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Interactive comment on "Northern Hemisphere storminess in the Norwegian Earth System Model (NorESM1-M)" *by* E. M. Knudsen and J. E. Walsh

Anonymous Referee #1

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General Comments

This paper gives a brief evaluation of two climate models relative to ERA-Interim, and the future projections with RCP8.5 with respect to sea level pressure, storm-track density and intensity, and mean precipitation.

The authors indicated in the introduction that the focus would be on the impact of sea ice loss and the polar amplification on the storm tracks, but as they also state in their introduction, this is merely implied. This paper has the potential to be very interesting if this aspect is addressed explicitly (e.g. by investigating the relationship between sea ice and storm tracks).

Unfortunately, as it stands, I find the paper lacking in novelty and insight. There are a

few papers published in the last couple of years with more thorough evaluation of storm-tracks in CMIP5 models, and more robust estimates of future changes (e.g. Zappa et al 2013a, 2013b, Chang et al 2012, Colle et al 2013, Mizuta 2012).

Specific Comments

1. The authors state that they are looking at RCP8.5 and RCP4.5, however only results from RCP8.5 are ever shown. One of the conclusions of the study is related to the linearity of the response in the two simulations, so this should really have been included in the paper. This should also be compared to the results of Zappa et al 2013b, who indicate that the larger the forcing, the larger the intermodel spread in responses of the storm tracks. Also, other studies show large differences in the responses when considering different models (e.g. Laine et al 2009) or different forcings (e.g. Catto et al 2011).

2. The precipitation projections are assumed to be due to the storm track changes. The changes shown may indeed be consistent with the changes in the storm tracks, but this is not the same as showing there is a causal relationship. Hawcroft et al 2012, and Catto et al 2012 showed the proportion of precipitation associated with extratropical cyclones and fronts respectively. Only through this type of linkage can a causal relationship be implied. The study of Zappa et al (2013b) does indeed show increases in the precipitation intensity of cyclones in the projections.

3. There is very little discussion or consideration of the reasons for the errors in the models, the differences between the errors, and the differences in the projections. Recent work by Harvey et al (2015) has attempted to investigate the sensitivity to various aspects of the climate system, including changes in sea ice.

4. The changes in mean intensity given here do not necessarily give any information about changes in the winds, which are really one of the aspects of the cyclones that are the most impactful.

5. Showing differences for the model evaluation for all the fields considered (i.e. model minus ERAI) would make it much easier for the reader to see the biases.

6. Page 8977, lines 19-22: Are there references for the statements about the impacts of storms relating to sea-ice loss?

7. Page 8978, lines 10-15: The paper does not really attempt to unravel this issue, so these lines are misleading. 8. The description of the CMIP5 experiment should reference Taylor et al (2012).

9. The introduction of the ERA-Interim states that a 0.5 degree grid is used, but I do not understand where or how. If the highest available resolution of ERA-Interim is 0.75 degrees and the models have lower resolution than that, what need is there to artificially increase the resolution of ERA-Interim?

10. Page 8981, line 12: The table referenced does not show the sea ice extent estimates for the observations as stated here.

11. Section 3.1.1: Most studies using MSLP as any sort of storminess measure would actually use the 2-6 day filtered variance of MSLP, rather than the full field. It would be better to also use this if the storminess is really what is of interest here. It would also be good to show difference plots so that the errors in the models can be seen.

12. Page 8985, last line: Does this mean deep in height, or deep in intensity? This line is unclear.

13. Page 8986, line5: This sentence needs clarification. Do the cyclones vary with temperature, or does the average intensity of the cyclones change with temperature, or is it to do with the temperature gradient?

14. Page 8987, lines 2-3 and 14: The precipitation shown is the average total precipitation. The authors have not shown that this is frontal precipitation, and the similarity of the storm tracks and precipitation field does not give this link explicitly (see e.g. Catto et al 2012).

15. Page 8987, lines 26-: The assumption here is that the models actually produce rainfall for the correct reasons. Catto et al 2013 show that the intensity of frontal precipitation is too low in a GCM and the frequency of precipitation is much too high (a problem common to many models).

16. Section 3.2.4: The discussion on precipitation changes in the Mediterranean and Northeast Atlantic Ocean region could do with some work. The eastern North Atlantic region only becomes significantly drier during September, which only matches the changes in storm tracks in CCSM. In December the changes here are mostly positive, and do not strongly relate to the storm track changes. Could this be related to a change in the precipitation intensity of the cyclones? The Mediterranean region has been shown in other studies to have large decreases in track density in projections (e.g. Zappa et al 2014). It is not clear to which region the expansion of the Hadley cell refers, or the lesser warming seen.

17. Section 4, first bullet point: This first point is not really a finding of the study, it is what was expected. This has not been shown here. Table 3 shows that there are just as large (sometimes larger) changes in the variables investigated in December as September.

18. Section 4, second bullet point: This point is not consistent with the reference to DeWeaver and Bitz (2006) given in section 3.1.1.

19. Section 4, third bullet point: This linear scaling is not shown in the paper, and the point is inconsistent with previous studies. This needs to be discussed.

20. Section 4, fourth bullet point: The link between the diminishing sea ice cover is consistent with the decrease in SLP, but a causal relationship has not been shown. This, and linking the sea ice loss to the storm tracks in some explicit way, would make the paper much more interesting.

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Interactive comment on Geosci. Model Dev. Discuss., 7, 8975, 2014.