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Ocean Science Discussions

Interactive comment on "Interannual-to-decadal variability of North Atlantic air-sea CO_2 fluxes" *by* S. Raynaud et al.

Anonymous Referee #1

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Interannual-to-decadal variability of North Atlantic air-sea CO2 fluxes by S. Raynaud, O. Aumont, B. Rodgers, P. Yiou and C. Orr.

Summary: The authors analyze output from two simulations with a coupled ocean, sea ice, biogeochemical model and investigate interannual to decadal variability of CO2 fluxes in the North Atlantic. Previous work in this field focused on the region since it is one of the few locations where a time series of observational data is available. They identify several modes of variability that also show some correlation with the North Atlantic Oscillation.

General comment: I would like to recommend the paper to be published, however, I listed a few issues below that I feel require clarification before this paper is finally accepted.

Abstract: There is a statement in the first paragraph of the abstract informing the reader

that differences in CO2 fluxes between atmosphere and ocean models is due, in parts, to the difference in spatial resolution. I was expecting this to be addressed in the introduction in detail since questions relating to other potential problems come to mind. For example: What are the other potential causes for these differences? How do parameterizations of ocean physics such as sub-grid scale motion impact upon the results? What about parameterizations of the air/sea exchange processes for heat, freshwater, gases? Where are the key uncertainties associated with the biogeochemical model parameterizations? Etc. Probably some key references could be referred to when comparing outcome from ocean/atmosphere models and some issue should be expanded on in the introduction. While a detailed evaluation of the model goes beyond the scope of this paper, I believe some more detail should be presented.

Introduction: I would like to make two suggestions here: (1) I am not clear what the question is the authors attempt to answer in their paper, i.e. what is the exact objective of their paper. It would help the reader if this could be clearly stated somewhere in the introduction, and if the conclusion would relate back to this. (2) Ocean models and newer atmospheric models seem to produce CO2 fluxes that are in good agreement but differ from Gruber's (2002) findings, although Gruber found good agreement with observations. What is this problem here? Why are Gruber's model data and observations consistent but differ from the newer atmospheric and ocean models? And how does this paper contribute to the current debate. This is not discussed and should be explained in more details.

Climate simulation: The reader would benefit from a discussion that focuses upon the model's ability to simulate climate. This is a global circulation/climate model and the analysis shown here is investigating variability of CO2 fluxes in the Atlantic region only. Is the circulation simulated and applied here discussed somewhere in the literature? How well is climate presented in this model? It would help the reader to provide some details on key model evaluation criteria e.g. overturning, water mass distribution, ACC, equatorial upwelling, cold tongue, sea ice production etc., some information on the key

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features that are usually discussed in the modeling literature in order to assess global simulations.

Other comments: p. 438, line 2: it might be better to replace the term inversions here, with inverse models, models, etc.

p. 438, line 8: remove “the”

p. 439: line 3: remove ‘.’

p. 439: line 3: I suggest to replace “arealy” (I am not sure if this word exists in English?) and replace the sentence with something like: “and that the spatially integrated flux exhibits large variability”.

p. 440, line 2 & 3: modify sentences beginning both with “SuchĚ”.

p. 442, line 14: What are the climatological circulation fields used in this study? Or these coming from ORCA2?

p. 444, line 15: remove one “to”

p. 444, line 20: I commented earlier that some discussion of the general circulation simulated would be helpful. Is the Gulf Stream the only problem? Is there a reference where this problem has been discussed in details? Why is this a problem?

p. 445, line 8-11: There is something missing here! The sentence needs to be restructured.

p. 445, line 18: Is “significant correlation” used in a statistical sense here, meaning the correlation has been tested for its significance? Please clarify.

p. 446, line 2-4: Would it be possible to comment here on the significance of the analysis in the light of the significant difference between model data and observations (Fig 1 and 2).

Figure 1: I suggested rearranging panels, for example, observations on the left, sim-

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ulations on the right. What data is shown here? Is this an annual cycle found form averaging the last 55 years cycle of the simulation? Or is this just the last year of the simulation?

Figure 8-10: The titles Extreme (above panels on the left) and Transitory (above panels on the right) seem not to correspond to the text in the figure caption?

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