Reviewer’s report

Title: Outcomes when Congenital Heart Disease is diagnosed antenatally versus postnatally in the UK: a retrospective population-based study

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Reviewer: Katherine Brown

Reviewer’s report:

This is an important and interesting topic. The paper is well written and clear, with the intro setting things up nicely done. This kind of data is relatively rare, and hence interesting to see. My comments + queries are:

1) The small numbers are a concern. The study contains data from one English county and neither condition is common, hence the number of included patients after various dropped out is small. Other congenital heart case series and papers contain small numbers for similar reasons. The small numbers limit what one can do with / conclude from the data. Could the authors comment on why data on patients operated at other centres could not be obtained? Have they considered widening / enlarging the dataset? (I accept that this would be challenging).

2) The outcome measures – I found myself reading the paper wondering exactly what the outcome measures were. The paper is set up so that you have to get to the tables at the end before you know or else jump back and forth once you find out where they have been put. Would the authors consider changing this element of the structure of the report? Comment – queries on the outcomes are as follows:

a) Acidosis – My understanding is that the authors have chosen to use PH<7.35 to define presence of acidosis, which I agree strictly speaking does indicate this. Hence a very high proportion of patients have 'acidosis' – (of the patient groups - 100%, 100%, 100%, 83%) – this is not particularly informative. Why limit the scope of this aspect of the analysis to such a blunt comparison? There is a big difference between a PH of 6.9 (likely to be damaging to end organs), which is a feature of postnatal diagnosis patients who collapse (and TGA with restrictive atrial septum whenever this is diagnosed) and PH 7.30, which nearly every baby with HLHS has even if he or she is stable pre operatively. I do not think this approach provides enough discrimination in particular given the small numbers.

b) Intubation – yes this is important, and suggests that the child may have been unstable. However, babies may be intubated for transfer if on prostaglandins or for a Rashkind septostomy. Therefore a measure of the child being unwell and hence intubated for this reason would be useful. Can the authors comment on the indication for intubation? If these were mainly emergency intubations or done for instability then certainly this is important.

c) Survival – given the small numbers the study may be underpowered to detect a difference, can the authors comment on this? Has any power calculation been
d) Neurological abnormality – In babies and small children with CHD neurological and developmental abnormalities are under detected, and many families report problems once their child is at nursery, at primary school or even later. Therefore an objective test such as Bayley, Mullen or other standard test at a given age is more likely to determine whether or not there is a difference in neuro-developmental outcome between the two groups. Given the retrospective nature of the data this type of assessment is unavailable. Can the authors comment on any potential bias in their approach to this element of outcome?

e) Ongoing cardiac problems is partly evidenced by the need for medication. Do the authors have any concerns about the objectivity of this assessment?

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests