Attribution of sea ice model biases: Authors' Response

This response is set out in the following way:

1. Response to reviewer comment 1 (review quoted and comments addressed inline)
2. Response to reviewer comment 2 (")
3. Description of proposed changes to manuscript arising from both reviews

1. Reply to Reviewer Comment 1 (Anonymous)

We thank the reviewer for taking the time to read our manuscript, and for his/her useful suggestions for its improvement, which we address inline below. The reviewer's comments are quoted in italics.

West et al propose a new analysis framework to understand model biases in Arctic sea ice which they apply to HadGEM2-ES, a model with known biases in sea ice characteristics.

The attribution of climate model errors in the sea ice zone is a very important open topic and the paper provides original and likely efficient means to evaluate such errors. The main problem I think is writing, which I found often imprecise, and renders a proper evaluation of the paper difficult.

In particular, the methods absolutely require clarification and should use better and simpler terminology. Because I did not fully get the methods, it was thereafter really complicated to follow, in particular the discussion and conclusions.

A second requirement to make this paper acceptable is to early on in the result section to explain that the induced surface flux method works - eg. to describe how well the different methods to compute surface flux biases converge. Now this is done here and there, and I have constantly been doubting of the quality of the methods, because of the absence of such evaluation.

The reviewer is right that the convergence of the different methods, demonstrated in Figure 6, deserves better discussion, and probably quantification, which we propose to carry out by more thorough analysis of the errors of the induced surface flux method in Appendix B, and discussion of these in Section 4, as described below.

Our view is that the spread amongst the different estimates is caused predominantly by observational uncertainty, with errors introduced by the induced surface flux method assumptions relatively small in magnitude by comparison. This is supported by the fact that the difference between the sum of the induced surface flux contributions and each surface flux anomaly is comparable in magnitude to the differences between surface flux anomalies wrt the different datasets (ERAI, ISCCP-FD, CERES).

We think that it would be difficult to show that the induced surface flux (ISF) method works purely by comparison with the direct surface radiation evaluation, because the difference between the different estimates are dominated by the observational uncertainty. We think a better way would be by a more thorough evaluation of the impact of assumptions made by the ISF method in Appendix B. For example:

- Evaluation of errors introduced by the simple model by comparing modelled fields of net radiation to those predicted by the formulae, as suggested by Reviewer 2
- Evaluation of errors caused by ignoring higher-order derivatives, by calculating these terms

The magnitude of these errors would then be compared to the observational uncertainty, as estimated by the difference between the direct radiation evaluations shown in Figure 6.

There is a more fundamental point: the ISF method is not just a way of calculating surface flux anomalies due to a particular process, but also of characterising them, because their definition is to some degree subjective. For example, suppose for the month of May a model shows mean downwelling SW of 300 Wm^{-2}, and albedo 0.8, given net SW of 60Wm^{-2}, but observational estimates shows mean downwelling SW of 250 Wm^{-2}, and albedo of 0.7, giving net SW of 75 Wm^{-2}, with a model anomaly of 15 Wm^{-2}. Clearly the downwelling SW anomaly induces a positive surface flux anomaly, the albedo anomaly induces a negative one, and the total surface flux anomaly is -15 Wm^{-2}. But the exact induced anomalies are subjective. The approach used in the paper is equivalent to multiplying the anomaly in one process by the mean in the other, giving contributions of +12.5 Wm^{-2} and -27.5 Wm^{-2} by the downwelling SW and albedo anomalies respectively.

Hence while it would in theory possible to say whether the *sum* of the ISF contributions was correct (if we knew the exact actual surface flux error), it would not be possible to say whether each individual contribution were correct. The main requirement is that each contribution is physically realistic, and provides useful information.

A third thing I would have enjoyed to see is a specific discussion of how the ice-albedo and growth-thickness feedbacks can be diagnosed from the method. It is claimed in the abstract that your method can separate these effects, and I am in trouble to see how that statement is presently supported in the text. I can guess feedbacks are acting from Fig. 6, but I think this topic deserves a bit more to support the claim made in the abstract.

This does require greater justification. The sentence in which the relevant anomalies are identified with the surface albedo feedback and thickness-growth feedback (page 10, line 31) will be expanded accordingly.

The thickness-growth feedback, for example, ostensibly acts by altering the energy balance at the base of the ice; as the ice thickens, the temperature gradient decreases, basal conduction decreases, and the energy balance becomes less strongly negative, so the ice thickens more slowly. But by energy conservation, this process must also have some manifestation in the external fluxes: as the ice is losing energy less quickly, some external energy flux must also have changed. Under the assumptions of the simple model used (similar to Thorndike 1992 as you note below), which ignores sensible heat storage, it is the upwelling LW term that changes: as the ice thickens, its top surface also cools to maintain flux continuity.

Hence any change to the ice energy balance resulting from the ice thickening, and therefore conducting less efficiently, can be diagnosed as the contribution of the ice thickness to the change in upwelling LW radiation.

I have also not understood why energetic errors of oceanic origin have been ignored from the discussion, especially in the North Atlantic sector of the Arctic - where there is a low bias.

Energy passing to the ice through the ocean-to-ice heat flux has two main sources: solar input to the ocean, and oceanic heat convergence. The first is implicitly taken account of through the analysis of the effect of ice fraction anomalies on net SW radiation. We make the case that Arctic-wide, the importance of the oceanic heat convergence in driving summer basal ice melt is small by comparison to direct solar input, and this will be more thoroughly justified in the revision, referencing model results by e.g. Steele et al 2010 in addition to the observational references originally included.

However, you are right that the contribution of the oceanic heat convergence should be properly quantified. In the revised version of the paper, we will include estimates of Arctic-wide ocean heat convergence (for model and observations), and set the results shown in Figure 6 in this context.

Finally, the authors claim in the conclusions that they can "quantify" the origin of errors, but apart from Fig. 6 (which I liked a lot), I did not really see a quantification of the errors. Is that quantification the main point - or is it the consistent comparison of the different sources of error ?

We do quantify individual contributions to the surface flux biases as shown in Figure 6 for a few illustrative months, in section 4. However, we will examine whether there is scope for a more systematic approach, for example quoting the annual average flux contribution for each state variable in a table.

Also, it was difficult to ultimately figure out whether biases in external forcings or in the sea ice model are the ultimate cause of the biases. Is your method capable to tell after all ?

Briefly, the answer is no: the method cannot tell the ultimate cause of the surface flux biases. It is designed to diagnose the proximate cause of the biases.

The induced surface flux (ISF) method, alone, can determine only the first-order cause of the net surface flux bias. The state variables examined (downwelling SW, downwelling LW, melt onset occurrence, ice fraction, ice thickness) affect the surface flux on very short timescales, and are unambiguously properties of the atmosphere (radiation) and sea ice (melt onset, fraction, thickness). Hence the ISF method allows the short-term causes of surface flux bias to be decomposed into those arising from the atmosphere, and from the sea ice.

It's recognised that the causes of biases in the state variables themselves may lie in different systems. For example, ice thickness biases will have some ultimate cause in the atmosphere – indeed, that is one finding of the paper. Conversely, while the case is made in Section 5 that cloud errors are to blame for downwelling LW biases, it is likely that the sea ice simulation will nevertheless have some influence on how this is manifested. However, all these effects act on relatively long timescales, compared to the almost instantaneous timescale on which the state variables affect the net surface flux.

A last general comment - the logics of the arguments should be better presented.

We apologise that the presentation of logic is unsatisfactory. This paper has been through several rounds of restructuring as the analysis has developed, and the coherence of argument has probably suffered due to this. We will try to significantly improve this in the revision.

I am pretty confident that - if these presentation issues are seriously addressed by the team of coauthors, this will make an excellent contribution to their favourite cryospheric journal.

A few specific comments.

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** I have tried to understand what the generic approach is. Here is what I have understood. The present presentation is too lengthy, misses the essential elements and overdiscusses details. A synthetic view is missing. There are three means to evaluate errors in surface energy budget (I have understood two of them)*

1) The direct computation of surface flux bias, i.e. the difference between simulated and observed surface flux (or one of its components)

This is correct although we would add the caveat that this is still only an estimate of the actual surface flux bias – the observational uncertainty is very large.

2) The induced surface flux bias, which is the contribution of bias in a specific variable to surface flux bias, namely calculated as $\Delta Fx = dFx / dx\ \Delta x$(mod-obs).

To evaluate derivatives, the SEB is simplified using two different approximations during the cold and warm seasons, based on ideas from Thorndike et al 1992.

I don't think there is a need to calculate those derivatives in the body of the paper.

We broadly agree but note the concern by Reviewer 2 that some of the calculations by which the induced surface fluxes are arrived at are incompletely explained. It may be necessary to show at least one derivative, for illustration, with the additional detail that is planned for the revision.

If the derivatives are well calculated and if the non-linearities are not too important, the sum of Δ Fx should hopefully approach the surface flux bias.

3) The third diagnostic is "the sea ice latent heat flux uptake anomaly implied by the ice volume anomalies relative to PIOMAS".

I have tried to figure out what the authors mean, but I did not really managed. The wording is not precise enough for the reader to what is meant by this and what is gained by comparing that to the surface flux biases. I guess "latent heat flux" is confusing in the context of the surface energy budget. But whether that thing is a heat storage anomaly divided by time or something else, I don't know. Maybe an "ice thickness bias converted to Joules" or "an energetic equivalent ice thickness bias" ?

This is correct. For each month, the modelled field of rate of change of ice thickness is calculated as half the difference between the following and the preceding month. A similar field is calculated for the reference dataset, PIOMAS. The reference field is subtracted from the modelled field to create a model anomaly of ice thickness change. This is then multiplied by ice density, specific latent heat of fusion, reversed in sign, and divided by the number of seconds in a month, to create an equivalent sea ice latent heat storage anomaly in Wm-2.

For the reasons discussed in Section 2.3, the surface heat flux is viewed as the main source of this latent heat storage. It's noted however that it would be useful to provide an estimate of modelled and observed oceanic heat convergence which provides an additional input to the latent heat storage.

We will clarify this point in the revised manuscript.

Besides an explanation of what it means, we would need an explanation of what should be taken from that diagnostic.

It is important to clarify this point because a lot of the argumentation was based on that.

** The two methods to compute the surface flux derivatives is called "a model". I think it is a "computation method". It is actually inspired from Thorndike et al (1992) – which should be acknowledged - and maybe from earlier works in EBMs. What you are doing is to derive the surface energy budget wrt anything.*

The 'model' versus 'computation method' is an interesting distinction – our interpretation is that it hinges on wither the formulae are viewed as a way of calculating induced surface fluxes (model) or characterising them (computation method), as discussed above. As in our view both are valid interpretations, either phrase might be more appropriate depending on the circumstances.

Thorndike et al will be cited.

2. Reply to reviewer comment 2 (Francois Massonnet)

We thank Francois Massonnet for his helpful and thorough review of our manuscript. Below, we address in turn his major and minor comments inline, which are quoted in italics.

Major comments

1) *It is not always easy to follow the authors' methodology. I see the general idea behind the approach: expressing the net flux to the ice as a function of state variables, then linearizing around a reference state to obtain the flux bias resulting from the bias in one of the model components. However, I could surely not reproduce the results myself, just based on the text. I appreciate the efforts to publish the code in Supplementary Material, but the text itself should have all elements. For example, I do not understand how the bias in F_sfc attributed to error in melt onset is derived (i.e., from eq. A6 to 4). Furthermore, the attribution of flux error to melt onset error seems to not be a function of the melt onset itself, but rather a function of concentration difference. This is confusing: melt onset is defined by the time of the day where surface melting commences, and the right hand side of Eq. 4 does not display surface melting terms. At some point it came to my mind that the authors were perhaps using "melt onset" for "ice retreat", but I'm not sure. In all cases, this is confusing.*

The definition of the state variable 'melt onset occurrence', and its relation to the net surface flux, is not very clearly explained in the paper, and certainly requires expansion. This will be altered in the revised version of the paper, and we will ensure more generally the replicability of the calculations of the induced surface fluxes (for example, noting the use of local ice and snow thicknesses as you suggest below). However, a brief explanation of this particular component, melt onset occurrence, is also provided here. Very simply, its purpose is to capture the effect of meltpond formation on the surface flux.

HadGEM2-ES parameterises the effect of meltponds by reducing surface albedo linearly from 0.8 to 0.65 as the surface temperature goes from -1℃ to 0℃, after Curry et al (2001). Because we have daily surface temperature fields, we can judge for each modelled year which day 'melt onset' – defined as the day surface temperature first goes above -0.5℃ – occurs. Comparison of these dates to the observational SSMI estimates referenced in the paper shows that modelled melt onset occurs, on average, 20-25 days earlier across most of the Arctic Ocean in the model than in observations.

In the induced surface flux analysis, we examine the effect on modelled surface flux of the melt onset process occurring at the wrong time of year. The relevant state variable here is 'melt onset occurrence', which takes the value 0 or 1 depending on whether a grid cell on a particular day has yet exceeded -0.5℃ (model definition) or whether a liquid water microwave signature has been detected (observational definition). In a similar way to the other state variables used in the paper, the observations are averaged over the period 1980-1999 to obtain a daily climatology of melt onset occurrence. For each modelled year, this climatology is subtracted from the modelled melt onset occurrence fields to obtain a modelled melt onset anomaly. This anomaly is then multiplied by the relevant partial derivative – in this case, (downwelling SW) * (cold snow albedo – melting snow albedo) to produce the induced anomaly in net SW.

2) *Besides the need for clarity in the methodology, a key question is to what extent the assumption of linearity holds, in particular for what bias range Eqs 3 and 4 would be valid. Will the methods work for models with very large biases? Another point is that this linearization involves the use of Eqs 1 and 2, that are themselves derived using linearity assumptions. I trust that the approach is valid, because the sum of individual contributions (Fig. 6) seems to match the flux errors from datasets and from volume estimates, but a quantification of this match should be done (perhaps by calculating residuals). Overall I find that the authors have not discussed the validity of this assumption, and this is critical given how non linearly the ice behaves.*

You are right that this needs to be quantified. For some of the state variables the dependence of surface flux is linear, and the second partial derivatives go to zero (e.g. all state variables in the melting season, and downwelling LW in the freezing season). However the dependence of surface flux on ice and snow thickness is nonlinear and it would be useful to examine the circumstances in which higher derivatives are important.

As you mention, additional assumptions are made in deriving equation (1): linearity of upwelling longwave dependence on surface temperature, and uniform conduction of heat within the ice. We will try to quantify the impact of these also by comparing actual net surface flux fields to predicted fields, as you suggest below in point 5).

3) *I would also like to see if the method is robust to internal variability. Could the authors take one or several of the four other members of the HadGEM2-ES model and run the same analysis? In other words, is the 1980-1999 period long enough to identify and attribute the biases?*

Analysis of other ensemble members would be a valuable enhancement of the study. For the revised version of the paper, we plan to carry out the same analysis on the other three ensemble members, and to quantify the consistency with the results from the first member.

4) *The authors have not cited an important study: Holland et al., 2008 (doi:10.1007/s00382-008-0493-4). In that study, the inter-model scatter in the sea ice mass budget (present-day conditions) is shown to be explained by the way models absorb shortwave radiation. This is directly relevant to the study here, and I think the authors should go through the Holland et al. study to position their results with respect to theirs. In particular the claim that turbulent fluxes are of relatively minor importance relative to radiative fluxes in setting the surface energy balance (Fig. 2, and p. 5 line 11-16) should be put in perspective with that study. As the Arctic sea ice mean state changes, turbulent fluxes appear to be of increasing importance.*

Holland et al (2008) show annual sea ice melt rates to be strongly correlated with summer net SW across the CMIP3 ensemble. The causality here could go in either, or most likely both, directions. This appears to be consistent with the finding in the current study that the excessive sea ice melt in HadGEM2-ES is driven by surface albedo and net SW issues. This will be referenced.

The neglecting of turbulent fluxes is a shortcoming of our study. As you point out, while they are comparatively small in an absolute sense, they may nevertheless be important in driving future changes. In a similar way, model anomalies in turbulent fluxes may be of comparable size to those in radiative fluxes even if the model absolute values are much smaller. We will expand on this point in the Discussion.

5) *The authors should prove, with a figure, that the model developed in the appendix is good enough to do the investigations. Could they plot, for one or several grid cells and one or several freezing seasons, the reconstructed flux F_sfc (Eq. A4) and the actual flux from the model? A quantification of the correspondence would be a plus.*

This would also be a valuable exercise. Actual and calculated modelled fields will be compared for a few sample years, and the correspondence described, quantified and illustrated. The most appropriate place for this would probably be the discussion of errors in Appendix B.

6) *Nothing is said about the treatment of snow in the HadGEM2-ES model. How many snow layers are there, what is snow conductivity, etc.?*

There is only one snow layer, and the conductivity is 0.33 Wm-1K-1. Like the ice, the snow has no heat capacity. It should be made clear, however (here and in the revised version of the paper) that sensible heat storage is parameterised in the top 10cm of the snow-ice column during surface exchange calculations, to aid stability.

7) *The authors repeatedly use the word "anomaly" to describe the difference between modeled and reference quantities, but I would avoid this word and use "bias" or "error" instead. To me, an "anomaly" is used to described the deviation of a signal with respect to its own mean*

We have some concern is that use of the word 'bias' might suggest that HadGEM2-ES is being evaluated with respect to the 'truth', but most datasets used are only very rough approximations to this. We will change 'anomaly' to 'bias', but clearly define at the outset the meaning of the word 'bias' for the purposes of the paper – the difference of the model relative to a particular observational estimate.

8) *I'm unclear about whether ocean surface temperature biases are accounted for in the analysis. From Fig. 6, it looks like they are not. On the other hand, p. 5 lines 6-10*

seem to suggest that the Arctic Ocean is critical in setting the ice energetic balance (and this is also seen in Keen et al (https://link.springer.com/article/10.1007/s00382-013-1679-y, their Fig. 4). So, I'm puzzled: is the contribution of oceanic surface temperature bias taken into account or not in the analysis?

It is true that the ocean contributes a significant amount of heat to the ice in the summer. However, we make the case in our study that the major part of this heat comes from direct solar heating of the ocean, an effect which is taken account of through analysis of the effect of ice fraction on the net SW bias. This case will be strengthened in the revision by citing evidence from models (e.g. Steele et al, 2010) in addition to the evidence from observations already referenced.

This is also relevant to the minor comment at 5-6/10 below.

Minor comments

1-18 - "countered by a counteracting" is a bit odd.

'Counteracting' is superfluous and will be removed.

1-24 - "from 1986-2015" –> from 1986 to 2015

Change will be made as suggested.

1-28 - Along with an earlier melt onset date, you can mention that freeze up has been delayed: Stammerjohn et al., 2012, their Fig.2 (doi:10.1029/2012GL050874)

This will be done.

1-29 - "whoseloss" –> "the loss of which"

Change will be made as suggested.

2-1 - Evaluation against observations of volume is quite impossible (even extent observations are not direct observations), so I would use "observational or reanalysis reference datasets"

Change will be made as suggested.

2-18 Instead of "anomalies" I would use "biases"

See response to point 7) above.

3-24 The period 1980-1999 is used for evaluation, because it "predates the rapid sea ice loss". Why is it a problem to have a period with strong trend in the analysis? Is it expecting that the SEB would change too rapidly during a period with strong trends? Would the analysis be robust if the model output was evaluated on a distinct and later period (2000-2015 for instance, using historical + RCP8.5 runs). Please elaborate.

The trend itself is not problematic. Our motivation for using this period was to evaluate the model on a 'reference' time period at least partially independent of the time period that is usually used to evaluate sea ice trends. We will make this clear in the revised version.

3-35 Reanalysis data also suffer from biases because of errors in atmospheric forcing, this could be stated as well.

This will be stated.

5-11/16 Can the authors explain exactly what they mean by "Heat flux due to snowfall". The presence of snow affects heat conduction fluxes and acts to reduce bottom growth, is that what the authors are talking about?

The presence of snow matters only because it takes energy to melt it; snow falling on ice changes the enthalpy of the snow-ice system. A 3m column of bare fresh ice at 0℃, for example, will take ~9.2×10^8 Jm^{-2} to melt it. If 50cm fresh snow falls on the ice, the combined snow-ice column will take ~9.8×10^8 Jm^{-2} to melt it. Hence the falling of snow on ice represents a transfer of negative latent heat from the atmosphere system to the snow-ice system, which must be taken account of in calculating the total surface flux. We will try to explain this more fully in the revised version of the paper.

5-26 I assume h_I and h_s refer to in-situ / actual thickness (this is the one that matters for vertical thermodynamics). It would be good to mention that here, as there is usually a lot of confusion between that quantity and the grid cell average thickness.

Yes, this is the case and that will be clarified.

5-24 Eq. 1: Maybe I missed it, but what is the value for ice albedo? Does albedo depend on the ice state?

The ice albedo used is 0.61. This does not depend on ice thickness, but falls linearly to 0.535 as ice surface temperature rises from -1℃ to 0℃. This information will be added to the text.

5-27 The subscript for snow thickness is "S" here while it is "s" elsewhere

Change will be made as suggested.

5-28 The symbol for albedo is \alpha_I while in the equation it's \alpha_ice

Change will be made as suggested.

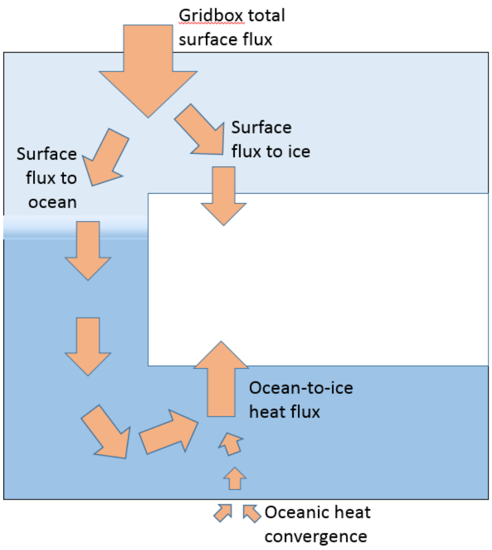
5-30 Eq. 2: the big "dot" is a bit disturbing, it makes me think at a scalar product. I would use a simple dot or no dot at all.

Change will be made as suggested.

5-6/10 The sentence "Because of this, although advection-derived ocean heating..." is unclear to me. First, can you demonstrate the oceanic heat convergence (that is not accounted for in your framework) is a small contributer to volume changes? Second, I do not follow the logical articulation with the next sencen "Hence the surface energy...". Please clarify. In the same line, reading the recent paper by Lei et al. (2018, doi:10.1002/2017JC013548) could be useful to add up to the discussion.

Regarding your first question, Reviewer 1 also suggested that it would be sensible for the oceanic heat convergence to be properly quantified, and this will be done in the revised version.

Regarding your second question, see first our response to your major comment 8) above. We agree our wording here is confusing. The case we are making is probably best illustrated by this schematic:



The point made is that the source energy for the ocean-to-ice heat flux derives, in the main, from the surface heat flux (specifically, solar heating in summer), and not from oceanic heat convergence, over most parts of the Arctic Ocean (clearly there are regions where this is not true, e.g. near the ice edge in the Atlantic sector). Therefore the surface flux analysis (specifically, the effect of ice fraction anomalies on net SW) implicitly accounts for a large part of the ocean-to-ice heat flux.

As we mention above, additional evidence will be cited for this in the revision, as well as rewording this sentence.

Thank you for drawing our attention to Lei et al (2018), which draws together a very wide range of observational data to investigate mechanisms of sea ice growth and melt. If we have understood this study correctly, it deduces a strong role for direct solar heating in driving summer sea ice basal melting by noting an association between areas of low summer sea ice concentration and high early autumn oceanic heat fluxes (as measured by ice mass balance buoys). Hence this would also be a valuable study to quote in this context.

6-1 Please give the albedo values used.

These will be provided in the revised version: 0.535 for melting ice, 0.61 for cold ice, 0.65 for melting snow, 0.8 for cold snow.

6-3 "summarises" –> "summarise"

Change will be made as suggested.

6-10 Eq.3: please describe the meaning of the terms of the equation. In particular, what is h_I_eff? It is necessary to have this information in the text somewhere.

These will be described fully.

6-23 The partial derivatives are to be evaluated at a reference state, and I understand here that a mid-point between observations and model is taken ("Where observational datasets were available, the reference quantities in the partial derivative fields were calculated as model-observation means"). The authors should explain why it was done this way. I assume that the reconstructed flux error would be mathematically closer to the actual error than if the reference was taken as either the model or the osbserved value.

I don't think that's the case. For any function *f*, evaluated at two values x_1 and x_2, if we try to approximate $f(x_2)-f(x_1)$ using the first term of the Taylor series evaluated at some point $\lambda x_1+(1-\lambda)x_2$, where $0\leq\lambda\leq1$, the coefficient of the second Taylor series term is minimised when $\lambda=1/2$, i.e. at the midpoint of x_1 and x_2. Hence evaluating the partial derivatives at the model-observation midpoint provides our best guess. We will briefly note our reasoning for using this in the revision.

6-30 The paragraph starts by saying that 4 ensemble members were run, but Fig. 3 only shows one. Can you clarify?

Only one ensemble member is used for the analysis – this will be clarified, although as indicated above reference will be made in the revised version to results for the other three members.

8-28 The word "save" should be removed, I think

It would probably be best replaced by ‘except for’.

10-25/28 Can you go a bit more quantitative here? From Fig. 6, the residual of the analysis can be calculated as the sum of individual contributions (the stars) and the actual flux error.

But what is the ‘actual flux error’? The surface flux anomalies wrt ERAI, ISCCP-FD, CERES cannot be regarded as such because of the observational uncertainties – this is clear, because the difference between the sum of the contributions and each surface flux anomaly is comparable in magnitude to the differences between surface flux anomalies wrt the different datasets. Each is only an estimate of the actual flux error, which cannot be exactly known – it is equally possible that the sum of the contributions is a more accurate estimate than any.

We can provide the residual of the analysis for completion with respect to ERAI, ISCCP-FD or CERES – but the actual numbers will differ greatly depending on which dataset is used.

10-31/32 The surface albedo feedback is not just a sea ice concentration thing. The melting of snow, the thinning of the ice are also key players in the surface albedo reduction, even though ice concentration remains unchanged. Wouldn’t it make more sense to include these factors as well in the definition of surface albedo feedback?

It is true that the surface albedo feedback also includes these effects. However, it would not be possible to include the effect of snow melting because of the lack of reference dataset. The effect of ice thickness on albedo is not actually modelled by HadGEM2-ES – the albedo switches abruptly to the open ocean value when the sea ice thickness falls to zero. Hence there would be two separate effects to estimate here: the direct effect of ice thickness anomalies on albedo, and the effect of HadGEM2-ES not modelling this. Given this, as well as large uncertainties in observations of the link between albedo and ice thickness for thin ice, we think this effect is outside the scope of the study. However, it will be mentioned as a possible additional contributing factor to the summer net SW bias.

11-15 The sentence "Hence the melt onset anomaly, acting alone, would induce a seasonal cycle of sea ice thickness both lower, and more amplified, than that observed..." is unclear, especially regarding the "lower" part. Can you please rephrase?

How would the following be:

‘Hence the melt onset anomaly, acting alone, would induce a seasonal cycle of sea ice thickness lower in the annual mean, but also more amplified, than that observed, because the surface albedo and thickness-growth feedbacks act to translate lower ice thicknesses into faster melt and growth. For similar reasons, the downwelling LW anomaly, acting alone, would induce a seasonal cycle of sea ice thickness higher in the annual mean, and also less amplified, than that observed.’

12-1 "concludethat" –> conclude that

Change will be made as suggested.

Fig. 3. I'm puzzled by panel (a). Sea ice extent seems small. Is that because the domain "Arctic Ocean" is restricted to the seas of Fig. 1? In other observational records, like NSIDC, winter sea ice extent is more in the 14-16 million km2 range.

Yes, the extent is calculated over only the Arctic Ocean domain, like other variables in this paper, and so the winter extent appears much lower than in the well-known NSIDC figures. We think that this is appropriate, because much of the winter variability in whole-Arctic sea ice extent is due to processes in the subpolar seas, which are not relevant to the Arctic Ocean process analysis in this study.

References

Steele, M.; Zhang, J.; Ermold, W. (2010): Mechanisms of summer Arctic Ocean warming, J. Geophys. Res. (Oceans), 115, C11, doi: 10.1029/2009JC005849

3. Summary of proposed modifications to the paper based on both reviews

Minor revisions

All minor comments made by Reviewer 2 addressed as indicated above.

Thorndike et al (1992) cited for the simple models used in section 2.3, as requested by Reviewer 1.

Abstract and introduction

Wording will be altered to clarify that the induced surface flux method is only able to unambiguously identify the proximate cause of sea ice biases, and that the ultimate cause remains a more complex question.

Section 2 – data and methods

The treatment of snow in HadGEM2-ES will be described in more detail.

The meaning of the 'heat flux due to snowfall' will be explained more clearly – although this will be done as briefly as possible, as this flux is not important for the analysis.

The methodology description in Section 2.3 will be refined to explain more clearly how the individual components were calculated. If there is scope for removing some details (e.g. calculation of partial derivatives) this will be done.

The discussion of the basal energy balance, and the role of oceanic heat convergence versus solar heating, will be made more explicit, and additional evidence will be cited.

Section 4 – induced surface flux results

Effort will be made to quantify the induced surface flux anomalies shown in Figure 6 in a more systematic way, possibly in a table. Residuals with respect to the direct surface radiation evaluation, and the sea ice latent heat storage evaluation, will be quoted in the context of the large observational uncertainty.

Internal variability in the induced surface fluxes will be discussed, noting results from applying the analysis to the other three ensemble members of HadGEM2-ES.

The difference between the various estimates of surface radiation anomaly will be discussed in relation to the additional error analysis of Appendix B (see below).

The effect of ice thickness on albedo, and its neglect by HadGEM2-ES, will be noted as a 'missing process', alongside the albedo effect of snow on ice.

The logical link between the surface radiation anomalies and the sea ice latent heat storage anomaly will be explained more clearly.

The model error in Arctic Ocean oceanic heat convergence will be quantified as far as possible given current observational estimates, and Figure 6 will be interpreted in this context.

Section 5 – discussion

The means by which the surface albedo and thickness-growth feedbacks can be diagnosed from the induced surface fluxes will be described more fully, as requested by Reviewer 1.

The role of turbulent fluxes will be discussed more fully, with reference to Holland et al (2008). This study will also be quoted in the discussion of drivers of the summer net SW anomalies.

The paragraph discussing the separate effects of the melt onset and downwelling LW anomalies on the sea ice will be rewritten as indicated above.

Appendix B – error analysis

The discussion of errors will be enhanced to consider the impact of other assumptions, e.g. linearising the Stefan-Boltzmann relation, ignoring higher-order derivatives. As part of this, model net radiation fields will be compared to fields calculated from the formulae used in the ISF analysis, as requested by Reviewer 2.