register, so patients in the study are defined by prescribing data. This has the effect of excluding all diet controlled patients with T2DM. These patients are likely to have good HbA1c results but they may see deterioration over the period of the study. Of more relevance is the exclusion of patients on 3 or more insulin injections. This does not make any sense. Many Type 1 patients are on only 2 daily injections and many Type 2 patients are on 3 or more daily injections. The authors argue that patients will be excluded equally in both intervention and control regions. However, this may not be the case if diabetologists in the two regions prefer different insulin regimes. Equally, it may account for some of the improvement in HbA1c over time as it may serve to exclude the more poorly controlled type 1 and type 2 patients in both regions who need to be treated with more complex insulin regimes and whose management is more challenging. The result is that the study is likely to be based on a heterogeneous group and I would have thought the inclusion of type 1 patients would have been more robust than the present population that cannot be easily defined. The major impact of this is the weakness in making any international comparisons as in the discussion.
The absence of hospital based diabetes centres in the control region deserves some comment. How is care delivered in the hospitals there? Are there the same number of diabetologists?

The quality indicators are more based on the availability of data in the research database than on ‘current scientific evidence’. This should be made more explicit. I am unsure what scientific evidence would suggest that keeping HbA1c below 10% is important?? Where did this indicator come from? The targets for HbA1c and total cholesterol are quite high – were these the appropriate targets in 2008? HbA1c of <7% and total cholesterol <4mmol/l would be more appropriate.

The research questions are clear and focus on quality improvement in process and intermediate outcomes. However, much of the text in the background and discussion relates to aspects of the chronic disease programme that have already been described in previous publications – namely the ACIC scores. The relevance to this paper is hard to see as any quality improvement cannot be directly linked to any one aspect of the model but is more likely the result of the complex intervention as a whole. Much of this could easily be removed without affecting the conclusions and would provide a greater focus for the paper.

As previously mentioned, I cannot see the relevance of the whole section in the discussion labeled ‘Results in relation to the ACIC score’. The discussion is otherwise well balanced.

I am not used to the phrase ‘Effect evaluation’ as in the title. Quality improvement would be a more appropriate description.

The paper does not add substantially to the published literature but is of some relevance in terms of the development of diabetes care in Belgium.

Please number your comments and divide them into

- Major Compulsory Revisions
  1. Improve the description of the population and their site of care and model of shared care.
  2. Either discuss more fully the weakness in defining patients with T2DM or include all patients on anti-diabetic agents.
  3. Explain of what relevance HbA1c<10% is as a quality indicator and how the cutoffs were decided.
  4. Rewrite and reduce the sections ACIC scores.

- Minor Essential Revisions
  1. There are a few typos through the text. These are particularly prominent in the references e.g. Ref 2 ‘tot’ instead of ‘to’. Ref 12 ‘populationj’ instead of ‘population’ etc.
- Discretionary Revisions

- Describe the data cleaning process.
- Describe the hospital care of diabetes in the control region.
- Consider changing the title.

**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 interests'