Reviewer 1

Review comments on "Sea Ice Assimilation into a Coupled Ocean-Sea Ice Adjoint Model of the Arctic Ocean" by Koldunov et al.

1. ***Summary***

The paper deals with an ocean-sea ice data assimilation experiment in the Arctic Ocean by an adjoint method. The data used for the assimilation are ocean hydrography (in-situ and remotely sensed measurements and climatology) and sea ice concentration. These data are assimilated to a regional coupled ocean-sea ice model (MITgcm + Hibler-type sea ice model) for a state estimation of 2000 - 2008 period, by adjusting the initial and boundary conditions for the model (the control vector is composed of ocean initial conditions and atmospheric boundary conditions). The authors report substantial improvement of modeled sea ice concentration and position of ice edge in summer, as well as improvement of seasonal cycle of sea ice cover and ice thickness distribution. The authors also report that the improvements occur mainly due to corrections to the surface atmospheric temperature.

We thank the Reviewer for thoroughly evaluating our manuscript. In the following Reviewer's comments are in italic, our answers are in usual font, text from the manuscript is in the quotation marks and the new text is in blue color.

2. ***General comments***

The adjoint method is one of the promising data assimilation methodologies for state estimations, since the method can preserve modeled physics in the estimated state. The estimated state is not only usable as a 4-dimensional interpolation of observation, but also applicable for dynamical interpretations of the system. However, setting up the adjoint model, which has the consistent physics with the corresponding forward model, sometimes requires substantial efforts, and in addition running the adjoint model with a long assimilation window needs linearization of the code. The authors seem to suffer from these technical issues, although the details were not provided in the manuscript. They run the adjoint model without some ocean modules nor sea ice dynamics. The latter issue (adjoint run without sea ice dynamics) is crucial for the current experiment design, since the study focuses on improvement of sea ice status and associated corrections to the control variables. In addition, the authors divide the 2000 - 2008 assimilation window into 1 year chunk. This division seriously deteriorates the advantage of the adjoint method, since the observed data cannot contribute to improve the model status of preceding years. This issue may affect one of the conclusions of this study that the authors don't find sizable improvements of ocean field, since the spin-up/spin-down time for ocean is much longer than that for sea ice. For these reasons, the present study did not take the advantage of the adjoint method. Since the authors did not provide the details of the experiment design (e.g., definition of the cost function, penalty term, uncertainty for each observational data), some of the results in the manuscript are difficult to interpret. As a whole, unfortunately I cannot find any new technical achievement nor sound scientific findings for Arctic ocean-sea ice system in this manuscript.

We agree with the Reviewer that not all of the possible advantages of the adjoint assimilation method were used in this study and that by employing all of them the study would benefit greatly. However, as the Reviewer have mentioned, getting the adjoint assimilation to work is a technically challenging task and it is hard to expect that all the technical issues would be solved in the pilot study we present here, in which we are only beginning to gain experience with adjoint sea ice assimilation.

Saying that the present study "*did not take the advantage of the adjoint method*", while we agree in principle that long-term processes are not present in the sensitivities, at the same time is in our opinion an overstatement. Adjoint assimilation techniques are successfully used in ocean and atmospheric sciences with short assimilation windows and certain approximations of the full forward model are made in the adjoint mode.

It seems that a lot of the Reviewer's judgment about our results is based on the erroneous assumption that the sea ice in the adjoint model was not allowed to move. We admittedly made a bad selection of words by saying in the "Methods" section that the "sea ice dynamics were switched off" in the adjoint mode. What should have been said is that the sea ice is in the "free drift" mode and that the ice rheology is not taken into account.

It certainly is wishful to have rheology included as well as being able to benefit from long assimilation windows. However, the adjoint method has strong limitations for nonlinear systems, which cannot easily be circumvented and that unfortunately prevent taking full advantage of capabilities one might expect from applications with more linear models. In particular, the initial goal was to do the assimilation in one sweep and we tried extending the window to periods longer than one year. Unfortunately, the gradient information was no longer useful for the improvement of the state. All this explanation is now included in the text. In addition, following the Reviewer's request, we also added more information about the design of our experiment.

More detailed answers to reviewer's criticism are provided below.

3. Major points

- Why the authors switched off the sea ice dynamics of the adjoint model? Since the main focus of this study is to examine the effect of sea ice concentration assimilation on the modeled field and correction to the control variables, the adjoint run without sea ice dynamics may seriously deteriorate the results. Particularly, I have concerns about the result that the system obtained the optimal solution of sea ice concentration by the corrections to the 2 m air temperature. Since the adjoint model doesn't take the sea ice dynamics into account, the dynamical forcing, such as wind forcing and/or ocean drag, cannot directly contribute to improve sea ice concentration by the assimilation. I would guess this is one of the reasons why the ice concentration improvement occurs mainly due to thermal forcing (2 m air temperature), and occurs not by dynamical forcing. To obtain the conclusions in this study, the sea ice dynamics is indispensable in the adjoint model.

As stated above, we have only switched off the sea ice rheology, so that the sea ice is in a free drift mode. We admittedly did a very poor selection of words by saying that "sea ice dynamics were switched off" and it is fixed now in the text as follows:

"The sea ice module was active in the adjoint integration, but the part of the sea ice dynamics that treats rheology was switched off, so the sea ice is in a free drift configuration."

The reason for switching off rheology was that we were not able to get useful sensitivities for the sea concentration for long periods of time with the rheology on. This is undoubtedly a flaw in our methodology, but simplification of the adjoint model to provide more useful gradients is a common practice in the adjoint data assimilation community. We note that in the published study of Fenty et al. (2015) similar simplifications were done to the adjoint model. Liu et al. (2012) showed that removing a certain part from the adjoint has little effect on the adjoint model during short time periods, while it prevents the adjoint integration to become useless on longer time scales.

Realizing that in a free drift mode the model does not consider internal sea ice stresses, the dynamical forcing actually seems to contribute more to the improvements of the sea ice than if the rheology would have been switched on. This actually emphasizes corrections to the wind stress in comparison to other components. As a result of the additional analysis of the corrections to the controls (requested by Reviewer 2), we have removed the statement about the relative contribution of thermo-dynamical forcing to the improvement of the model state.

- The division of the 2000 - 2008 assimilation window into 1 year chunks deteriorates the advantage of the adjoint method. Due to the current set-up of the system, the system cannot use the observed data to improve the model status for preceding years. I am afraid that this short assimilation window might be a cause of the small improvement of ocean status compared to sea ice in the current assimilation (ocean needs longer spin-up/spin-down time than sea ice, and 1 year window is too short even for layers shallower than Arctic halocline). I also would like to point out that even for sea ice, 1 year chunk is too short, if the authors intend to examine improvement of ice thickness. In order to extend the assimilation window, I think further linearization of the code (including consistency check with the original code) is necessary.

The reasons for choosing 1 year-long chunks are purely technical in nature (this will be explained in more detail in the answer to a specific question below). The limited time scale of the assimilation is a valid concern, however, as it is shown in our work and in Fenty et al. (2015), one year is enough to significantly reduce model-data differences for the sea ice and not only in one specific year (as in Fenty et al., 2015), but consistently for several years in the 2000s.

The small assimilation window certainly limits the extent to which the data can affect the state. Particularly, processes that act on long time scales such as in the interior of the ocean will not adjust much, which may explain the small ocean improvements. We believe that more important is the much larger amount of sea ice concentration and SST data compared to the very limited amount of data in the Arctic Ocean water column, although one may also hope for ocean improvements though the assimilation of sea ice data. Reviewer 2 proposed a similar explanation below. This conclusion is based on the spatial distribution of adjustments to control variables that obviously mostly reduce discrepancies between modelled and observed sea ice area.

- The authors should describe how the cost function is defined (i.e., the objective function, the gradient of which is estimated by the adjoint), since the definition of the cost (i.e., weighting between different types of observed data, error estimates for observational data, and definition of penalty term, etc.) strongly affect the behavior of the system. Due to the lack of these information, some results shown in the manuscript are difficult to interpret.

We have added the information requested by Reviewer in the text. The changes are described in detail in the answers to specific questions below.

- How do the authors control or constrain the allowable corrections to the control variables? Since the spatial pattern of the correction to the 2 m air temperature (Fig. 8) is quite similar to the bias of the free run (Fig. 3), I have a concern that the system changes the control variables without reasonable constraint. The authors cite Köhl (2015) as a reference for the

atmospheric condition. In this paper, the errors of atmospheric field are prescribed by the standard deviation of the NCEP field. What does this mean? The standard deviation of the NCEP field is the ensemble spread of the NCEP climate model? If so, the standard deviation does not provide error of the reanalysis field, but provides the magnitude of the natural variability of the modeled climate system. Since the adjoint method tries to impose all the deficiencies of the model status to the control variables, the validity of the allowable corrections to the control variables should be carefully examined.

The constraint on the control variables is separated into a mean and a time varying part. For the time varying part we use the standard deviation of the NCEP fields. Arguably, this choice would reflect a very pessimistic view of the NCEP data. The reason for this generous choice is that parameters are updated on a daily frequency and background information are mapped fields, which render the data of the constraints much larger than the actual ocean or sea ice data. Since errors of the controls are correlated in space and time, the actual degree of freedom is much smaller than the number of data provided for the constraints. The generous error in constrains compensates for the lack of correlation in the error weights. We believe that the standard deviation gives a very good approximation for the relative errors, although not for the absolute errors. The posterior evaluation of the corrections does not reveal any unrealistically large change in parameters which would point to a critical influence of the weights.

We added the following information in the text:

"For the atmospheric control variables, uncertainties were specified as the maximum of the STD of the NCEP fields and the errors for the mean components of air temperature, humidity, precipitation, downward shortwave radiation and wind were specified as 1°C, 0.001 kg/kg, 1.5×10^{-8} mm/s, 20 W/m^2 and 2 m/s, respectively. For the downward shortwave radiation, both mean and time varying parts were set to 20 W/m^2."

- What is the new technical achievement or new scientific finding(s) of this study, particularly, in comparison with the number of former sea ice (and partly ocean) data assimilation studies for Arctic region? Since the sea ice dynamics of the adjoint model is not consistent with the forward model and the assimilation window is only 1 year, I hardly find any advantages on this experiment compared to the former studies using another assimilation method (e.g., optimal interpolation, 3D-Var, Green function method and EnKF).

We perform the first multi-year adjoint data assimilation for the coupled Arctic Ocean sea-ice system. To our knowledge, only one other Arctic data assimilation exercise exists that assimilates sea ice and hydrographic information based on the adjoint method (Fenty et al., 2015). But those authors first assimilate hydrographic data and then sea ice, while we do both simultaneously. The adjoint method, as explained in detail by Fenty et al. (2015) and also in our text, is able to adjust the model in a dynamically consistent way, which the other methods (except the Green's function) are not able to do. The Green's function method, due to its limited amount of adjustable parameters, is at the edge of being an ocean-sea ice synthesis method. Although it does assimilate data, the influence of the few parameters is so limited that they are mostly only able to improve the climate of the model. Being a pioneering work, our effort is certainly not without potential for further improvement; however providing a description of the experience and lessons we have learned in the process will certainly be important and useful for the data assimilation community, which will pick up on this work and carry it forward.

As we already mentioned, the Reviewer's criticism of the sea ice dynamics in the adjoint not being consistent with the forward model is due to the wrong interpretation of our (faulty) model description, in particular, the assumption that the sea ice is not moving in the adjoint. In reality the sea ice in the adjoint model is in the free drift mode. Simplification of the adjoint model is a standard technic and similar modifications in the adjoint model were made, for example, by Fenty et al. (2015).

In summary:
- we believe that in our experiment design we use practices that are common in the adjoint assimilation community and which lead to dynamically consistent state estimates of the ocean-sea ice system.
- for the first time pan-Arctic multiyear coupled ice-ocean adjoint state estimate is performed and results are of significant interest for the data assimilation community and, in particular, to researchers dealing with the adjoint method.

4. Minor points

- Page 4, line 16: Which module is excluded from the adjoint code? What effect do the authors expect by this exclusion, particularly, in relation to the conclusion of this study? –

We modify the paragraph to explicitly mention the code modules inclusion/exclusion and possible effects:

"The adjoint model was modified here similarly to Köhl and Stammer (2008) to exclude KPP modules and increase diffusivity values compared to the forward run. This is done to avoid exponentially growing adjoint variables. The sea ice module was active in the adjoint integration, but the part of the sea ice dynamics which treats rheology was switched off, so that the sea ice model was in a free drift configuration. This approach led to a reduced (approximate) adjoint producing smoother adjoint gradients. These gradients can still be successfully used to improve the large scale state of the model (see Köhl and Willebrand (2002) and Köhl and Stammer (2008) for more details). Similar simplifications of the adjoint model were used by Fenty at al. (2015) and Liu et al. (2012) provided an evaluation of the effect of modifications in the parameterizations on the adjoint. They confirm mostly small changes, although regionally some patterns of the gradients may shift. Since the gradients are only a means to find the cost function minimum and the forward code (and thus the minimum itself) is unmodified, changes to the gradient may lead to lower performance in finding the minimum but not to different states once the minimum is found."

Page 4, line 16-17: Why the sea ice dynamics are switched off? Due to the absence of sea ice dynamics, the adjoint model cannot provide the optimal gradient, and therefore the correction to the control variable may not reflect (modeled) reality.

As mentioned in the response to the major points, we have switched off only the rheology of the sea ice model, so that the sea ice in the adjoint model was in a free drift mode. We apologise for the confusion this might have caused since it might have given the impression that the adjoint variables to sea ice don't move at all.

As explained already in the answer to the previous point, we believe that simplifying the adjoint of the forward model is a necessary condition to get gradients that are instrumental in effectively reducing the cost function. We are not aware of any realistic long-term ocean adjoint data assimilation study that uses the full adjoint of the forward model.

- Page 4, line 20-23: Are the control variables used to define penalty term in the cost (object) function? Please provide the exact definition of the penalty term for reproducibility of the study.
- Page 4: Please describe exact form of the cost function (including penalty terms) used in this study.

We have added a brief definition of the cost function in the text, but we point the reader to Fenty et al. (2015) for more details, who have identical cost function formulation. The text now reads as follows:

The cost function J is defined as follows:

$$J = \sum_{t=1}^{t_f} [y(t) - E(t)x(t)]^T R(t)^{-1} [y(t) - E(t)x(t)] + v^T P(0)^{-1} v + u_m^T Q_m^{-1} u_m + \sum_{t=0}^{t_f - 1} u_a(t)^T Q_a(t)^{-1} u_a(t) \quad (1)$$

where y(t) is a vector of assimilated data in time t, x(t) is a vector of the model state, E(t) is a matrix which maps the model state to the assimilated data, v is a first guess initial condition, u_m is a mean atmospheric state and $u_a(t)$ is a time-varying atmospheric state. Additional weights $R(t)^{-1}$, $P(0)^{-1}$, Q_m^{-1} and $Q_a(t)^{-1}$ control the relative contribution of different terms in the cost function. More detailed description of the cost function and optimization procedure can be found in Fenty et al. (2015).

- Page 4, line 23: How did the authors define the uncertainty for hydrographic (e.g., EN3, NISE, etc.) and satellite data? Although the author cited Köhl (2015) as a reference, the representation error of in-situ measurements should be different since the resolution of the model differs.

The representation error unfortunately will remain the same for two reasons. First, in comparison to a Rossby radius of less than 5 km in the Arctic, a resolution of 16 km is still not sufficient to resolve eddies and the related processes in the Arctic. In relation to the Rossby radius, the Arctic configuration is probably of similar resolution as the global configuration on average. Second, even for a truly eddy resolving version, the problem of the representation error remains for all assimilation windows longer than a few eddy turnover times scales, because on these longer time scales we loose the ability to reconstruct individual eddy development and movement due to the chaotic dynamics. The eddy field becomes statistical and only an adjustment of the statistical properties remains feasible (see Köhl and Willebrand (2003) on how this can be achieved).

- Page 4, line 24: Why did the authors apply constant error of 50% for sea ice concentration? OSI-SAF provides uncertainty estimates of ice concentration at every grid points. The constant error does not take into account the large uncertainties over the marginal ice zone, while it underestimates weight of reliable data over the central arctic.

We added the following explanation in the text:

"We verified the sensitivity of our results by using space-time varying sea ice uncertainty estimates as they became available, as well as different values of a constant error. Results of the sea ice assimilation with variable uncertainties were very similar to the ones with a constant error value of 50%.“

- Page 4, line 24: Why the data assimilation is performed in one year chunk? Due to this experiment design, the authors cannot take the advantage of adjoint method. Particularly, the authors cannot examine the effect of data assimilation to the ocean variables, since 1 year chunk is too short even for layers above Arctic halocline.

The use of one year chunks indeed has limited our ability to get improvements related to the long-term ocean variability. However, we disagree with the Reviewer's strong statements that "*the authors cannot take the advantage of the adjoint method*" and "*cannot examine the effect of data assimilation to the ocean variables"*. We believe that our study actually demonstrates the opposite. A one year time scale seems to be enough to successfully assimilate sea ice concentrations, which, as mentioned in the subsection "2.2 Adjoint data assimilation approach", is the main focus of the study. It is also enough to considerably alter the surface layers of the ocean, which are most important for the short-term ocean-atmosphere exchange.

We added the following text to the manuscript:

"The use of one year segments is related to technical reasons; we are not able to get useful sensitivities for a time period longer than a year for all years of our 2000-2008 assimilation period. We were successful in completing a 2-year assimilation at one occasion (2005-2004), but the results for sea ice area and thickness were not noticeably different from the 1-year chunk assimilation."

Getting stable and useful adjoint gradients on longer time scales for sea ice concentration is a challenge, which to our knowledge groups around the world did not solve to date. Sea ice is a faster moving medium compared to the ocean and in addition is not a smooth global field, making it hard to handle in the adjoint. This study exploits achievements in adjoint sea ice assimilation that are currently available. We believe that in our manuscript we have demonstrated that, even for short assimilation periods, the use of the adjoint method in the Arctic is useful.

- Page 5, line 8-10: How did the authors define the relative weight between different types of observation? Satellite measurements generally cover the large area with constant time interval, whereas the errors of the data are not independent but covariant. On the other hand, in-situ ocean measurements are very sparse, while the measurement errors are almost negligible (and therefore assumed to be independent each other) compared to the representation error. In addition, the magnitude of the representation error depends on the size of model's grid cell. How did the authors handle these issues? Description is needed.

Little is known about the covariance of errors, and error covariances for data terms are difficult to implement into the adjoint method (a feature not implemented so far). For most of the data, the error covariance is not a large problem because the data is sparse. Exceptions are mapped data that are processed via objective analysis. We took care of the reduced degree of freedom for the climatological data by increasing their error by a factor of two. All other data errors are not adjusted since only relative errors matter for the cost function and similar constraints apply for all data. For instance, in situ data error is correlated with depth while along track data is correlated along the track. Lacking the ability to specify anything clearly better, we settled with the simplest approach to assume that reduction in degrees of freedom is similar across all data types.

- Page 6, line 23-25: I don't understand the meaning of this sentence. More explanations are needed.

We rewrote the sentence as follows:

"Both metrics suffer from the inability to guarantee that improvements in this metric also lead to an overall improved match in the spatial sea ice coverage, since a perfect total SIC or SIE evolution may still correspond to considerable differences to the data in their regional distribution."

- Page 6, line 28-30: Please explain relation between the Hausdorff distance (Dukohvskoy et al. 2015) and the metrics used in this study (sum of the RMS errors). I do not understand why the authors introduced Dukohvskoy et al., (2015) here.

The reviewer is correct – we mentioned the Hausdorff distance from Dukohvskoy et al. (2015) without actually using it. We therefore removed the two last sentences in the paragraph and moved the reference to Dukohvskoy et al. (2015) to the end of the previous sentence:

"This calls for changing the common practice of model evaluation by only comparing their ability to simulate present day SIE without considering the sea ice spatial distribution (e.g. Dukhovskoy et al., 2015)."

- Page 7, line 18-24: It is hard to believe that the spatial pattern of the bias of 2 m air temperature of NCEP reanalysis coincide with that of modeled ice concentration bias. If I understand correctly, the uncertainties of the NCEP reanalysis data used in this study are given by the standard deviation of ensemble runs of NCEP climate model. Does the difference between the ensembles has such a sharp gradient like the correction to the 2 m air temperature (Fig. 8 the first row, left)?

The NCEP reanalysis uncertainties were determined as the standard deviation of the whole NCEP time series not as deviation of ensemble runs of the NCEP model. We are not aware of any ensemble runs of the NCEP RA1. This reanalysis is based on 3DVAR and does not produce an ensemble as part of the method. Maybe the reviewer has a different reanalysis in mind, but why would results from that be more appropriate? In any case, the shape of the correction field should not correspond to the uncertainties field of the control variable because corrections are related to the errors, while uncertainties describe only statistical properties of errors. Values of the corrections of course should be within the range of the uncertainties, which is the case.

- Page 7-8: The authors described that the correction to the 2 m air temperature is the main driver to improve the sea ice concentration in the assimilated field, and the contributions from other control variables, such as wind forcing, are very small. I am afraid this may be an artifact due to the lack of sea ice dynamics in the adjoint model, as described in major point.

As described in the answer provided above to the major points, our previously incomplete description must have led to the assumption that the adjoint variables to the sea ice are not moving at all, while the actual approximation in the adjoint would lead to the opposite effect. We completely understand the Reviewers' confusion and therefore we tried to make description of the adjoint formulation clearer. We have also considerably modified the section

“Control variables” and the statement about the relative contribution of thermo-dynamical forcing to the improvement of the model state is removed.

- Page 7, line 31-33: The wind can play a role not only in local redistribution of the sea ice along the shore and ice edge, but also in large-scale sea ice distributions, although such effect is not seeable in the present experiment design.

For the case of the free drift used in our work we expect that the gradients come from a model in which the sea ice is actually more responsive to the wind forcing compared to the case where the rheology was switched on. In the new version of the “Control variables” section, the statement the Reviewer is referring to was removed.

- Page 8, line 8: “.., both making atmospheric forcing actually worse”. I do not understand the meaning of this sentence.

We modified the text in the following way:

“But it could equally also point to problems of the correct attribution of sea ice concentrations from satellite data. In both cases, corrections to atmospheric control variables will not improve the quality of the original atmospheric forcing, but on the contrary may make it worse.“

- Page 9, line 24-25: This is interesting. Could the authors provide specifications of the mechanism?

In our opinion the most probable reason for the slight reduction of the positive temperature bias in the Eurasian Basin of the Arctic Ocean is a modification of the Atlantic Water upstream, before it enters the Arctic Ocean. Polyakov et al. (2005) estimated the travel time of the Atlantic Water temperature anomalies between the Svinoy section and Fram Strait to be about 1.5 years. Taking into account the relatively good observational coverage of the North Atlantic Ocean, even with short assimilation periods, the modifications of the near-surface ocean layers before they enter the Arctic Ocean and dive under the halocline can be enough to alter properties in the deeper layers of the Eurasian Basin. In other words, the model probably fixes the problem of too warm Atlantic Water entering the Arctic, and the reduced temperature bias in the Arctic Ocean itself is a consequence.

- Page 9, line 33 – Page 10, line 1: How much is the ratio of relative contributions to the cost function between ocean variables and sea ice variable? If the contribution from sea ice variables dominates, the system tries to change the control variables which have large impact on sea ice, and then it is natural that the changes of the ocean variables are small.

In Section 2.2 we wrote the following:

“Taking into account differences in the amount of sea ice concentration and sea surface temperature data compared to the amount of hydrography data, it is not surprising that most of the contributions to the total reduction of the cost function are from SIC and SST. Hence, most of the improvements can be expected to happen in these fields, while changes in the state of the ocean are expected to be small.”

However, the reviewer is correct in that a similar statement is appropriate in the discussion about ocean changes. We therefore modify the text as follows:

"This is probably due to the fact that the volume flux is mostly controlled by the wind stress, which means that the corrections of the control variables discussed above do not contribute considerably to changes in the ocean circulation. This is expected since the amount of sea ice concentration data is much larger than the number of hydrographic observations in the Arctic Ocean, so that the assimilation system tries to change control variables in a way that will have larger impact on the sea ice. However, episodically, significant changes can be observed (for example in summer 2008) when modifications in the throughflows at Fram Strait are noticed, which are about 60% larger than in the forward simulation (Fig. 11a)."

- Page 10, line 1-3: The fluxes shown in Fig. 11 are improved by the assimilation? As far as I know there are some flux estimates through Arctic gateways based on observations (e.g., Tsubouchi et al., 2014, JGR).

Thank you for pointing us to this publication. However, the flux estimates presented in Tsubouchi et al. (2012) are for a very short period of time (August-September). We therefore calculated fluxes for the same period of time and added them to the table, along with estimates from Tsubouchi et al. (2012).

The following text was now added to the manuscript:

"We also show mean fluxes for August-September of year 2005 and compare them to the results of Tsubouchi et al. (2012), who applied an inverse model to data obtained in summer 2005 to calculate net fluxes of volume, heat and freshwater around the Arctic Ocean boundary."

"Considering Tsubouchi et al. (2012) to be a good approximation of observed values in August-September 2005, it is hard to definitely conclude if ocean fluxes become better or worse after the assimilation (Table 2). Some values, such as the volume flux through Davis Strait and the Barents Sea Opening, or the freshwater flux in the Fram and Davis Straits, have changed and became closer to the values of Tsubouchi et al. (2012). Other values moved even further away from their estimates."

- Page 10, line 25-28: see the comments above.

We removed the statement about the contribution of 2m temperature to the improvement of the sea ice state.

- Page 11, line 3-4: This result is interesting, but I am still afraid that this improvement might be achieved by wrong reason, due to the absence of sea ice dynamics in the adjoint, since the ice dynamics is important for the redistribution and accumulation of sea ice.

This comment again is related to the erroneous interpretation of our model setup description (see several comments above) by assuming that the sea ice in the adjoint model is not allowed to move.

- Page 11, line 11-13: I agree that the estimated state of sea ice is consistent with the modeled physics, whereas due to the lack of ice dynamics in the adjoint, we cannot make sure the correction to the control variables are realistic. In other words, we cannot exclude a

possibility that the estimated state is achieved by artificial forcing different from reality, and therefore by thermodynamic and dynamical balance different from reality.

Since the forward model is uncompromised by the approximations made in the adjoint, the effect of these approximations is always secondary, i.e., that a minimum cost function has not been found. In any case, the forward model is certainly also flawed in many ways and there is no guarantee that the estimated state is not achieved by an artificial forcing that has little to do with reality. Our comment at the end of Section 4 is exactly about that. For instance, there are many processes, like tides or ice-wave interaction that are not included in the forward model, which may be responsible for certain biases of the model and ultimately in the estimated atmospheric state. We, of course, act in the framework of approximations included in our model configuration and can only make conclusions about this system.

- Figure 3 caption, line 3: "third" should be "forth".

Thank you. This was fixed.

- Figure 3, 4, 8 and 10: It would be helpful for comparison of the spatial patterns, if the longitude and latitude lines (as in Fig. 9) are embedded in these figures.

To comply with the Reviewer's request, we redraw Figures 3, 4, 8 and 10 to add longitude/latitude grid lines.