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Interactive comment on "Modeled Arctic sea ice evolution through 2300 in CMIP5 extended RCPs" *by* P. J. Hezel et al.

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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.

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.

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.

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.

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...).

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

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.

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

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

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

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

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.

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

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

l.19: style: replace "dramatic"

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.

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?

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

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.

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

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".

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

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

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

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

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.

Interactive comment on The Cryosphere Discuss., 8, 1383, 2014.