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Biogeosciences Discussions

Interactive comment on "Annual hypoxia dynamics in an enclosed gulf" *by* K. Kountoura and I. Zacharias

Anonymous Referee #1

Received and published: 19 June 2012

Dear Dr. Zacharias,

I apologize for having used the word "deceptive" in my review. Your response indicates that you were offended by that. My mother tongue is not english either. I checked out the definition in the dictionary and realized it was not appropriate since the word could be understood as "to deliberately mislead". This is not what I intended to insinuate. I should have used the word "disappointing", i.e. "not up to expectations". So please read my sentence as "The use of the model results is disappointing".

I take your reply as an opportunity to feed this discussion. I will respond to some of your concerns and add a few more specific comments that could help improving the manuscript.

1. The tidal range in the Atlantic Ocean, just outside the Gulf of St. Lawrence, close to Cabot Strait is not 10 m! At Cabot Strait the M2 tidal range is 0.8 m. The tidal range in the middle of the Gulf of St. Lawrence is even less, something like 0.4 m.

2. Arguing that the Amvrakikos Gulf is not comparable to the Gulf of St. Lawrence because the absolute sizes are different is not very meaningful. Comparisons between systems make more sense in relative terms. For example, lengthscale ratios could be compared. In the Gulf of St. Lawrence the order of magnitude of the width at Cabot Strait is $W \sim 100$ km. This could be compared to the typical horizontal size of the Gulf, say $L \sim 1000$ km. So $W / L \sim 0.1$. In the Amvrakikos Gulf the relevant corresponding values are something like $W \sim 1$ km and $L \sim 10$ km, i.e. $W / L \sim 0.1$. This argues that the two systems are in fact geometrically similar. More convincing arguments of this sort are required to argue that the Amvrakikos is either geometrically or dynamically unique compared to other semi-enclosed systems subject to hypoxia.

3. I reiterate that the model needs to be described in more details, that the model needs to be quantitatively and statistically assessed against observations and that much more model results need to be presented. You asked for suggestions. I searched online for good recent examples of model simulations of small coastal systems. Check out for example the work of Li et al. (Simulations of Chesapeake Bay estuary: Sensitivity to turbulence mixing parameterizations and comparison with observations, JGR, 2005). This is a good example of the expectations I have. Readers expect to see modeled vector and scalar fields, both shown as horizontal and vertical slices through the domain (see for example Figure 5, 6, 7 and 8 in Li et al.). Readers expect to see a clear visual comparison between the simulated circulation for autumn-winter conditions (lateral oxygenation) compared to the spring-summer conditions (no lateral intrusion). The skill of the model needs to be assessed as in Li et al. and as in your own previous work (e.g. Gianni and Zacharias, 2012).

4. Since the paper argues in the introduction that vertical mixing is small, one would also expect to see some modeled quantities that would support this quantitatively. The

model you use has a turbulence close scheme (which one?). Vertical slices of the vertical eddy diffusivity could be presented. The comparison between seasons and during inflow events would be interesting to examine. Again, see Li et al for examples (e.g. Fig. 10 and 13).

5. You're saying that simulating one month real time takes 20 hours of computer time? That's not that much. Simulating one year would take something like 10 days. That's feasible. Many of my numerical simulations take weeks to run. Now that your model is calibrated why not send a run for a year and wait 10 days? I don't see this as a major constraint.

Sincerely,

Interactive comment on Biogeosciences Discuss., 9, 5049, 2012.