Author's response to reviews

Title: Does a local financial incentive scheme reduce inequalities in the delivery of clinical care in a socially deprived community? A longitudinal data analysis

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Response to reviews

We are grateful to all three reviewers for their helpful reviews, and have improved our manuscript accordingly.

Reviewer 1

I found this paper a little confusing because the authors sought to answer two questions. The first was in the title: ‘Does a local financial incentive scheme reduce inequalities in the delivery of clinical care in a socially deprived community?’ The second question was how income earned per patient varied according to practice characteristics including levels of deprivation in the practice population.

We considered whether to work income into the title but decided against doing so because (i) our primary focus was on clinical achievement and (ii) we wanted to keep the title length sufficiently descriptive but reasonably contained.

The presentation and explanation of the results could be made more clear.

In the results (Line 42) the authors state that ‘Higher practice deprivation was associated with poorer performance for five indicators’. Are the ORs for comparisons at baseline? This should be made clearer.

The local scheme was introduced in 2007 without a period of pre-intervention data collection and ran until 2011. We analysed all data over the four year period of the scheme. In our discussion we stated that the absence of pre-intervention data means that we cannot attribute any reductions to the scheme. We have added the following text to the study design and setting section “The scheme was introduced in 2007 and ran until 2011, there was no pre-intervention data period.” and the results section “four-year incentivised period”.

Table 1 shows numbers of practices participating in each indicator by year. Did the regression analysis take participation into account?

The longitudinal regression uses all the observations in each year. That is all available data contributes to the fit. If some practices participate for only two of four years then the values for those two years are included. We have clarified this in our methods.

Table 3 shows changes in indicators over time overall and dichotomised into change for practices in more or less socioeconomically deprived areas. I think it would be helpful to include this in the paper rather than as online supplementary material since this provides
easily understandable data on baseline and final performance for the indicators. The titles should be changed from more or less ‘deprived practices’ to practices in more or less deprived areas.

We did not include Table 3 within the main body of the manuscript as the journal guidance suggests that it is too large. However, we have now incorporated it and await any further instruction from the editorial team. We have adjusted the title of figure 1 and references in the body of our manuscript accordingly.

It would help readers to have a more detailed explanation of what Table 4 shows.

We examined this and found that we had rather unhelpfully omitted mention of Tables 4 and 5 in the text, making interpretation of the tables much more difficult. We have now aligned these Tables with the text and accompanying explanations “Table 4 provides the coefficients in the regression for each indicator. In Table 5 we present statistically significant effects as odds ratios.”

The authors state high ethnic minority population (20% Asian) but do not account for this in their analysis. This should be stated a limitation of the analysis.

We provided contextual background data in our introduction from census data to describe the setting of our research. It was not possible to include ethnicity in our analyses given the unreliability of data coding in primary care. We have added the following text to our methods “It was not possible to analyse the effect of ethnicity in this study given the unreliability of data coded in primary care”.

The authors found that practices serving more deprived areas achieved lower performance, and therefore also financial rewards, at the beginning and end of the scheme with little evidence of narrowing for most indicators. The authors conclude ‘that financial rewards penalised practices serving more deprived patients, future pay-for-performance schemes also need to address fairness of rewards in relation to workload’. It would be helpful to understand why practices in more deprived areas had lower performance before suggesting that rewards need to be increased for these practice to improve performance.

Thank you for this point. We agree that it would be helpful to understand why practices in more deprived areas had lower levels of performance. We have therefore addressed this within our results and discussion section avoiding the use of the word penalised and we have included supporting data from our accompanying qualitative accompanying paper.

Reviewer 2
This is a descriptive but well outlined study describing a local pay for performance (P4P) scheme. The authors questioned whether a P4P would narrow the gap between practices serving affluent areas compared to those serving more economically deprived ones. While the study has several limitations, these are well described in the discussion.

We appreciate your feedback, thank you.

Reviewer 3

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

Change the headings on both axes to ‘practice income’ rather than cash – which will make it more consistent with the wording in the title as well

Figure one headings have been amended.

Table 3

How are 'More deprived practices' and 'Less deprived practices' defined. Is it practices above or below the mean of all practices or is the national average used? How many practices were in each category? The same question arises in Figure 1.

Deprivation was treated as a continuous variable. Practices in more or less deprived areas were categorised as above or below the median. This is now reflected in our methods and Figure 1.

Figure 1

How was 'More' or 'Less' deprived defined? There is no definition of 'More' or 'Less' deprived in the body of the article. Have the authors divided practices above or below the national 'average' or was practice average used? The number of 'More' and 'Less' deprived practices should be stated in the body of the article and in Figure 1. The line within the box is presumably the mean but can this be clearly stated. What does each box represents, is it the 95% CI or the inter-quartile range?

Information has been added to our methods and figure 1 to indicate that the median and inter-quartile are presented in our manuscript.

Discretionary Revisions - These are recommendations for improvement which the author can choose to ignore.
The authors state that "IMD ... was the focus of this evaluation". However nothing is said about how deprivation is distributed amongst the 83 practices. The range of IMD between 5.6 and 62.3 around the mean of 37.93 suggests that IMD was reasonably symmetrical about the mean but were there a few really deprived practices or many slightly deprived? A little more information about the distribution of IMD scores between practices would help interpretation as would some indication as to which domain of IMD contributed to a high score.

IMD scores were provided by the former Primary Care Trust therefore it was not possible for us to determine which domain of IMD contributed to a high score.

Table 1

On page 6 the authors state that 'the number of practices participating in the local scheme increased over time'. Can these numbers (N) be shown in head of each column?

We presented the number of practices contributing to each indicator by year in Table 1. As the number of practices participating varied by indicator and the number of indicators varied each year we have not added this to each column to avoid confusion.

Table 5

Can the authors explain in the table why some rows are entirely blank e.g. Weight Management?

We have added notes to each table to highlight where a blank indicates that an indicator was not yet included or excluded from the local scheme (Tables 1 3 and 4), or if the odds ratio was not statistically significant (Table 5)

Table 6

Would presenting the co-efficient and se for List Size and IMD in scientific notation to the base 10 be preferable and more readable than up to six places of decimals?

We also considered this option but decided to present our results as they are following advice from our statistician a former copy editor for Oxford University Press. We would be happy to change the format following guidance from the journal.