Reviewer's report

Title: The association between improved quality diabetes indicators, health outcomes and costs: towards constructing a "business case" for quality of diabetes care- a time series study.

Version: 1 Date: 4 July 2014

Reviewer: Evangelos Kontopantelis

Reviewer's report:

Thank you for giving me the opportunity to review this interesting paper. The authors use data from Israel to address an important question, whether quality improvement in diabetes management leads to better outcomes. In general they have done a good job. However, I have a few major and minor points to make which I feel need to be addressed before the work is publishable.

Major compulsory revisions

1) I would say HbA1c>9% is very poor rather than poor. The thresholds used needs to be put into context: US, UK (NICE) or WHO recommendations? I would be more interested in the risks associated with very low HbA1c levels as well since these are associated with hypoglycaemia. In general there appear to exist U-shaped relationships between these biological parameters and outcomes (hospitalisation, survival) and you seem to be the very high values vs all others. I would be interested to see categories rather than a dichotomised variable. How did the authors decide on the thresholds they used?

2) The authors give a decent overview of the informatics revolution but fail to mention the Quality Outcomes Framework, the largest incentivisation and performance monitoring system in the world, while submitting a paper on quality of care to a British journal. Incentivisation and monitoring though the system has led to improvements in quality of care (http://www.ncbi.nlm.nih.gov/pubmed/19625717, http://www.ncbi.nlm.nih.gov/pubmed/21712336), reduced inequalities (http://www.ncbi.nlm.nih.gov/pubmed/18701159) and these effects were also observed for diabetes patients (http://www.ncbi.nlm.nih.gov/pubmed/22918988) while hospitalisation for diabetes patients who received better QOF care appeared to be lower (http://www.ncbi.nlm.nih.gov/pubmed/20880046). Finally, the measurement indicators have more or less been removed from the scheme, when a ‘paired’ control indicator is still in, and it has been shown that the levels of care were not affected by the withdrawal of indicators (http://www.ncbi.nlm.nih.gov/pubmed/24468469) and the control indicators should be the focus. Also note that the diabetes clinical domain of the scheme was the biggest with 17 clinical indicators (now reduced to 15 I think but still the biggest). All this is relevant (especially the last three papers) and I would expect the authors to dedicate a para in the introduction to discuss the QOF and its diabetes domain (and perhaps overall as well).
3) The section regarding GEE in the statistical analysis section is unclear: what is the purpose of the correlation structure i.e. where is it used, what does ‘within-group’ refer to i.e. what are the groups? If this is relevant to the nested structure of the dataset, which has not been properly explained, see point 4 below. The role of the global and dynamic models and why their coefficients difference answers the question is unclear and needs to be properly explained.

4) The nested structure of the data needs to be properly explained. Lines of data are not an elaborate way to do so. For example I would say “The data had a nested structure with patients nested within areas (if relevant) and crossed with year (since same patients more or less over different time points, rather than different). Also the Poisson and Cox regression analyses need to account for the structure of the data and robust standard errors need to be calculated. In Stata clusters for the patients need to be defined with xtpoisson and the vce(cluster) option (the latter will allow robust error calculation within patient, who have been defined in stset using the id option). Of course all this relates to Stata and I don’t know what the authors used for their analyses.

5) Discussion is too long a bit all over the place. Please use a standard reporting such as : key findings / strengths and limitation of the study / results in context (I.e. in relation to other work) / conclusion. And try to reduce a bit.

6) Results section is poor. The results are not presented clearly enough and the section needs to be expanded. For example there is no reference to the results from the Poisson regression (table 3) other than mention of a significant association. In sharp contrast to the massive discussion section. Also report incidence-rate ratios rather that betas for the Poisson analyses, since the irrs can be interpreted much more easily.

Minor essential revisions
1) Abstract introduction is unclear. Grammatically, needs “constructs” instead of “construct” but still doesn’t make much sense to me and I think needs to be rephrased. i.e. replace “business case” which would need to be defined with something like “cost-effectiveness”?
2) List covariates in the methods section of the abstract
3) Mention and reference the software used.
4) Interaction term is first mentioned in the results, not the methods – relates to main point 3.
5) In the results section report confidence intervals rather

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:
I declare that I have no competing interests