Author's response to reviews

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

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Author's response to reviews: see over
Response to the reviewers

Reviewer #1 – Ian Lee

- The level of care group competition was measured by number of care groups in a GGD region. I am not convinced this is an accurate measure, as the authors point out earlier that the population in each GGD region varies widely. The number of care groups in each GGD region is hence a distorted indicator of competition. This might have contributed to the lack of statistical significance in the analyses.

We agree with reviewer that the number of care groups in a GGD region is not the best way to measure competition. However, as mentioned we have no information about the number of diabetes patients per care group. Therefore, we have included two indicators for the competition: 1) the number of care groups in a GGD region and 2) the care group market share based on the number of general practitioners. The second one is probably the better one, since it takes the number of GPs in a region into account (indicator of the population in the region). To be better able to distinguish both associations, and as both indicators of care group competition are strongly related, we separated the multivariate analyses in one with the number of care groups in a GGD region and the other with the care group market share. In this analyses the number of care groups is significantly associated with the price of DMPs.

- The authors mention that no consumer panel data were available for one GGD region, but do not mention how this issue is treated/rectified.

These cases were excluded. This is mentioned in the revised manuscript

- The lack of statistical significance in the regressions is a concern. This can be interpreted in one of two ways. It might be said that competition and price are not linked, which is what the authors conclude. Another possible reason, however, is that the model is misspecified or that the data is inconclusive. In other words, the evidence is not compelling enough for the conclusions of the paper to be supported. Indeed, the lack of information or confounding effects (which are not measurable based on the data) are discussed in the limitations section of this study.

After untangling both indicators for care group competition, one of the indicators showed significant associations with the price of DMPs. In addition, we have adjusted our analyses for the percentage of persons with low income and percentage of persons with a western and non-western nationality in the GGD-region. We think that these revisions have strengthened our analyses, and therefore our revised conclusion. We still think the price of DMPs is more dependent on the particular insurer, but as well a little bit dependent on the care group competition. In accordance, a recent Dutch report showed that health insurers work with calculation models to make financial agreements. Not in all calculation models care of all involved health care providers, such as dieticians, is included. This could also have influenced the large difference in the price of DMPs between health insurers.

- One other general comment that I have, is that the paper was hard to read. The train of thought of the authors was not easy to follow, and some terms were not adequately defined or explained. For example, the authors mention ’managed competition’ (page 4) or ’exit-option’ (page 5) but do not explain what they actually mean. There are also numerous editorial and grammatical errors in the manuscript, which either distracts the reader or distorts the meaning being conveyed.

We have revised several parts of the manuscript with regard to not adequately defined terms.
Reviewer #2 – Rachael E. Moorin

Major comments

- 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.

The reviewer is right that mostly cost data has a skewed distribution, and we expected so. However, our cost data showed good normality (fairly normal distribution in histogram and non-significant Skewness-Kurtosis test). We reported the outcome of the normality test in the method section. In addition, we reported R squared values for the multivariate analyses.

- 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.

In the revised manuscript we adjusted the analyses in table 1 and table 3 for the percentage of persons with a low income and the percentage of persons with a western or non-western nationality in the GGD region. Part of the analyses were multivariate models already (table 1 and table 3), but we now clearly mention so in the tables. We could not develop a multivariate model for table 4, since the number of cases per insurer is too low to include more variables.

Minor comments

- 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.

We included additional information in the manuscript to clarify this better: “Two out of eight insurers agreed to participate in the study. One insurer sent all their contracts via e-mail; the contracts of the other insurer were viewed at the insurance company. Although, only two out of eight insurers participated, we were able to include contracts of seven health insurers. In the Netherlands, the tendency of health care providers was to negotiate with the health insurer with the highest market share and to propose this contract to the other health insurers in the region. In the time period 2008-2010, a lot of health insurers agreed upon the proposed contract of another health insurer without additional precondition or changes in tariffs. So, the two health insurers participating in our study agreed with contracts and prices of DMPs made by other health insurers. For this reason, contracts of other health insurers were included also. The two insurers supplied data of 76 contracts of seven insurers”.

- 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 GPs 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.
We agree that the additional information given was not clear. In the revised manuscript we have excluded the sentence “The postal codes of GPs' practice addresses were divided into the GGD regions. This resulted in the total number of GPs per GGD region”.

- 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.

  We added the following sentence to the method section: “In the multivariate linear regression analyses all independent variables were forced in the model, regardless of the significance level”.

- 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.

  We have revised the manuscript to only mention care groups and not provider groups

- 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.

  We added a footnote to table 2 with additional information

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

  In the revised manuscript ‘multivariate regression’ is stated in the table title.

- 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.

  In the revised manuscript ‘univariate regression’ is stated in the table title.