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Interactive comment on "The competing impacts of climate change and nutrient reductions on dissolved oxygen in Chesapeake Bay" *by* Isaac D. Irby et al.

Anonymous Referee #2

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The study on impacts of climate change and nutrient reductions on dissolved oxygen in Chesapeake Bay is well written and addresses an interesting topic. However, there are many shortcomings of the chosen approach:

1) The simulations did not consider the impact of temperature changes on hydrodynamics. Wind and evaporation did not change be definition. Only the impacts of increases in air temperature, global sea level, river flow and nutrient loads (related to river flow) were considered. Hence, the approach is not dynamically consistent.

2) A time slice approach was chosen and the transient behavior was neglected. A period of only three years was investigated. Uncertainty caused by natural variability

was not investigated. In particular, the impacts of the large variability of sea level pressure and wind fields on the simulation results were not considered because the time slices are too short. Hence, it is not clear to me whether the calculated changes in dissolved oxygen concentrations are statistically significant.

3) The applied changes in air temperature, sea level and river flow were estimated from ensemble mean values from global model simulations from the literature (partly grey literature) and are not consistent results of changing climate in the region. In particular, it was impossible for me to understand how the watershed simulations were done. A regional climate model with sufficient horizontal resolution was not applied. Hence, the simulations are not dynamically consistent projections. I suggest to call them sensitivity studies.

4) The uncertainty of projected future climate caused by biases of global climate models was not assessed. Usually there is a large spread of projected changes around the ensemble mean. The spread of the calculated changes in dissolved oxygen concentrations may be larger than the differences between the impact of nutrient load changes and the impact of climate change on the results. Hence, the conclusions on the competing impacts may not hold in a multi-model ensemble approach.

5) A greenhouse gas emission scenario RCP 4.5 was chosen. The question whether the conclusions would also hold for RCP 8.5, which is not necessarily less likely than RCP 4.5, was not addressed.

6) Quantitative figures of changing nutrient concentrations and nutrient supply are not given. Hence, it is difficult to compare with other coastal seas with comparable environmental situation.

7) Why has sea level rise a positive impact on dissolved oxygen concentrations in regions B and C? This result is unexpected. The authors state that the impact of increased stratification and residence time is smaller than the impact of increased estuarine circulation. Is this result supported by other model studies or possibly a short-

coming of the present model?

In summary, the present study is not about changing climate with all its uncertainties and the approach does not support the provided conclusions that otherwise would have large impact on marine management. Hence, I recommend to perform longer simulations to estimate uncertainties caused by natural variability (usually 30-year long simulations are recommended to address the statistics of weather). To estimate uncertainties due to model biases an ensemble of simulations driven by various global model results should be performed. Further, a high-end emission scenario like RCP 8.5 should be investigated to be able to conclude (perhaps) that the impact of changing climate does not counteract the impact of nutrient load reductions. I recommend a major revision.

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