Reviewer's report

Title: Market competition and price of disease management programs: an observational study

Version: 1 Date: 10 July 2014

Reviewer: Rachael E Moorin

Reviewer's report:

Major Compulsory Revisions

This was a well written paper on a topic of interest. I have some concerns regarding the statistical analysis as outlined below.

1. The authors have used linear regression to evaluate the association between competition among care groups, health insurers and the price of DMPs. Generalised linear models are more appropriate for cost data due to the typically skewed distribution of these data. The authors do not present any information regarding the normality of the data or the model fit parameters, therefore the appropriateness of the model to these data are difficult to judge. Given that linear regression has been used and that this technique has the best and most easily interpretable diagnostics for model fit, I would have expected at least an R squared value to have been provided. In addition normal plots would have helped determine the appropriateness of the data.

2. A large number of the analyses undertaken are univariate and therefore confounding is not taken into account (eg table 4). Why was a multivariate model not developed? The lack of inclusion of regional differences in socio-demographics and population risks is particularly concerning. Adjusting for risk in models such as these is very important. While the authors have alluded to this limitation in the discussion - this remains a serious limitation in the paper, which they have not adequately justified.

Minor Essential Revisions

1. The authors state in the methods that two out of eight insurers agreed to participate in the study. They then go on to say that "two insurers supplied data of 76 contracts of seven insurers." This is confusing and the terminology should be clarified.

2. Towards the end of the methods : Data collection section the authors state that the GP's practice addresses were used to divide the sample into GGD regions - then they state that this "resulted in the total number of GP's per GGD region". Do the authors mean GP practices per region or were actual number of GP's practicing in that region known. This should be clarified in the text.

3. In the multivariate models a description of how the covariates entered the model should be provided - were they simply forced as it appears since non
significant covariates remain in the final model.

4. Terminology - throughout the text the term care group is used; however in the tables the term provider group is used - presumably for the same construct. Terminology should be made consistent.

5. No text is offered to pick out the key results presented in table 2 - only a description of the contents of the table is given. Since economic terms are used in the body of the table (dominant, competitive) these terms should be defined explicitly in foot notes for the non-health economic reader.

6. Table 3 presumably this is a multivariate regression (given the reference category) - not stated in text or table.

7. Table 4 was poorly formatted and difficult to read. In the text this analysis is stated to be univariate - should be stated in the table heading or footnote. Tables should stand alone - this does not as it is not explicit as to the analysis undertaken.

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

**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