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.

Reviewer 2

Summary

In this work the authors demonstrate the synthesis of hydrographic and sea ice concentration data into a 16-km horizontal resolution Arctic and North Atlantic coupled sea ice-ocean model. The reduction of an uncertainty-weighted model-data difference cost function was achieved by iteratively optimizing a set of adjustments to a set of atmospheric and initial condition control variables using gradient information provided by the adjoint of the numerical sea ice-ocean model. The final multiyear state estimate was constructed by optimizing each single year between 2000 and 2008 in succession - the final optimized state of year X is defines the initial state for year X+1. The authors demonstrate improvements of the model's reproduction of the data. The largest reduction in terms of percentage is found with sea ice concentration and SST with lower relative cost reduction for other data, including T and S profiles, SSH, and mean dynamic topography. The largest sea ice concentration cost reductions in terms of RMS are found during summer months. Discrepancies between simulated and observed sea ice extent are found to increase in some months even when discrepancies in simulated and observed total sea ice area decrease. After synthesizing ocean and sea ice data, little impact is seen in ocean volume, heat, and freshwater fluxes through Fram Strait and Davis Strait.

We thank the Reviewer for the thoughtful evaluation of 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.

Specific Comments

1) With respect to the title, assimilation is not "into a Coupled Ocean-Sea Ice Adjoint Model". The assimilation is "into a Coupled Ocean-Sea Ice Model using its adjoint".

We thank the Reviewer for this suggestion and changed the title accordingly.

2) Abstract: Better to provide the actual spatial resolution of the satellite sea ice concentration data that is assimilated rather than refer to it as 'high resolution'.

We decided to remove the reference to the resolution altogether because the sea ice data are assimilated, as the other data, on the model grid. The sentence now reads as follows:

"Satellite sea ice concentrations (SIC), together with several ocean parameters, are assimilated into a regional Arctic coupled ocean-sea ice model covering the period 2000-2008 using the adjoint method."

3) Page 1, Line 6: 'values of sea ice extent become underestimated' doesn't define a metric. Is the metric the sum of model minus data or weighted model minus data difference or the RMS of model minus data or something else?

We have tried to make the statement more precise, now it reads as follows:

"During summer months, values of sea ice extent (SIE) integrated over the model domain become underestimated compared to observations,…"

4) Page 1, Line 6-7: Characterizing a state estimate of a system as complex as the Arctic Ocean requires that one analyzes a suite of metrics. The author's statement that one the sea ice extent metric is "not suitable to characterize the quality of the sea ice simulation" is odd and out of place. To whom is this statement aimed? This seems to be a straw man argument.

The statement is more related to the practice of characterising the quality of a sea ice simulation (not the complete Arctic system) in the model by only considering one metric, namely the integrated Northern Hemisphere September sea ice extent. This is still common in many publications and authors are also guilty of this sin. However, we agree with the Reviewer in that the abstract is not the right place for such a statement and now the sentence reads as follows:

"During summer months, values of sea ice extent (SIE) integrated over the model domain become underestimated compared to observations, however the root-mean-square difference of mean SIE to the data is reduced in nearly all months and years."

5) Page 1, Lines 10-11: The atmospheric control variable adjustments that one finds during any optimization are intimately related to the magnitudes of the prior uncertainties of the individual terms of the first-guess atmospheric state. The author's statement that biases in sea ice are reduced 'mainly due to corrections to the surface atmosphere temperature' is difficult to interpret because the reader does not know the magnitude of prior uncertainties used during e optimization. Are you referring to the sum of the squared normalized adjustments? How is surface atmosphere temperature identified as the main control variable correction since atmospheric forcing units are arbitrary?

As suggested by the Reviewer, we have re-evaluated our analysis of the corrections to control variables and decided to remove the statement about the 2-m air temperature contribution to the improvement of the model state.

6) Page 2, Line 8. The authors may consider using the term 'state estimation' to describe the model-data synthesis methodology used in this study instead of the term 'data assimilation'. An uninformed reader may think that the work conducted here referring to sequential data assimilation, a technique that has been applied to sea ice data for decades. The adjoint method used in this work is rather special and yields quite a different product (namely a physically-consistent ocean and sea ice state). Below I post an excerpt from Wunsch and Heimbach, 2007 in which they argue for their choice of the term 'state estimation' when describing the application of the adjoint method to combine data with a model (emphasis mine): "In physical oceanography, the problem of combining observations with numerical models differs in a number of significant ways from its practice in the atmospheric sciences. It is these differences that lead us to use the terminology "state estimation" to distinguish the oceanographers' problems and methods from those employed under the label "data assimilation" in numerical weather prediction. "Data assimilation" is an apt term, and were it not for its prior use in the meteorological forecast community, it would be the terminology of choice. But meteorologists, faced with the goal of daily weather forecasting, have developed sophisticated techniques directed at their own particular problems, along with an opaque terminology not easily penetrable by outsiders. Because much of oceanography has goals distinct from forecasting, the direct application of meteorological methods is often not appropriate."

The term “adjoint data assimilation” is used in the community and even the usage of the term “state estimation” exist for applications with the Kalman filter. Since there is no agreement in the community, the reader has to anyway carefully read the methods section of the paper in order to understand how exactly data were used to improve the model. We agree with the Reviewer that the term “state estimation” is probably better to put the reader on the right path. However, “state estimation” inherited the flavour of trying to estimate a static climatological state, as it was attempted in the first applications of the adjoint method during the WOCE era, therefore we find it to be a less appropriate term; but again, current usage of both terms show that the nomenclature is not well defined and although we could live with the term “state estimation”, we don’t find it really better.

7) Page 4 Line 10-11: List the control variables.

The list of all control variables can be found in the text below. We did not use the longwave radiation as a control variable for final simulations. We adjusted the text accordingly.

8) P4 Line 20: Describe why the atmospheric control variable frequency was changed to daily.

We incorrectly used information from a different experiment setup, so the frequency of updates is actually once per three days, which is still higher than 10 days used by Köhl (2015), who has chosen 10 days due to computational (memory) limitations. We added the requested information and now the sentence reads as follows:

“In contrast to Köhl (2015), additional control variables are optimized and the frequency of the updates is enhanced to once per 3 days in order to reflect shorter time scales of sea ice variability.”

9) P4 Line 23: As atmospheric adjustments are an important control parameter in this work, the authors should (a) explicitly state how they were derived as Kohl (2015): "For the atmospheric state, errors are calculated as before from the [standard deviation] of the NCEP fields." And (b) show maps of their magnitudes in the main text or in supplemental materials. Also, because they are so important, more discussion about your choice of standard deviation of NCEP fields is appropriate. The standard deviation of Arctic near-surface atmosphere temperatures is considerable given the large seasonal cycle. In much earlier versions of ECCO/GECCO the use of atmospheric state standard deviations could be justified because in mid-latitudes and the tropics they partially captured "random" variations due to synoptic variability. At high latitudes the standard deviation for near-surface atmosphere temperature and shortwave radiation is mostly due to the seasonal cycle.

Although there is a large seasonal cycle the difference between the STD with and without the seasonal cycle is actually not that large for most of the globe; but it is true that in the Arctic region the error is with values around 12-30˚C overestimated by a factor of 2. The STD is in both cases relatively homogeneous, such that a figure would not provide valuable information.

We have added the following text to the manuscript:

”For the atmospheric control variables, uncertainties are 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."

10) Page 4, Line 24: Why are the sea ice data assigned a constant 50% error? Satellite SIC products have errors that are far smaller than that everywhere except in the MIZ and in summer when meltponds are present.

We added an explanation in the text:

"We verified the sensitivity of our results by using space-time varying 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 constant error value of 50%."

11) Page 4, Line 27: To clarify, each year after the first uses initial conditions that are identical to the final state of the previous year, correct?

Yes, this is correct. We feel that the sentence: "After the first year assimilation, we move to the next year using the final state of the previous year's successful iteration as initial conditions.", describes this sufficiently.

12) Page 4, Lines 3-4: Some SST products have nonzero values beneath sea ice. Is that the case in the RSS dataset?

The RSS data have a "sea ice" flag that, in practice, means missing value. We didn't take the SST data if the "sea ice" flag was set.

13) Table 1: I understand that the PHC climatology had large biases relative to modern Arctic T and S because it was derived with observations mainly from the 1970's and 1980's and before the recent shifts in Arctic heat and freshwater (McPhee et al, 2009). Can you comment on how the simultaneous use of the PHC climatology alongside contemporary data may have affected the T and S cost reduction?

We used the PHC climatology only for model initialisation and it was not used in the data assimilation. We have removed it from Table 1. Since the model was started from PHC and in situ data is sparse, the model cannot be corrected very much away from the first guess, a point made in the text.

As mentioned below in the paper, the changes of the deep ocean state are quite small due to both the predominance of sea ice and sea surface temperature observations over interior hydrographic observations and the short assimilation periods (yearly chunks). Using a more recent Arctic Ocean state as initial conditions would certainly be beneficial due to an initially smaller cost in T and S. However, judging from the experience we gained during this exercise, we believe that there would still be hardly any significant cost reduction of T and S beyond surface layers in the Arctic Ocean, mainly again because of our experiment design and a much larger amount of sea ice data compared to hydrography data.

14) Page 4, Paragraph 1: Cost function reduction percentages are important but obviously they are dependent on how close to the data you were when you began your simulations. The *first-guess solution of Fenty et al., (2015) could have been further from the data than your first-guess solution. While both may end up in the same state, their reduction percentage would be higher. The most important information is how well one's final state estimate fits the data. Much less important is the magnitude of the improvement relative to one's (somewhat arbitrary) starting point.*

We agree that just stating the percentage reduction is problematic, but it is nevertheless important information about the performance of the assimilation. We cannot really assume that we found a minimum, and therefore success is usually evaluated by the amount of reduction. It would not be a too bad assumption that both controls perform about equally well. We have added the caveat of this comparison in the text:

"In 2004 the cost reduction of sea ice area was about 30%, less than that reported by Fenty (2015) (49%), which may partly be explained by differences in the first guess solution."

15) Page 4, before line 31: It may be useful to mention how many iterations were conducted before the 1% threshold was achieved. In Figure 3 I see "iteration 3" as the final iteration for 2005 and 2007. That strikes me as unusual. If your cost was dominated by SIC and SST data and the adjoint method quickly reduced the misfits of those data, then I can see how you hit the 1% total cost reduction threshold quickly. However, it is possible that if those two datasets were ignored, the adjoint machinery could have continued to substantially reduce misfits in other datasets. Can you comment on that?

The Reviewer is correct. The cost is dominated by SIC and SST. These data easily respond to the surface controls. We added this explanation:

"The cost is dominated by SIC and SST data, which easily respond to the surface controls, and the adjoint method quickly reduced the misfits of those data, so that the number of iterations was usually less than five."

16) Page 5, Line 15: There may be a missing figure. I cannot match up Figure 2 to the description offered here. Fig 2 is % cost reduction in different years vs. data.

Thank you very much for spotting this. The first paragraph of the section "Sea ice concentration changes" was from a previous draft version. We did not intend to include it in the manuscript. It is now removed.

17) Page 5, Line 24: Good to additionally mention why most models overestimate sea ice in the Greenland Sea with a reference.

This statement was referring more to results of climate models (see for example Figure 9.23 in IPCC AR5 Chapter 9). It is not correct to transfer this result to regional ocean-sea ice models because much of the bias in climate models result from biases in the atmosphere. So we have removed this part of the sentence. Now the end of the sentence reads as follows:

"Most noticeable is the decrease in the SIC along the east coast of Greenland after data assimilation"

18) Page 5, Line 35: This is probably because in these extreme months the location of the sea ice edge is relatively stable compared to spring and fall months when the ice pack contracting and expanding.

We thank reviewer for this explanation, which we added in the text with a slight modification. Now the text reads as follows:

"Interesting to note, values of RMSE in March and September are quite similar, despite the large differences in ice cover in the two months. One of the possible reasons is that the location of the ice edge in those extreme months is relatively stable compared to spring and fall when the ice pack is contracting and expanding."

19) Section 4: This entire discussion must be rewritten. Atmospheric control variable adjustments seem to be compared by their relative magnitudes but their relative magnitudes are not meaningful because these physical variables have different, arbitrary, units. By all means show the magnitude of the adjustments but to make a meaningful comparison one should first normalize them by their prior uncertainties. a. This includes Figure 8, which should be updated to show all control variable adjustments normalized by their uncertainties. Also include longwave radiation.

Neither dimensional, nor normalized values provide the impact of the changes per se. The prior errors of controls have nothing to do with the impact or even the anticipated impact but describe only our knowledge about them. Moreover, our choice of STD does not make a difference because there is no reason why one STD of perturbation should have a similar impact across all parameters.

Nevertheless, in the optimization the parameters enter normalized, and corrections are generated according to the normalized sensitivities and the approximation of the Hessian matrix. Since we have only a few iterations completed, the Hessian stays not very far away from its initial value, which is the identity. Therefore, in this special case the normalized corrections will still more or less reflect normalized sensitivities. Since the impact is the product of the sensitivity and the corrections, normalized corrections will provide a reasonable measure of the relative importance of the parameters.

As suggested by the Reviewer we have added the normalized corrections to Fig. 8 and reworked the section. The additional text reads as follows:

"Dimensional values of the corrections do not directly provide information about the relative importance of changes in the controls for bringing the model into consistency with observations. However, due to the relatively small number of iterations, we can use values of the corrections normalized by uncertainties as a reasonable measure of the relative importance of changes in control parameters. Spatial distributions and monthly means of absolute values of normalized corrections for the year 2005 are shown in Fig. \ref{fig:8}.

Wind corrections seem to play integrally a larger role, with a maximum in May. This agrees well with results of \citep{Kauker2009}, who used an adjoint sensitivity analysis to determine the relative contribution of different atmospheric and ocean fields to the September 2007 sea ice minimum and found that the May-June wind conditions are one of the main factors in setting up extremely low sea ice conditions in Summer 2007. The maximum contribution of air temperature corrections occurs in June and it is about a factor of five smaller than the contribution of the wind corrections. However, using free drift in the adjoint biases the sensitivities towards larger sensitivities of sea ice to wind changes. Since measuring the impact by the normalized corrections relies on the assumption of correct sensitivities, the results may be also biased to too large an impact by the wind.

Given the absence of proper sea ice dynamics in the adjoint model (only free drift is used) and lack of many important processes in the forward model (such as tides or waves), the question remains to what extent corrections to control variables reflect deficiencies in the forcing fields or a compensation to the sea ice model or sea ice data deficiencies, particularly since in the Arctic the NCEP reanalysis seems to perform well near the surface \citep{Jakobson2012}."

Showing only normalized adjustments maybe tells a lot to people involved in the adjoint community, but for most people it's just easier to look at absolute values that have some physical meaning.

The long wave radiation was not a control variable in our final simulations, so we can't show it. We have changed the text in the method description accordingly.

20) Page 8, Line 20-22. The "probably realistic" spatial distribution of the Kwok Arctic sea ice thickness field deserves a reference. Are the 0.7 m errors spatially correlated or uncorrelated?

The 0.7 m is a mean error; the individual values would vary of course, depending on the sea ice thickness. Reference to Kwok et al. (2008) was added.

21) Page 9, Line 22-24: Neither the length of the simulation nor the number of T/S profiles is a fundamental impediment to magnitude of model-data misfit reduction. An iteration 0 state with T and S close to the data as measured by the prior uncertainty could be responsible. Maybe averaged normalized costs should be added to Figure 2 for each cost category for iteration 0 and the final iteration.

The reason why we think the number of data and time period of assimilation matter is that the data information has to be able to reach the sensitivities to the controls in the adjoint model. All data from year 2 and later is excluded from modifying the initial condition due to the separation into 1 year windows. The time window of one year, on the other hand, is too short for deep data signals to be able to reach the surface. Sparse data is in general a problem because, due to the lack of covariance information, sparse data is likely to produce unphysically small-scale corrections, which are likely to be not beneficial for the simulation of the dense SST and SIC data that determine most of the cost.

22) I may be incorrect but it seems that no Arctic Ocean T and S pro- files were used in this work. I do not see Arctic Ocean data in the Ingleby and Huddleston report and the NISE database doesn't show data north of the Norwegian Sea. Given that the assimilation period overlaps with the existence of ice-tethered profilers, why were ice-tethered profile data not included (http://www.whoi.edu/page.do?pid=20781)? As for the CTD data in the Arctic, both the ICES database (http://www.ices.dk/marine-data/data-portals/Pages/ocean.aspx) and the World Ocean Database v3 (https://www.nodc.noaa.gov/OC5/WOD13/) have data for the time period considered in this work. There may be perfectly fine reasons for excluding these data but the reasons should be offered.

At the time we have started our assimilation efforts (year 2012), the combination of the EN3 (which includes a good amount of Arctic Ocean T and S profiles) and NISE dataset was the best available option in terms of data coverage and technical efforts were required to interpolate observations to the model grid. Later we decide to stick with this choice for consistency.

We now added the following:

“The collection of hydrographic observational data in the Arctic Ocean used in the present work is not comprehensive and does not include, for example, ice-tethered profile data. In the present pilot study we decided to stick to two well-structured data sets available at the time we have started our efforts.“

Technical Corrections

1. Page 1, Line 5: change ’become’ to ’are’ as in ’values of sea ice extent are underestimated’

Corrected.

2. Page 1, Line 5: first comma to semicolon. Or split this long sentence into two before ’however’

We changed it to semicolon.

3. Page 1, Line 14: strike ’to date’

Corrected.

4. Page 1, Line 16: reference?

We now cite Overland and Wang (2013).

5. Page 1, Line 17: strike comma before ’is therefore of utmost importance’

Corrected.

6. Page 1, Line 24: strike ’if not possible’

Corrected.

7. Page 2, line 2, strike comma before ’the community’. Strike ’heavily’.

Corrected.

8. Your doi for Detlef’s 2016 paper is incorrect. It should be DOI: 10.1146/annurev-marine-122414-034113

We double checked and could not find a difference between the DOI that you have provided and what appears in the paper. Can you please specify what exactly is wrong in the DOI?

9. Page 2, Line 19: strike "usually in general"

Corrected.

10. Page 5, Line 12: strike "are going to"

Corrected.

11. Page 5, Line 28: replace "very good" with "improved"

Corrected.

12. Page 5, Line 29: strike "thus"

Corrected.

13. Page 5, Line 31-32: This sentence deserves a rewrite for clarity. As mentioned above, relative percentage sea ice cost reductions are also a function of the (unknown) first guess states.

The year in Fenty et al. (2015) is different as well as the first guess, so we remove the sentence completely.

14. Add 'bears' before 'a good resemblance'

Corrected.

15. Page 5, Line 24: For clarity consider saying 'since a perfect total sea ice area evolution...' and the following sentence is redundant.

We removed the redundant sentence and modified the sentence in question to comply with Reviewer 1 request 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."

16. Page 8, Line 20-22. Strike "except for the" and simply say that "Sea ice thickness are not provided by Kwok for the Barents and Kara Seas and the Canadian Archipelago because ..." with a reference.

We deleted part of the sentence after "except for the" since it does not make sense to discuss uncertainty or realism of the data in the regions where they are not present.

17. Page 8, Line 26: change "variables" to "variables'"

We guess the Reviewer meant "to variable's". Corrected.

18. Page 9, Lines 1-2: Why is it hard to provide quantitative estimates? You could plot time series of the uncertainty-weighted squared model-data misfit (normalized cost) before and after the assimilation.

The Reviewer is right. Quantitative metrics are not hard to provide in general; we think the visual comparison of spatial distribution is more instructive than just a few numbers. We removed the respective sentence.

19. Plotting model minus data or model minus data squared in Fig 5 might simplify comparison.

Although your suggestion allows for an easier evaluation of the improvement, we decided to continue showing absolute values since we believe it is easier for most readers to interpret. Adding separate panels with differences would just duplicate the information and make the figure unnecessarily verbose.

20. Section 4: Fonts on the time series of Fig 8 are also small and difficult to read. One subplot is cut off. After normalizing the summed control variable adjustments they could all be shown in the together in the same plot.

We now made the fonts of the time series in Fig. 8 larger.

Sea Ice Assimilation into a Coupled Ocean-Sea Ice ~~Adjoint~~ Model ~~of the Arctic Ocean~~Using its Adjoint

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Abstract. ~~High-resolution satellite~~ Satellite sea ice concentrations (SIC), together with several ocean parameters, are assimilated into a regional Arctic coupled ocean-sea ice model covering the period 2000-2008 using the adjoint method. There is substantial improvement in the representation of the SIC spatial distribution, in particular with respect to the position of the ice edge and to the concentrations in the central parts of the Arctic Ocean during summer months. Seasonal cycles of total Arctic sea ice area show an overall improvement. During summer months, values of sea ice extent ~~become underestimated, however, it is shown that this metric is not suitable to characterize the quality of the sea ice simulation, as the~~ (SIE) integrated over the model domain become underestimated compared to observations, however the root-mean-square difference of mean SIE to the data is reduced in nearly all months and years. Along with the SIC, the sea ice thickness fields also become closer to observations, providing added-value by the assimilation. Very sparse ocean data in the Arctic~~ocean~~, corresponding to a very small contribution to the cost function, prevent sizable improvements of assimilated ocean variables, with the exception of the sea surface temperature. ~~The bias between simulated and observed SIC decreases mainly due to corrections to the surface atmospheric temperature, while contributions of other control variables remain small.~~

1 Introduction

The Arctic region is expected to experience a dramatic anthropogenic temperature increase over the years to come (IPCC, Stocker et al. (2014)). ~~Already to date, a~~ A major decline in Arctic sea ice is already observed (Kwok and Rothrock, 2009; Comiso et al., 2008) and climate change projections suggest that, due to rising temperatures, a complete disappearance of summer sea ice could occur as soon as ~~2050.~~ 2050 (Overland and Wang, 2013). Obtaining an improved understanding of the changing Arctic Ocean, its transport properties of heat, freshwater as well as carbon and nutrients, and its interaction with sea ice and the overlying atmosphere ~~,~~ is therefore of utmost importance.

Despite recent improvements in observing capabilities (Lee et al., 2010), the Arctic Ocean remains one of the least explored areas of the World Ocean. This is due to the harsh environmental conditions of the region, but also due to logistical and political difficulties in maintaining sustained Arctic-wide, ideally autonomous, ocean observations. Fortunately, many polar-orbiting satellites obtain important ocean and sea ice parameters over the sub-Arctic region, such as sea surface height (SSH), sea surface temperature (SST), ocean color and sea surface salinity (SSS). However, over sea ice covered regions satellite measurements of the ocean surface are limited~~if not possible~~. To enhance our insight into the Arctic environment a joint analysis of observational efforts is therefore required. However, to understand large scale circulation processes in the Arctic Ocean ~~,~~ the community will have to rely ~~heavily~~ on numerical ocean circulation models due to the continued substantial under-sampling of the Arctic ~~ocean~~ under sea ice cover.

The representation of the Arctic Ocean circulation in existing ocean models considerably improved during the last 10 years, to the point that today many models reasonably well reproduce variability of SSH (Koldunov et al., 2014), while for the components of the freshwater balance the picture is mixed (Jahn et al., 2012) and for circulation and water masses models show significant discrepancies (Proshutinsky et al., 2011).

One method to further increase the resemblance between models and available observations is data assimilation. The models with data assimilation can be used to draw conclusions about variations in Arctic Ocean parameters on decadal scales, and to reveal mechanisms which drive changes in Arctic circulation.

Stammer et al. (2016) described the state of ocean data assimilation in the context of climate research. As described there, ocean data assimilation became a mature field for the ice-free ocean. However, assimilation in coupled ocean-sea ice or fully coupled climate models is still at its infancy and needs considerable attention. This also includes the use of sea ice parameters to constrain coupled ocean-sea ice models and to understand the coupling between sea ice and the underlying ocean and the atmosphere.

Chevallier et al. (2016) recently reported results from the ORA-IP inter-comparison project for Arctic sea ice parameters using global ocean-sea ice reanalyses with and without assimilation of sea ice data. They found good agreement in the reconstructed concentration but a large spread in sea ice thickness due to biases related to the sea ice model components.

The approaches to the sea ice assimilation ~~usually in general~~ are similar to the way ocean variables are assimilated in ~~particular ocean model and ranges~~ ocean models and range from nudging (e.g. Lindsay and Zhang (2006); Tietsche et al. (2013)) to the use of ensemble Kalman filter (e.g. Lisæter et al. (2003); Xie et al. (2016)). The sea ice sensitivity study of Koldunov et al. (2013) was among the first prerequisites to a full data assimilation attempt in the Arctic with the adjoint method. The authors looked at the sensitivity of sea ice parameters to external atmospheric forcing parameters (see also Kauker et al. (2009)). The former study revealed the impact of spring atmospheric temperatures on summer sea ice concentration and extent. The study of Kauker et al. (2009) underlined that wind stress changes are important for changing summer sea ice thickness.

More recently, Fenty et al. (2015) studied the impact of assimilating sea ice concentration (and ocean) data into a global, eddy permitting ocean circulation model using the adjoint method. In that study the circulation for the year 2004 was reconstructed. By comparing a setup with and without assimilation of sea ice concentration, the authors demonstrate that sea ice concentration data reduce model misfits in the Arctic with respect to upper ocean stratification and reduces ICESat-derived Arctic ice thickness errors.

The present study builds on the work of Fenty et al. (2015) and advances it by performing a multi-year data assimilation for the coupled Arctic Ocean-sea ice system. To be computationally feasible, the study is based on a regional Arctic configuration, nested laterally into a North Atlantic-Arctic solution (Serra et al., 2010). The goal of the study is to investigate the changes in the Arctic during the period 2000 - 2008. This period is characterized by significant changes in the Arctic Ocean and by increased amounts of Arctic observations. This makes it a good test period for the assimilation system and can provide first scientific applications. At the same time, the consistency of the assimilated EUMETSAT sea ice data (OSI-SAF, 2015) with the used sea ice model is being tested, as are its impact on the estimate of the ocean circulation and unobserved ice parameters ~~,~~ such as sea ice thickness.

The remaining paper is structured as follows: after an introduction to the model configuration and the assimilation method in Section 2, the impact of the assimilation on the sea ice concentration is discussed in Section 3. Section 4 focuses on how the sea ice state is adjusted by changing the control variables and Section 5 summarizes the impact on the ocean state and the sea ice thickness. Concluding remarks follow in Section 6.

2 Methods

Our study is based on a regional configuration of the MITgcm coupled ocean-sea ice model (Marshall et al., 1997) and the respective ECCO adjoint framework. The model set-up, the data assimilation and the optimization results are described in the following subsections.

2.1 Model set-up

The model domain covers the northern North Atlantic and the Arctic Ocean (Fig. 1) with the model grid being curvilinear and a subset of the 16-km resolution Atlantic-Arctic model (ATL06) reported in (Serra et al., 2010). The model uses z-coordinates and has 50 levels, with resolution varying from 10 meters in the top layers of the water column to 550 meters in the deep parts of the ocean. The bathymetry is based on the ETOPO2 database (Smith, 1997) with no artificial deepening or widening of the Nordic Seas passages being applied.

As atmospheric forcing, the model uses the atmospheric state from the 6-hourly NCEP R1 reanalysis (Kalnay et al., 1996), including ~~2-meter~~ 2-meters air temperature, precipitation rate, ~~2-meter~~ 2-meters specific humidity, downward shortwave radiation flux, net shortwave radiation flux, downward longwave radiation flux, ~~10-meter~~ 10-meters zonal wind component and ~~10-meter~~ 10-meters meridional wind component. The surface fluxes of heat, freshwater and momentum are derived via bulk formulas. At the open southern boundary, roughly at 48° N in the Atlantic, results from a 60-year long integration of the ATL06 model are used. The ATL06 was in turn forced laterally at 33° S by a 1° resolution global solution of the MITgcm forced by the same NCEP data set (see (Serra et al., 2010) for details). At the northern boundary a barotropic net inflow of 0.9 Sv into the Arctic is prescribed at Bering Strait, balancing the corresponding outflow through the southern boundary. An annual averaged river run-off (Fekete et al., 1999) is applied in the North Atlantic, while seasonally varying run-off is used for the Arctic rivers.

The MITgcm offers a wide variety of modules that can simulate different aspects of the unresolved ocean physics. For the vertical mixing ~~parametrization~~ parameterization we use the K-Profile Paremeterization (KPP) scheme of (Large et al., 1994). The model is operated in a hydrostatic configuration with an implicit free surface. The sea ice component is based on a Hibler-type (Hibler, 1979, 1980) viscous-plastic dynamic-thermodynamic sea ice model. The thermodynamic part of the model is the so-called zero-layer formulation following Semtner (1976) with snow cover as in Zhang et al. (1998). The temperature profile in the ice is assumed to be linear, with constant ice conductivity. Such a formulation implies that the sea ice does not store heat, and, as a result, the seasonal variability of sea ice is exaggerated (Semtner, 1984). To reduce this effect we use the sub-grid scale heat flux ~~parametrization~~ parameterization following Hibler (1984). Moreover, we use the viscous-plastic rheology scheme of Hibler (1979) with an extended line successive over-relaxation (LSOR) method (Zhang and Hibler, 1997). A comparison of the effect of different rheology schemes in MITgcm is provided by Losch et al. (2010). Recently, Nguyen et al. (2011) applied the coupled MITgcm in a regional Arctic Ocean study and reported values for many model parameters used in our study.

2.2 Adjoint data assimilation approach

Similar to the work of Fenty et al. (2015), our assimilation also employs the ECCO adjoint methodology to bring the coupled sea ice-ocean general circulation model into consistency with assimilated data and prior uncertainties. The particular implementation used here builds on the ~~set up~~ set-up of the GECCO2 synthesis (Köhl, 2015) but was extended to facilitate the additional assimilation of sea ice parameters. A complete list of parameters assimilated and their sources are presented in Table 1. The collection of hydrographic observational data in the Arctic Ocean used in the present work is not comprehensive and does not include, for example, ice-tethered profile data. In the present pilot study we decided to stick to two well-structured data sets available at the time we have started our efforts.

While using the adjoint method, an uncertainty-weighted sum of squares of model-data misfits is minimized in an iterative fashion using the gradient of the ~~costfunction~~ cost function with respect to a number of control variables. 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).

The MITgcm is suitable for the automatic generation of adjoint code by the Transformation of Algorithms in FORTRAN (TAF) source-to-source translator (Giering and Kaminski, 1998; Giering et al., 2005). Koldunov et al. (2013) used the MITgcm and its adjoint to perform an analysis of the Arctic-wide adjoint-based sea ice sensitivities to atmospheric forcing.

Here we use a version of the MITgcm with an improved adjoint of a thermodynamic ice model (Fenty and Heimbach, 2013a, b). The adjoint model was modified here similarly to Köhl and Stammer (2008) to exclude ~~some modules .~~ 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 ~~were switched off. The cost function~~

~~was defined similarly to Köhl (2015), however the list of data sources is modified to include additional observations from the Arctic Ocean. A complete list of parameters assimilated and their sources are presented in Table 1~~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 et 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.

In contrast to Köhl (2015), additional control variables are optimized and the frequency of the updates is enhanced to ~~daily~~once per 3 days in order to reflect shorter time scales of sea ice variability. The final list of control variables is: surface (2m) air temperature, surface (2m) specific humidity, surface (10m) zonal and meridional wind velocity, precipitation rate, downward shortwave ~~and longwave~~ radiation, and initial temperature and salinity for the first year of assimilation. 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 x 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.

We employ the same uncertainty weights for hydrographic and satellite data as Köhl (2015), while for sea ice concentration we specify a constant error of 50%. 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%.

The data assimilation is performed in one year chunks. The use of one year segments is related to technical reasons; we are not able to get useful sensitivities for the 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.

Each of the iterative ~~costfunction~~ cost function reductions is performed until the ~~costfunction~~ cost function differs by less than 1% in two consecutive iterations. The cost is dominated by SIC and SST data, which easily respond to the surface controls, and the adjoint method quickly reduced the misfits of those data, so that the number of iterations was usually less than five. After the first year assimilation, we move to the next year using the final state of the previous year's successful iteration as initial conditions. Therefore, the iteration termed 0 in the following makes already use of an improved initial condition from the assimilation in the previous year, and is thus not equivalent to a free run starting from climatology. For the impact on the ocean circulation, we consider also the free run to demonstrate the impact of changing the initial conditions by assimilating data during the ~~preceeding~~ preceding year.

Fig. 2 shows the percentage decrease in model-data differences. The red color indicates reduction in total model-data difference (FC), while other colors indicate the reduction of the differences for individual variables. Negative values mean that there is an increase in model-data difference for that variable.

The largest total reduction (about 16%) is obtained for the year 2008, while the smallest (about 2%) is obtained for the year 2005. The average reduction for all years is about 9%. The strongest cost reductions for individual variables is obtained for the sea surface temperature (SST) and sea ice area (SIA), with an overall average of about 23% and 26%, respectively. The least successful cost reduction is obtained for the mean dynamic topography (MDT), with many years in which the model-data differences for this variable slightly increased. In 2004 the cost reduction of sea ice area was about 30%, less than that reported by Fenty et al. (2015) (49%)~~. The reasons for this difference include the lower number of iterations and that in our synthesis only the inital conditions of year 2000 were adjusted.~~, which may partly be explained by differences in the first guess solution.

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 is expected to be small.

In the following we ~~are going to~~ concentrate mainly on results related to changes of the sea ice conditions, with only a brief discussion of ocean state changes later on.

3 Sea ice concentration changes

Fig. ~~2 illustrates the improvement of sea ice parameters as basin distributions to gain insight into the regional improvement of the model SIC through data assimilation. The SIC spatial distribution before and after assimilation is compared for March (month of the maximum ice coverage) and September (month of the minimum sea ice coverage). For the comparison we selected the years 2005, representing an intermediate maximum of summer SIC, and 2007, representing the minimum in summer SIC for the period in consideration.~~

~~Fig.~~ 3 shows in the top two rows the sea ice concentration for the winter time period (March of the year 2005) from satellite and from model runs, before and after data assimilation, together with the changes of the latter two relative to observations. Since most of the Arctic Ocean is covered by sea ice with high concentrations, the largest improvements are in the position of the ice edge. Most noticeable is the decrease in the SIC along the east coast of Greenland after data assimilation~~, a region where most models tend to overestimate sea ice~~. During the initial run of the model, there is a tongue of the sea ice extending towards the open ocean. After data assimilation the tongue did not disappear completely, however, it declined considerably.

During the summer period (September 2005), shown in the bottom two rows of Fig. 3, there are improvements both in the sea ice edge and in the SIC of the interior sea ice field. Initially, the sea ice edge was not very far from observations, but after data assimilation the match between model and data is ~~very good~~improved. The SIC in the central parts of the Arctic Ocean increased and became ~~thus~~ closer to the satellite data. A direct comparison to the results by Fenty et al. (2015) is hindered by the fact that differences less than 15% are blanked out in their study and by the different years analyzed.

~~Since year 2005 shows in our case only a 20% reduction in sea ice, the match in ice edge after assimilation is less good than theirs for the summer and the winter case.~~

In contrast to 2005, identifying changes in the SIC for March 2007 (Fig. 4) is more challenging. Practically all the differences between simulations and satellite data are along the ice edge and there seems to be not much ~~changes~~ change between the initial state of the model and the state after assimilation. For example, the noticeable negative anomaly around Franz Joseph Land is not developed further after SIC assimilation. This particular negative SIC anomaly is most probably dynamical in nature, and can not be handled properly by the simplified ice dynamics scheme (free drift) used in the adjoint model to calculate changes of the model parameters. The spatial distribution of SIC ~~shows~~ during September 2007 (Fig. 4) already bears a good resemblance to the satellite data before the assimilation. Improvements are mostly visible in the central parts of the Arctic Ocean, where the too low SIC is increased. The ice edge also became closer to observations, but the amount of sea ice in the Amerasian basin remains larger compared to observations. In this region the SIC in the unconstrained run is high (with also thicker sea ice), which is not easy to remove by thermodynamic corrections of the forcing and, due to the high SIC and thickness, not easy to move by changes in wind forcing. This possibly indicates some limitations of the approach, where the corrections mostly come from the thermodynamic forcing and the assimilation period is short.

The seasonal cycle of sea ice area (SIA) and sea ice extent (SIE) are shown in Fig. 5, again for years 2005 and 2007. Results for SIA for both years show that values of SIA in general are getting closer to satellite observations as a result of the SIC assimilation. One would expect that, close to the beginning of the assimilation period (1st of January), corrections of the atmospheric forcing did not have enough time to considerably influence sea ice parameters. This is true for SIA in 2007, when sizable differences between initial and last iterations only first appear in May. However, SIA in 2005 gets considerably closer to observations already in February, indicating that atmospheric corrections actually can affect sea ice parameters relatively fast even during winter.

For both years, SIA shows overall improvement during the whole year; but this is not the case for the SIE. In 2005 the SIE good match between initial iteration and satellite data during summer months disappears after assimilation, with considerable underestimation of SIE. In 2007 there is an overall SIE improvement after the assimilation, but there are again months with a considerable SIE underestimation. 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 ~~area~~ total SIC or SIE evolution may still correspond to considerable differences to the data in their regional distribution. ~~Therefore, those commonly-used integral parameters show limited ability to characterize the quality of sea ice simulations.~~ Chances of having SIE distribution close to observations with quite different spatial shape of the sea ice field are very high. 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 ~~. To address this issue, Dukhovskoy et al. (2015)have investigated several norms to measure the differences between two sea ice fields. They found that Hausdorff Distances have the best skill to quantifying the similarity between two-dimensional fields.~~ (e.g. Dukhovskoy et al. (2015)).

With respect to the model performance, two better metrics are the sum of the RMS errors (RMSE) for SIA and SIE, which at least to some extent consider differences in spatial distribution by penalizing positive and negative differences at every grid point. Monthly values of the SIA ~~RMS error~~ RMSE before assimilation, after assimilation and the respective differences between the two (in percent) are shown in Fig. 6. Before assimilation, largest RMSE appear during summer months (> 2x10^6 km^2), while in other seasons they are about 1.5x10^6 km^2. Interesting to note, values of RMSE in March and September are quite similar, despite the large differences in ice cover in the two months. One of the possible reasons is that location of the ice edge in those extreme months is relatively stable compared to spring and fall when the ice pack is contracting and expanding. After the assimilation the most notable improvements also occur for summer months, but with the addition of September. After the assimilation, March values show only about 10% improvement, while September values have about 25% improvement on average. There is no clear indication that assimilation of SIC on the yearly basis gradually improves the simulated sea ice, due to, for instance, better initial conditions in January. For some months the decrease in SIA RMSE after assimilation can be as little as 1%, although it is always getting smaller. The same is not the case for the SIE RMSE.

As expected, SIE RMSE values (Fig. 7) are larger, with a maximum in summer and September before the data assimilation. Assimilation is most effective for a reduction of SIE RMSE in September (about 25% on average). After the assimilation October becomes, in addition to summer months, one of the months with relatively large SIE RMSE differences. October is also a month when (during 5 out of ~~total~~ 9 years) after assimilation the SIE RMSE increased. The SIE RMSE, similarly to the SIA RMSE, do not show any obvious tendency from the first year to the last.

4 Control variables

As mentioned in Section 2.2, the model is brought into consistency with observations by adjusting a number of control variables. The strength and spatial distribution of the adjustments carry important information about the way the optimization procedure changes the forcing and the initial conditions in order to bring the state of the model closer to the observed state. Figure 8 shows the area-mean temporal variation of the corrections to several control variables over the year ~~2005. Also shown is~~ 2005 in absolute values and normalized by the uncertainties. Also shown are the spatial distribution of the corrections for the month when their strength is at its maximum.

As expected, there are strong changes in the surface atmospheric temperature. Its modification is probably the easiest way to change the sea ice concentration by increasing temperature when/where a reduction of SIC is required and vice-versa. The spatial distribution of corrections in 2005 (Fig. 8, top row, left column) compares very well to the difference between first guess and satellite SIC data in the central Arctic (Fig. 3). In order to increase SIC in the Eurasian Basin, the optimization reduces the surface atmospheric temperature in June by about 2 degrees in this region on average, reaching 3 degrees in some places. Positive SAT corrections over the Arctic shelf seas helps to reduce extra sea ice generated there by the model during summer months (not shown).

The corrections to the downward shortwave radiation (Fig. 8, second row) show temporal variations and a spatial distribution similar to the SAT corrections, but the magnitudes are quite small. Corrections ~~for the downward longwave radiation are even smaller and not shown. Corrections~~ to the zonal and meridional wind components (Fig. 8, third and last rows) are on average quite small in absolute values, but locally can reach 10 m/s. The wind corrections are mainly concentrated along the shore and summer ice edge and, contrary to the SAT corrections, it is difficult to associate them to some particular large-scale sea ice change.

~~Given the size of the correction it is suggested that the optimization brings the model towards sea ice observationsmainly by changing thermodynamic-related control variables, especially the 2-m SAT. Wind corrections might play some rolein local redistribution of the sea ice along the shore and ice edge, but due to the~~ Dimensional values of the corrections do not directly provide information about the relative importance of changes in the controls for bringing the model into consistency with observations. However, due to the relatively small number of iterations, we can use values of the corrections normalized by uncertainties as a reasonable measure of the relative importance of changes in control parameters. Spatial distributions and monthly means of absolute values of normalized corrections for the year 2005 are shown in Fig. 8.

Wind corrections seem to play integrally a larger role, with a maximum in May. This agrees well with results of (Kauker et al., 2009), who used an adjoint sensitivity analysis to determine the relative contribution of different atmospheric and ocean fields to the September 2007 sea ice minimum and found that the May-June wind conditions are one of the main factors in setting up extremely low sea ice conditions in Summer 2007. The maximum contribution of air temperature corrections occurs in June and it is about a factor of five smaller than the contribution of the wind corrections. However, using free drift in the adjoint biases the sensitivities towards larger sensitivities of sea ice to wind changes. Since measuring the impact by the normalized corrections relies on the assumption of correct sensitivities, the results may be also biased to too large an impact by the wind.

Given the absence of proper sea ice dynamics in the adjoint model (only free drift is used) ~~it remains unclear how suitable the estimated wind corrections are for correcting the model sea ice. Nevertheless, the wind can be important factor for the redistribution of the surface temperature and salinity properties along the ice edge, as shown below.~~

~~The~~ and lack of many important processes in the forward model (such as tides or waves), the question remains to what extent corrections to control variables reflect deficiencies in the forcing fields or a compensation to the sea ice model or sea ice data deficiencies, particularly since in the Arctic the NCEP reanalysis seems to perform well near the surface (Jakobson et al., 2012). For example, temperatures decreasing over areas with high SIC during summer months in order to grow ice and temperatures increasing over low SIC areas, could be an attempt of the assimilation system to fix problems associated with the sea ice movement. But it could equally also point ~~out~~ to problems of the correct attribution of sea ice concentrations from satellite data~~, both making atmospheric forcing actually~~. 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.

5 Improvements in sea ice thickness and ocean state

The adjoint assimilation leads to dynamically consistent model solutions, which along with directly assimilated variables may considerably improve variables of the simulation for which no observations are available. In case of SIC assimilation, one obvious candidate for improvement is the sea ice thickness (SIT). We also consider changes in the ocean state which result from the combined effect of assimilating ocean parameters and indirectly of the SIC assimilation, due to the coupled nature of the assimilation procedure and the forward model.

5.1 Sea ice thickness

Changes in SIT as a result of SIC assimilation and comparisons of the former with satellite data are shown in Fig. 9. The satellite ice thickness data are obtained from ICESat campaigns (Kwok et al., 2007), distributed on a 25-km grid and available from the NASA Jet Propulsion Laboratory (http://rkwok.jpl.nasa.gov/icesat/index.html). ICEsat sea ice thickness estimates are considerably larger than those in the simulations, especially in the Canadian sector of the Arctic Ocean. One should note that the uncertainty for this observational data is quite large (just better than 0.7 m, Kwok et al. (2007)), while the spatial distribution of the thickness is probably realistic ~~, except for the Barents and Kara Seas as well as the Canadian Archipelago, where ice thickness estimates are not provided~~(Kwok and Cunningham, 2008).

The ice in October-November during 2005 became thicker in the Eurasian Basin of the Arctic Ocean after assimilation and in general became closer to the observed thickness distribution. The thickness increase is considerable, reaching 0.5 m in some places. The shape of the region with the largest thickness increase in the Eurasian Basin resembles the shape of the September SIC distribution (Fig. 3) and because of its similarity in pattern it is probably a result of the control ~~variables~~ variable's corrections that aim to thermodynamically increase SIC in this region. Results for October-November 2007 are similar, with improved thickness along the continental shelf of the Eurasian Basin. However, thickness increase is not as strong as for 2005, reaching only about 0.3 m. A general tendency of these improvements is an increase in thickness in the central Arctic and the Canadian Basin, while regions with thin ice over the shelf seas tend to decrease in thickness. This tendency was also shown by Fenty et al. (2015) for the year 2004.

~~Considering the limited amount of the sea ice thickness data and their large uncertainty over the study period, it is hard to provide quantitative estimates of the SIT improvement due to SIC assimilation. Nevertheless, the~~ To summarize, the visual comparison with available satellite data hint to a general improvement of the SIT spatial distribution.

5.2 Ocean changes

Local changes of the SIC are caused by corrected atmospheric conditions (see above), which in the coupled system will also affect near-surface ocean parameters. To some extent changes can also come about through change in the ocean circulation and we want to investigate therefore how large those changes are and to what extent they could contribute to the sea ice improvements.

Fig. 10 shows differences in temperature and salinity between the initial and final iterations of the assimilation system for June and September of year 2005. The month of June is chosen because corrections to thermodynamic control variables during this month are largest (see above in Section 4). The sea surface temperature differences are mostly positive along the ice edge, where the model produces too much ice in the initial iteration (Fig. 3), and lower in magnitude in the central part of the Arctic Ocean. In June, considerable temperature differences cover a much smaller area, since most of the shelf seas are still covered by high concentrations of sea ice and most of the additional energy resulting from the correction to thermodynamic control variables is spent directly in the sea ice melting.

The surface salinity (Fig. 10, right column) shows an increase in the Eurasian Basin, caused by additional sea ice production (or less melting). There is a decrease of salinity around the sea ice edge due to melting of excessive sea ice formed in the initial iteration. In September, however, there is a pronounced increase in salinity in most of the Arctic shelf seas. This might be a result of the local increase in sea ice production in areas which become free of ice due to the summer corrections (e.g. Laptev Sea), but still have quite negative temperatures in the original forcing which are not corrected in September (corrections in September are quite small) at the onset of the freezing period.

Due to the relatively short assimilation periods (1 year) and to the extremely low amount of vertical temperature/salinity profile observations, improvements in the vertical distribution of temperature and salinity after 9 years of assimilation are quite small. Nevertheless, the positive bias in the Atlantic Water layer temperature of the Eurasian Basin, which is characteristic for the forward run, has been slightly reduced (not shown). On the other hand, changes in the upper part of the water column due to sea ice corrections, although hardly penetrating deeper than the first 50 meters, may influence integral fluxes at the borders of the Arctic Ocean.

We have calculated volume, heat and freshwater fluxes (Table 2) through the main passages of the Arctic Ocean (except for Bering Strait, where fluxes are largely prescribed in the model by the boundary conditions). Along with the initial and final iterations, results for a no-assimilation forward run were analyzed in order to remove the effect of changing the initial conditions at the beginning of ~~ech~~ each assimilation year. These may lead to changes of long-term variability and may affect the fluxes towards the end of the assimilation period. 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.

Differences in the total mean volume flux are quite small for all passages. 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 changes~~) when modifications in the throughflows at ~~the~~ Fram Strait are noticed, which are about 60% larger than in the forward simulation (Fig. 11a).

Differences in the heat flux (Fig. 11b) at Fram and Davis Straits can be episodically relatively large, but they do not show any particular tendency and may be related to the local heating or cooling in the vicinity of the sections. Table 2 summarizes the mean differences for the analyzed passages and, although hardly visible in the time series (not shown), heat flux differences for the St. Anna Trough are the largest on average, reducing the heat export from the Arctic Ocean by about 80%.

The freshwater flux differences (Fig. 11c) are most visible in the Fram Strait time series, but positive and negative differences remain comparable to the forward run and compensate each other, such that on average the relative difference is only about 3%. Large relative differences ~~are again occure~~ again occur for the St. Anna Trough (Table 2), which is located in an area with strong atmospheric corrections during most of the years.

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.

From the combined analysis of Fig. 11 and Table 2 one can conclude that, while on average most of the transports are hardly affected by the assimilation, during some periods relative large differences between the simulations with assimilation and the forward run without assimilation can be seen and may reach 60-100% for major straits.

6 Concluding remarks

Results from a multi-year data assimilation attempt based on a coupled Arctic Ocean-sea ice system were presented. The largest improvements relative to simulations without data assimilation were seen for the sea ice concentration (SIC) and sea surface temperature. Most of the improvements in the SIC happened during summer months and manifest themselves in a more realistic position of the sea ice edge and in SIC values closer to observations in the central Arctic.

The seasonal cycle of the monthly mean sea ice area (SIA) shows an overall improvement after assimilation, while sea ice extent (SIE) becomes worse during some months. The later fact demonstrates that the total mean SIE and SIA are not good measures for the model success in simulating sea ice, particularly considering the obvious improvements in spatial sea ice distribution. In order to obtain more meaningful estimates of the sea ice improvements, we consider sums of the ~~RMS error~~ root-mean-squared error (RMSE) for SIA and SIE. The largest reduction of the RMSE happened during the summer months.

~~Most of the sea ice changes during the assimilation procedure are induced by thermodynamic variables, especially the 2-m atmospheric temperature. Corrections to the wind do not have any obvious large-scale influence on the sea ice redistribution, but can potentially have some influence on the surface ocean variables. From the results above it seems unlikely that the estimated changes in the ocean state would play a major role in driving the improvements in the estimated sea ice state.~~

An obvious suggestion for improving the sea ice estimation is to consider larger assimilation periods or even best to use a single assimilation window. By this, data from later years may influence the corrections and the state of all preceding years. However, a long memory of the system seems to be not very evident in the assimilation. We have assimilated data in yearly chunks and one could expect that RMSE between observations and initial simulations (before assimilation) would gradually improve due to better initial conditions, at least over the first few years. However, we do not observe this effect in our experiments.

The comparison to available but limited sea ice thickness observations shows that SIC assimilation reveals some improvements in ~~SIT~~sea ice thickness (SIT), despite these observations not being directly assimilated. The amount of assimilated ocean observations in the water column of the Arctic Ocean is almost negligible compared to the amount of SIC data. However, the ocean state is affected indirectly by SIC assimilation, for example due to the freshwater fluxes related to the additional melting or freezing and by changes in the ocean exposure to the atmosphere caused by changes in SIC. The transports of ocean properties do not change on average after the assimilation, but episodically they can be quite different from the corresponding transports in simulations without assimilation. The latter can still be important for local process studies or model validation against observations that are limited in time.

With the use of the adjoint assimilation technique, we produced a model simulation that is considerably closer to observations and at the same time dynamically consistent. This data can be used for further understanding of the reasons and consequences of changes in the Arctic Ocean.

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Table 1. Datasets used in the assimilation procedure.

Dataset	Source
~~Monthly PHC climatology PHC 3.0, Steele et al. 2001~~ Mean Dynamic Topography	MDT from Technical University of Denmark (Knudsen Knudsen and Andersen, 2013; Cheng et al., 2014)
Monthly SST	Remote Sensing Systems [CIT]
Sea Level Anomalies	TOPEX/Poseidon, ERS-1,2 and Envisat, AVISO [CIT]
EN3 hydrographic data	Ingleby and Huddleston (2007)
NISE hydrographic data	Nilsen et al. (2008)
Sea ice concentration	OSI-SAF (2015)

Table 2. Mean values of different fluxes through Arctic Ocean passages.

Parameter and passage	~~Forward~~ Forward	~~After assimilation~~ After assimilation	~~Difference in %~~ Difference in %	Forwa 2005
Volume flux (Sv)				
Fram St.	-3.12	-3.12	-0.02	-4.0
Davis St.	-0.50	-0.55	4.72	0.44
Barents Sea Op.	2.78	2.81	0.88	3.5
St. Anna Tr.	-2.01	-2.01	0.18	
Heat flux (TW)				
Fram St.	38.76	38.62	-0.36	41.5
Davis St.	7.94	7.69	-3.12	8.6
Barents Sea Op.	83.10	84.07	1.17	111.8
St. Anna Tr.	1.02	0.20	-80.13	
Freshwater flux (mSv)				
Fram St.	-113.50	-109.80	-3.20	-173.0
Davis St.	-25.60	-27.27	6.50	13.5
Barents Sea Op.	-21.81	-22.37	2.57	-22.5
St. Anna Tr.	6.84	8.44	23.32	

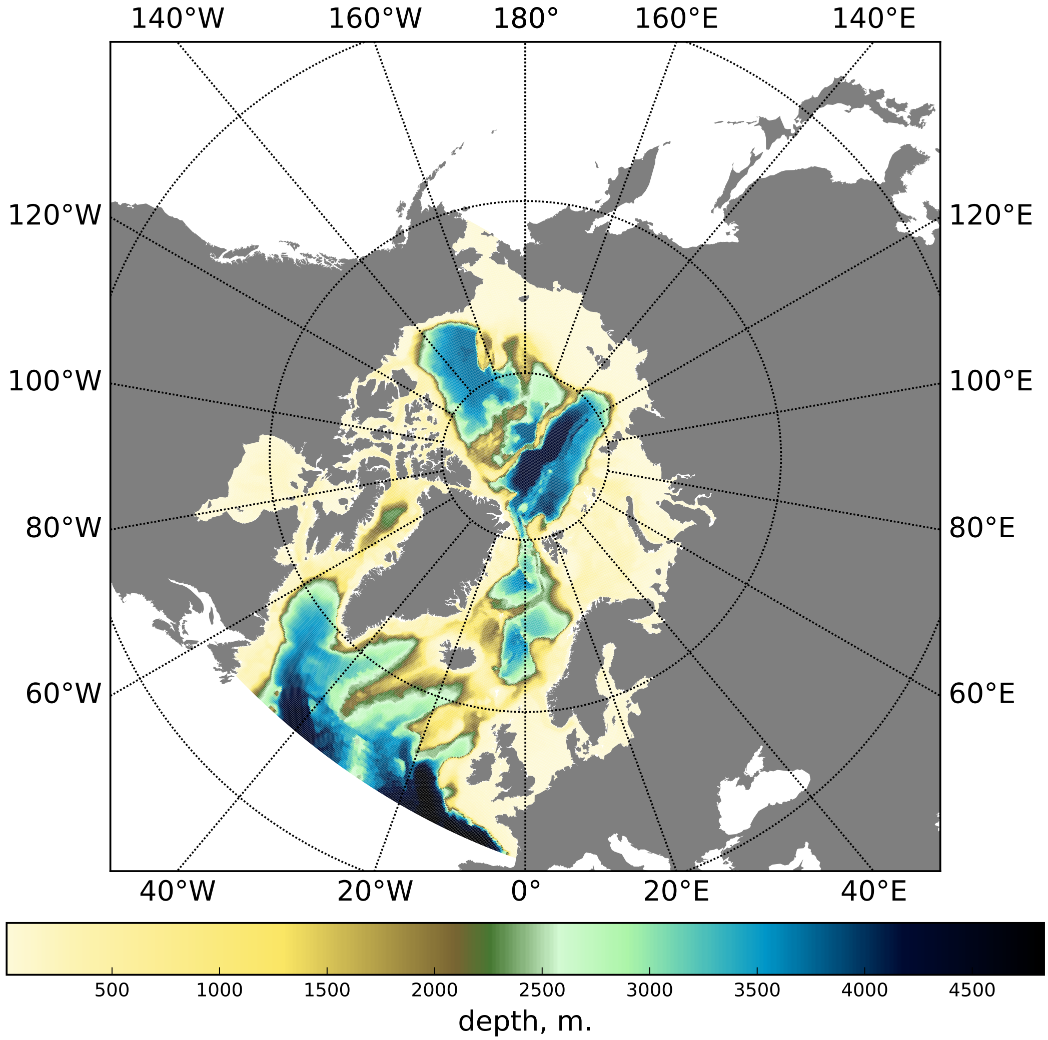

Figure 1. Model domain with bathymetry.

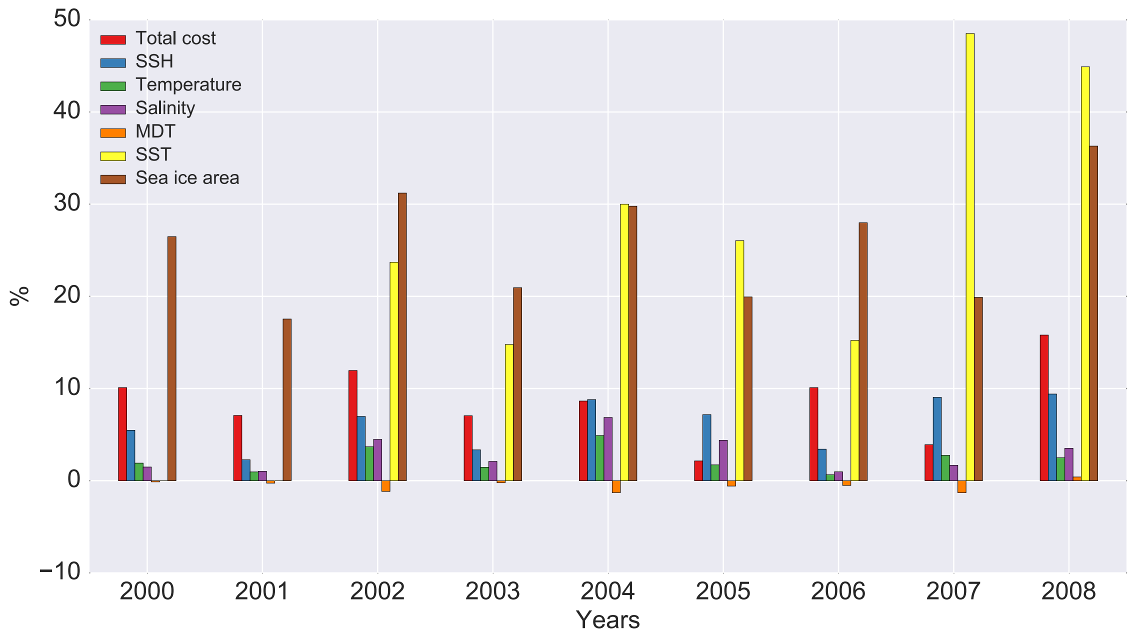

Figure 2. Total cost reduction and individual contributions to the reduction from different assimilated variables. During the first two years SST assimilation is not performed (no data).

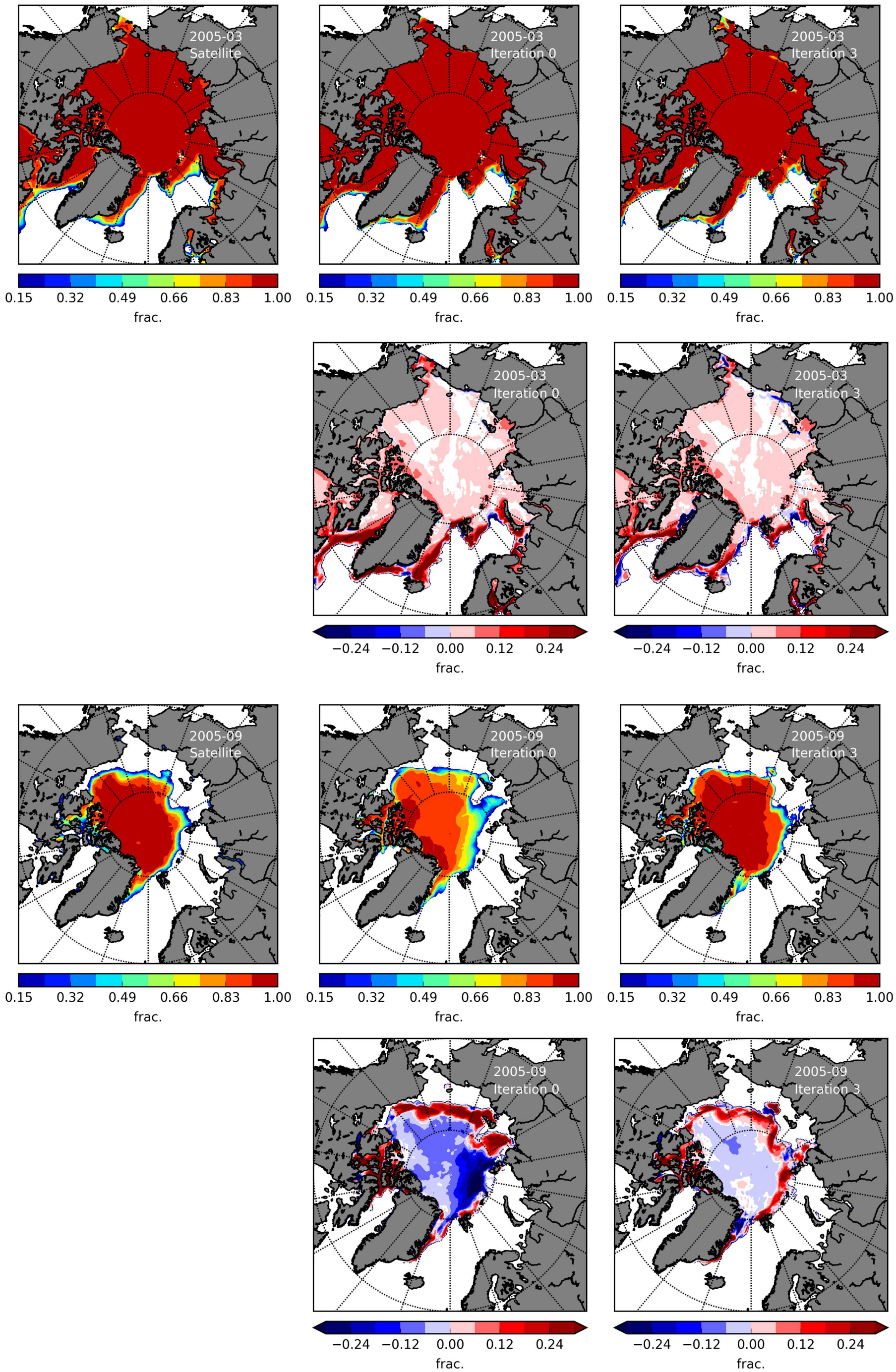

Figure 3. Spatial distribution of sea ice concentration (SIC) for the year 2005 (year of the local sea ice maximum) during March (first row) and September (third row). Assimilated satellite data (left column), model results from the run without corrections (middle column) and model ~~reults~~ results during the last assimilation iteration (right column) are shown. The second and ~~third~~ fourth rows correspond to the differences between the model solutions and the observations.

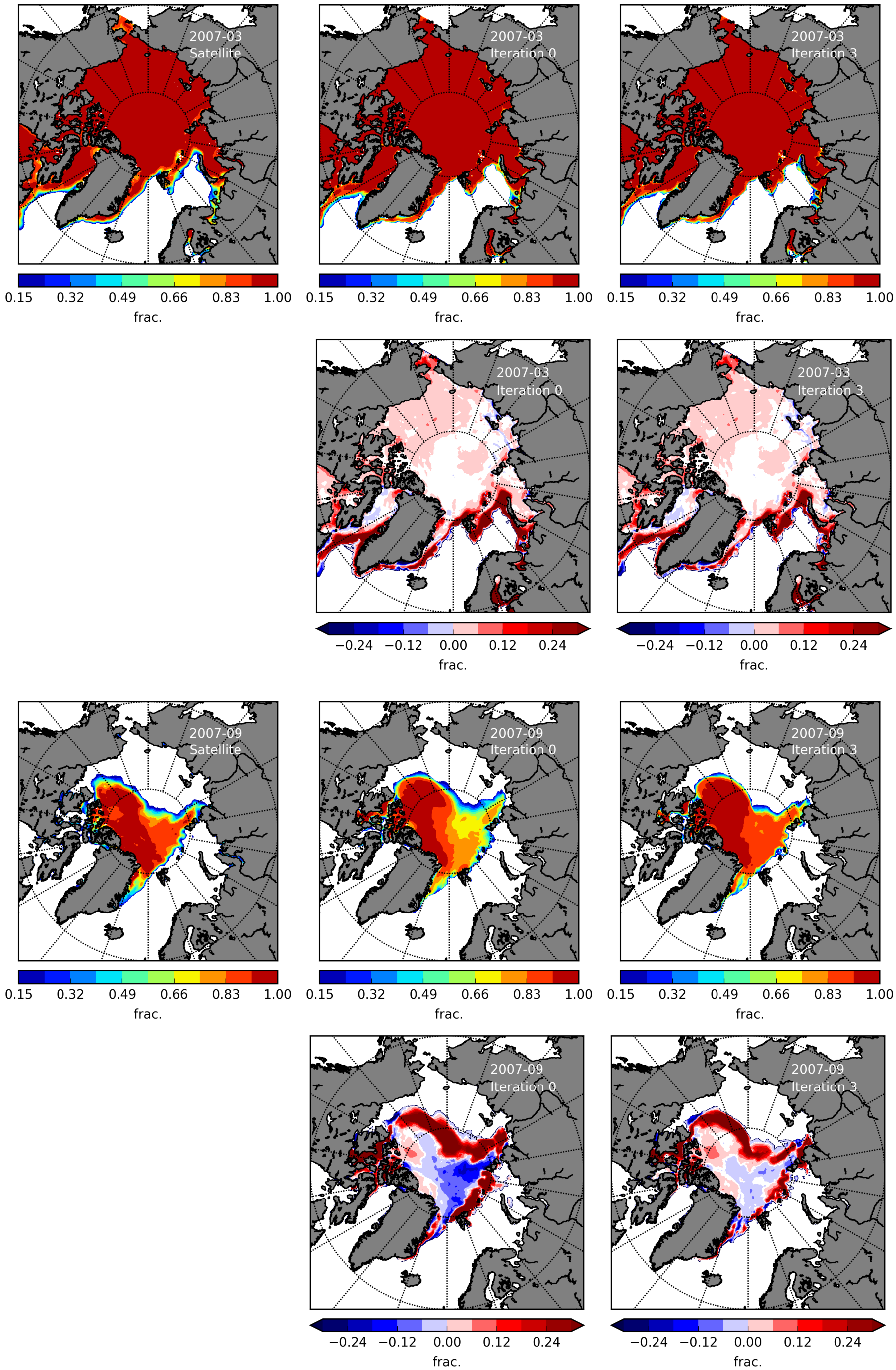

Figure 4. Same as Fig. 3, but for year 2007 (the year of the overall minimum sea ice.)

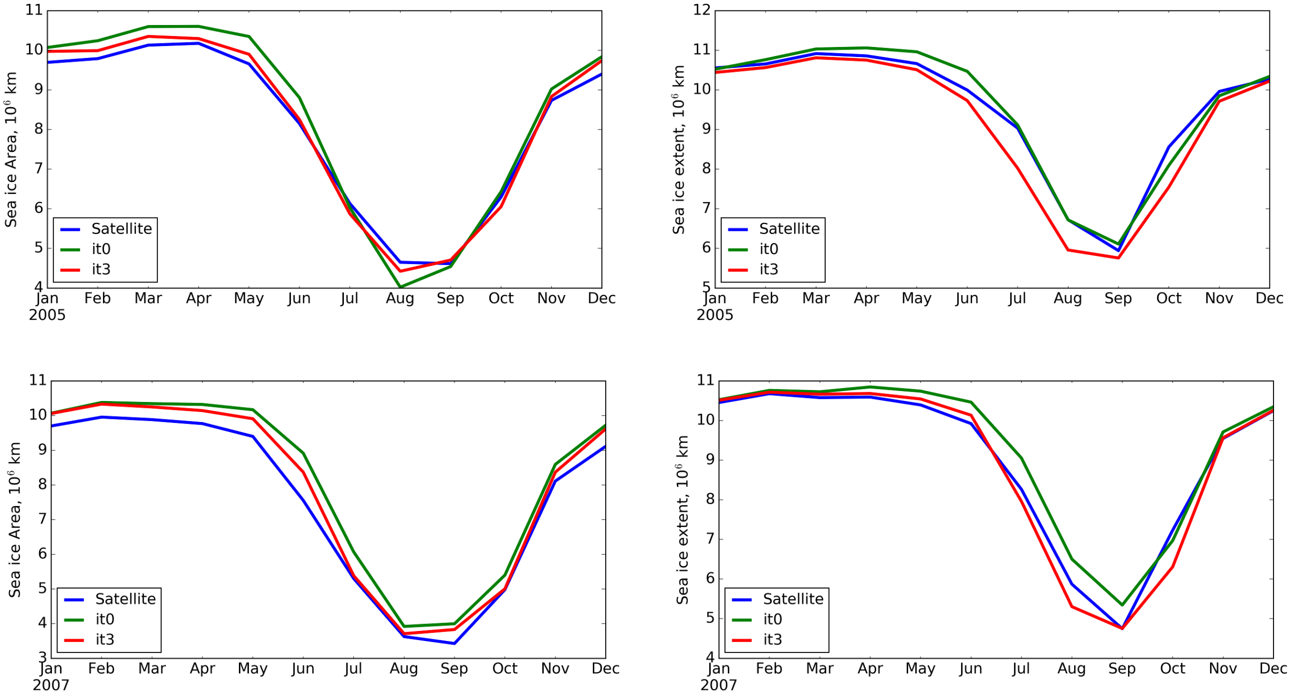

Figure 5. Monthly mean sea ice area (left) and extent (right) for the years 2005 (top) and 2007 (bottom). Assimilated satellite data is shown in blue, model solution without corrections is shown in green and the result from the last iteration is shown in red.

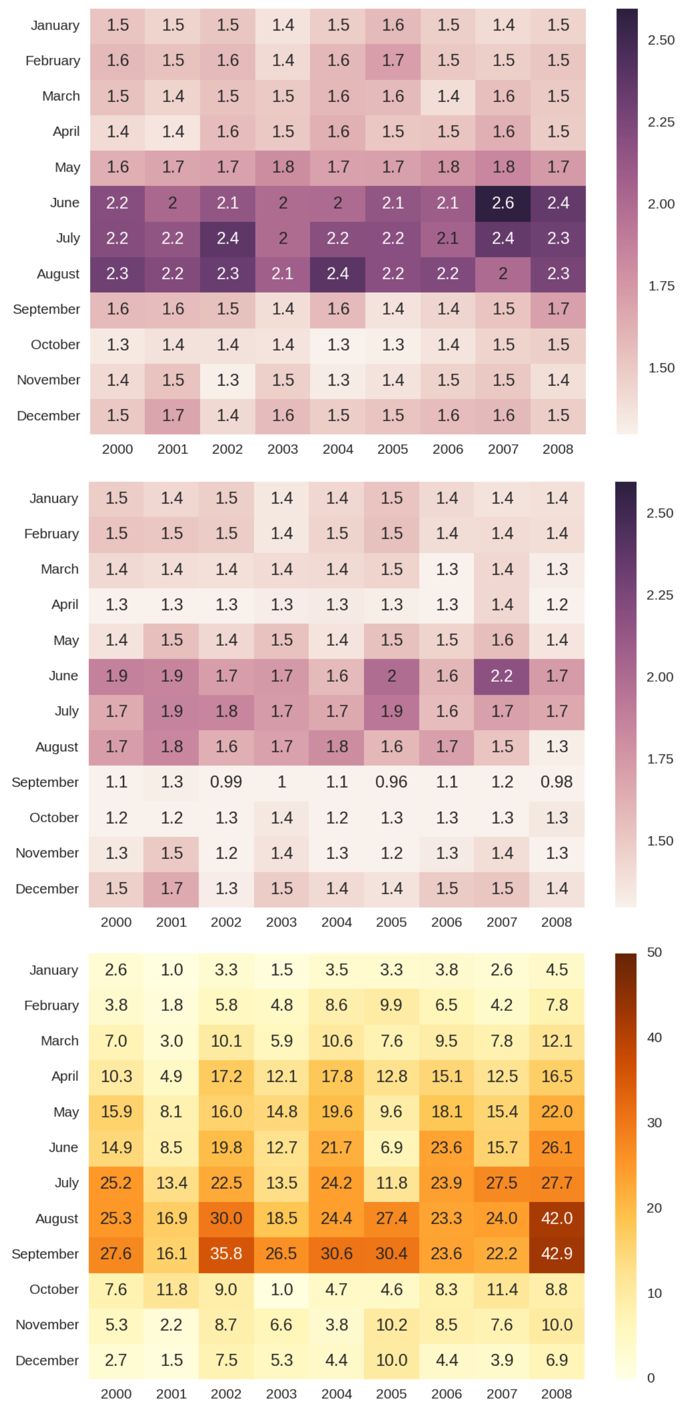

Figure 6. Sum of the sea ice area root-mean-square error (RMSE) (compared to assimilated sea ice at every grid location) for every month (in 10^6 km^2), before assimilation (top), after assimilation (middle) and the percent difference between the two (bottom). Positive differences correspond to a decrease of the RMSE and vice-versa.

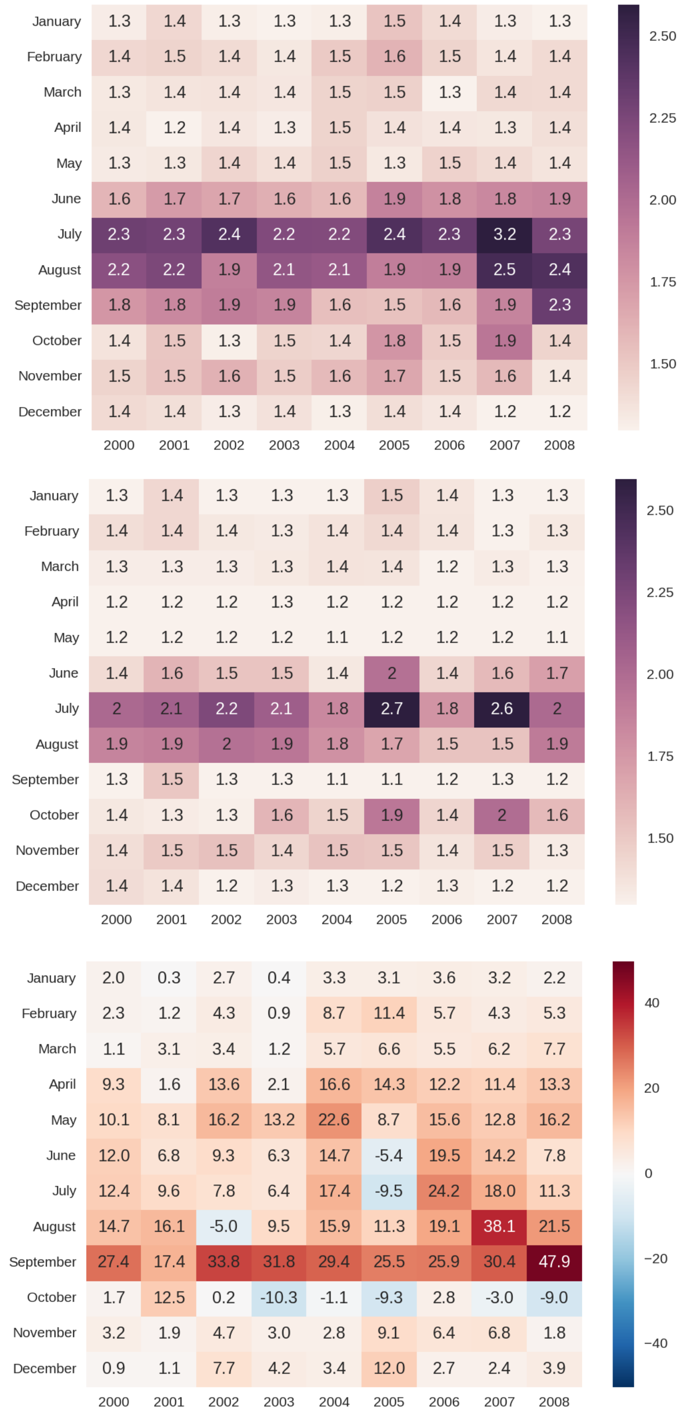

Figure 7. Same as Fig. 6, but for the sea ice extent.

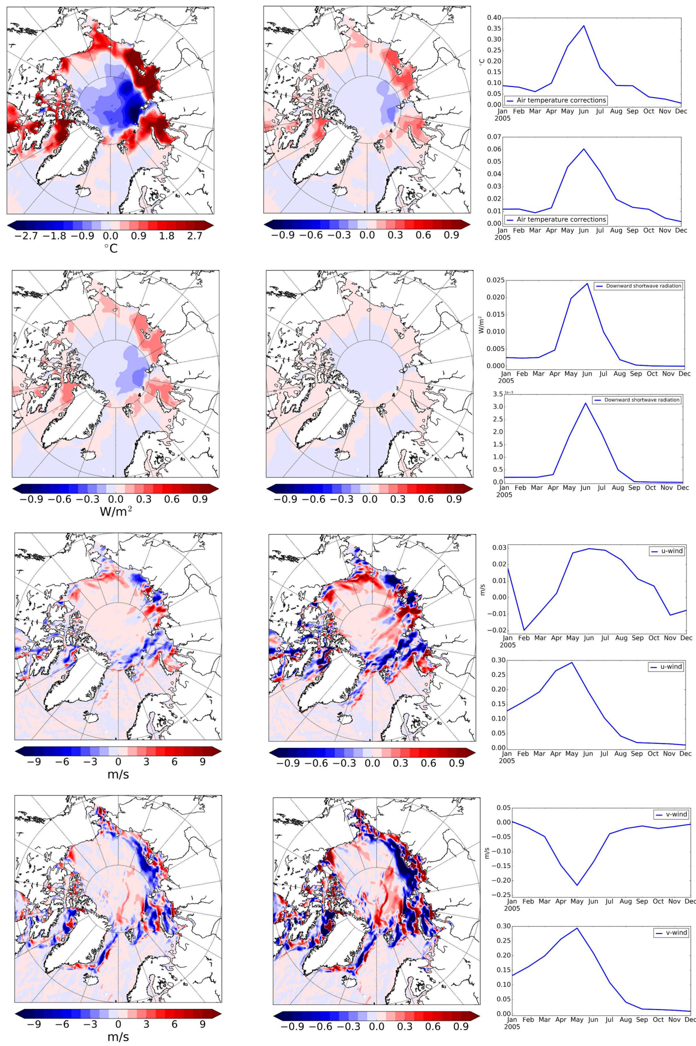

Figure 8. Corrections for different surface forcing variables: spatial distribution (left) and spatial distribution scaled by the uncertainty (middle) for the month with the largest absolute value of corrections in ~~year 2005 and (right)~~ 2005. Also shown is the monthly climatology ~~of~~ for the sea ice area mean ~~correction~~corrections (right column) averaged over the area north of 66.5° N (top panels for each variable) and the average of absolute values scaled by the uncertainties (lower panel for each variable). Corrections are shown for June 2005 2-m air temperature (first row), June 2005 downward shortwave radiation (second row), June 2005 zonal component of the wind (third row) and May 2005 meridional component of the wind (fourth row).

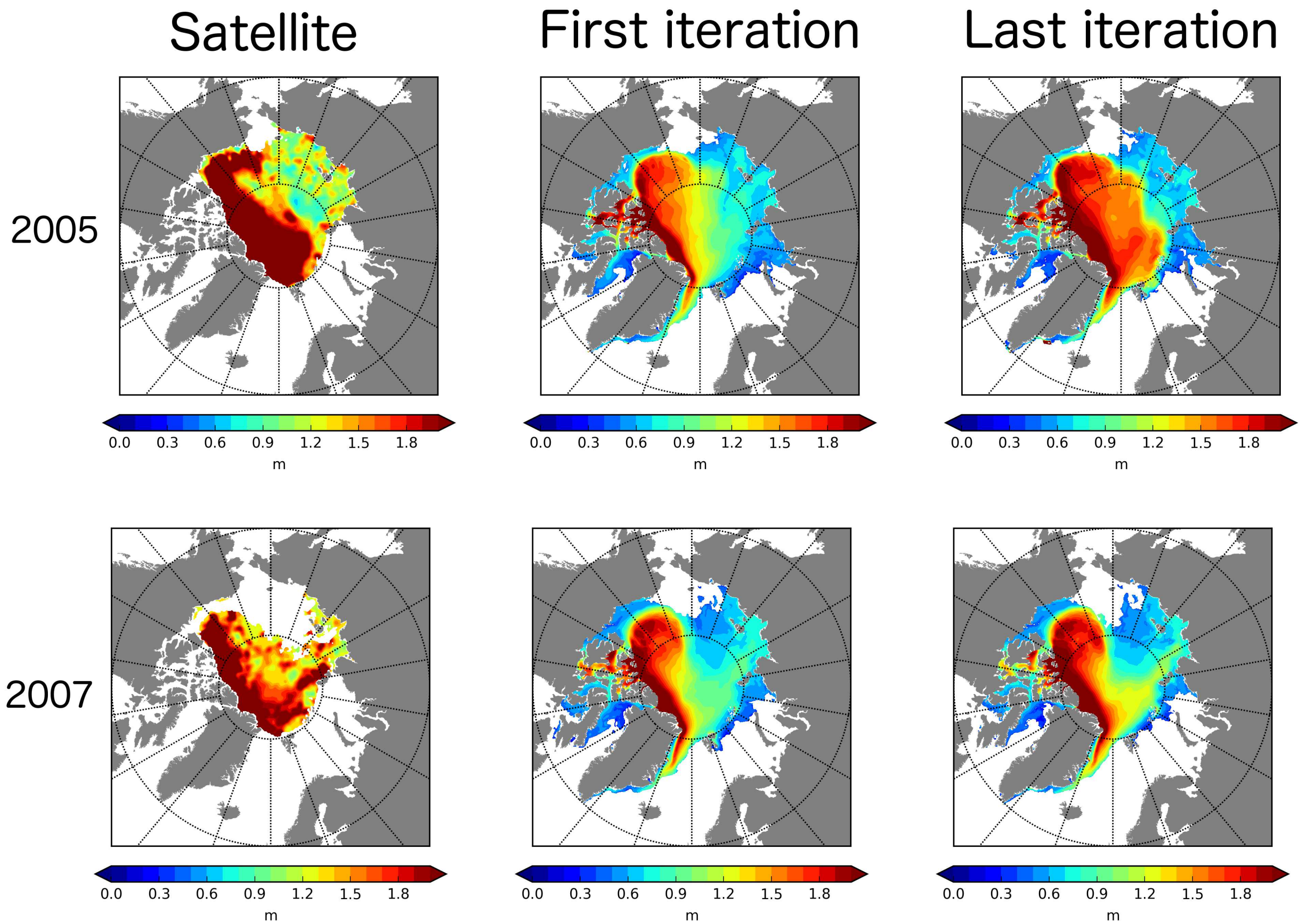

Figure 9. Sea ice thickness in October-November for years 2005 (top row) and 2007 (bottom row). Left column presents satellite data (ICESat, Kwok data); middle column are model results before assimilation (first iteration); right column corresponds to model results after assimilation (last iteration).

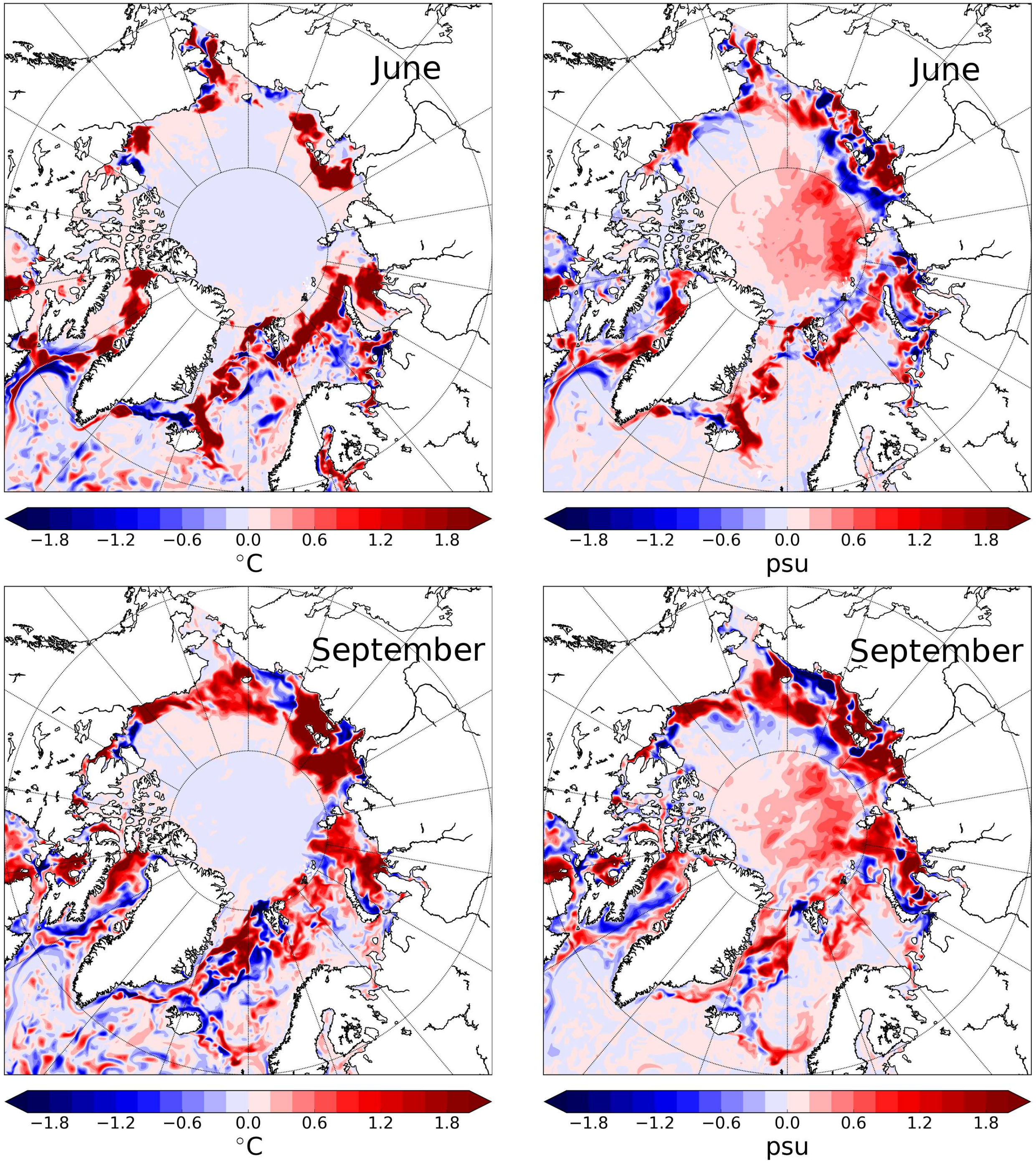

Figure 10. Differences in ocean surface temperature (left column) and salinity (right column) between first guess and last iteration for June 2005 (top row) and September 2005 (bottom row).

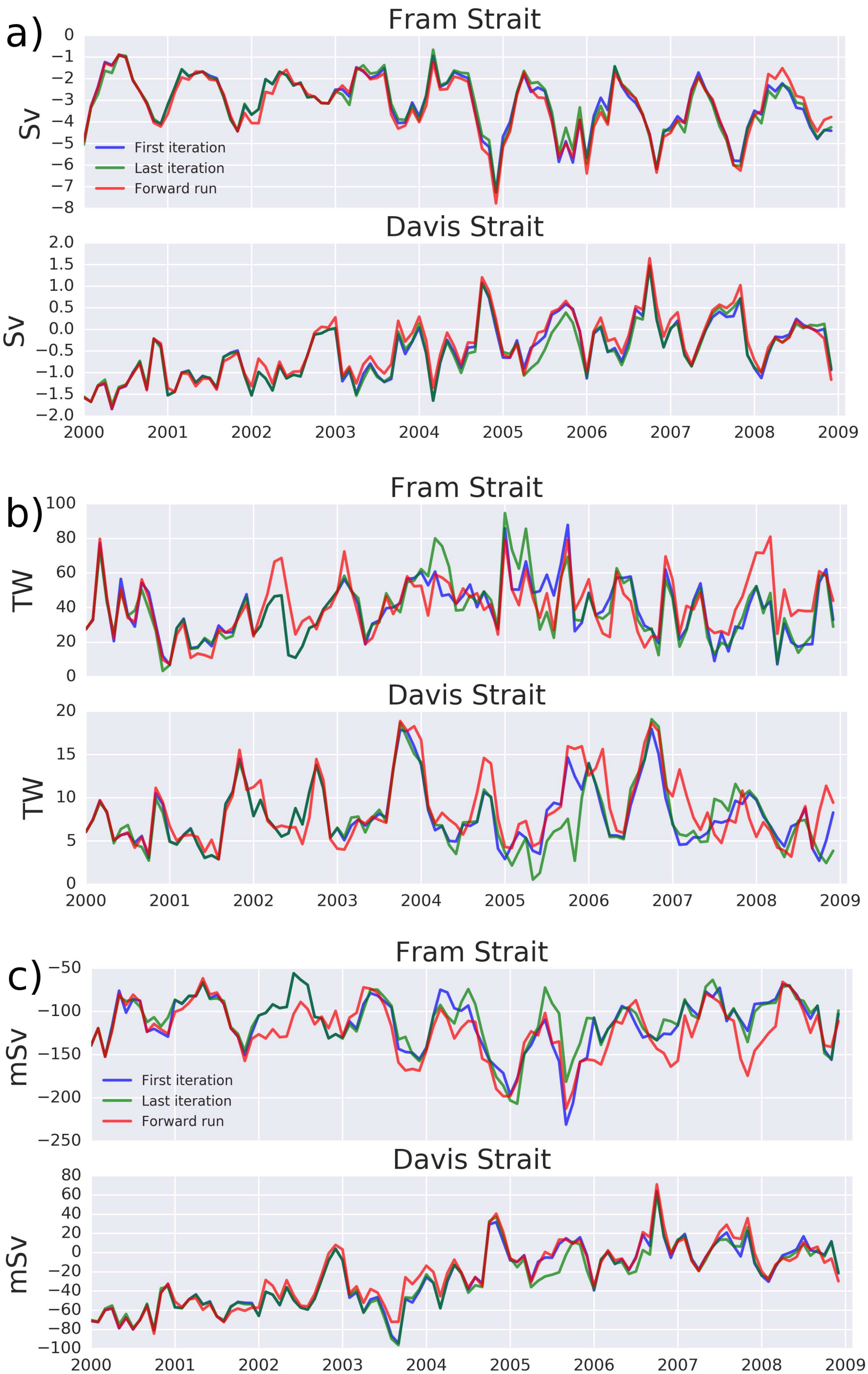

Figure 11. Fluxes through the Fram and Davis Straits of (a) volume, (b) heat and (c) freshwater. Positive fluxes are into the Arctic Ocean. Results are shown for the forward run (red), for the run before assimilation (blue) and for the run after assimilation (green).