Author's response to reviews

Title: Pedigree analysis and epidemiological features of idiopathic congenital talipes equinovarus in the United Kingdom: A case-control study

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Author's response to reviews: see over
We thank the reviewers for their comments and address their specific concerns below:

**Reviewer 1**
The reviewer rightly points out that the response rate is less than ideal and suggests we discuss in detail our perception of the reasons for the incomplete dataset and the potential effect this has on our major conclusions. The reviewer also acknowledges that we have assessed the impact of this in that we highlight that the sex ratio, predominance of bilateral disease and right over left-sided involvement agrees with other reported series.

Unfortunately we are unable to assess the impact of low participation as much as we would like as our data on non-participants is limited. We have clearly stated our participation rate in the text, but have not discussed our perceptions of the reasons for this in detail since the problems of poor participation in case-control studies are well known. We have added a few words to the end of the 1st paragraph of the discussion to re-state that we had a relatively poor response rate, but that we do not believe the sample to be severely biased.

The reviewer suggests it would be useful if the reasons for caesarean section could be explored. We agree, but unfortunately do not have this data. We have addressed this issue by explicitly stating the lack of data in the text (paragraph 3 of discussion).

**Reviewer 2**
The reviewer suggests that we have emphasized statistically non-significant findings such as paternal smoking and maternal folic acid use. We have amended the text throughout so that non-significant associations are explicitly stated to be such (eg: results section of abstract). In doing this we feel that we have addressed the first of the major compulsory revisions, where the reviewer asks that we make clear what are just ‘suggestions’ of associations and those that are more certain.

We have amended the tables to include p values as well as odds ratios and 95% confidence intervals, although general statistical thinking holds that CIs should be preferred over p values.

We have not adjusted for multiple statistical tests as this would not be standard practice in the context of a case-control study of environmental risk factors. We built an adjusted regression model to deal with multiple risk factors and confounders.

Regarding the discretionary comments, it is widely considered that education is a marker of socioeconomic status. A sentence has been added to paragraph 4 of the discussion to highlight the link between socioeconomic status and education.

Regarding the differences between our studies and others, heterogeneity is a feature of studies of clubfoot and this probably reflects the complexity of its aetiology. We have added a paragraph to the discussion to this effect.

We hope that you find our responses to the comments satisfactory.

In addition to these changes, we have amended the family history results to make them more comprehensive and to ensure that all the relevant figures are in the text. We hope that you are happy with these amendments.
Best wishes

Amanda Cardy
On behalf of all co-authors.