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Interactive comment on "Adjoint sensitivity of the air-sea CO_2 flux to ecosystem parameterization in a three-dimensional global ocean carbon cycle model" by J. F. Tjiputra and A. M. E. Winguth

Anonymous Referee #1

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In this paper, the authors use the adjoint of a global ocean carbon cycle model to perform a set of sensitivity tests on the simulated air-sea flux of CO2. Several parameters are varied and their respective impacts on the CO2 flux are discussed. They show that, except in the tropics, temperature and DIC changes control the marine pCO2. Then they analyze the variability of DIC concentrations and show that the marine biology is a key factor in most regions. Finally they perform a second set of sensitivity tests on the impact of ecosystem characteristics on the air-sea CO2 flux and conclude on the most sensitive components of the marine ecosystem with respect to future climate change.

My overall opinion is that this manuscript cannot be published as is in Biogeosciences

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without a major revision. I however think that the authors should be able to address my concerns quite easily and I recommend not rejecting the manuscript.

I am really convinced that the approach proposed in this manuscript (i.e., the use of an adjont model) is promising. However, I have some concerns with the way the authors are using their model. The overall objectives of the manuscript are indeed somehow confusing. Do the authors aim at understanding the potential impact of the climate change on air-sea CO2 fluxes or do they want to understand how their model behaves? There is a permanent ambiguity between these two objectives throughout the text that makes it very confusing.

The abstract is particularly symptomatic: It is said that "reducing the herbivores' ingestion parameter in the model by 25% could increase the global uptake of atmospheric carbon by 6 PgC", which looks to me as the result of a sensitivity test for a better understanding of the model since it is very unlikely that the climate change will affect only herbivores in the future. However the next (and last) sentence is "Thus, climate induced changes in the marine ecosystem structure are of importance for the future uptake of atmospheric CO2". This is likely true, but I do not see anything in this study that support this conclusion (and certainly not the previous sentence).

The conclusion is also weird, with its long last paragraph about what are the weaknesses and limitations of the current study. I suggest that the authors do reconsider the structure of their manuscript. This last paragraph should be a part of a "discussion" section in which they should also explain their strategy to use this adjoint model approach to address the impact of future climate change on ecosystem and thus on the air-sea flux. In section 3, they should try to avoid ambiguities about the significance of their results with respect to climate change by making it more "technical and complete" about what we learn about the model with these sensitivity tests.

I have a last comment. Figure 1 shows that there is a large discrepancy between the seasonal cycle of the air-sea flux of the model and that of the Takahashi et al. (2002)

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climatology. It is unclear how was performed the comparison since there is very little data available in the climatology. Is there a bias due to the lack of data or is it a problem with the model itself? This is of importance since the authors discuss in details the case of the Southern Ocean throughout the paper. This problem should at least be address in the "discussion" section.

Interactive comment on Biogeosciences Discuss., 4, 1377, 2007.

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