Answers to reviewer #1

We thank the reviewer for his/her careful reading of our paper, and for his/her remarks that will help enhancing the clarity and the generality of the main ideas. We did our best to take them into account as explained below.

Answer to specific comments:

1. Yes, we agree with the reviewer that the sketch in Figure 1 is not complete, and that there are other possible sources of uncertainty in ocean models. The introduction actually focuses on uncertainties resulting from everything that is not resolved by the model (external world, unresolved scales, unresolved diversity, unresolved processes,...). This includes all approximations in the specification of the forcing (simulating the effect of the external world) and in the parameterization of the missing physics (simulating the effect of unresolved processes). However, as the reviewer points out, this is not correctly summarized in Fig. 1. In addition, there are also other possible sources of uncertainty that were not mentioned in the introduction itself. We have thus modified the introduction (and Fig. 1) to enhance the generality of the approach:

   - First, Fig. 1 was indeed misleading because it was specific to a model resolving the 'large scales'. We have thus modified this figure to make it more generic and more consistent with the text of the introduction: the description of system A has been generalized to: "limited ocean system, reduced spectral window, simplified physics, simplified biogeochemistry", and the description of system B has been generalized to: "external world (e.g. atmosphere), unresolved scales, unresolved processes, unresolved diversity".
   - Second, the following text has been added in the introduction to mention that this does not include all possible sources of uncertainty, with a reference to Palmer et al. (2014) for more information: "The most direct approach to introduce an appropriate level of randomness in ocean models is to use stochastic processes to mimick the effect of uncertainties. In the discussion above (summarized in Fig. 1), a specific focus was given to uncertainties resulting from the effect that unresolved processes (in $\mathcal{B}$) produce on the system ($\mathcal{A}$). However, there is a variety of other sources of uncertainty in ocean models (e.g. numerical schemes, machine accuracy,...) that do not enter this particular sketch, and that may also require a stochastic approach (Palmer et al., 2014)."

2. Yes, we agree that the tuning of the perturbation parameters is a very important question, and that general guidelines could be very helpful to users. However, as explained in the last paragraph of section 2.1, the tuning of the parameters is out of the scope of the present paper. We have tried to modify this paragraph to introduce the ideas raised by the reviewer, but it was difficult to give very precise guidelines. The text of the paper has been modified as follows:

   "Referring to the sketch presented in Fig. 1, the general idea to tune the parameters is to obtain reliable probabilistic information on what happens in system $\mathcal{B}$, and to reduce this information to a simple statistical model (e.g. the autoregressive model described above). More precisely, the probability distribution simulating the effect of $\mathcal{B}$ should also be conditioned on what happens in system $\mathcal{A}$. For instance, it can be very important that the probability distribution for the state of the atmosphere (e.g. surface winds) be conditioned on the state of the ocean model (e.g. mesoscale eddies), to simulate the interaction between $\mathcal{A}$ and $\mathcal{B}$. Similarly, the probability distribution for unresolved scales or unresolved diversity usually depends on what happens in system $\mathcal{A}$. This need to correctly simulate conditional probability distributions explains why the tuning of the parameters is not easy, and why an extensive database to learn the statistical behaviour of the coupling between $\mathcal{A}$ and $\mathcal{B}$ is often necessary. In practice, this learning information can be obtained either from observations of the two systems or from other models explicitly simulating the coupling between $\mathcal{A}$ and $\mathcal{B}$. For instance, high-resolution observations or high-resolution models can be used to tune a statistical model for unresolved scales; a model of the atmospheric boundary layer can be used to learn the statistical dependence of the state of the atmosphere to the ocean conditions; a generic biogeochemical model involving a large number of species can be used to understand the statistical effect that unresolved diversity can produce in a simple ecosystem model.

The identification of an appropriate statistical model is thus an important intermediate step that is far from straightforward, and for which it is difficult to provide very precise guidelines. Despite of these difficulties, our point of view is that the tuning of the system is usually even more problematic with a deterministic parameterization of unresolved processes, since no deterministic simulation could exactly fit the real behaviour of the system.(...)"

3. Yes, we agree that this is useful complement. We have added a new equation (similar to Eqs. 5 and 7) to introduce explicit perturbation of the model forcing. This equation is introduced by the following additional text (at the end of section 2.3): "On the other hand, the external forcing **u** (e.g. atmospheric data, river runoff, open-sea boundary conditions,...) can also be a major source of uncertainty in the model, which can be explictly simulated using a formulation similar to Eq. (5):

$$\frac{d\mathbf{x}}{dt} = \frac{1}{m}\sum_{i=1}^{m}\mathcal{M}(\mathbf{x},\mathbf{u}+\delta\mathbf{u}^{(i)},\mathbf{p},t) \tag{1}$$

where the fluctuations $\delta\mathbf{u}^{(i)}$ must be tuned to correctly reproduce the effect of uncertainties in the forcing. Introducing appropriate perturbations of the atmospheric data can for instance be useful to include them in the control vector of ocean data assimilation systems (Skandrani et al., 2009; Meinvielle et al., 2013)."

4. Yes, we agree that the reference to 'weak constraint data assimilation' was a bit too specific. This has been reformulated to include assimilation systems in a wider sense.

5. Yes, we agree that the expression "unresolved diversity" may not be generic enough in the sense that it does not encompass all possible model simplifications. However, we believe that it is useful to make the connection between similar simplifications in different model components (e.g. biological diversity and diversity of ice dynamical behaviours), and thus to use a similar approach to simulated these uncertainties. What was probably missing in the paper is the description of the model simplification process that leads to "unresolved diversity". This simplification process is "aggregation" of several system components (e.g. several biogeochemical species or several ice categories) using one single state variable and one single set of parameters. To clarify this point, the text of the paper has been modified as follows.

   In section 2.4, we have tried to clarify what we mean by "unresolved diversity" in general: "Another general source of uncertainty in ocean models is the simplification of the system by aggregation of several system components using one single state variable and one single set of parameters.".

   In section 3.2, we have tried to clarify the concept of "unresolved biologic diversity" in particular: "On the one hand, the most common simplification in biogeochemical model (Le Quéré et al., 2005) is to aggregate the biogeochemical components of the ocean in a limited number of categories (defining system $\mathcal{A}$ in Fig. 1). This reduces the number of state variables and parameters, and introduces uncertainties in the model equations since the various components included in one single category (unresolved diversity, in system $\mathcal{B}$) do not usually display the same dynamical behaviour. To simulate this first class of uncertainty, we will use (...)".

Answer to technical comments:

All technical comments have been taken into account. In particular, the following modifications have been introduced in the paper:

- P632L5: The likely explanation is that neglecting fluctuations of $P^\star$ has the direct effect of increasing the effective ice strength, which leads to a systematic under-estimation of ice thickness. The effect of $P^\star$ is indeed nonlinear: during the periods of small $P^\star$, the ice thickness has the opportunity to increase, and this increase is not counterbalanced by a symmetric decrease of thickness during the periods of large $P^\star$. This explanation has been added in section 3.3.

- P635, Algorithm 2: Yes, this is correct. If there is a restart file, the seeds from the restart file must override the initial seeds of the stochastic simulation. A word of explanation has been added in the algorithm.
- P636L7-14: The last paragraph of the appendix has been moved to the conclusion (with slight modifications).

Additional references:

Le Quéré C., S. P. Harrison, I. C. Prentice, E. T. Buitenhuis, O. Aumont, et al. (2005). Ecosystem dynamics based on plankton functional types for global ocean biogeochemistry models. Global Change Biology, 11 (11), 2016–2040.

Meinvielle M., Brankart J.-M., Brasseur P., Barnier B., Dussin R., and Verron J. Optimal adjustment of the atmospheric forcing parameters of ocean models using sea surface temperature data assimilation. Ocean Science, 9, 867-883, 2013.

Palmer, T. N. P. Düben , H. McNamara (2014). Stochastic modelling and energy-efficient computing for weather and climate prediction, Phil. Trans. A, 372, issue 2018.

Skandrani C., Brankart J.-M., Ferry N., Verron J., Brasseur P. and Barnier B. Controlling atmospheric forcing parameters of global ocean models: sequential assimilation of sea surface Mercator-Ocean reanalysis data. Ocean Science, 5, 403-419, 2009.

Answers to reviewer #2

We thank the reviewer for his/her careful reading of our paper, and for his constructive suggestions to improve the quality of the manuscript. They have been carefully taken into account as explained below.

Concerning comments 1 and 5 by reviewer #1, please see our answers to his/her comments.

Thank you also for the additional litterature, they have been added in the paper as a complement information. In particular, the reference to Palmer at al. (2014) has been used to answer to the first comment of reviewer 1; the reference to Palmer at al. (2001) has been used as an additional reference to the development of stochastic parameterization in meteorology; and the reference to Palmer at al. (2008) has been used as an additional reference to the implementation of stochastic parameterization at ECMWF.

Concerning the stochastically perturbed backscatter of kinetic energy, we agree that it should be mentioned in the paper, and that there could be some ways of using it in ocean models (even if we have not investigated this possibility). The following text has been added at the end of section 2.3: "Before concluding this section, it is important to remember that the above discussion only provides one possible framework for simulating the effect of unresolved fluctuations, and that other approaches can be imagined. For instance, a specific stochastic parameterization is already routinely applied at ECMWF to simulate the backscatter of kinetic energy from unresolved scales to the smaller scales that are resolved by the model (Shutts, 2005). This scheme has been developed for atmospheric applications but might also be applicable to ocean models.(...)"

Concerning unresolved biologic diversity, we have tried to give more background information, with an addtional reference (see also answser to comment 5 by reviewer 1). The following text has been added to the paper: "On the one hand, the most common simplification in biogeochemical model (Le Quéré et al., 2005) is to aggregate the biogeochemical components of the ocean in a limited number of categories (defining system $\mathcal{A}$ in Fig. 1). This reduces the number of state variables and parameters, and introduces uncertainties in the model equations since the various components included in one single category (unresolved diversity, in system $\mathcal{B}$) do not usually display the same dynamical behaviour. To simulate this first class of uncertainty, we will use (...)".

Typos have been corrected. Thanks.

Additional references:

Le Quéré C., S. P. Harrison, I. C. Prentice, E. T. Buitenhuis, O. Aumont, et al. (2005). Ecosystem dynamics based on plankton functional types for global ocean biogeochemistry models. Global Change Biology, 11 (11), 2016–2040.

Palmer, T. N. (2001). A nonlinear dynamical perspective on model error: A proposal for non-local stochastic-dynamic parametrization in weather and climate prediction models. Q.J.R. Meteorol. Soc., 127: 279304. doi: 10.1002/qj.49712757202.

Palmer, T. N., R. Buizza, F. Doblas-Reyes, T. Jung, M. Leutbecher, G.J. Shutts, M. Steinheimer and A. Weisheimer (2009). Stochastic parametrization and model uncertainty. ECMWF Tech. Memo. 598, 42pp.

Palmer, T. N. P. Düben , H. McNamara (2014). Stochastic modelling and energy-efficient computing for weather and climate prediction, Phil. Trans. A, 372, issue 2018.

Shutts, G. (2005). A kinetic energy backscatter algorithm for use in ensemble prediction systems. Q.J.R. Meteorol. Soc., 131: 30793102.