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

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General comments:

1. BATS comparison suggests model underestimates variability, much more than other models.

As already mentioned above (see response to the 3rd to last remark of Referee #1) the revised manuscript now provides a more quantitative evaluation of model-data agreement at BATS. That analysis reveals that the ORCA2 model underestimates the data-based estimates of variability at BATS as do all other models published to date. Although the most realistic simulated amplitude at BATS is from the MIT model, the most realistic phasing comes from our historical simulation (ORCA-2006). Furthermore, at the only other long subtropical ocean time series station, station ALOHA in the North Pacific, the amplitude of our model and the MIT model are comparable and model-data agreement is much better. Finally, variability at BATS is not representative of the

North Atlantic variability overall. For example, our historical simulation estimates 50% greater variability when anomalies are area-integrated over the North Atlantic Basin. More details about this concern are now provided in the revised text along with possible reasons for why our model, amongst others, underestimates variability at BATS (see Discussion in the revised text).

2. This underestimation is not made sufficiently clear in the text

The revised manuscript makes this clearer and much discussion is now devoted to this issue.

3. Statistical analysis is the central focus, but it is not sufficiently linked to mechanisms of the carbon cycle

The revised text now provides clearer links between the statistical analysis and mechanisms. Although just a tool, our statistical approach was also fundamental, for instance, to extract the decadal mode (from amongst others), to evaluate the effect of increased atmospheric CO2 on decadal variability, and to evaluate lags between climate forcing and air-sea CO2 flux. We are the first to use MSSA in such a context and it yields original findings for which we are able to offer some mechanistic insight. More details concerning our response to this general concern are given below in the response to the specific comments.

Specific comments:

1a. Meaning of the lag correlation?

Indeed our lag correlation of the air-sea CO2 flux with the NAO index (r = 0.43 with a 2.5-year lag) does appear weak. Putting it in context though, correlations of the complex NAO signal with climatic variables such as SST are seldom stronger (Visbeck et al., 2003). Thus relatively speaking, this correlation for the air-sea CO2 flux is actually rather large. In the Discussion, we now discuss possible mechanisms for the 2.5-year lag while also providing references to previous work (Hakkinen, 1999; Gulev et al., 2003; Follows and Williams, 2004; Palter et al., 2005). In short, lateral advection from different regimes is known to explain such lags in the physical system (Follows and Williams, 2004). Lags greater than one year may also be explained by spatially varying forcing of the mixed layer DIC content. As is the case for SST, seasonal re-emergence is also expected to directly affect quantities such as DIC and nutrients, not only indirectly via the effect of SST on pCO2). Palter et al. (2005) show that mode water formation and re-emergence can explain lags longer than one year.

1b. Where is the other 75% of the variability?

Briefly, as now pointed out in the revised text, the remaining variability is in the climate trend and noise. In more detail, we are interested in the most coherent patterns of the variability. This coherency is important in both space and time. Spatially, we want to be able to study relationships between locations and between different variables. Temporally, we are interested in identifiable features that have also been found at numerous times during the simulation. Thus, we were required to strongly filter the signal to extract the most robust properties from the variability. Such filtering is not necessary when only making model-data comparison, without trying to understand underlying mechanisms. More explanation is provided below (see response to 3a). As an example, Moron et al.(1998) applied MSSA to century-scale SST records in different regions of the world. Their analysis reveals that over the similar timescale in the North Atlantic, the contribution to the variance explained by the sum of dominant modes was around 29%, which is very close to our result.

In the revised manuscript, we now emphasise that technically the decadal mode may be considered as more "poorly-resolved" in the sense that its period is longer than the standard values we used for the MSSA window parameter. However, our tests with different values of the window parameter in the MSSA analysis always extracted the same decadal mode. In section 3.7, we also show that the difference in the structure of this mode between our two simulations (high and low atmospheric CO2) can be explained using a decomposition of the air-sea flux equation. This result offers a straight-forward way to evaluate this deduced decadal mode, both statistically and dynamically. In summary, our decadal mode must be considered as adequately resolved.

1c. Hardly any attempt to gain understanding of interactions between the complex ecosystem model and the carbon cycle.

It is true that unlike previous work (Le Quere et al., 2003), our focus has not been on trying to assess the advantages of using a more complex ocean biogeochemical-ecosystem model. That is, the complexity of the ecosystem model is useful here only to the extent that it is more realistic (Aumont and Bopp, 2006). Instead, our aim was to learn from being able to distinguishing the major modes of interannual-to-decadal variability while simultaneously taking into account lags in the system. These aspects are original, and we think adequate to stand alone. A clear priority for future work will be to systematically assess through a series of sensitivity tests how stepwise increases in complexity and changes in parameters will affect simulated air-sea CO2 fluxes.

2. Provide more careful explanation of MSSA technique

A detailed description of MSSA is now given as an Appendix.

3a. Make it clearer that plots suggest small variability relative to global variability from data and othermodels

In the revised text, we now make it clearer that the amplitude of the interannual and decadal modes (e.g., Fig.6) explain a small part of the total variability, particularly in terms of global variability (+-0.5 Pg C yr-1). We also clarify that this finding is consistent with previous work focusing on climate variability in the North Atlantic. Furthermore, we now explain why this small part of the signal retains the most important spatiotemporal features of the variability. Concerning Fig. 6 and small variability in the North Atlantic in general, see the response to 3c just below. In Fig. 6, the 3 oscillatory modes are presented separately, each explaining only part of the variability. The amplitude of these models is consistent with the filtering process (see 3c). Therefore, these amplitudes cannot be directly compared to results from other studies although they are representative of the North Atlantic variability . For prope rcomparison with results from other models, all results would have to undergo the same filtering method.

3b. Compare simulated total variability, globally and in the North Atlantic

We now mention in the revised text that the standard deviation of the simulated, area-integrated, global variability is 0.25 Pg C yr-1 and that that for the North Atlantic (north of 15N) is 0.045 Pg C yr-1, i.e. 20% of the global variability. As mentioned previously, we also offer a more detailed comparison of our results to those from other models as well as to the data based estimates (see Section 3.2 Variability at the BATS station). Global variability in our model is similar to other models, and overall North Atlantic variability is 50% higher than found by (McKinley et al., 2004b), even though their model shows higher variability at BATS.

3c. Address question of small variability at BATS.

As requested, more details concerning the small variability at BATS are provided in the revised text and in this document (see previous responses above). The revised text also now discusses how the variability in the North Atlantic is relatively small but discernible thanks to filtering in our methodology. We filtered in two preliminary steps and within the MSSA approach itself. As described in Section 2.3, as a 1st preliminary step, a 6-month running mean was applied to remove uninteresting intra-seasonal variability, and the climatological annual cycle was computed over the 55 years and removed. Then as a 2nd step of prefiltering, we used EOF to retain only 15 degrees of freedom for the computer-intensive MSSA analysis. That retained 80% of the signal. Subsequently, that signal was reduced further when the MSSA analysis was applied to see through the naturally noisy North-Atlantic climate. The NAO is much noisier than is ENSO. The resulting first mode (interdecadal) extracted by MSSA is indeed associated with a slow drift and is not further analysed. The MSSA analysis also extracted 3 higher frequency oscillatory modes, which together explain 25% of the prefiltered 80%

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variance. Thus we retained 20% (0.25 * 0.80) of the total variance. In the Discussion, we also now offer possible explanations for the generally weak variability at BATS: uncertainties in data-based derived fluxes, coarse model resolution, weak NCEP forcing, reduced lateral advective impacts (Palter et al., 2005)) from weak NCEP forcing.

Detailed comments

1a. Abstract: Qualify differences in variability between inverse and ocean models.

We have now qualified differences in variability estimates in the 2nd sentence of the abstract.

1b. State what proportion of the simulated global variability comes from the North Atlantic

The 3rd sentence of the abstract now includes this statement.

1c. Provide regional and mechanistic details about the correlation of r = 0.64 with a lag of 1 to 3 years .

In the revised text, the lags are presented at the end of section 3.5 and they are discussed further in the 3rd paragraph of the Discussion. The 1-3 year lags concern the North Atlantic's area-integrated, basin-wide (from 20N to 70N), air-sea CO2 flux vs. the wind (climate) forcing. As now mentioned in the text, the 1-year lag may be explained at least in part by vertical processes, namely the slow air-sea equilibration time of about 1 year needed for mixed layer DIC to equilibrate with the atmosphere (Broecker and Peng, 1974). Related lags of up to 3 years have been documented previously as being due to the effects of lateral ocean transport (Hakkinen, 1999; Gulev et al., 2003; Follows and Williams, 2004; Palter et al., 2005).

1d. Be clearer about increased variability actually being a trend due to increased delta pCO2 over time(instead of a decadal oscillation).

It is now clarified in the revised text that the increased variability concerns the timescale

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of the oscillatory decadal mode, and that this is due to the anthropogenic CO2 increase.

2a.

Corrected.

2b. Vague statement. Has it been shown that "...the same holds for patterns of air-sea CO2 fluxes"?

The revised text has been corrected to make this a logical supposition, not a statement of fact.

3a. Clarify that this is a global model.

Done.

4. Section 2.3

4a. More detail needed on MSSA.

An appendix has been added on MSSA.

4b. Are monthly/daily/annual anomalies considered in the analysis?

As now mentioned in section 2.3 (Analysis), we analysed monthly averages.

4c. Why is EOF used for preprocessing to MSSA and how does that affect results as well as earlier statements that are negative towards EOF?

We now clarify in the revised text that we use EOF analysis only as a preprocessing tool following for example Plaut and Vautard (1994). In filtering with EOF, the aim was to decrease the number of degrees of freedom while limiting loss of important information. This preprocessing is necessary because MSSA is computationally very expensive. Furthermore, when we exploit the EOF results, we do not consider the separate EOFs individually. Instead, we sum enough of the EOFs together to represent most of the variability. This limited sum of EOFs only serves to reduce the number of degrees of freedom. Subsequently, the MSSA modes are extracted from this sum of EOFs. Thus

EOF preprocessing followed by MSSA analysis allows us to fully account for lags, thus going beyond an EOF-only analysis.

4d. How does weighting of active and passive variables work?

More details about this weighting are provided in section 2.3 as well as in responses to remarks from the Editor (see above).

4e. Is analysis focused on Control or anthropogenic run?

We have added two sentences at the end of the Section 2.3 to clarify that analysis primarily focuses on the anthropogenic run.

5. Section 3.2

5a. Explain much smaller variability at "BATS"

Section 3.2 now provides a detailed explanation of the smaller variability at BATS, as also mentioned in previous responses to Referee #2 concerning this same question.

5b. Make clear that neither this model or previous model simulations have accounted for eddies.

In response to this remark, we have completely rewritten this section and no longer mention eddies here. Instead, we have added a paragraph to the Discussion (see 2nd paragraph) focusing on resolution and making the requested statement.

6. Section 3.3

6a and 6b.

Explain why it is "important" to take lags into account, particularly in terms of enhanced processunderstanding.

By "important", we meant that simple local adjustments of DIC content may not be the only factors driving theresponse of air-sea CO2 flux to climate variability. We have clarified what we meant in this sentence. Furthermore,in the Discussion, we also go into further detail concerning mechanisms.

7. Section 3.4

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

We now clarify in the text that these modes are not imposed, and that they come from the MSSA analysis, which selects the dominant oscillatory modes.

7b. Explain if interdecadal mode may be due to model drift or deep-ocean adjustment?

In the text, we now acknowledge that the interdecadal model could in part be due to model drift. Yet model drift is small because as described in the Methods section, simulations were spun up for 5000 years and followed by two 55-year cycles where forcing was repeated, before the final 55-year period that we analyse here. In any case, besides the potential for even small model drift, the relatively short 55-year forcing period is reason enough to focus on the 3 shorter modes (3.2, 5-7, and 13-year frequencies) as we have done throughout the document. Interestingly, the interdecadal mode was previously identified by Moron et al. (1998) from an analysis of observations.

7c. The "poorly-resolved" 13-year mode is a concern because of its low statistical significance.

One cannot precisely quantify the significance of an MSSA mode. As mentioned previously, technically one might say (as we did in the submitted manuscript) that this mode is poorly resolved simply because our MSSA temporal window parameter M is slightly narrower than the period of the mode. Yet when we made sensitivity tests using different M 's, particularly wider ones, the MSSA analysis always yielded this same mode with the same spatiotemporal signature. To avoid confusion, we have reformulated this text and no longer use the term "poorly resolved".

Furthermore, this mode is found in both our simulations with only slight differences due to increased atmospheric CO2 (see revised section 3.7 as well as section 3.4). Our sensitivity tests reveal that the decadal mode is both statistically and physically robust.

7d. Need complete description of MSSA to understand discussion starting at the end of p. 446.

A complete description of MSSA is now provided in a dedicated Appendix.

7e, i. and ii.

1. Mention that increase in decadal variability is due to increasing atmospheric CO2.

Done: At the end of section 3.4 (Spectral properties) we mention that the 30% increase of the decadal variability is due to the difference in Delta pCO2.

2. ii. Mention that increased pCO2 will increase only North decadal variability in the North Atlantic

Done here (section 3.4) as well as in section 3.7 and in the Conclusions.

8. Section 3.5

8a. Lag between wind and basin-scale CO2 flux needs mechanistic connection.

This mechanistic connection (air-sea CO2 exchange time and lateral transport) has now been provided in Section 3.5 with a link to more details that are given in the Discussion.

9. Section 3.6

9a. May need a figure and deeper explanation as to why the 5-7 year mode lags the forcing.

Following this advice, we have now added two new maps (Figs. 15a and 15b) as well as additional text to clarify this point.

9b. Replace "upwelling" with "convective supply"

Done.

9c. Explain where there is a dipole in Fig. 13b, while there is a positive in Fig. 14b (in the high N. Atlantic), not opposite phasing as suggested in the text

We now clarify that the region where the variations in mixed layer depth appears to explain Delta pCO2 variation is limited to south and south-east of Greenland and does not extend to east of Newfoundland and southward. Moreover, we have fixed an error in the Figure number that was referred to in the submitted manuscript, which seems to be what provoked this remark.

9d. Specify implied reference to Fig. 8d

We have now explicitly mentioned this Figure, as requested.

9e. Further show and explain how the 5-7 year mode may cause the delayed response.

As specified in the revised text at the end of Section 3.6 (Inferred processes), we have now added a new figure(Fig. 17) showing the export production associated with the 5-7 year mode. During the transitory phase north of 40N and in the east, this term qualitatively resembles the Kg * Delta pCO2' term of Fig. 15a. Thus biological processes may contribute to the large area-integrated air-sea CO2 flux anomaly of the 5-7 year mode (Fig. 10) through the Delta pCO2 anomaly. Since this term is maximal during the transitory phase and not during the extreme phase, it necessarily introduces a lag of the air-sea CO2 flux response to the forcing.

10. Conclusion 10a. Remind reader of small amount of variance contributed by the modes.

We now mention in the 2nd paragraph of the Discussion that all modes summed up contribute 25% of the total variability and that the remaining variability is due to the trend in climate and to noise.

10b.

In the Discussion we also focus on how simulated variability at BATS appears to be too low, while providing details of possible causes.

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11. Figures 11a-c. Confusing organisation and labelling of figures.

There was a problem with labelling in the submitted manuscript, which has now been corrected in the revised manuscript. Regarding smoothing, we have added to the caption of Fig. 5 that we applied a 12-month running mean to that data.

11d. Clarify meaning of negative or positive sign in Fig. 15.

We now make it clearer in the revised manuscript that we are actually talking about an increase in the absolute value (or magnitude) of Delta pCO2, a term which actually becomes more negative as atmospheric CO2 increases. The increase in the absolute value of this term increases the area-integrated decadal anomaly.

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