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Interactive comment on "Arctic Mission Benefit Analysis: Impact of Sea Ice Thickness, Freeboard, and Snow Depth Products on Sea Ice Forecast Performance" *by* Thomas Kaminski et al.

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Response to Reviewer comments to manuscript "Arctic Mission Benefit Analysis: Impact of Sea Ice Thickness, Freeboard, and Snow Depth Products on Sea Ice Forecast Performance"

May 9, 2018

We thank the reviewers for their careful inspection of the manuscript. In the following we address their comments point-by-point. We use *text in italics* to repeat the reviewer comments, normal text for our response, and **bold faced text** for quotations from the manuscript, with changes marked in colour. Where we use line or Figure numbers these refer to the manuscript version published in TCD.

We provide the revised manuscript (with and without changes highlighted) in the supplement.

1 comments by Anonymous Referee #1

This is a well written and detailed paper in which CryoSat-2-derived ice freeboard, *sea ice thickness and snow depth products are used to assess a coupled ice-ocean model's forecast performance for a region that includes the East Siberian Sea, Outer New Siberian islands and the West Laptev Sea. A comprehensive list of control variables ranging from atmospheric forcing, initialization fields and physical processes (e.g., density of sea ice) are used. The paper presents a very thorough description of the Quantitative Network Design (QND) and how it is used to assess the observational impact of remotely sensed ice freeboard on the uncertainty reduction on sea ice volume and snow volume. The substance of the study is highlighted in Figure 16 which shows the uncertainty reduction in the three areas for sea ice volume and snow volume when evaluating quantities such as sea ice thickness, radar freeboard, and lidar freeboard.*

General Comments:

There is a wealth of information provided to the reader in terms of detailed tables and figures. The paper title is somewhat misleading as only the last day of the model forecast (May 28, 2015) is used in the evaluation. I expected a much longer period of analysis (e.g., weeks to months). Figures 14 and 15 (see comments below) are too difficult to read in their present format. The manuscript appears to include all relevant references.

We'll respond in detail below, where we address the specific comments, which take up all the above points again.

Specific Comments:

The *paper title implies that the use of sea ice thickness, freeboard, and snow depth products will be used to assess sea ice forecast performance. However, I only see an evaluation of the model forecast on May 28, 2015. Could an extended period (entire month of May 2015) be evaluated? The Northern Sea Route is mentioned many times in the text. The maritime transport*

industry should have interest in how ArcMBA and the QND approach could be used to predict the ice conditions for June, July and possibly August as well. Could this work be extended in this manner for a future study?

Yes, the QND approach is flexible in that respect, but, of course, this requires that the corresponding target Jacobians for June, July and August be computed and the QND analysis be performed (which we consider beyond the scope of the present study).

Page 6 (line 9-10): *The paper states "We perform these predictions for May 28, 2015, a point where there is still sufficient snow cover for our prediction to be relevant". How- ever, on page 12 (lines 23-24) the paper states "Note that on May 28 parts of the target regions are almost snow free already". How does this impact the first statement about "sufficient snow cover"?*

Snow is already reduced strongly on May 28 especially in target region ESS (reduced by about 90%) but it still shows enough sensitivity to perturbations in the control vector. To clarify, we have changed the sentence on page 12 to read:

Note that on May 28 parts of over the target regions are almost snow free already a large fraction of snow cover of has already melted. The misfit to the modified Warren climatology in the target area East Siberian Sea is on the order of about $10cm$ (50% relative error) but much less for the other target areas.

Also *in this paragraph, to make sure I understand; the model was spun up for a period beginning January 1, 1979. A restart file from March 31, 2015 was used to initialize MPIOM and the modeling system was run with data assimilation through April 30, 2015. The 4-week model forecasts begin May 1, 2015 and I assume are forced with the ERA-Interim reanalysis, but without any ocean/ice data assimilation? Is this correct?*

We confirm that, in the spin-up, the model is driven by ERAinterim and no data assimilation is performed. We added a clarification:

As we base our QND experiments on simulations from April 1 to May 28, we Next we address Arctic MPIOM performance over our assimilation and forecasting period (see Figure 4). We show the April mean and the May 28 mean of the modelled SIT and the misfit of the April mean thickness to that retrieved from CryoSat-2 (Figure 8). For a comparison of CryoSat-2 thickness to in situ observations we refer to Haas et al. (2017).

Please *provide a more detailed caption for Figure 2 and provide some additional text about the trajectories (notional) depicted in this figure.*

We added detail to the caption:

Schematic Presentation presentation of the QND procedure: Each coloured line illustrates a model trajectory that simulates from a given value of the control vector (x) counterparts of the observations (d_1 and d_2) and a target quantity (y). Through the model, the observations act as constraints on the control vector, which reduces its uncertainty from $C(x_0)$ to $C(x)$. This uncertainty reduction on the control vector translates into an uncertainty reduction in the target quantity from $\sigma(y_0)$ to $\sigma(y)$.

Page 12 (lines 17-18): *Fig. 8c depicts the mean April 2015 misfit of the modeled SIT to AWI CryoSat-2 ice thickness. How does the April 2015 AWI CryoSat-2 data compare to NASA OIB for this period? Please provide an additional plot showing the NASA OIB data overlaid on the 2015 mean CryoSat-2 SIT. How does OIB compare with the AWI data?*

The validation of the CryoSat-2 product is beyond the scope of this study, and to our knowledge no such comparison exists with CryoSat-2 from AWI. That is mainly due to the fact that OIB observations are remotely sensed as well (no direct observations); OIB uses Laser altimetry to measure the Laser

freeboard and utilises, as the CryosSat-2 algorithm, assumptions on the sea ice and snow densities to calculate the sea ice thickness. In contrast to the CryosSat-2 algorithm, no snow depth climatology is taken into account but snow depth measurements from a snow radar. But these snow depth observations are uncertain as well. The estimation of the uncertainty of OIB snow depth observations taken by OIB is an active research area.

Better suited for a validation of the CryoSat-2 thickness are more direct measurements as, for instance, electro-magnetic (EM) in-situ measurements because these are (largely) independent of sea ice and snow densities. EM-thickness measurements deliver the thickness of sea ice and snow. We added to the manuscript a reference to Haas et al. (2017) who show in-situ EM-thickness observations compared to CryoSat-2 sea ice and snow thickness in the Lincoln Sea (see revised part of manuscript in response to above comment starting *Also in this paragraph ...*). The Figure that (due to technical problems with the web interface for comment submission) is only available in the file response.pdf of the supplement) depicts a scatter plot of areal EM-thickness observations and Cryosat-2 thickness observation taken in April 2017 during the PAMARCMIP2017 campaign on the Chukchi Shelf, Northwind Ridge and in the Lincoln Sea and Fram Strait.

Page 12 (line 22): *There is mention of "modified Warren climatology", but no explanation on how the modified snowcover was used in the CryoSat-2 ice freeboard retrievals. Please explain and provide specific details.*

The main challenge for sea-ice thickness retrieval with satellite altimeters is the parameterisation of snow depth on sea ice, which is not measured routinely. The current processors use a snow climatology instead of remotely-sensed data. Warren et al. (1999) established this climatology with results from drifting stations mainly on multi-year sea ice collected over the past decades. However, since the Arctic Ocean shows a significant higher fraction of first-year sea ice in recent years, the approach proposed by Kurtz and Farrell (2011) is followed and the climatological snow depth values are multiplied over first year ice with a factor of 0.5. We revised the manuscript accordingly:

Figure 9 depicts the April mean and the May 28 mean of the modelled snow depth and the misfit to the modified Warren climatology (Warren et al., 1999) that is used in the CryoSat-2 retrieval (see Section 2.5). The main challenge for sea ice thickness retrieval with satellite altimeters is the parameterisation of snow depth on sea ice, which is still not measured routinely. The current CryoSat-2 retrieval uses a modified snow climatology that addresses shortcomings of the Warren et al. (1999) climatology that was based largely on data from drifting stations mainly on multi-year sea ice collected over the past decades, and hence is not reflective of a much younger, more seasonal Arctic ice cover. Given the increased fraction of first-year ice in the Arctic Ocean, the approach proposed by Kurtz and Farrell (2011) is used and the climatological snow depth values used in the retrieval are multiplied over first-year ice by a factor of 0.5.

Table 3 *shows significant reduction in the uncertainties for SIV and SNV. I am surprised there is very little mention of these results in the text. Please expand on this in the text.*

In fact we describe these uncertainty reductions in depth on the two pages following the presentation of Table 3, together with Figure 16.

Page 28: *Graphs in Figure 14 are very difficult to read (too small). Perhaps graphs for Reg 1, 2, 3, 4, 5, 7, 8, 9 can be removed and the remaining graphs could be enlarged.*

We think it is instructive to show sensitivities to the full control vector, so the reader understands "where the action is". And then we followed the suggestion to show enlarged plots of the Jacobian rows for Region 6 and

the model parameters in a new Figure.

Page 29: *Figure 15 is a little easier to read than Fig 14, but still a challenge to read the individual plots.*

Similarly, also here we show the full control vector first and then add a new Figure with Jacobian rows for Regions 5 and 6 and the model parameters.

Although *mentioned briefly in the Summary and Conclusions, it would be of value to assess the impact from this study on the ice drift. Are there ice drift observations available in May 2015 to perform an analysis?*

The system could indeed be extended by including either an ice drift product into the set of products to be evaluated or by using ice drift as an additional target quantity to be predicted by the model. The former would require an extension of the observational Jacobian and the latter an extension of the target Jacobian. Both could be topics for a follow-up study. An example of an ice drift product to be evaluated could be that of OSI SAF (Lavergne et al., 2010). We have extended the suggestions of possible ArcMBA extensions in the conclusions section.

Technical Corrections:

- *Page 2 (line 1): Spell out EO as this is first time referenced.*
  Done.
- *Page 2 (line 12): Don't spell out EO here.*
  Done.
- *Page 2 (line 14): Is there a better term for "rawer"? Also "rawer" is used in several instances through page 9.*
  Yes, "rawer" may not be ideal, now we also use "lower-level" to clarify (and in the following use either according to context):

The constraint from rawer EO products that constraints from lower-level EO products (i.e. rawer products that more directly related to the actual measurement) that are used to derive SIT products may be even stronger, because these rawer products such products that conform more closely to the raw EO data are typically more accurate.

- *Page 2 (line 21): I suggest deleting phrase "products of further".*
  Done.
- *Page 2 (lines 22-23). Add comma after approach, and delete QND on line 23 to make 1 sentence.*
  Done.
- *Page 3 (line 3): LFB has already been defined.*
  Only in the abstract, and we think the journal policy is to repeat the definition in the main text, to be checked by the copy editor anyhow ...
- *Page 4: Figure 1 caption should read "Oval boxes", not "Ovals boxes".*
  Done.
- *Page 6: Figure 2 caption should read "presentation" (lower case p).*
  Done.
- *Page 7: Fig. 3 blue background is too dark. Please modify for better clarity?*
  We modified the plot.
- *Page 8 (line 10): Can a reference be given for "Gent and McWilliams style"?*
  Reference is given.
- *Page 9 (line 15): Replace "will be" to "are".*
  Done.
- *Page 9 (line 21): How do you come up with 34 years? Jan 1 1979 to March 31, 2015 should be 36 years.*
  Oh, an embarrassing error. Many thanks for helping us with the basic algebra!

- *Page 9 (line 24): Spell out OSI SAF.*
  Done.
- *Page 9 (line 28): delete "by" and put Lindsay and Schweiger (2015) in parenthesis.*
  Done.
- *Page 10 (line 5): typo xxx should be "regions".*
  Done.
- *Page 13 (line 4) remove "could".*
  Page 12. Done.
- *Page 21 (line 5): spell out EASE.*
  Done.
- *Page 22 (line 5): should be "For later use 'it' also lists".*
  Done.

2 comments by Anonymous Referee #2

General comment:
The authors present a formalism to assess possible benefits of different Earth Observation (EO) products for reanalysis Arctic sea ice data. The authors consider seven satellite products: sea ice thickness and free board, radar free board (derived from satellite data), and the hypothetical data laser freeboard and snow depth, the latter both in higher and lower accuracy. The question focused on in the assessment is how uncertainties of EO products are reflected in (user) defined variables, so called target quantities. An outcome of this study could be to identify those kind of EO products, which lead to the fewest uncertainties in the target quantities. The authors consider snow volume and and sea ice volume as target quantities. Sources of uncertainties

are not only found in the EO products, but also in the model and experimental setup, such as initial and boundary conditions, parameterization and a formulation of the physics. To identify the impact of these onto the uncertainty propagation towards the target quantities, a so called control vector finds application in the formalism, containing representations of these sources.
Their findings are different for the target quantities:
Discussing the satellite EO products:
In an attempt to forecast sea ice volume with the MPI-OM, it appears most beneficial to use either SIT or RFB as EO product, compared to SIFB.
If one attempts to forecast snow volume, the results are different: it is most beneficial to use RFB, while SIT lead to highest uncertainties. SIFB appeared to be in the middle.
Second, using the hypothetical products:
The authors conclude, that using a hypothetical LFB product with low accuracy is better (for both SIV and SNV) than using SIT but could not reach the performance of RFB. Improving the accuracy of the LFB product improves the performance. Using an approach where any of the above EO products is used in combination with snow depth products leads to improved performance. Again, EO products with higher accuracy lead to improved performance.

As such, I consider the work the authors introduced to be a novel and valuable contribution in the process of optimizing the use of EO products in reanalysis and thus in prediction frameworks. However, I consider the presentation of the work poor, which strongly hinders an easy approach.

The manuscript lacks conciseness and does not follow basic rules of scientific writing. For instance, notions are either wrongly introduced (such as the Jacobian), or not explained , such as M and N or the "Jacobians" or the perturbations, which appear

to be crucial in the QND formalism. The explanation of the basic equations are erroneous and in the introduction of the sea ice-ocean model MPI-OM it is explained, that this model consists of the equation of the ocean – while neglecting the sea ice. It is added later in the text. A reader not familiar with the set of equations will be confused. There are partly wrong explanations – widely extended – of topics irrelevant for the understanding of the proposed algorithms of the manuscript while relevant explanations are missing. Moreover, the captions of Figures do not (sufficiently) explain the graphs, graphs are lacking labeling of the axes, units are lacking, captions do not fit with the graphs/tables; Figures are neither properly explained in the text. The discussion of results (most likely shown in the graphs) lack references to the graphs at all, and if they refer to a graph (which might be quite complex), they do not explain, which bar and which of the many boxes in the graph they are referring to. This makes the argumentation very hard to follow.

There is a lot of jumping within the graphs, which are spread over the entire manuscript, such that the reader often finds himself in searching the graphs/tables, than in following the argumentation. I suggest to move them all to the end of the manuscript.

Moreover, the authors introduce the QND formalism, but in the development of the text it is not clear, what is precisely done. There are some indications on the procedure, for instance on how sensitivities are derived. It is not clear (for instance), how and when the EO products or the information on uncertainties are incorporated into the QND formalism.

Due to the poor/sloppy form and logic of the paper, I may have missed some principal issues that will appear better in a reviewed version of the paper.

We are glad that the reviewer recognises the novelty and value of the manuscript and appreciate the effort he/she put into further improving our manuscript. An iteration with the editorial office revealed that the reviewer inspected an earlier version of the manuscript, rather than the version published in TCD. As a consequence some of the reviewer comments were already addressed in the TCD manuscript. For a few comments we failed to identify the location in (either version of) the manuscript they refer to, and we agreed with the editorial office to ignore those. Furthermore the reviewer criticises presentation aspects that are out of the authors' control, because we need to follow the journal's guidelines. For example, the editorial office had confirmed that the Figures must not be moved to the end of the manuscript. We'll list more examples below. We hope that without these complications the reviewer would have come to a better rating of the presentation quality and, hence, the manuscript overall (given that the non-public part of the report in the journal's web interface explicitly states "Please note that this rating only refers to this version of the manuscript!"). These complications also render part of the reviewer's very long list of comments difficult to address. Having said this, we would like to stress that many of the reviewer's comments are very helpful and have led to significant improvements of the manuscript's readability (see detailed response below). We also note that sometimes different comments address similar questions. In such cases, in order to be concise and avoid redundancy, we tried to refer to responses already provided instead of repeating responses. Often this resulted in forward references, as, in writing the response, we moved backwards from the specific comments to the general comments.

Specific comments:
<u>Comments on the arrangement of the manuscript</u>
The current sectioning of the article is:
1. Introduction
2. Methods
2.1 QND

2.2 Target Quantities
2.3 Model
2.4 Control Vector
2.5 Data Sets and Observation Operators
3. Target and Observation Jacobians
4. Sea ice and snow volume uncertainty (Rename: "Uncertainties in the target values")
5. Discussion
6. Summary and conclusions

This is unfortunate. For instance, in the methods subsections the authors use terms (such as "the model", the control vector, ...) before introducing them. I suggest to first introduce the QND formalism, then to introduce the model, followed by the Data Sets and Observation Operators, Control Vector and Target Quantities. Beside, the model section (as also mentioned below) contains topics, that should be shifted into a separate section that contains a concise description of the experimental setup. This is missing so far. Yet, it is not clear to me, why hindcast experiments are discussed in this section. This is definitely not part of a model description and should be moved into a section, where results are presented and discussed.

The order of subsections of section 2 was deliberately selected. We first present the QND formalism in an abstract way (with all relevant terms: target quantity, model, control vector, Jacobians, mappings M and N). Then in 2.2. we specify the target quantities for our study, i.e. we start from our objective. When this is formulated, we can present in section 2.3 the numerical model we are going to use and can refer to the target quantities, to judge whether the model in appropriate. Based on the description of the model, we can describe the control vector (which depends on the model). Our goal is to minimise the uncertainty in the control vector through observations, so 2.5 follows naturally.

A set of clarifications (also in response to the detailed comments below) are inserted to support this logic. For example, to clarify that 2.1 takes an abstract point we included the following clarification (revised text shown below with response to content comment 4c).

And we have changed the section title of section 2.3 to “Sea ice-ocean model” to stress the distinction from the abstract model (introduced in section 2.1).

Section 3 belongs also into a section regarding the experimental setup. In such, it should also stated clearly (among a concise explanation of what and how the authors perform in the QND formalism), that and how hindcast experiments are performed and assessed. The authors should also consider to properly introduce M and N – and what they call Jacobian, as these appear to be crucial part of the algorithm.
I suppose, that section 4 is meant to be a discussion on results of the QND scheme. If so, then it should be named along that line.
The authors mention that the mean state is of little importance although it obviously impacts the derivatives: the model bias is not accounted as model uncertainty and should lead to even more optimistic benefit analysis, even with larger control vectors. This issue should be flagged upfront and in the discussions of the results.

For the experimental setup we have introduced a dedicated section (by splitting off the start of the results section) In the context of our manuscript, with focus on evaluation of EO data sets, the experimental setup consists in the description of the cases we investigate.

Section 3 takes an intermediate role: The Jacobians are a component of the QND system so they could have been presented under the method section. On the other hand they are interesting objects of study on their own. This is why we dedicated a separate section to their presentation. As mentioned above, all relevant terms (including M, N and their Jacobians) are introduced in 2.1. along with their symbols. Even if

we later justify, why we have not merged our estimates of the model error contribution into the uncertainty of the target variables, but prefer to report it separately, we need to have the complete equation in section 2.1, so the reader knows where and how model uncertainty contributes.

<u>*Content*</u>

1. *Referring to abstract, l.7 and throughout the paper: It is not clear, what you exactly did in your experiments.*

   We hope the responses to the comments clarify this. Part of the problem may also be attributed to the fact that the reviewer did not read the TCD version of the manuscript (see above).

2. *Introduction p.3 l.15f: Do not refer to results in this paper in the introductory part! This section is dedicated to the documentation of already existing work and for motivating the content of the manuscript at hand. Instead of referring to your own (unpublished) work of this manuscript, cite (published) articles supporting your suggestions. If there aren‘t any, I suggest you to reformulate your statements as hypothesis and provide reasons/indications for its validity.*

   The introduction of the TCD manuscript does exactly what you suggest: *...for doumentation of already existing work and for motivating the content of the manuscript at hand.*. No unpublished own work is referred to, and the problem is formulated.

3. *p.4 l.12 ff: I would slightly restructure the enumeration to something like (which you could refer to these by naming or referring to the numbering):*
   1. Structural uncertainty: caused by the representation of individual processes and their numerical implementation.

2. Parametric uncertainty: of the constants in the parameterization of these processes
3. Boundary value and forcing uncertainty: of relevant processes, e.g. uncertainties in the forcings such as surface winds or precipitation.
4. Initial state uncertainty.
In the following I would also rename "factor" as "uncertainty type". E.g. in l.19: it could be rephrased along the line: "The choice of the control vector is subjective. A good choice should take into account all input uncertainty categories (2. to 4. in the upper list)"

To clarify we have revised the wording and use "category" for the above uncertainty types 1-4 but "input quantity" for the components of the control vector (of which more than one typically fall into any given category). We prefer to first describe the category and then (where applicable) define a name for it.

As mentioned, the QND formalism performs a rigorous uncertainty propagation from the observations via the control vector to a target quantity of interest through a dedicated modelling chain. Hence, it is worth recalling the four influence factors which produce relying on the indirect link from the observations to the target variables established by a numerical model. We distinguish between four sources of uncertainty in a model simulation:

- **(a) Uncertainty caused by the formulation of individual process representations and their numerical implementation (structural uncertainty).**
- **(b) Uncertainty in constants (process parameters) in the formulation of these processes (parametric uncertainty).**
- **(c) Uncertainty in external forcing/boundary values (such as surface winds or precipitation) driving the relevant processes.**
- **(d) Uncertainty in the state of the system at the beginning of the simulation (initial state uncertainty).**

The first factor category reflects the implementation of the relevant processes into the model (code) while the others can be understood as represented by a set of input quantities controlling the behaviour of a simulation using the given model implementation. The QND procedure formalises the selection of these input quantities through the definition of a control vector, x. The choice of the control vector is a subjective element in the QND procedure. A good choice covers all input factors quantities with high uncertainty and high impact on simulated observations d_{mod} or target quantities y (Kaminski et al., 2012; Rayner et al., 2016).

4. *Be more concise and introduce the notions and used quantities and mechanisms thoroughly:*

   a) *p.4 l.26: Clarify what the "observational information" is. Is this the uncertainties in the observations?*

   We think at this point the general phrasing is fine, later in that section we'll be more formal. Also note response to phrasing comment 37.

   b) *p.4 l.28ff: A motivation for the use of the PDF covariance matrices, the assumption of their Gaussianity is lacking. Where is it used? Explicitly in the backpropagation step? As well, you have constants in the control vector, don't you (see Table 1, rows 1-31 – out of 45)? How are they transformed into the required structure?*
   Indicate, how the PDF covariances are constructed . In this section it could be referred to Section 2.4 Control Vector. In that section (2.4), it should be mentioned, how the PDF covariance matrix is build for each type of entry. Currently, in this section it is explained, that a perturbation is added to the fields themselves and all the discussion is about the fields, but not about the control vector itself. This is confusing. Beside, it is lacking, which law the perturbations follow – the N(0,sigma) would be a natural choice, but it

is not mentioned, neither the size of sigma. Motivate the necessity of the perturbations.

We hope that our above explanation of the logic behind the order of the sections (first general then specific) answers most of the difficulties. Also the (slightly revised) section 2.4 on the control vector (see response to phrasing comment 33) should (now) be sufficiently clear.

c) *p.5 l3: "For the first QND step we use the model M as a mapping from control variables onto equivalents of the observations." - It is unfortunate to say "the model M" without introducing it before. If M is just the mapping from control variables to the observational space, then it might be better to write: "In the inverse step we use a mapping from the control vars onto the observational space. In the upcoming we refer to this operator as the model M."*

Manuscript revised as follows:

For the first QND step we use the model a mapping M as a mapping from control variables onto equivalents of the observations. In our notation the observation operators that map the model state onto the individual data streams (see Kaminski and Mathieu (2017) and Section 2.5) are absorbed incorporated in M. Here we refer to M as model.

d) *In Section 2.1 QND explain concisely the role of the control vector, what the outcome for the target vector is dependent on the observation products and their (which?) additional information. Moreover, you list the sources of uncertainties for the model, but not for the EO products. Elaborate on these as well!*

After the explanation of the terms in equation (3) on page 5: It is not clear, how the forecast/assimilation is involved. It is not clarified throughout the manuscript.

It might be beneficial to introduce N more properly. It is not really clear to me, which role the control vector plays at this stage, not how it is involved in

the QND structure.

While M is a mapping from the EO product to the model equivalent, I guess, that the ocean ice model is already somehow involved here and some of the parameterizations etc (see uncertainty types) are involved (explanation, how this is done, is missing).

In step one you thus estimate the sensitivities of this mapping (how?). In the second step, you basically aim to assess the propagation of the uncertainties within the sea ice ocean model (how?), if I understood you right. As an outcome of this step 2 you also get an estimate of the uncertainty quality of the model parameterization on the uncertainty of the target quantities. It is not clear to me, how/if the EO products are incorporated into the process.

Particularly, it is not clear, how the scheme as sketched in Fig.1 is related to the procedure as sketched in Fig. 12, which comes into play without any motivation.

These questions should be clarified.

Most of these questions are clarified in the responses to other comments. Role of control vector: phrasing comment 33. Incorporation of EO products: phrasing comment 37.

An elaboration of the sources of observational uncertainty is beyond the scope of this study, as long as it does not come in in the section of the retrieval chain between the rawer and higher level products we evaluate. Such sources of observational uncertainty are discussed in section 2.5.

The forecast of target quantities depends on the problem at hand, for the present problem it is explained in section 2.2.

MPIOM (including parametrisations) is presented in section 2.3, observation operators (including parametrisations) in section 2.5.

In step 1 we do not estimate the sensitivity of M, i.e. M' but we use it. The approximation of M' is presented in section 3.

e) *p.6 l.5: In the QND it is mentioned that there are two models involved: represented by the operators N and M. Moreover, in Section 2.3, a sea ice-ocean model is being introduced, that seems to be not incorporated into the QND (see the definition of M and N). This is confusing. A clarifying explanation on this is strongly desired.*
Moreover, the authors mention, that it is crucial to have a realistic propagation of the sensitivities of the uncertainties to the target quantities (via both, N and M, I guess), instead of a realistic representation of the simulation of the target quantities. I do not understand, how these two are disconnected. In particular, the authors compare model output with EO products, (see e.g. Fig.6-9) which contradicts their own argumentation. This needs to be clarified.
How do the authors access that the sensitivities are represented realistically?
4f) Figure 2: caption: Explain what it is seen, what are the shaded lines, what the darker? What do the x-axis and the y-axis represent? What are the units? Why are there two d_i involved and how and why at different time steps? This is explained neither in the caption nor at any point in the manuscript! What is contained in $C(d_i)$, what in $sigma(y_i)$?
These are basics. The graph is not self-explaining and does not help the reader to understand the graph nor the algorithm.
This confusion also occurs in p.7 l.7, where it has not been clarified beforehand, how the observations are incorporated into the "model" (whichever model). In the abstract you also talk about forecasting. How does this agree with a scenario which appears to be a reanalysis scenario? How is this Figure 2 connected to Figure 1 and how to Figure 12?

Link between MPIOM and equations of section 2.1 explained in revised first sentence of section 2.3:

The requirement on the dynamical To simulate observation equivalents

(M in Equation (1)) and target quantities (N in Equation (3)) we employ a coupled model of the coupled sea ice-ocean systemis that it simulates in a realistic manner . The model is required to provide realistic simulations of the sensitivity of the observation equivalents and the target quantities to changes in the control variables.

Need for realistic model sensitivities: See response to comment 9. For clarification we also added an example:

To conduct a valuable QND assessment, the requirement on the model is not that it simulates the target quantities and observations under investigation realistically, but the requirement is that it provides a realistic *sensitivity* of the target quantities and observations under investigation with respect to a change in the control vector. If these sensitivities , (As a hypothetical example we can think of a perfect regional tracer model that is run with an offset in the initial or boundary conditions for a passive tracer. The simulated tracer concentration will carry this offset, but all sensitivities will be perfect.) If the sensitivities of the target quantities and observations (i.e. the Jacobians,) are realistic, but the simulation of target quantities and observations incorrect, we can always make a valuable QND assessment with appropriate model uncertainty.

More detailed caption for Figure 2 was provided above (with changes to the manuscript pasted in) in response to a comment by reviewer 1. See also response to phrasing comments 20 and 25 (on change of symbols).

See response to phrasing comment 37 on inclusion of the observations and observation operators.

The forecasting scenario is described in section 2.2.

5. *Deducing from (5), where you define the uncertainty reduction as $(sigma(y0) - sigma(y))/sigma(y0)$, the posterior target uncertainty in equ. (4) is not $sigma^2$*

but $sigma$! Moreover, it is confusing, that in the text above you mentioned, that you do not consider $sigma(ymod)$, and come up with it here.

There is no role for $\sigma(y_{mod})$ in the formalism before equation 3. We added (before the equations that provide the squares of $\sigma(y)$ and $\sigma(y_0)$) a "via" to clarify that the square root has to be taken. For $\sigma(y)$ the resulting text change is shown with response to "Phrasing comment" 22.

6. *p.7 l.26: Here it is said that predictions are performed, but from the preceding it appears that (in some way) the incorporation of the EO products into the model appears in a reanalysis framework (see e.g. Fig. 2). It is not clear, how the QND procedure fits with the argumentation. What I make up from the preceding is that in some way you will use different types of observations and will get different SNV and SIV . If so, it is not clear how uncertainties/sensitivities are then derived. The entire procedure needs clarification!*

   We hope the clarifications we added in response to the other comments (on inclusion of observations, forecasting, etc...) have resolved these difficulties.

7. *Section 2.3 Model: The detailed explanation is not of relevance for the purpose of the manuscript. It is not relevant to explain, what an ocean-sea ice model is, and what the particularities for MPI-OM are. Just refer to Jungclaus et al. (2013); Niederdrenk (2013). Beside, the description has parts which are seriously wrong:*

   - *p.8 l.7 ff: A short explanation: Due to the complexity of the 3D Navier-Stokes equations, it is common practice to apply a couple of approximations, such as the the hydrostatic approximation or the Boussinesq approximation. You can skip that information, this is nothing special. What follows is incorrect and should be skipped due to the already mentioned non-explicitness of the MPI-OM with respect to the primitive equations and an equation for the balance of the thermodynamics.*

   - *Particularly, you introduce the MPI-OM by saying, that is consisting of the three balance equations – which are solely related to the ocean (without mentioning) while skipping the second set of equations for the sea-ice component.*

   If you really want to make a distinction, then cite the articles related to the ocean models and those related to the ice models. You can discuss the relevant parts (like snow loading treatment in the discussion section, as you already do) when it is needed (and refer then in the discussion to the literature). Also, the discussion of the mesh is unnecessary. If it is really necessary (which I do not see) I recommend to mention the structure in short and provide a source. If there is anything particular you implemented due to the necessity of the algorithm, then mention it along the line "In addition to the standard MPI-OM we implemented... in order to ... based on [literature]".
   The part starting from p.7 l.30 to p.8 l.5 is OK. If I understand the authors correctly, then they use the last sentence in there to justify/indicate that the MPI-OM gives realistic dependencies. If this is the case, then I would formulate exactly this – e.g. by "Thus, we consider the model results to be reasonably realistic." The remainder of the model description should be removed.

   Not all readers of the article are familiar with the MPIOM (and its development status), hence we consider a short presentation of the model relevant. Nevertheless, this model description has been shortened (even though it is unclear which parts the reviewer considered "incorrect"). We skipped the very general part about the MPIOM description but maintain the part about the recent development of MPIOM and the brief description of the ocean model because we think that it is essential for the readers to have some idea about the implemented processes.:

   MPIOM is based on the primitive equations, a set of nonlinear differential equations that approximate the oceanic flow and are used in most oceanic

models. They consist of three main sets of balance equations: A continuity equation representing the conservation of mass, the Navier-Stokes equations ensuring conservation of momentum, and a thermal energy equation relating the overall temperature of the system to heat sources and sinks. Diagnostic treatment of pressure and density is used to close the momentum balance. Density is taken to be a function of model pressure, temperature and salinity (UNESCO, 1983). Recent development of the model Recent development of the ocean part of the model includes the treatment of horizontal discretisation which has undergone a transition from a staggered E-grid to an orthogonal curvilinear C-grid. The treatment of subgrid-scale mixing has been improved by through the inclusion of a new formulation of bottom boundary layer slope convection, an isopycnal diffusion scheme, and a Gent and McWilliams style eddy-induced mixing parameterisation . Along-isopycnic (Gent and McWilliams, 1990). Along-isopycnal diffusion is formulated following Redi (1982) and Griffies (1998). Isopycnal tracer mixing by unresolved eddies is parameterised following Gent et al. (1995). For the vertical eddy viscosity and diffusion the Richardson number–dependent scheme of Pacanowski and Philander (1981) is used. An additional wind mixing proportional to the cube of the 10-m wind speed (decaying exponentially with depth) compensates for too low turbulent mixing close to the surface. Static instabilities are removed through enhanced vertical diffusion.

A viscous–plastic rheology (Hibler, 1979) is used for the sea ice dynamics. The thermodynamics is Sea ice thermodynamics are formulated using a Semtner (1976) zero-layer model relating changes in sea ice thickness to a balance of radiant, turbulent, and oceanic heat fluxes. In the zero-layer model the conductive heat flux within the sea ice/snow layer is assumed to be directly proportional to the temperature gradient across the sea ice/snow layer and inversely proportional to the thickness of that layer,

i.e. the sea ice does not store heat. The effect of snow accumulation on sea ice is included, along with snow–ice transformation when the snow/ice interface sinks below the sea level because of snow loading (flooding). The effect of ice formation and melting is accounted for within the model assuming a sea ice salinity of 5 psu.

Regarding the resolution, we have included in the text why this is important:

This setup achieves a spatial resolution as high as that of the EO products we analyse (in fact over the target regions the model resolution is higher) without major computational constraints, which allows an evaluation of the full spatial information content provided by the respective EO products. Here, we will refer to this particular model configuration as Arctic MPIOM.

8. *Remark on Section 2.3 Model: I understand that in this section the authors introduce the model and refer to related literature, introduce the forcing (though it should be indicated in the Section title as well). Starting from p.10 l.11, the authors describe the initialization of the MPI-OM. This belongs to the presentation of the experimental design. I suggest to separate the experimental setup from the description of the model. I suggest to dedicate a separate section with a clear description of the experimental setup, starting from initialisation, perturbation strategies of the control vector variables, etc.*

   As mentioned above we agree on an extra section for the experimental setup, but it addresses the observational cases we investigate. The ocean model, including its setup is regarded as a component of the system, the components of which we describe in section 2.

9. *p.10 l.20ff- until the end of the section: A motivation of the upcoming paragraphs is missing and I do not see the point why it is placed in the model description section. Place it into a different section with an appropriate title. Moreover, if you aim to present an assessment of the MPI-OM hindcasts due to observations*

and a discussion on their uncertainties, then indicate this in the abstract – and motivate this in the beginning of a possible new section, where you perform this discussion.

We added a motivation for the validation part:

For a successful QND assessment it is essential that MPIOM provides realistic sensitivities of the observation equivalent and the target quantities to the changes in the control vector (Equation (1) and Equation (3)). However, observations are not available to validate these sensitivities. The only validation of MPIOM possible is against observations of the state of the sea ice and ocean. In the following we present comparisons with selected observation based products first for the hindcasting period, and then for assimilation window and forecasting period.

10. *Alternatively, the authors could shortly indicate, that they consider the MPI-OM to represent the physics well, and present a summary. At this point this is not clear, how this discussion is related to the QND.*

    See response to item 9.

11. *p.10 l.26 and the discussion related: In earlier passages, the authors stated, that they are not interested in the realm of the model results, but rather in the sensitivities. This is not reflected/discussed in the comparison of concrete values against observations.*

    See response to item 9.

12. *p.10 l.28: "only small misfits": you should exclude the marginal ice zones out of this, as I consider a misfit of about 50% as noticeable. And it could be explained by stronger transport and errors in the advection schemes. As well, it is possible that in those regions there are different (weaker) tolerances in the accuracies of the observations.*

    We revised:

    In March (panel d) and June (panel e) only small relatively small scale misfits to the OSI SAF ice concentration are found but they can reach up to 50% (here and in the following we use the term "misfit" for the model-observation difference)

    We don't want to speculate about the reasons but just want to describe the performance of MPIOM.

13. *p.11 l.3: it is not clear to me that you look at hindcasts. Clarify this beforehand, for instance in a separate section explaining the experimental setup.*

    We introduced the hindcast in the paragraph about the model initialisation. We rephrased the beginning of the following paragraph:

    The hindcast with Arctic MPIOM has been validated against remotely sensed ice concentration from the reprocessed OSI SAF Ocean and Sea Ice Satellite Application Facility (OSI SAF) sea ice concentration product ...

14. *p.13 3f: How much sense does it make to compare multi-annual means in a period of sea ice decline? Is the interdecadal trend insignificant?*

    Indeed the value of a comparison of the mean state is limited in a strongly changing climate but we think that a more detailed validation is beyond the scope of this paper. For the QND approach only the state in April and May 2015 is of relevance which we discuss in Figure 8 with respect to SIT and in Figure 9 with respect to SND.

15. *p.15 l.2: Describe where the uncertainties are derived from and how.*

    This is exactly what the section does, it describes the PDF of the control vector, i.e. mean and uncertainty:

For process parameters this standard deviation is estimated from the range of values typically used within the modelling community. The standard deviation for the components of the initial state is based on a model simulation over the past 37 years and computed for the 37 member ensemble corresponding to all states on the same day of the year. Likewise the standard deviation of the surface boundary conditions is computed for the 37 member ensemble corresponding to the April-October means of the respective year.

16. *p.15 l.5: If you want to be indepth: you could explain, why it is numerically cheaper to divide big vectors into several smaller ones. Or is it rather due to the fact, that it is beneficial to get to know where the uncertainties stem from? At least this was the impression in the extensive argumentation that comes later in the manuscript.*

    We added a bit of detail:

    The largest possible control vector in our modelling system is the superset of initial and surface boundary conditions as well as all parameters in the process formulations, including the observation operators. As described in section 3, the Jacobian computation requires an extra run for each additional component of the control vector. To keep our ArcMBA system numerically efficient, two and three-dimensional fields are partitioned into regions.

17. *p.15 l.12ff: How can uncertainties have diagonal form? It looks like what you mean by uncertainties also contains information about cross-correlations between the different control variables. Elaborate more on that, or repeat it here in a concise way. Otherwise it does not make sense. Uncertainties themselves will form no matrix but a vector.*

    Response to phrasing comment 20 should have clarified this.

18. *p.20 l.1 : What is the retrieval chain and how can this (as well as Fig. 12) be brought into agreement with the QND formalism introduced in Fig. 1. This section lacks explanation on how this incorporates into the QND formalism.*

    See response to phrasing comment 37.

19. *p.20 l.5: the Jacobian is a matrix which contains derivatives. This I do not see reflected in the right hand side of Fig. 12. What I see is that the observational equivalents of the left hand side products are being derived and – so it seems – compared. Maybe, sensitivities is a better word. Anyhow, I do not see this reflected in the graph. If it is a Jacobian, it could be useful to give a formula.*

    Revised formulation to:

    The right-hand side of the graph illustrates how this Jacobian is derived from the Jacobians of the the equivalents of the respective products are simulated from the relevant model variables, which are denoted in violet colour.

20. *-p.20 l.7f: I do not see where you derive variables that describe the changes in the variable (due to changes in the control vector) – this is why you have the control vector, right? Moreover, the comparison regarding the complexity is not clear and should be explained.*

    The observational Jacobian M' (sensitivity of observation equivalent with respect to control vector) is described in section 3. For incorporation of EO products into M see phrasing comment 37. The complexity refers to extra computations that require extra input, as described in the next sentence.

21. *p.20 l.12f: "SIT refers to the grid cell average, i.e. for the Jacobian...": grid cell average vs dividing by SIC is not coherent to me. Please correct.*

    Is is common practise that observers define sea ice variables on a grid as the mean over the ice-covered grid cell while modellers define the mean as the average over the grid cell. The model analogue of the former quantity can be calculated by dividing the latter quantity by the sea ice concentration in the grid cell, i.e. by SIC. No need for any correction of the text, we think.

22. *p.23 l.14: relating to the Beaufort Gyre: If this is the case, shouldn't there be then a negative correlation seen in those regions, 7 or 8?*

    We added:

    WIX is positive for eastward wind stress. A positive perturbation on WIX is most distinct in region 6 (but also evident in regions 7 and 8) and slows down the Beaufort Gyre which advects less sea ice into the target region (sea ice behaves, at least in April and May, to a large extent like a rigid body, i.e. the impact in regions 7 and 8 acts almost instantaneously on the target regions) resulting in a negative sensitivity.

23. *p.31 l9: it is not clear how your assessment is linked to this forecast. When did you apply your QND framework? In which period? How did you treat the nonstationarity?*

    We did it for one specific period as described in section 2.2.

The language could be improved throughout the manuscript. Here, I give suggestions to some of the parts, which I considered most worthy to be improved. The author should consider to use short and flat structured sentences.:
- abstract l.10: remove the institutes name, it appears awkward, just "all derived from CryoSat-2".

See response to technical comment 20.

Phrasing&Structuring

1. *Abstract l.7 "observation impact (added value)": replace by "added value of observations" or "We assess the added value of different EO data products in terms of ..."*

   "observation impact" is a standard term in the data assimilation community (see, e.g., Todling (2013)). One occurrence had been removed already in the TCD manuscript (see below).

2. *Abstract l.9: "the assessments cover" replace by "We assess seven..."*

   Done as suggested.

3. *Abstract l.11f: concerning the phrases in brackets: I suggest to replace both by "(low and high accuracy)" each.*

   Done as suggested.

4. *Abstract l.20: "Providing" instead of "the provision of".*

   Done.

5. *p.2 l.7: Mention that this forecast is done with one particular model, namely MPIOM.*

   Done.

6. *p.3 l.7: Write instead "Forecasts of the ice and the ocean state are ...", as the sea ice-ocean models not only contain equations describing the dynamics of the system, as you also introduce later in the manuscript.*

   Done as suggested.

7. *p.3 l.8: A minor suggestion: formulate in a positive way: "In order to derive reliable forecasts, uncertainties in the model initial state, of the atmospheric b.c.s and in the parameterizations of physical processes should be minimized."*

   Done as suggested.

8. *p.3 l.9: remove "only". For instance, observations of bad quality are of no advantage. And improvements in modeling, parameterization etc. also contribute to improved model output.*

   Done.

9. *p.3 l.21: "observation impact" : change to "the impact of observations"*

   Was already changed in the TCD manuscript.

10. *p.3 l.23: optimized for what?*

    Sentence extended:

    The technique originates from seismology (Hardt and Scherbaum, 1994) and was first applied to the climate system by Rayner et al. (1996), who optimised the spatial distribution of in situ observations of atmospheric carbon dioxide to achieve minimum uncertainty in inferred surface fluxes.

11. *p.3 l.27f: "successfully demonstrated" sounds weird.*

    Replaced by "successfully applied for".

12. *p.4 l.9: Do not use control vector at this stage, it is confusing, when it is not introduced yet and does not lead to a further understanding. I would just skip it. Furthermore, maybe it is better to formulate, that with the QND formalism you are able to assess how the uncertainty propagates from the observations (raw data?) to a certain target quantity. To my mind it is not of interest at this point to add information on the modeling chain. It is just confusing.*

    Done, resulting text was shown above in response to comment 3 related to content.

13. *p.4 l.10f: I would remove "hence", as this is the 4 factors you identify. ("We distinguish 4 types of ..."). Remove "influence" at end of line 10, as it is redundant and*

    confusing. Instead you could consider to use the phrase "sources of uncertainties".

    Done, resulting text was shown above in response to comment 3 related to content.

14. *p.4 l.17: remove "(code)", this is redundant.*

    Yes, a deliberate redundancy to be really clear.

15. *p.4 l.22: Keep the message as short as possible to maintain comprehensibility. For instance remove "any potential model output" and replace ", for example a process parameter such as the albedo of the snow" by "(such as the albedo of snow)". The phrase "process parameter" only adds confusion.*

    First suggestion followed, second not, as we think it is useful for a better understanding of the generality of the concept to mention the "class" of quantities the albedo of snow belongs to.

16. *p.4 l.26f: A suggestion to rephrase: "In a first step, we reduce the uncertainty in the control vector by making use of a given inverse model and information (to be specified by the authors) on the observations." Then start a new sentence for the second step.*

    We split in two sentences, but prefer our wording.

17. *p.4 l.28ff: You could shorten it to "Within the QND formalism, we present all involved variables/quantities by probability density functions (PDF)." The explanation does not add new information.*

    Done, but "variables/" deleted.

18. *p.4 l.8: "based on algebra" sounds weird. I would just phrase it as "and is partly based on..."*

    Done.

19. *p.5 l.5: I would replace "absorbed" by "incorporated".*

Done.

20. *p.5 l.7: "with covariance $C(x)$, i.e. the uncertainty is given by": There is an inconsistency. Why is $C(x)^{-1}$ the uncertainty, and why are the data uncertainties $C(..)$ and not $C(..)^{-1}$? I would rather replace that by: "with covariance $C(x)$, which is given by/defined as".*

We have clarified our use of the term uncertainty where we introduce the PDF notation through the following additional text:

In the context of these PDFs we will use the term uncertainty to refer to its full covariance matrix in the case of a vector quantity, and in the case of a scalar quantity or a given vector component it refers to the square root of the entry on the diagonal of the full covariance matrix corresponding to that particular vector component. In the latter case the uncertainty refers to one standard deviation of the marginal PDF corresponding to that component, and we use the notation $\sigma(d_2)$ to denote, for example, the standard deviation of the second component of d.

21. *p.5 l.12: Is "observational constraint" the correct word? Shouldn't it rather be the given uncertainty of the observations? Furthermore, to improve readability, use $C(dmod)$ instead of "the second term". Also mention here, that this is a subjective choice, instead of coming back to that 10 lines later when discussing different equations. For a better understanding, I suggest a reformulation from line 9: "where the data uncertainty $C(d)$ is a combination of two factors: [formula]. The term $C(dobs)$ expresses the uncertainty in the observations and $C(dmod)$ the uncertainty in the projection operator M . Its/Their (both?) formulation is a subjective choice.". For the formula (2) you could also shortly explanation/indication, why you used the quadratic form. I guess, the reason is smoothness and higher regularity due to the inversion step. Or do you aim to account more for larger*

uncertainties than for smaller? (Which is what the L2 norm does compared to the L1 norm).

Squares unintended (in fact left overs from an earlier version of eq 2 that was formulated in terms of the σ). We think it is o.k. to be general here and discuss the role of model uncertainty together for eqs 2 and 3. We included phrasing suggestions as follows:

where the data uncertainty $\mathbf{C}(d)$ combines is the combination of two contributions:

$$\mathbf{C}(d) = \mathbf{C}(d_{\mathbf{obs}}) + \mathbf{C}(d_{\mathbf{mod}}) \quad (1)$$

The term $\mathbf{C}(d_{\mathbf{obs}})$ with the uncertainty expresses the uncertainty in the observations and $\mathbf{C}(d_{\mathbf{mod}})$ the uncertainty in the simulated equivalents of the observations $M(x)$:

$$\mathbf{C}(d)^2 = \mathbf{C}(d_{\mathbf{obs}})^2 + \mathbf{C}(d_{\mathbf{mod}})^2$$

. The first term in Equation 1 expresses the observational constraint impact of the observations and the second term the prior information contentimpact of the prior information.

22. *p.5 l.14: replace "in the second step" by "in the propagation step" ... you already introduced that notion. As well it is now confusing, which model you consider. Better to first introduce the model and then what is done in this propagation step. A proposition: "The model N involved in the second, the propagation step, is the mapping from the control vector onto the target quantities. The Jacobian of N, (N') is used to estimate how the posterior uncertainties in C(x) propagate to the" - I am confused here: before equation (1) you say, that $C(x)$ is the covariance of the Gaussian PDF of the posterior control vector. And here you say, that that $C(x)$ is the control vector. Use unique formulation.*

Use of uncertainty clarified in response to comment 20.

Revised phrasing according to suggestion:

In the secondstep, the Jacobian matrix $\mathbf{N}'$ of the model (now used as a The mapping N involved in the second, the uncertainty propagation step, is the mapping from the control vector onto target quantities and denoted by a target quantity, y. The Jacobian matrix $\mathbf{N}'$ of the mapping N) is employed to propagate approximate the propagation of the posterior uncertainty in the control vector $\mathbf{C}(x)$ forward to the uncertainty in a target quantity, $\sigma(y)$ via...

23. *p.5 l.18: For improved readability, I would proceed chronologically in the order of occurrence of the terms (start from the beginning of the equations), introduce the meaning of the single terms and indicate subject choices then. My suggestion for p.5 l.18-p.6 l.4: "The first term, $N`C(x)N`T$, reflects the propagation of the posterior uncertainty $C(x)$ to the target uncertainties via the model N, while $sigma(ymod)$ reflects the remaining uncertainties (see types 2-4 in the list above), that are not yet represented in the control vector. Like $C(dmod)$, this quantity is set due to subjective choice. In our work, we skip this term in order to sharpen the contrast between the EO products, and only mention two plausible estimates."*

    After the revision in response to comment 23, we think the presentation of Eq 3 reads well, no need for further modification. We do not skip $\sigma(y_{mod})$ but report it separately.

24. *p.7 l.13: "does not require real observations": This phrase is unnecessary. Instead you could just say, that the QND formalism can be used to assess/evaluate hypothetical...*

    Done as suggested.

25. *p.7 l.15: here you use d as the set of observations, and in l.7 you use d1 and d2. Introduce the definition of d (using vector notation) before you use it (or*

components of the vector without mentioning). For instance, Fig. 2 could be introduced after such a definition. A suggestion: First say, that it is possible to evaluate a network of observations, that do not need to have the same structure, nor be available on the same grid. In particular, this enables the study of the benefit of using hypothetical data networks. As is done in this work.

See response to comment 20. Figure revised from "$C(d_i)$" to $\sigma(d_i)$

26. *p.10 l.11: "from a restart file a dd.mm.yyyy generated ...", remove (start time of ERA- Interim).*

    Done.

27. *p.10 l.15: The initial ocean state is assumed to be at rest, the initial sea ice...*

    We added in the manuscript that the sea ice is at rest as well.

28. *p.12 Fig 6: Explain what blue and what red colors mean! How is misfit defined? How do you assess with this comparison the sensitivities instead of real values?*

    Explanation of colours added to caption. For the rest see response to comments 9 and 12 related to "content".

29. *p.12 l.7: "is linear in time plus a quadratic time-dependent component, i.e. it does not contain year-to-year variability." this correlation is not clear to me. Explain or remove!*

    We think that the information is important to understand the assessment and refer to the Lindsay and Schweiger (2015) paper for a detailed explanation, see also response to next comment.

30. *p.12 l4: explain the ice thickness regression procedure.*

    A detailed description of the procedure would be outside the scope of the paper. We refer to the Lindsay and Schweiger (2015) paper.

31. *p.14 caption Fig. 8: Needs to be improved along the line already mentioned (Think of self- explaining!, colors, notions, etc.)*

    Done.

32. *p. 15 Add to the title of the control vector: "and Uncertainty specification".*

    The control vector is represented by a PDF, which implies that the section addresses both mean and uncertainty.

33. *Section 2.4: Give a little introduction into the purpose of the control vector. Do you gain information by using that one? What is the difference in the outcome when using a large or a small control vector? Somewhat trivial: Add, why you do not modify the control vector, while you do so with the observations.*

    We have revised the text as follows:

    The definition Criteria for the choice of the control vector and the specification on prior uncertainty follows Kaminski et al. (2015)The components and their prior uncertainty are presented in Section 2.1. The specification of prior, both mean (x_0) and uncertainty ($C(x_0)$), follow Kaminski et al. (2015), and is listed in Table 1.

    Exploring the sensitivity of the results with respect to the specification of the control vector could be the topic of a follow up study, as is mentioned in the conclusions (variations from year to year).

34. *p.15 l.2: Consider to add ", C(x0)," after "uncertainties". Moreover, I would shorten: "(2015), and are listed in Table 1."*

    Done.

35. *p.15 l.7ff: In order to avoid confusion, the part in the brackets where it is said that perturbation is added to the entire part of the simulation, should be put out of the brackets.*

    Done.

36. *p.15 l.11: Either use present tense ("results"), or reformulate: "Thus, the control vector contains in total 157 control variables."*

    We use present tense ("results").

37. *Section 2.5: This section is not understandable at all. As introduction of this section clarify where you apply the data sets and where the observation operators in the QND framework!*

    We added a sentence to the first paragraph:

    Recall that the (combination of) data set(s) enters the QND algorithm through its uncertainty $C(d)$ and that the observation operator is incorporated in the model M (see Section 2.1)

38. *p.16 l.8: when you use the word "link", you should say between what. Right now you only use from model's state variables, but lack the to-part.*

    We added "to the respective data sets".

39. *p.17 check table caption against the table: column one lists the indices/place of occurrence of the quantities in the control vector, while column 2 the abbreviation. To enable easier reading you could section the table in 3 parts, the first being process parameters, the second initial fields and the third forcing fields. You could remove the third column and section by horizontal lines and note the type by writing "process parameters" etc in vertical style left beside the index. Alternatively, insert additional rows that only contain "process parameters" etc as sectioning of the table.*

    The last column can be removed and instead it should be explained in the caption, that the parameters are unique values, while initial and forcing are given in the control vector individually for each of the 9 regions (and refer to the figure 10 *where they are introduced). Column 5 lacks units in most of the entries. Caption and head of table disagree.*

Units added where missing. Typos corrected. The last column is useful to identify the location of individual components in the Jacobian plots. But we have followed the suggestion to add horizontal lines to section the table into the three compartments.

40. *Fig. 12: What effect do the assessment boxes have? Which role do they have in the upcoming of the manuscript? Explain abbreviations in the graph, that have not been introduced yet, such as MSS.*

    The assessment boxes indicate where the model and the retrievals "shake hand". Definition of MSS added.

41. *The first time the notion "Archimedes' principle" shows up, it could be shortly explained, if the author want to be self-explanatory.*

    We thought that Archimedes' principle needs no explanation in a scientific paper, but have now added a reference (to Guerrier and Horley (1970)).

42. *p.20 l.20: for consistency in notation, use formula for snow depth or write the following formulas in words, i.e. "densities of snow, ice and water".*

    Symbol for snow depth was not used to avoid confusion with modelled snow depth.

43. *p.20 l.21: add names of f_i, f_r and f_l. It has so far only once been mentioned in Fig. 11.*

    The names are introduced on p.16 l.10.

44. *p.20 l.28: motivate $-0.22hs/c$: what is this and where do you take the formulas from.*

    A motivation is given in l.28 but we rephrased the sentence.

45. *p.21, l5: remove "provided by AWI", and use: the CryoSat-2 product files used in this work.*

    Done.

46. *p.21 Caption of Fig. 13: time is missing (April 2015). l.6f: How do the uncertainties in the other times look like? I do not see how you incorporate the uncertainties into your algorithm. And: l.8: you introduced before the diagonal structure of the "uncertainties". So I would refer to that by "Recall, that we assume uncertainties to be uncorrelated in space".*

    We added the time in the caption. Observational uncertainty enters via equation (1) and (2)). Rephrased as suggested.

47. *p.21 l.10: give a justification/reason, why you use the threshold 0.7 for SIC.*

    All altimeter retrievals have problems for large open water fraction. We selected the threshold in analogy to the CryoSat retrieval. We added that in the manuscript.

48. *p.22 l.10: Does M refer to model MPI-OM? With respect to what is the derivative?*

    M' is the derivative of the simulated EO product with respect to the control vector and was defined in section 2.1.

49. *p.22 l.17: where do you derive $sigma_i$ from – particularly for process parameters?*

    We had explained it at the end of section 2.4 and provided the extra text with our response to content comment 15.

50. *p.22 l.20: what is a 1-sigma change?*

    We rephrased the sentence (change by one standard deviation).

51. *p.23 l.2: It is easier if you explain, that this plot shows the sensitivities of the XXX due to changes in SIFB, LFB,...*

What does that mean: "the Jacobian for April means of SIT over a point"? One entry in the Jacobian is: $\partial f_j/\partial x_i$. Explain, what f_j, what x_i is?.

We had defined the Jacobian in 2.1 and had interpreted it as sensitivity in the preceding paragraph. We extended to read **a point in space**.

52. *In the caption of fig 14 clarify that each bars in the plot corresponds to the uncertainty/sensitivity (?) of one entry in the control vector due to the changes in the values XXX in the black dot! Then explain that for instance for SIT there are 4 bars for each region – one for each EO product. It is very hard to read this figure without any further explanation.*

    Done.

53. *p.23 l.4: add information where you are referring your discussion to, for instance "SIT sensitivity (indicated as the XXX bars in the graph)" – otherwise it is simply confusing. End of that sentence in l.6: add "in that region".*

    Done.

54. *p.23 l.9: this has not been indicated in your model description. Just give a reference here.*

    The dependence of the sea ice growth on the open water fraction is independent on the model formulation. We rephrased the sentence.

55. *p.23 l.17: "the various..." where do we see this in Fig.14? Do you still refer to this figure? Indicate which bars you are talking about! This applies for the entire section! Any statement you make – refer to the corresponding bars!*

    We added:

    The various freeboard products exhibit high sensitivity to initial SIT and SND (orange, red, and green bars in Figure 14).

56. *p.23 l.28: what is the model N? Are you still in Fig.14?*

    N' is defined in section 2.1 and we write that we are on Fig 15.

57. *p.24 l.11: put "region 6" out of the brackets, as this is a particular feature of region 6!*

    Done.

58. *p.25 l.3: Is "derive" the right word? If so, say how you do this. Else, use "use"/"introduce". In any case, motivate your choice.*

    Yes, exactly: We explain immediately how we do it.

59. *p.25 l.4 remove: "and listed in the last but one row".*

    Done.

60. *p.25 l4: "model that perfectly simulates"...: where do you use this result and how?*

    Exactly here, to translate a thickness into a volume.

61. *p.25 l.6 "and listed in the last row": remove.*

    Done.

62. *caption of table 3: 4-6 are 3 columns, whereas prior and posterior are 2 values, confusing! Moreover, you could refer to the figure where they are depicted.*
    Is low or high accuracy used? Explain where you find "without additional product", "with product with low accuracy" and "product with high accuracy in the table".

    Caption explains clearly that uncertainty is given per region and target quantity, i.e. we have 2x3 values. Accuracy (or absence) of snow product in column 3 as described.

63. *p.26 l.8: better phrasing (and indicating what you are referring to): "the performance of SIFB (bars with magenta color in Fig. 16) is similar for ".*

    Was already revised in the TCD manuscript.

64. *p.26 l.11: Figure 14: ...green bars in (?). explain what you are exactly comparing! This applies for the entire manuscript and I will not further mention any further occurrences.*

    We added to the caption:

    The sensitivities of the respective EO product to the control vector ("observational Jacobianrows ") for a April means of SIT, SIFBLFB (orange bars), RFB (red bars), SIFB (green bars), SIT (black bars) and LFB SND (cyan bars) over a single point indicated by the black dot (and by yellow black cross on Figure 3). The observational Jacobians with respect to the process parameters are shown in the left middle panel. The other panels show the observational Jacobians with respect to the initial and forcing fields (see Table 1 for an explanation of the abbreviations).

65. *p.26 l.17: "has so good performance already": and l.20 "the first thing to note", l.22 "with uncertain assumption primarily": improve phrasing.*

    We rephrased: **The This imbalance is lower for the high accuracy LFB producthas so good performance on SIV already,, because this product already performs excellently on SIV such that there is not much scope for yet better further increases in performance on SNV.**

    and

    The first thing to note is that the step First, we note that switching from SIT to SIFB drastically reduces the performance for SIV.

66. *p.26 l.20: which step? In which procedure? Refer to figure.*

We replaced "the step" by "switching" to prevent confusion with the two-step procedure of QND formalism.

67. *p.26 l.23: (right hand side of Fig. 12) instead of on the modeling side of Fig....*

    Done.

68. *p.27 l.20 Remove "We need to" and "here". And put "(Equation (2))" at the end of the sentence!*

    Done

69. *caption fig 14 and 15: write instead the dependencies/sensitivities of xxx to xxx. For instance it looks like in Fig. 14 you depict the outcome of step 1 (inverse step, see your Fig. 1) meaning the sensitivities of the control vector to the EO products, while in Fig.15 you depict the sensitivities of the target variables to the control vector. (forward step 2 in your Fig.1) – could that make sense?*

    Yes. Caption adapted.

70. *caption Fig.16: Uncertainty reduction due to what? Explain the different bars, the different color codes.*

    Done.

71. *p.31 l-5 which setup do you mean? Regarding the spatial resolution: It is clear, that it is finer than the target regions... why do you mention that here?*

    The setup of MPIOM (we added "of MPIOM" in the manuscript). By mentioning the size of the target regions we want to make clear that the sensitivity of the target regions is aggregated over many model grid boxes, and small-scale effects are averaged out.

72. *p.31 l.12: what does that mean that you are not resolving changes in the initial conditions? Does that mean that in the considered period of integration, the*

model state does not develop that much away from the initialization? Furthermore you emphasized several times in the manuscript that you are not interested in the real state but in the realistic representation of sensitivities. How does that fit here?

We are talking about the control vector. The initial and boundary condition have the full temporal and spatial scales included. The perturbations to the initial and surface boundary condition are per region, though. The model state can develop freely away from the initial state in response to the surface boundary conditions. We have not stated that we are not interested in the “real” state (we discussed the “real” state of the model in section 2.3) but we stated that the “real” state does not enter the QND formalism directly but only via the model sensitivities (which have some dependence on the “real” state, of course).

Technical corrections - compact listing of purely technical corrections, typing errors etc.

1. *Articles are lacking in many places, such as in (p.3 l.31), (p.4 l.28), (p.15 l.13), (caption in p.19), (p.23 l.4), (p.23 l.14), (caption of table 3), (p.25 l.21 and l.35), (p.26 l.2 and l.6), (p.33 l.14).*

   Difficult to follow as line references do not refer to TCD manuscript, some spots we could not identify (e.g. p.25 l.21 and l.35). Among those spots we could identify in the TCD manuscript, often articles were already present, in some cases we found that inserting an article not useful (p.3 l.31, p.4 l.28, p.15 l.13, (caption of table 3)), and in other cases we have inserted articles, we’ll see with the copy-editor ...

2. *Check for doubling of words such as in p.32 l.14 (than than) and in p.13 l.6 (the the), p.24 l.8: “compared”.*

   In all cases except for “compared compared’ the TCD manuscript was already correct.

3. *Check commas, they are missing in several places, such as in: (p.4 l 9: "as mentioned, ..."), (p.5 l.5: "In this case, ..."), (p.14 l.9), (p.16 l.8: after "In the following"), (p.20 l.12: after SIC in the brackets), (p.20, l.2 after “assessment”).*

   Done.

4. *Fullstops are missing: end of eq (6) , (7), and eq. (10), and p.20 l.29.*

   Done.

5. *Put the Tables and Figures all at the end of the manuscript. The authors jump a lot back and forth between their Figures and Tables, some of them are placed in sections that are unrelated to the Figures/Tables. Having them all in one place would make it easier to follow the argumentation.*

   This would be incompliant with the journal style.

6. *Addresses of authors should be consistent in their structure. For instance, (1) has street name, while others only list the town and the country. Address (3): Danish writing of Copenhagen, which should be changed to English.*

   Done.

7. *Abstract l.21: clarify the abbreviation EO when used the first time.*

   Done.

8. *p.3 l.9: typo: parametrisation: correct to parameterization (or to parameterization if BE is used).*

   Parametrisation was no typo, see here for BE (https://dict.leo.org/englisch-deutsch/parametrisation). We have changed to parameterisation but will check with the copy-editor.

9. *p.9 l.10: “Recent EO products”.*

   We think this refers to p 2. Switching to “recent” would change the meaning.

10. *p.3 l.12: "The constraints" (plural).*

    Done.

11. *p.4 l.19 and in other parts: use vector notation for vectors, such as the control vector.*

    We'll see with the copy editor.

12. *p.5 l.16: insert comma before sigma (y). otherwise sigma could be understood as target quantity.*

    ... is employed to propagate approximate the propagation of the posterior uncertainty in the control vector $C(x)$ forward to the uncertainty in a target quantity, $\sigma(y)$: via

13. *p.10 l.34: no new paragraph.*

    Done.

14. *p.10 l.34: "underestimates" instead of "is underestimating".*

    Done.

15. *p.10 l.35: "target regions".*

    Done.

16. *p.11 fig5: The figure does not add relevant information to the paper.*

    We think it is useful for the reader to get an impression of the spatial variation in the resolution of the model. On the relevance of the resolution we have commented above. But we have removed the "arctic zoom" panel.

17. *p.13 fig 7 (a-b) the color map is unfortunate. The reader does not see a lot of differences.*

    Maybe a problem with the printer? We had included isolines with a distance of 0.5 m to support readability, so we think they should be o.k. and will also cross-check with the copy editor.

18. *p.13 l 2 and p.23 l.11: no new paragraph.*

    Done.

19. *p.15 l 2: "prior uncertainties".*

    Added symbol to clarify.

20. *p.16 l.6: remove "by the AWI", this is not relevant here and does not follow common rules. Instead move "(Rickers et al. 2014)" after "Cryosat2 mission". Also remove "by AWI" in l.10.*

    To our knowledge there exist three different CryoSat-2 products (respectively derived by AWI, UCL, and NSIDC) and we would like to make clear which product we used.

21. *p.19: Grey coloring not explained in the caption.*

    We added the word "emphasise" for the other colours to make clear that grey has no special meaning.

22. *p.22 l.5: "For later use it..." and "and the three...".*

    Done.

23. *p.23 l.2: "a April means" - correct.*

    Done

24. *p.24 l.14: the prior row is the first row and not the third.*

    It is the third row if you take the two header rows into account.

25. *p.24 l.15: "uncertainties" - It is not only 1 uncertainty.*

    No longer present, dropped with introduction of extra section.

26. *p.24 l.17-25: I do not see why you list them here.*

    With the new section title "experimental design" this gets probably obvious. We have also changed to enumeration.

27. *p.25 l2: rows 3-18: say to which table you refrer to.*

    Table reference added.

28. *p.26 l7: regions 5 and 6.*

    Done.

29. *p.26 l.9: "In contrast to" instead of "By contrast to".*

    Done.

30. *p.27 l1: remove brackets.*

    Brackets make sense because for WLS this is only one.

31. *p.27 l.6: comma after technically. This sentence is way to long. Split it!*

    Done.

32. *p.32 l.4: "of a grid cell to a grid-cell average": use uniform writing.*

    Thanks, we'll see with the copy-editor ...

33. *p32, l.12: comma after assessment.*

    We put it after SIFB:

    In the assessment of SIFBArchimides, Archimedes' principle is applied in the observation operator, where the input quantities including snow depth are taken from the model.

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