Final author response to reviewer comments for
"Modeled Arctic sea ice evolution through 2300 in CMIP5 extended RCPs"
by P. J. Hezel et al.

The Cryosphere Discuss., 8, 1383-1406, 2014
www.the-cryosphere-discuss.net/8/1383/2014/
doi:10.5194/tcd-8-1383-2014

19 May 2014

We thank Drs. Alex Jahn and Dirk Notz for their thoughtful comments and suggestions to improve the manuscript. We respond to each of the reviewer comments individually below, our response being in plain text.

- Paul Hezel

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Received and published: 26 March 2014

In this contribution, the authors analyse the sea-ice evolution in the extended CMIP5 simulations that cover the time period up to the year 2300.
The paper is well written, easy to grasp and scientifically largely sound. I would, nevertheless, have wished for a more in-depth analysis of some of the scienfic findings presented here. However, basing this review on the things that are there rather than on those that aren't, the current contribution contains sufficiently new material to warrant publication in The Cryosphere subject to some minor revision. Additional analysis (as sometimes indicated below) would, however, certainly increase the impact of this study, but is not strictly necessary to allow publication of this manuscript.

Specific comments:
p.1384, l.26: Why is the entire analysis focused on sea-ice extent rather than sea-ice area? In the former metric, spurious jumps in sea-ice cover could appear simply because individual grid cells become ice free in a threshold-type manner once their concentration drops below 15 % even if the sea-ice concentration decreases gradually. The only argument for using sea-ice extent would be its better comparability with observations. However, in this contribution the comparison to observations remains rather superficial and is in particular not followed up at all for the further analysis of model simulations. I hence suggest to change the analysis from sea-ice extent to sea-ice area unless there are good reasons to stick to the former metric.

Sea ice area is compared to sea ice extent for all 37 RCP8.5 models shown in IPCC AR5 in the figure below.

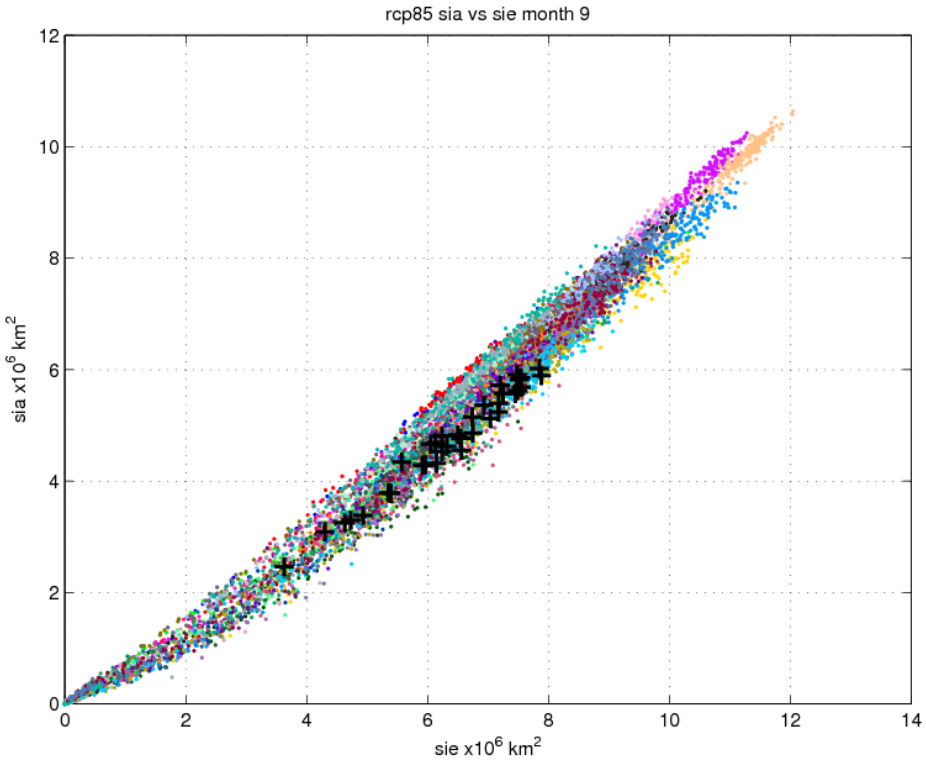

Figure caption: SIA vs SIE for unsmoothed September RCP8.5 time series (37 models) shows an approximately linear relationship between the two modeled quantities. Slopes for

each model are a mean of 0.85 (range 0.68-0.98). There are obvious non-linearities for SIE below 2x10^6 km^2, where the mean slope drops to 0.56. For SIE > 2x10^6 km^2, mean slope is 0.92 (range 0.73- 1.10). Black crosses indicate observations for SIE and SIA from NSIDC from 1979-present. The slope for the observations is 0.85.

We generally agree with your assessment of the problems with sea ice extent compared to sea ice area, and have added a reference to your paper in the methods section. We state, " Though sea ice area may be a better metric \citep{Notz2014}, we have chosen to use sea ice extent rather than sea ice area for its ease of comparison to observations and previous studies."

We note that the CMIP output of monthly averages of sea ice concentration (SIC) is used to create the SIE fields. We would expect that the averaging that goes into the monthly SIC field results in a somewhat random distribution of small SIC values about the 15% threshold, resulting in a smaller bias than one might find for example from looking at SIC fields for a single time step in the sea ice model. In this case, wind-driven divergence might drive relatively large areas of the ocean across the 15% SIC threshold, and hence greatly affect the calculation for sea ice extent.

There is a relationship between sea ice area and sea ice extent that is predictable for each model as shown by the figure above, and is consistent with the relationship in the observations. Since sea ice extent makes for easier comparison to previous studies which have focused primarily on sea ice extent, sea ice extent makes a better comparison to observations, and observed sea ice area is still subject to a greater uncertainty than extent, we strongly advocate for leaving the discussion in terms of sea ice extent. Furthermore, if one is interested in diagnosing the changes to the sea ice cover, the change in sea ice extent is larger than the equivalent change in sea ice area at low values of each (i.e., the slope in the figure decreases below 2 million km2). Any non-linearities that appear in sea ice sensitivity with respect to time or temperature or some other metric should therefore be clearer when using sea ice extent as compared to sea ice area.

*p.1385, l.4: Why is the response dominated by the forcing *or by changes in global mean annual surface temperature*? Arctic sea ice doesn't know much about global mean annual surface temperature. It's true that global mean temperature and sea ice co-vary, but the response of sea ice is not dominated by changes in global mean annual surface temperature.*

Thank you for pointing out this statement. The paragraphs starting p.1385 l.4 and ending l.25 have been reworked as follows to better convey the connection between global temperature and forcing, and why we discuss results with respect to the global temperature.:

"The Arctic sea ice in coupled climate models responds prominently to changes in forcing. The global mean surface temperature is proportional to the forcing as long as the forcing continues to increase (e.g., Long and Collins, 2013) and is indicative of both the forcing and the global feedbacks. As the rate of increase in forcing slows and the forcing becomes constant in RCP4.5 and RCP8.5, both the global mean surface air temperature and Arctic surface temperature continue to slowly increase as a~result of the adjustment of climate system to the continued energy imbalance at the top of the atmosphere (Hansen et al., 2005, Held et al., 2010). To demonstrate the relationship between forcing and global mean temperature, the forcing and the global surface temperature response are shown in Fig. 1 for the three RCPs examined here: RCP2.6, RCP4.5 and RCP8.5. Extended RCP6.0 had only two models with sea ice concentration data through 2300 and was not included in this analysis. For each RCP, the idealized net forcing shown here is calculated from the greenhouse gases and other forcing agents, including aerosol direct and indirect effects, and is described in Meinshausen et al. (2011).

In this paper we discuss the relationship of the changes in sea ice extent and volume with respect to the changes in global mean surface air temperature as a reflection of the forcing, although a~discussion with respect to forcing itself or greenhouse gas concentrations is equally applicable (e.g., Jahn and Holland, 2013). Changes in sea ice can

also be discussed with respect to Arctic regional surface temperatures (e.g., Zhang, 2010), but it is difficult to separate the effect of warmer surface temperatures driven by other causes (e.g., warm air advection, radiative changes due to clouds) and thus a driver of sea ice changes, from the surface temperature response due to reduced sea ice concentration, thinner sea ice, and thus increased oceanic heat flux to the atmosphere."

l.13ff: This point should also be made more clearly here. In particular, it's not fully clear why for global mean SST it is advantageous that it includes feedbacks, while for Arctic SST such inclusion of feedbacks is given as the main reason for not using this metric.

We have modified the text in the passage for the comment above, and now state:

"Changes in sea ice can also be discussed with respect to Arctic regional surface temperatures (e.g., Zhang, 2010), but it is difficult to separate the effect of warmer surface temperatures driven by other causes (e.g., warm air advection, radiative changes due to clouds) and thus a driver of sea ice changes, from the surface temperature response due to reduced sea ice concentration, thinner sea ice, and thus increased oceanic heat flux to the atmosphere."

p.1386, l.7ff / section 4: Mahlstein and Knutti went to great lengths to correct their analysis for model biases. Such analysis of model biases is apparently not done here, which makes it hard to judge in as how much the present findings relate to the real world. Some more work in this direction would be highly desirable.

Please see the discussion of the Mahlstein and Knutti 2012 (MK2012) paper in the comment for P. 1392 L.3 below.

p.1387, l.14: It'd be interesting to learn how much the different ensemble members differ from each other in terms of the analyses that you carry out here. And be it just one additional sentence that indicates that the findings remain unchanged (if that's the case...).

As Table 1 shows, only 4 of the 14 models analyzed have more than one ensemble member, and for RCP8.5 only one of 9 models has more than one ensemble member. As we are looking at the response of the sea ice on fairly long time scales, the additional information gained by including more than one ensemble member even if they were available is limited, as the ensemble members describe variability on interannual and slightly longer time scales. We added a sentence stating that 'the results do not change with the inclusion of additional ensemble members.' Furthermore, there are no additional ensemble in cases where variability may play a large role and be useful to evaluate its effect – e.g. in determining the lag in response to decreasing forcing in RCP2.6.

l.23: "ocean area" should be made more specific

Changed to 'ocean-covered area of the grid cells'.

p.1388, l.22: I don't see the contradiction (as indicated by the "but") between the first part and the second part of this sentence.

This was split to two sentences to clarify the point: "The spread in modeled sea ice volume is quite large in September at the end of the 20th century. This partially reflects the poor observed constraint on sea ice volume since time series estimates of sea ice thickness or volume have been difficult to assess until very recently."

p.1389, l.5: same here (related to "though")

This was changed to: "Soon after the forcing begins to decrease the global mean annual surface temperature also begins to decrease. It does remain higher, however, by a~multi-model mean of 0.73 C (range 0.35—1.4 C) at the end of the simulation compared to the period 1986--2005, which is the period of approximately equivalent forcing."

l.10: re-phrase: "large component of natural variability": of what is natural variability a component?

Rephrased to read:
“ For individual models there is a~time lag before the minimum SIE after 2044 in the individual models of up to 6 decades due to the large natural variability at reduced SIE and the relatively weak decline in forcing.

l.15: "minimum extent" should be made more specific, since it is often used synonymously with "summer extent".

Rephrased to read: “and the summer Arctic sea ice extent is larger at the end of the 23rd century than the minimum extent between 2006 and 2300 in all 9 models. “

*l.23: should it be "extent *and* volume"?*

Yes. Sentence clarified as follows: “All but two models (CESM1-CAM5.1 and GISS-E2-R) show roughly the same sensitivity (well within a factor of two) in September Arctic sea ice extent and volume per degree global temperature change when global temperature is on a decreasing trajectory compared to when global temperature is on an increasing trajectory.”

l.28ff: I have difficulties in re-conciling these findings with those discussed in ll. 21-24. There it says that the models show roughly the same sensitivity during warming and cooling, but now it says that there is no consistent change and hence no model agreement regarding hysteresis. If the statement in ll 22-24 is correct, the models apparently do agree that there is no hysteresis, it seems to me.

Yes – you are correct. We have changed the last two sentences to clarify this point: “Even though there is a~hysteresis in temperature with respect to the forcing due to the residual effect of the slower components of climate warming, there is no evidence of hysteresis in sea ice extent or volume with respect to increasing and decreasing temperatures.”

*p.1390, l.5: Add "continues *to remain constant* through"*

Changed as suggested.

l.15 ff: Where can this be seen? Would be nice to analyse this more quantitatively.

This can be seen in Fig. 3 by inspection. The reference to the figure has been added to the text.

l.19: style: replace "dramatic"

Changed to 'sharper than in RCP4.5'.

p.1391: l.7: were the changing trends in extent and volume calculated or simply visually taken from the time series? I know from our own analysis that this statement is not true for MPI-ESM-LR, which shows a sudden increase in volume loss.

The trends are assessed by inspection of Fig. 4. On closer inspection, it is clear that the MPI-ESM-LR and the HadGEM2-ES show an steeper trend, and MPI-ESM-LR does show a sudden volume decrease at the same time as the non-linear drop in SIE. The sea ice thickness field in the MPI-ESM-LR suggests however that this may be related to the minimum sea ice thickness (0.5 m) prescribed during ice growth.

The text was changed to: “In all but two models, however, sea ice volume demonstrates a continuing linear or slower rather than faster rate of decline through the disappearance of winter ice, and thus we conclude that apparent threshold behavior is not occurring in this set of models as the winter sea ice disappears.“

p.1392: l.3 (p.1394, l.22ff): Excluding these three models still seems to give a range of 2-4 ◦ C warming for summer sea-ice disappearance and of 7-10 ◦ C warming for winter sea-ice disappearing. Not sure I would call such ranges "broad agreement"... Can these ranges be lowered following the methods given by Mahlstein and Knutti?

"Broad agreement" in the text characterizes the additional warming to make winter ice disappear among CMIP5 models, not the comparison of CMIP5 with MK2012.

We choose not to reproduce the MK2012 analysis here. MK2012 use the recalibration from the models to inform the estimate of when the observations result in an ice free state. Our emphasis here is on the fact that the models give a more coherent picture of sea ice disappearance in terms of temperature than in terms of time, but it is still consistent with the temperature given by MK2012. The implication is that if observational estimates of the sensitivity of sea ice to global or Arctic temperature increases can be better constrained, then the estimate for sea ice disappearance (both in time and temperature) can be calculated. Though not explicitly stated, the MK2012 analysis results in fairly large uncertainties (with a range between 1.3 and 3 C).

The range in temperatures at which modeled sea ice disappears is the result of differences in mean states in the models, as well as sensitivity of the sea ice as discussed in MK2012. Disentangling the two sources of these contributing uncertainties is however not obvious, as the first is probably not independent of the second. The recalibration methods such as the one applied in MK2012 are potentially efficient in reducing uncertainties, but it is necessary to get sharp estimates of observed sensitivity of SIE to global or Arctic mean temperature changes. Due to the shortness of the observational record and the spread in modeled sensitivities and apparent nonlinearities in the system, the result is large uncertainties in estimates of temperature of sea ice disappearance evident in MK2012, Fig. 2 and 4. The sensitivity of sea ice to global temperature change is not necessarily linear in temperature as the sea ice declines, as shown in the figure below.

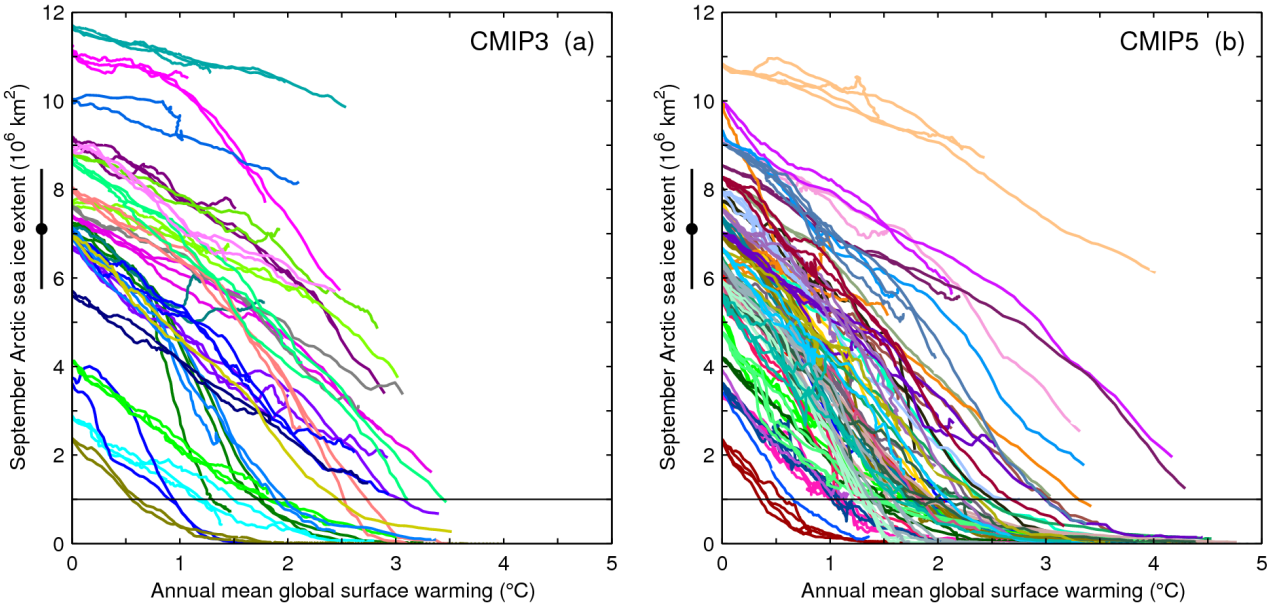

Figure caption: Sensitivity of September sea ice extent to global mean temperature change in CMIP3 (reference period 1980-1999) and CMIP5 (reference period 1986-2005). From IPCC AR5, Fig. 12.30.

l.8: Style: change to "rates of winter sea-ice decline".

Changed as suggested.

l.8: Again, only visually, but the rate of decline of GISS-E2-R doesn't seem constant to me, but to rather decrease towards the end of the simulation.

The rates of decline do decrease slightly towards the end of the simulation, but the point is that the rate of decline seems to be independent of the existence of summer sea ice, which is not the case for the other models. As to whether the trend is constant or decreases slightly does not matter.

l.15: style: "In the seven models that lose September sea ice in both their RCP4.5 and their RCP8.5 simulation, the mean..."

Changed as suggested.

l.21: this sentence is not fully clear and should maybe be split in two. This would allow a clearer description of the "maximum additional increase".

The second part of the sentence was removed, which clarifies it without changing the point.

l.24: I doubt that sea ice responds to changes in global mean surface temperature, it only responds to changes in local forcing.

Phrase changed to: " sea ice responds to the changes in global forcing reflected in the global mean surface temperature, regardless of the forcing trajectory to arrive at that temperature."

p.1393: l.2: split into two sentences.

Changed as suggested.

l.18: Why is this a contradiction as indicated by the "although"?

The next sentence indicates that a similarly short period of decreasing forcing for the CMIP5 models also fails to show a significant trend.

l.26ff: I don't understand how this discussion relates to the reversibility of sea-ice extent decline.

If the mismatch between observations and modeled rates of decline is real, then the sensitivity of sea ice to warming may be larger than modeled. For this possibility, then the sensitivity to a recovery should also be similarly more sensitive and could result in equivalently faster rates of recovery.

p.1394, l.16: This must be made more specific: Obviously, stabilization at a random level is not sufficient to prevent an ice-free Arctic, also RCP8.5 stabilizes.

The is addressed as the last two paragraphs were changed to read:

We have shown here the evolution of Arctic sea ice extent and volume in the extended RCP2.6, RCP4.5, and RCP8.5. RCP2.6 demonstrates an increase in September ice extent in all 9 models as the radiative forcing in that scenario decreases after 2044 through 2300. In RCP4.5, 9 of 14 models have already become seasonally ice-free in September before the peak in radiative forcing, and an additional 3 models are ice-free in September by 2145. In RCP8.5, the September Arctic sea ice disappears in all 9 models, and the winter sea ice also disappears in 7 of 9 models under this scenario. Though the timing of the disappearance of September sea ice is not well constrained by the CMIP5 models, the global mean annual temperature increase at which sea ice disappears is fairly robust, both across models and across RCP scenarios.

From a~policy perspective, extended RCP2.6 indicates that a~recovery of Arctic sea ice could begin if and when policies to reduce global greenhouse gas concentrations and hence radiative forcing are implemented. Extended RCP4.5 further shows that a~plateau in the forcing may not be sufficient to prevent continued Arctic sea ice loss and a~seasonally ice-free state even if the decrease in forcing begins before the disappearance of summer sea ice. In practice, a reduction in forcing to prevent further sea ice loss needs to be sufficiently large to dominate the recalcitrant warming expected from heat storage in slowly evolving parts of the climate system (e.g., deep ocean) (Held et al., 2010). The threshold at which a forcing reduction maintains a constant global mean temperature would itself be a function of the estimated equilibrium and transient climate sensitivities of the earth system. As the RCP scenarios do not incorporate interactive carbon cycle processes and feedbacks, the

impact of such processes would need to be considered in the design of any strategies to reduce radiative forcing.

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Received and published: 28 March 2014

General comments
The manuscript describes the sea ice evolution in the extended RCPs in the CMIP5 archive. It shows interesting results on the summer and winter sea ice decline past 2100 in the RCP4.5 and RCP8.5, and the sea ice recovery in the RCP2.6, as simulated by several CMIP5 models. As the first analysis of the extended sea ice simulations from several CMIP5 models, this analysis will be very relevant and novel, and it puts earlier studies with individual CMIP5 models into context of the other CMIP5 models. The methods used are well described, and the results are by themselves novel and should be published. Before publication, the manuscript text needs some careful editing for better clarity and to remove several statements that are not fully supported by data and/or are confusing. In some places the manuscript also lacks statements as to why things are as seen in the CMIP5 models and I hope the authors can add these, giving the paper more substance. I recommend publication after these relative minor revisions and look forward to reading the final paper.

Specific comments
Page 1384; Line 9-12: It is unclear to me how the RCP2.6 and RCP4.5 "imply that summer sea ice extent could begin to recover if and when radiative forcing from greenhouse gas concentrations were to decrease", when it is said before that in the RCP4.5 the sea ice continues to decline (as forcing only stabilizes, not declines). This sentence therefore needs to be removed or rephrased, as it is only true for RCP2.6, but not RCP4.5.

Our thinking on this is as follows. In RCP2.6, the sea ice on average responds to the decrease in radiative forcing, and the decrease dominates any residual warming in the climate system and the Arctic which would otherwise continue to cause sea ice loss. RCP4.5 stabilizes the radiative forcing, and that stabilization allows a continued Arctic warming which continues to cause ice loss at a slower rate.

Given the rigidity of the RCP scenarios, we think this conclusion is warranted, though we have not determined the threshold at which a reduction in forcing would dominate a continued warming at stable forcing. In Section 5, we state: 'It might be expected therefore that a similar reduction in forcing under the higher forcing scenario of RCP4.5 would result in a similar summer Arctic sea ice recovery.'

We have changed the statement here to read: "Based on the analysis of these two scenarios, we suggest summer sea ice extent could begin to recover if and when radiative forcing from greenhouse gas concentrations were to decrease."

Page 1384; Line 26; Page 1385, line 1: It needs to be made clear here that this manuscript documents the first analysis of the sea ice simulation past 2100 in a suit of CMIP5 models, by adding "from these extended RCPs in a suite of CMIP5 simulations in this paper", as other authors have analyzed the sea ice past 2100 in individual CMIP5 models (these are cited later, but it needs to be made clearer how this paper is new).

This is a good point. We have changed the phrase to:
"We document the first multi-model evaluation of the Arctic sea ice extent (SIE) from these extended RCPs in this paper, though SIE behavior has been documented elsewhere in some individual models (Li et al., 2013; Meehl et al., 2012, 2013; Jahn and Holland, 2013) ."

Page 1385, Line29: I do not believe this statement is true as written: "The CMIP5 archive itself does not constrain the dates of possible sea ice disappearance". What I assume the authors mean, is that it does not constrain the date of sea ice disappearance satisfactorily, but it does gives a range, so it does constrain it. Please rephrase

this statement.

This is a fair argument. We have added 'satisfactorily' to the phrase, so it reads '… does not satisfactorily constrain...'

Page 1388, Line 20: Please add that this is to be expected and why (because why would a multi-model mean be expected to match the single realization observed in the real world?), or remove this statement, as it makes it sound like this finding is surprising, which it is not.

We have separated the sentence into to parts to clarify the intent as a comparison of model performance to observations:

"The observations fall well within the model spread. The trends in observed extent and reanalysis volume are generally more negative than the trends in multi-model means, though the spread in trends of individual ensemble members encompasses the observations (Stroeve et al., 2012, Massonnet et al, 2012)."

Page 1390, Line 14/15: This change between ice-free and "perennial" ice cover isn't worth talking about in my opinion, as it is just a change across an arbitrary line (1 million km2). I would remove this sentence here and in the conclusions, as it makes it sound like as if there is something big and interesting going on, while all it is is interannual variability in sea ice extent (a variable that changes easily by a lot, due to its definition) that happens to cross some not-significant (in nature) threshold. The Arctic is already summer ice-free for all relevant purposes at 1.5 million km2, and calling it "perennially ice covered" when it is slightly above 1 million km2 only leads to confusions (even if correct by the definition of an ice-free state at 1 million km2).

We have changed this sentence to:

"Of the 12 models that reach summer ice-free conditions, five exhibit low frequency oscillations or high interannual variability in September sea ice extent through the period 2100--2300 (see Fig. 3)."

The corresponding sentence was deleted from the conclusions.

Page 1390, Line 16/17: Please add a sentence giving some physical insight into why this is seen, i.e. because as the ice gets thinner and more open water area exists, the wind can change the ice extent more than when the ocean is already covered by close to 100% of sea ice and the ice is thicker. Physical statements like this are somewhat lacking in the manuscript in my opinion and should be added where appropriate to make it stronger.

Point well taken. We have added the following:
"The lower mean thickness of sea ice means that the ice area subject to either complete seasonal melting or survival through the melt season increases. The sea ice extent therefore is more susceptible to interannual variations in both solar radiative (e.g., cloud cover) and advective temperature forcing variations as well as variations in wind-driven convergence."

Page 1392, Line 25: This was also described by Jahn and Holland (2013), in respect to the greenhouse gas forcing, and this agreement with previous work should be mentioned here.

Yes, thank you – the sentence was constructed in part based on the description in your paper and the reference had been accidentally deleted in a previous edit of the manuscript. It has been added again.

Page 1394, Line 17: Please add "summer" before ice-free Arctic, as it clearly refers to that, and it is important to note that it does not refer to Arctic sea ice in general, because the stabilization in the RCP4.5 does not lead to winter ice free conditions in any of the models.

We have added 'seasonally' to this line.

Page 1394, Line 17/18: Please remove this sentence "Five models exhibit oscillatory behavior after 2100 alternating between seasonally ice-free and ice-covered states".

As noted earlier, the Arctic is still basically ice-free (and defiantly not in an "ice-covered state" even at 1.1-1.5 million km2, and someone who just reads the conclusion gets the wrong idea here.

This was removed from the conclusions as noted after previous comment.

Page 1394, Line 25: I don't think the analysis supports the use of the word "rapidly" here, as before it was said that the trends are close to zero until 2100, and that it takes up to 60 years to see a recovery in some models. "Rapid" implies something faster than 60 years in my mind. Please remove "rapid", then this sentence is okay.

'Rapidly' was removed in this instance. Assuming the multi-model mean filters interannual and slightly longer variability among the models and highlights the forced response, the multi-model mean does indeed suggest that the response is nearly immediate. Sixty years encompassed the maximum of the range of times between the forcing reversal and the absolute minimum in sea ice extent, and so either demonstrated the extreme end of internal variability, or the case of a model that was not as sensitive to the decrease in forcing.

Technical corrections
Thank you for suggesting the following corrections – all were followed.

Page 1384; Line 15: I would add a "the" before reversibility here, to make the sentence clearer.

Changed as suggested.

Page 1385, Line 3: Please add "of sea ice loss" after reversibility, and a "the" before reversibility, to make this sentence easier to understand.

Changed as suggested.

Page 1385, Line 15: It should be "slow adjustment of the climate system" (not slower, as there is no comparison made to something that response faster as far as I can tell).
For clarity, this was changed to: '... continue to slowly increase as a result of the adjustment of climate system to the continued energy imbalance ...'

Page 1386, Line 16: Please add "summer" before sea ice here, as that is what is being talked about.

Changed as suggested.

Page 1386, Line28: Please rephrase or split this sentence, it is too long and unclear towards the end. For example, "... into an ice free state. This is true both for annually ice free conditions achieved through radiative forcing (...) and for seasonally ice-free conditions by imposed removal (. . .)".

Changed as suggested. The following sentence was also removed for its repetition: "All coupled atmosphere-ocean modeling studies that we are aware of demonstrate Arctic sea ice recovery upon reversal of the forcing."

Page 1393, Line 2: This sentence needs to be rewritten and shortened, as it is difficult to follow. For example "which follows a future trajectory of increasing followed by decreasing radiative forcing" needs to be "which follows a future trajectory of increasing radiative forcing, followed by decreasing radiative forcing" or something similar. Please rephrase

Changed as suggested.

Figures: The figures are too small in the print version of the discussion paper, and I hope they can be printed larger in the final print-version on the manuscript.

The aspect ratio of the TCD format contributed to this, and it will be taken care of in final publication.

---- END OF RESPONSE