Please express our gratitude to all the reviews. Their comments and suggestions have greatly improved the manuscript. We are also most grateful to Julia Slingo for her comments. We have communicated directly with, and have asked her to join the author team. She has declined. Every change that Julia suggested has been adopted in the revised manuscript.

In the following we address each comment of the reviewers. Our responses are in bold and the reviewers’ comments are in regular font.

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**Reviewer A**

This white paper offers a well documented review of the scientific activity in the domain of atmosphere and ocean prediction beyond the limit of a couple of weeks imposed to a meteorological forecast by the chaotic nature of the atmosphere. The monthly to decadal climate prediction is possible because the atmosphere interacts with slowly evolving components of the earth system. This white paper lists in an exhaustive way these components, the recent progresses and the reasonable expectations. Its merit is to concentrate in less than 50 pages an information spread in thousands of pages in working group reports or peer reviewed literature. I would like to suggest a few additions to this synthesis.

Page 2 line 15: “... crop production, hydrological resources, and other ...” Since several examples are given for weather forecasting, why not quoting at least two for seasonal forecasting ?

**Response: These particular lines have been removed from the revised manuscript.**

Page 2 line 6 from bottom: The use of a common core model for various applications in the “seamless” approach is also an opportunity to save human time when porting a system to a new computer generation.

**Response: Revised as suggested.**

Page 3 line 22: Statistical methods are also useful:

-to correct model errors (not just the mean bias) so that impact users can incorporate forecast series into impact models

-to downscale the predicted information, since the cost of numerical forecasts (in particular decadal forecasts) imposes a rather low resolution to modellers

**Response: Revised as suggested.**

Page 5, end of paragraph 2: there is a caveat about the use of perfect model assumption, in particular in seasonal forecasting. The evaluation of seasonal probability forecasts indicates that ensembles based on a single model are overconfident. Efforts are made to increase the ensemble dispersion. The direct consequence of this increase is that in “perfect mode” the model is degraded.

**Response: The text corresponding to this comment has been removed.**

**Reviewer B**

General Comments:

I find this to be a useful summary of the sources of predictability across a range of time scales, including some comments on the state of the art and challenges ahead. It will therefore be a useful contribution for the Monograph that documents the Open Science Conference. My comments below are fairly minor.

Specific Comments:

1. p. 2, lines 14 and 20 from the bottom: In each case, I suggest that the word "will" is too strong; a more qualified word or phrase, such as "could" or "will likely" seems more appropriate to me. Similarly, on line 6 from the bottom, the word "necessitates" is absolute; perhaps a phrase such as "could be accelerated through" might be a better choice.

**Response: Revised as suggested.**

2. p. 5, line 1 Please provide full name and reference for "POAMA"

**Response: No longer relevant to revised manuscript.**

3. p. 5, line 8 Please provide full name for "IOD" and "SAM".

**Response: Modified as suggested.**

4. On the seasonal or seasonal-to-interannual time scale, I suggest some discussion of the reemergence mechanism, whereby oceanic anomalies are formed in the mixed layer in one winter, reside beneath the mixed layer for the summer, and reemerge in the winter (see references by Mike Alexander, among others). This is somewhat different than the upper ocean heat content discussion on page 8.

**Response: Manuscript has been revised as follows:**

**“Ocean heat can also be sequestered below the surface to re-emerge months later and provide a source of predictability (e.g., Alexander and Deser 1994). This occurs in the North Pacific and has been well documented in the North Atlantic where Spring atmospheric circulation patterns associated with a strong (weak) Atlantic jet drive positive (negative) tripolar anomalies in Atlantic ocean heat content (Hurrell et al. 2003). A positive tripole here indicates cold anomalies in the Labrador and subtropical Atlantic and warm anomalies just south of Newfoundland. The shoaling of the thermocline in summer then preserves these heat content anomalies in the subsurface until late Autumn or early winter when the more vigorous storm track deepens the mixed layer and the original heat content anomalies can “re-emerge” at the surface (Timlin et al. 2002) to influence the atmosphere again. This has been the basis of some statistical methods of seasonal forecasting (Folland et al. 2011) and it appears to have played a role in some recent extreme events (Taws et al. 2011). However it is still the case that models produce only a weak response to Atlantic ocean heat content anomalies, and higher resolution (e.g. Minobe et al 2008, Nakamura et al 2005) or other atmosphere-ocean interactions may need to be represented if the levels of predictability suggested in some studies from this coupling are to be fully realized.”**

5. p. 5, line 21 Please provide a reference for the discussion of operational Arctic prediction.

**Response: Reference to Blanchard-Wrigglesworth, Edward, Kyle C. Armour, Cecilia M. Bitz, Eric DeWeaver, 2011: Persistence and Inherent Predictability of Arctic Sea Ice in a GCM Ensemble and Observations. *J. Climate*, 24, 231–250. doi:**[**http://dx.doi.org/10.1175/2010JCLI3775.1**](http://dx.doi.org/10.1175/2010JCLI3775.1) **added to revised manuscript.**

6. p. 5, end of 3rd paragraph: I suspect that this discussion of multi-year predictability would be more appropriate later in the decadal predictability section.

**Response: Discussion moved as suggested.**

7. p. 6, line 8: The Bronimann reference is not listed.

**Response: Reference added**.

8. p. 9, first sentence of "Stratosphere" paragraph: There appears to be a word missing after "stratospheric"

**Response: Corrected.**

9. p. 9, Vegetation section: This paragraph is rather weak, with only one citation from 1999. This paragraph should either be strengthened or eliminated.

**Response: Reference to Osborn et al. 2009 added to section.**

10. p. 9, line 8 from bottom: Suggest replacing "synoptic" with an explicit time scale ("weeks", perhaps)

**Response: Modified as suggested.**

11. Throughout the text the term "prediction" is used both for initial value problems and boundary value problems, while the word "projection" is not used. In some contexts, "projection" is used for boundary value problems where there is no information coming from the initial conditions. The authors may wish to discuss these terms, and why they only use the term "prediction".

**Response: The revised manuscript includes the following foot note:**

**“In some of the literature a “prediction” corresponds to an initial value problem and the “projection” corresponds to a boundary forced problem. Here we recognize that decadal prediction and even seasonal prediction is a both an initial value and a boundary value problem. Throughout the text we refer to the combined initial value and boundary value problem as prediction problem.”**

12. p. 10, first sentence of section 2.3.1 How are the anthropogenic forcings incorporated in the initial conditions? Please clarify.

**Response: In the revised manuscript we note that “The forcing from greenhouse gases and aerosols are included in the initial condition in that they affect the current state of the climate system.”**

13. p. 10 The Ineson et al 2011 reference is not listed.

**Response: Corrected.**

14. p. 11, top paragraph seems somewhat out of place in this section

**Response: Removed from revised manuscript.**

15. p. 11, second paragraph, second from last sentence: This argues for an enhanced AMOC in response to volcanic activity, but Miller et al (2012) argue for a reduction of the AMOC in association with volcanism (as a contributor to the Little Ice Age onset). Thus, there may be some uncertainty with regard to the effect of volcanic activity on the AMOC, and this could be noted.

**Response: Revised as follows:**

**“Volcanic eruptions are not predictable in advance, but once they have occurred they are a potential source of forecast skill (e.g. Marshall *et al*. 2009). A similar approach has been considered for seasonal forecasting; once the atmospheric loading has been estimated based on the severity and type of explosion, this could be used in the forecast model. Furthermore, volcanoes impact ocean heat and circulation for many years, even decades (Stenchikov *et al*. 2009). In particular, the Atlantic meridional overturning circulation (AMOC) tends to be strengthened by volcanic eruptions. Volcanoes could therefore be a crucial source of decadal prediction skill (Otterå *et al*. 2010), although further research is needed to establish robust atmospheric signals on these timescales. Moreover, there is also evidence that volcanism can reduce the AMOC and may have been a contributor to the Little Ice Age onset (e.g., Miller et al. 2012).”**

16. p. 11, line 9 from bottom: Please clarify on what time scale the trend is being compared to the variability (interannual, decadal, multi-decadal?)

**Response: Text removed.**

17. p. 12, middle The Pohlmann et al 2012 reference is listed as "To be submitted"; I suggest this not be cited until it is submitted. Also in that paragraph, it is not clear to me how "… the AMOC is skillfully predicted up to 5 years ahead …" when earlier in the paragraph it is stated that skill in Atlantic SST predictions cannot be related to AMOC variations due to a lack of observations. I recommend that the statement on skillful AMOC prediction be modified to perhaps emphasize the model-based evidence for potential predictability.

**Response: Revised manuscript now reads as follows:**

**“Based on model predictability experiments, improved skill in north Atlantic SST is expected to be related to skilful predictions of the Atlantic meridional overturning circulation (AMOC), but this cannot be verified directly because of a lack of observations. However, recent multi-model ocean analyses (Pohlmann *et al*. 2012) provide a consistent signal that the AMOC at 45oN increased from the 1960s to the mid-1990s, and decreased thereafter. This is in agreement with related observations of the NAO, Labrador Sea convection and north Atlantic sub-polar gyre strength. Furthermore, the multi-model AMOC is skilfully predicted up to 5 years ahead. However, models forced only by external factors showed no skill, highlighting the importance of initialization.”**

**The Pohlmann manuscript has been accepted and received favorable reviews.**

18. I am not sure that Table 1 is really necessary.

**Response: The text has been revised to include more discussion of Table 1, making it a more relevant to the manuscript.**

**“Not only is the MJO important in the tropics, there is growing evidence that it has an important influence on northern hemisphere weather in the PNA (Pacific North American pattern) and even in the Atlantic and European sectors. Cassou (2008) and Lin et al. (2009) have studied the link from the MJO to modes of the northern hemisphere including the North Atlantic Oscillation. In Lin et al. (2009) time-lagged composites and probability analysis of the NAO index for different phases of the MJO reveal a statistically significant two-way relationship between the NAO and the tropical convection of the MJO (see Table 1). A significant increase of the NAO amplitude happens about one to two weeks after the MJO-related convection anomaly reaches the tropical Indian Ocean and western Pacific region. The development of the NAO is associated with a Rossby wave train in the upstream Pacific and North American region. In the Atlantic and African sector, there is an extratropical influence on the tropical intraseasonal variability. Certain phases of the MJO are preceded by two to four weeks by the occurrence of strong NAOs. A significant change of upper zonal wind in the tropical Atlantic is caused by a modulated transient westerly momentum flux convergence associated with the NAO.”**

19. For Figure 3, I am not sure that listing the statistical significance adds much here.

**Response: We have included the statistical significance for completeness.**

**Reviewer C**

Julia Slingo (attached)

**Response: We have adopted all of the suggested comments from this reviewer.**