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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 #2

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Formal review of manuscript for Atmospheric Science Letters Manuscript identification number: gmdd-7-8975-2014

Title: Northern Hemisphere storminess in the Norwegian Earth System Model (NorESM1-M) Authors: E. M. Knudsen and J. E. Walsh

Recommendation: major revision

General Comments: The authors analyse cyclone activity in the NorESM1-M GCM betwenn September and December and compare it to ERA-Interim and CCSM4 results. Additionally, climate change scenario simulations for the 21st century are analysed. While the paper presents some interesting results, the approach is not always the best and several of the conclusions are in my opinion unjustified or at least highly specu-

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lative. The manuscript will need a major revision before it is in publishable form. The major comments follow the bullet points in the conclusions. I have also added a number of minor comments to help the authors further improve the manuscript.

Major Comments:

The main conclusions in the paper are often highly speculative and/or unjustified. Please see comments regarding bullet points in the conclusions (Pages 8996-8998)

a) Page 8996 lines 7-8: One thing is not necessarily a cause of the other. Please rephrase

b) Page 8996 lines 14-16: This conclusion cannot be reached based on the slim data sample (two GCMs and one run per scenario, see also minor comments). Please delete.

c) Page 8996 lines 17-21: Causality cannot be established in this way. I do not recognise a clear shift of the storm track in the results (see minor comments). Please rephrase.

d) Page 8996 lines 22-26: The patterns of change in Fig. 7 do not show clearly show this. Please rephrase and discuss September and December separately.

e) Page 8997 lines 9-12: Please rephrase first sentence as the methodology is not considering the cyclone related precipitation, but the full precipitation field. Please delete second sentence as it is highly speculative.

f) Page 8997 lines 13-28: Please reformulate based on the above comments and minor comments. Please delete lines 15-20.

Minor Comments:

1: Page 8976 line 6: It is unclear if the CCSM4 results are a “benchmark” or if NorESM1-M and CCSM4 are both validated against ERA-interim. There are non-consistent statements in the manuscript (see e.g. page 8978 lines 17-21 and page

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8979 lines 8-13).

2: Page 8977 lines 25-26. Polar amplication (linked to enhanced temperature over the Arctic at lower levels) decreases LOW LEVEL meridional temperature gradients. However, upper level meridional temperature gradients actually increase (due to upper level temperature increase over the tropics). Other factors also contribute to cyclone intensitfication, e.g. diabatic processe. See Harvey et al (2015) for reasoning on temperature gradients, Barnes and Screen (2015) on arctic amplification, and Ulbrich et al. (2009) for a general review on cyclone activity. Please rephrase.

3: Page 8979 lines 12-13: As the authors are also evaluating CCSM4, it would be important to add more information on this GCM.

4: Page 8980 lines 16-21: I understand the data limitations but a simulation per scenario and GCM is an extremely thin base of evaluation. This is the reason why several of the statements in the conclusions cannot hold.

5: Page 8982 lines 3-9: I wonder if limiting the data to T42 is the best option for high latitude cyclones. Please check Zappa et al. (2014) on polar lows.

6: Page 8983, lines 15-17: I strongly disagree with this statement. There is no reason to believe why the relationship forcing / cyclone change should be linear, and based on only one simulation per run and scenario the conclusion cannot hold. See also Catto et al. (2011) on changes of cyclone activity for high end scenarios, the results are anything but linear . . ..

7: Page 8984, line 3: Misleading. SLP is not a real measure of storminess, but only a very indirect one. Relevant would rather the core pressure of (developing) cyclones. Just looking at SLP is too simple and not adequate as a measure of storminess.

8: Page 8985, lines 15-22: Very misleading please rephrase.

9: Page 8986, line 11: I believe the authors mean rather a bias than a shift . . .

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10: Page 8986, line 20-23: Please provide an adequate discussion based on the literature references included below (not exhaustive).

11: Page 8986, line 25: Misleading. Precipitation is not a real measure of storminess. Depending on the area of the globe, the precipitation is more or less associated with low pressure systems (see e.g. Hawcroft et al. 2013). Therefore, it is not appropriate to evaluate total precipitation. What would make sense would be to evaluate the cyclone related precipitation (see. also Zappa et al., 2013).

12: Page 8987, lines 1-29: A separation on convective, orographic and large-scale precipitation cannot be done “by eye”. The whole section 3.1.4. is very misleading as it is, particularly lines 19-25. Please reformulate.

13: Page 8989, lines 8-11: Please compare with projected changes in jet stream in cmip5 models (e.g. Barnes and Polvani, 2013).

14: Page 8989, lines 13-23: Please check review paper of Feser et al. (2015) on this topic. The issue with the “poleward shift of the storm track” is clearly overstated.

15: Page 8990: I cannot see a “poleward shift of the storm track”, except maybe for the Norwegian model in September over the North Pacific (Fig. 6a). Please rephrase.

16: Page 8991, lines 23-25: Please do not compare trends in reanalysis with climate projections directly, as they must not be necessarily the same.

17: Page 8996, lines 12-13: Please delete this sentence: “Such diferences can be expected to decrease with potential higher resolution in newer model versions”

18: Page 8997, lines 3-4: Please add “locally” or “limited” the phrase.

References:

Barnes and Polvani (2013) Response of the Midlatitude Jets, and of Their Variability, to Increased Greenhouse Gases in the CMIP5 Models. J Clim, 26, 7117–7135. Barnes and Screen (2015) The impact of Arctic warming on the midlatitude jetâĂŘstream:

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Can it? Has it? Will it? WIREs Clim Change doi: 10.1002/wcc.337 Catto et al. (2011) Northern Hemisphere extratropical cyclones in a warming climate in the HiGEM high-resolution climate model. J Clim, 24, 5336-5352. doi: 10.1175/2011JCLI4181.1 Feser et al (2015), Storminess over the North Atlantic and northwestern EuropeâĂŤA review. Q.J.R. Meteorol. Soc., 141, 350–382. doi: 10.1002/qj.2364 Harvey et al (2012) How large are projected 21st century storm track changes? Geophys Res Lett, 39, L18707. doi: 10.1029/2012GL052873 Harvey et al (2015) Deconstructing the climate change response of the Northern Hemisphere wintertime storm tracks. Clim Dynam. doi: 10.1007/s00382-015-2510-8 Hawcroft et al (2012) How much Northern Hemisphere precipitation is associated with extratropical cyclones? Geophys Res Lett, 39, L24809, doi:10.1029/2012GL053866. Ulbrich et al. (2009) Extra-tropical cyclones in the present and future climate: A review. Theor. Appl. Climatol. 96, 117–131, doi:10.1007/s00704-008-0083-8. Zappa et al. (2013) A multimodel assessment of future projections of North Atlantic and European extratropical cyclones in the CMIP5 climate models. Journal of Climate, 26, 5846-5862. doi: 10.1175/JCLI-D-12-00573.1 Zappa et al. (2014) Can polar lows be objectively identified and tracked in the ECMWF operational analysis and the ERA-Interim reanalysis? Monthly Weather Review, 142, 2596-2608. doi: 10.1175/MWR-D-14-00064.1

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