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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 #2

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Review of Interannual-to-decadal variability of North Atlantic air-sea CO2 fluxes Raynaud, Aumont, Rodgers, Yiou and Orr

07-October-2005

A modeling study of North Atlantic CO2 fluxes is presented. A statistical methodology is used to investigate the modes of the flux variability and its associated spatial patterns.

General comments:

Overall, I am concerned. I recommend major revisions. (1) Comparisons to observations at BATS suggest the model is substantially underestimating variability, and also that this underestimation is substantially more than in other models; (2) this is not made sufficiently clear; and (3) a statistical analysis is made the central focus, but it is not sufficiently linked to mechanisms of the carbon cycle.

Specific comments:

1. Statistics as a guide to mechanistic investigation: More discussion of the physical mechanisms that are responsible for the correlations found is needed.

a. For example, on page 445, it is mentioned that the maximum correlation of the NAO to the CO2 flux in the intergyre region is r=0.43 with a lag of 2.5 years. What does this mean? The correlation is not very strong and the lag is longer than one would expect a direct response to forcing of the surface ocean. Is this the SST seasonal re-emergence mechanism imprinting itself on the CO2 flux? Is there a reasonable explanation? If not, then what is the meaning of this correlation?

b. The total variability explained by the MSSA appears to be only about 25%, with 12.7% of this "poorly-resolved" (pg 446, line 14). Where is the other 75%? Please be clear that the analysis discussed in only a small part of the total. Is it clear that this is the only coherent variability and the rest is noise? Are there other studies consistent with this?

c. A very complex ecosystem model is used in this study, but there is hardly any attempt to gain understanding of the interactions of this ecosystem with the carbon cycle.

2. Statistical technique: The MSSA technique needs to be more carefully explained either in text, or preferably with equations. It is presently presented as too much of a "black box" and the reader cannot determine what the statistical analysis is doing or evaluate its results.

3. Smallness of the variability:

a. The amplitudes of the variations illustrated by the plots suggest a very small variability (max +-0.005-0.010 PgC/yr in Fig 6) in comparison to the global +- 0.5 PgC/yr suggested from data and other models. This should be stated so that all readers, even those not heavily involved in this area of study, can be aware of this.

b. There needs to be a demonstration of the magnitude of the total flux variability in

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this model in comparison to previous models or other estimates. A figure of the global and North Atlantic total flux variability in this model would be instructive. At least a comparison in the text of the standard deviation or extremes of the flux to previous studies would be helpful, if not a graphical comparison.

c. Specifically considering the comparison at BATS in Figure 2, the question the small variability needs to be addressed. Is SST variability very small?

Detailed comments: 1. Abstract, pg 438

a. Line 2, please qualify "..some atmospheric inversions have large.." and "..ocean models and at least one inversion have smallĚ"

b. Line 9, include a statement of what percentage of the total global variability comes from the North Atlantic in this model.

c. Line 20, a max correlation of r=0.64 with lag of 1-3 years is also mentioned, but this is not further explained or graphed in the article. For what region and with exactly what lag does this occur? What is the likely mechanism?

d. Line 24 - this increased variability is actually the trend due to an increased delta pCO2 over time (section 3.7), but here it is presented as a decadal timescale oscillation. Please be more clear.

2. Introduction, pg 440

a. Line 1, ".. understanding of basin-wide variability of air-sea CO2 flux from observationsĚ"

b. Line 10, final sentence. This is a very vague statement. Do the authors or have previous authors shown that ".. the same holds for patterns of air-sea CO2 fluxes"?

3. Section 2.1

a. Clarify this is a global model

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4. Section 2.3

a. As stated above, more detail on the MSSA is needed.

b. Are monthly/daily/annual anomalies considered in the analysis?

c. The authors are negative toward EOF analysis earlier in the paper, and consider the MSSA a large improvement, but then it appears that they use the EOF analysis as a pre-processing to the MSSA. Why is this done? How does it effect results? How does it impact their earlier statements?

d. How does the weighting of the “active” and “passive” variables work? How does it fit into the MSSA?

e. Is it the “control” or “anthropogenic” run that is the focus of the analysis? Please make clear throughout the text and on figures.

5. Section 3.2

a. As mentioned above, offer some explanation of the much smaller IAV in this model at BATS than in previous.

b. Make clear that not only this model, but in none of the previous models were there eddies.

6. Section 3.3

a. It is stated that it is “important” to take lags into account, but an enhanced process understanding is not demonstrated by having taken this approach. Please explain more carefully what is “important” here.

b. If the MSSA “Ĕ accounts for the correlations between variables having complex spatiotemporal structure”, what does this teach us? Again, please show the reader why this is important.

7. Section 3,4

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a. Do the 3.2, 5-7, and 13 year modes come from the MSSA or are they imposed? Please clarify.

b. Mode 4, “interdecadal” is suspicious to me. Might this be model drift? Deep ocean adjustment? If so, it should be explained as such.

c. The “poorly-resolved” 13-year mode is the “dominant” one. This is clearly a concern since statistical significance seems to be low. When this mode is focused on later in the analysis, it should be emphasized that this mode is “poorly-resolved”.

d. The discussion starting on the end of pg 446 does not make sense without more explanation of the MSSA.

e. Paragraph starting line 4, pg 447.

i. In section 3.7, the increase in decadal variability is said to be due to increasing delta pCO2 with the anthropogenic run. Instead of leaving this a hanging question to that future section, please mention it here.

ii. Further the statement that ”.. decadal variability of air-sea CO2 fluxes could increase Ĕ” needs to be qualified somehow to not imply a global finding. Either by ”.. decadal variability of North Atlantic air-sea CO2 fluxesĔ” , or by otherwise stating that the increases seen here would not have much global impact since they are so small.

8. Section 3.5

a. Lines 11-12. Figure 11. What is the mechanistic connection between fluxes and wind speeds 1-3 years before?

9. Section 3.6

a. Line 22, pg 450 - the sentence starting here states that the transitory phase of the 5-7 year mode explains why the basic flux lags the wind forcing. This seems important and perhaps the figure is worth showing. Moreover, a deeper explanation of how this provides an explanation would be helpful. How large are the anomalies? How much is

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the lag?

b. Line 27, pg 450 - replace “upwelling” with “convective supply”

c. Line 2, pg 451 - Comparing 14b and 13b, there is a dipole in 13b when there is a positive in 14b in the high North Atlantic, not a opposite phasing as suggested in the text. Please explain.

d. Paragraph starting line 5, pg 451 - Comparisons to the CO2 flux with the particle flux are mentioned. There is an implied reference is to Figure 8d. Please specify.

e. Line 15, pg 451 - Again, the 5-7 yr mode seems to explain something of the delayed response, but it is not shown or sufficiently explained. This is also referred to in the Conclusions. More explanation is needed.

10. Conclusions

a. Line 20+, pg 452 - The total flux variances explained by the modes are small (5% for 3.2, 6.4% for 5-7, and 12.7% for decadal). The reader needs to be reminded of this in the conclusions.

b. The smallness of variability in this model in comparison to observations and to other models needs to be re-iterated here.

11. Figures

a. Fig 1 - Hard to match model panel to data panel. Reorganize so that the model is on the right and data on left.

b. Fig 5 - What smoothing is applied?

c. Fig 8-10, 14-15 - Labeling on figures not consistent with captions.

d. Fig 15 - Clarify what a negative or positive sign means. If the overall delta pCO2 is negative, then a positive anomaly means a smaller anomaly? Or is a positive anomaly a larger anomaly?

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