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Earth System Dynamics
Discussions

Interactive comment on "Short Communication: Atmospheric moisture transport, the bridge between ocean evaporation and Arctic ice melting" *by* L. Gimeno et al.

Anonymous Referee #3

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The authors use a Lagrangian particle tracking algorithm to identify the source regions of atmospheric moisture associated with the Arctic river basins in northern Eurasia (as defined by Zhang et al. 2012) and for the Arctic ocean north of Eurasia (as defined by Kapsch et al. 2013). They find that the sources of atmospheric moisture for these regions are located in the Pacific, Atlantic, and the Mediterranean; that there are increasing trends of evaporation in these moisture source regions; and that these increasing trends of evaporation in the source regions correspond to an increase in moisture transport into the regions defined by Zhang et al. (2012) and Kapsch et al. (2013).

I enjoyed reading the article and found it quite interesting and informative. I have enu-

merated a significant number of minor revisions and one major revision that I believe will improve the manuscript and thereby make it suitable for publication in ESD.

Major:

1) The abstract needs major revision. For example: a) The abstract, in its current form, does not present the main results of the study. It presents a great deal of background material and the methods used. However, I do not believe it states in a clear and concise fashion the problem being addressed nor the main conclusions of the paper. The background information should be shortened to make room for the authors to more clearly state the previous results, what is still "disconnecting" in those results, and what the authors find in their study. b) Page 1034 Line 2: The first sentence is confusing: "could affect the global climate in similarly asymmetric way with respect to other regions" c) Page 1034 Line 5: This line seems to suggest that the "atmospheric branch of the hydrological cycle" is the only thing linking the Arctic system and the global climate. d) Page 1034 Line 20: The main results of the paper should be here rather than a listing of mechanisms that contribute to sea-ice loss.

Minor:

2) The first paragraph of the introduction (Page 1034 Line 25 through Page 1035 Line 14) makes a strong case that the "hiatus" does not actually exist. However, to me, this discussion seems somewhat tangential and distracting (due to the controversy surrounding it) to the rest of the paper. I suggest the possibility of using this space to expand the discussion on Arctic climate change and/or possible mechanisms that are contributing to Arctic amplification.

3) Page 1035 Lines 20-26: The authors should discuss the seasonality of sea-ice loss and snow cover extent here, as I believe it is relevant to their study. With respect to SCE, there are clearly strong downward trends during spring and summer, but the trends actually reverse and are positive in fall and early winter (http://climate.rutgers.edu/snowcover/). Considering J. Cohen's identification of in-

creasing Eurasian snow cover build-up during the fall, I think these positive trends have ramifications with respect to enhanced river discharge during the following melt season.

4) Page 1036 Line 15: Baggett and Lee (2015) find the existence of a planetary-scale wave life cycle that is highly amplified (blocking) despite a reduced meridional temperature gradient (consistent with Francis et al. 2012). Furthermore, the life cycle is preceded by enhanced warm pool tropical convection (consistent with the hypothesis presented in Palmer (2014)).

5) Page 1036 Line 26: The confusing sentence from the abstract is more or less repeated here.

6) Page 1037 Lines 5-14: Woods et al. (2013) and Liu and Barnes (2015) may be good references here, as they have done some work with respect to the atmospheric branch of the hydrological cycle. They discuss extreme atmospheric moisture transport into the Arctic through Rossby wave breaking and atmospheric rivers.

7) Page 1037 Line 23: Do the authors mean “these methods” or “Lagrangian techniques”?

8) Page 1038 Lines 15-26: Zhang et al. (2012) makes a nice connection between atmospheric moisture transport and increased river discharge, but, in my opinion, they do not provide a strong link between an increase in discharge and an increase in sea-ice melt. They rely on the 2007 melt season as an example of a high discharge/high melt relationship. To me, the high discharge/high melt relationship is intuitive. However (and it may be outside the scope of this paper), it would be beneficial if there were quantitative evidence to support the relationship.

9) Page 1039 Lines 20-27: The description of the method provided by the authors is sufficient, but I needed to read Stohl et al. (2004) to fully understand. Perhaps the authors should emphasize that Stohl et al. (2004) is a good reference near Page 1040

Line 5-7.

10) Page 1039 Line 19: It is not clear to me what constitutes the 3-D wind field. The authors state that October-March trajectories are calculated to test Zhang et al. (2012)’s results. Is the climatological October-March wind field used or are daily wind fields used to calculate the trajectories? I.e., are backward trajectories calculated for each day during October through March for the entire 1979-2013 dataset (initialization on $\sim$ 6300 individual days)?

11) Page 1040 Lines 9-21: This may be a good area of the paper to discuss the increasing trend in SCE over Eurasia during fall and winter. Although, I am admittedly unsure if an increasing trend in SCE actually corresponds to an increasing trend in snow-liquid-water content found in the Eurasian Arctic river basins.

12) Page 1040 Line 23: The authors use OAFlux data to calculate trends in evaporation over the oceans. The authors use the wind field and q data from ERA-Interim to calculate E-P along the Lagrangian trajectories. Since q is clearly related to evaporation, it would be nice if the authors linked the two datasets to show that they are consistent with each other. I suggest recalculating the trend lines as seen in Figure 1 using ERA-Interim evaporation data to see how well they match the OAFlux results.

13) Page 1041 Line 2: Rather than referring to the panels as “lateral”, I suggest giving the panels letters to designate them clearly.

14) Page 1041 Line 4: How is the blue contour chosen? It appears that it is chosen via a specific contour line for each basin, but it also appears the value of the contour line is different for each basin. Also, there is no blue line for the Mediterranean. Is the whole Mediterranean basin used? If so, the blue line in that basin would not follow a particular contour. Is the OAFlux trend sensitive to the choice of the blue contour?

15) Page 1041 Line 13: Which months are used for spring trajectories? Also, referring to my question (10), are trajectories calculated for every spring day?

16) Page 1041 Lines 13-21: The discussion concerning the results of Kapsch et al. (2013) seem quite rushed (although the authors state the figure is similar, it may still be nice to show it). The net result of the short discussion and lack of figure is to seemingly emphasize the results of Zhang et al. 2012 when perhaps that is not the intention of the authors.

17) Page 1042 Lines 1-5: These lines should be incorporated in some fashion into the abstract.

18) Page 1045 Caption: a) Rather than say “reddish colours” maybe use “warm colours” or “contours only show positive moisture sources” b) What are the units for the green shading? c) The period 2000-2010 does not appear to be the highest period of evaporation for all source regions. For example, it appears the Pacific source regions peaked between 1995-2005. Perhaps the authors are just being consistent by using the same period for all three sources, but, regardless, I think something needs to be corrected here.

Baggett, C., and S. Lee, 2015: Arctic warming induced by tropically forced tapping of available potential energy by planetary scale waves. J. Atmos. Sci., 72, 1562-1568.

Liu, C., and E. A. Barnes, 2015: Extreme moisture transport into the Arctic linked to Rossby wave breaking. J. Geophys. Res., 120, 3774–3788, doi: 10.1002/2014JD022796.

Woods, C., R. Caballero, and G. Svensson, 2013: Large-scale circulation associated with moisture intrusions into the Arctic during winter, Geophys. Res. Lett., 40, 4717-4721.

Interactive comment on Earth Syst. Dynam. Discuss., 6, 1033, 2015.