Reviewer's report

Title: Projected growth of the adult congenital heart disease population in the United States to 2050: an integrative systems modeling approach

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Reviewer: Suzanne Gilboa

Reviewer's report:

Review: Projected growth of the adult congenital heart disease population in the United States to 2050: an integrative systems modeling approach

General comments: This is a creative and clever approach to tackling this problem of estimating and projecting the size of the adult CHD population in the US. Empirical surveillance data beyond birth prevalence are lacking in the US and the authors understand the seriousness of this large gap in current knowledge; the authors are correct in their assessment that a mathematical model is a reasonable approach to take. The authors do a very nice job of laying out the need for this work as well as the data inputs for the model. The paper is, in general, well-written and clear. The authors do a nice job of explaining their study’s limitations as well.

Major Compulsory Revisions:

1. The paper would benefit from a more thorough explanation of “integrative systems modeling”. The description of the method provided is not adequate for someone unfamiliar with the approach. It was not possible for this reviewer to assess the validity of the model given its description; a reviewer with expertise in DisMod-PDE should have a chance to review this paper.

2. Although the authors recognize that NHIS estimates of “recalled” CHD prevalence are low compared to measured birth prevalence from population-based birth defects surveillance systems, this is a major limitation of this work. The extent of the under-estimation should ideally be quantified using some kind of sensitivity analysis. For example, could the same analyses have been possible using other estimates of birth prevalence – such as those reported by the Metropolitan Atlanta Congenital Defects Program? See Reller et al. J Pediatr. 2008 Dec;153(6):807-13. Even if the authors choose to retain the NHIS data this model and not use a more realistic data input for the birth prevalence, then the results should probably be expressed as “minimal” estimates – and the results of a sensitivity analysis be reported. The fact that these are minimal estimates is mentioned on page 14 in the Discussion, but it should be more prominently demonstrated – incorporated into the abstract, and results – and possibly even the title of the paper.

Minor Essential Revisions:

1. The paper would also benefit from data tables – and not just figures.
2. NHIS is the National Health Interview Survey – not Information – see: http://www.cdc.gov/nchs/nhis.htm

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

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I have a published abstract in which my collaborators and I took a different mathematical modeling approach to estimate the number of individuals with CHD in the United States. We are working those analyses into a manuscript that will be submitted for peer review in the coming year. I have never worked with (nor had any interaction with) the authors of this paper and did not perceive that I had any conflict of interest in reviewing this manuscript. I believe that I have provided the Editors with a fair and unbiased assessment.

Thank you for the opportunity to review this paper.