

## ***Interactive comment on “The perfect pattern of moisture transport for precipitation for Arctic sea ice melting” by Luis Gimeno-Sotelo et al.***

**Anonymous Referee #1**

Received and published: 24 January 2018

“The perfect pattern of moisture transport for precipitation for Arctic sea ice melting” (by Gimeno-Sotelo et al.) studies the changes in the patterns of moisture transport before and after 2003, identified as the main change point in Arctic sea ice extension series. Moisture transport decreases in summer and is enhanced in autumn and early winter. These results are shown to be consistent with other approximations to the problem and provide a reasonable explanation about the observed reduction in sea ice extension.

I find this paper to be an interesting addition to previous knowledge about mechanisms related to Arctic sea ice reduction in the last decades. But, before accepting this paper for publication, some new explanations and discussions have to be added and some figures and tables should be corrected or improved.

Before any comments about the text or the figures, I have to say that I do not feel very

C1

comfortable with the title of the paper. In the paper, it is shown that there has been a modification in seasonal transport of moisture toward the Arctic and in the prevailing circulation patterns in the Arctic and that those new conditions lead to a reduction in sea ice extension. But I do not understand why the new pattern is considered as ‘perfect’. Is the new pattern the one that maximizes the reduction in sea ice extension (if that is what is called ‘perfect’)? I would suggest to change the title.

Some methodological and conceptual issues:

Section 2.2.3: ‘...To compute moisture transport for precipitation (MTP) from each source to each sink for the AO, the trajectories of particles from the moisture sources for the Arctic (AR) were followed forward in time from every source region detected by Vazquez et al. (2016) (figure 1c).’ I find that further discussion is needed about this sentence (and paragraph) and what it implies. It is difficult for me to understand why those (and only those) particles are tracked. In fact, my interpretation is that source regions are not source regions anymore since authors follow 10 days into the future all the particles within those regions, had them gained water vapor within those source regions or not. Thus, water does not necessarily come from those regions and they stop being ‘source regions’. In addition, not all the precipitated water comes from those ‘sources’, so, what happens with other particles that produce precipitation but were not within those ‘source regions’ ten days before precipitation? Another question, are there enough ‘particles’ to properly characterize what happens with the smallest sub-regions defined in figure 1b (I am thinking in the results presented in figure 8)? Section 2.2.3 (should be 2.2.4): ‘circulation types’ are identified for four sections selected ‘according to the positions of the major sources of moisture’. What are the sizes of those sections? Is there a minimum recommendable size? Is the method used to identify ‘circulation types’ sensible to the area selected? Are your results robust if you modify (not much) those sections? Some of those sections share some common areas, does it affect to the latter interpretation of the circulation types? In addition, it would be advisable to identify those four sections in figure 1c.

C2

Results:

Section 3.2: Authors state that results in figure 6 suggest that 2003 is the most appropriate CP year. I do not find it so obvious. DS and MS series (figure 6) suggest that 2004 would be a better selection. Have authors tested if selecting one year or the other produces any difference? And, when writing about CPs, some explanation about change points identified using BinSeg and PELT should be provided. It is not clear in the text if more than one CP has been identified using those methods nor the implications that the existence of more than one CP in the SIE series would have in the interpretation of the results of this paper.

Some additional comments and typos:

P7, paragraph describing figure 7: It is not explained anywhere that figure 7 includes the differences between mean values of MTP until 2003 and mean values after 2003 for every source region (this is my interpretation of what is represented in figure 7). The caption of figure 7 doesn't include this information either. Same comments can be applied to figure 8.

I would suggest to re-plot figures 7 and 8 in order to include the information from the table in figure 7 and from table 1. It would be as easy as to plot with a thick (or filled) bar those differences that are statistically significant and with a thinner (empty) bar those differences that are not. In addition, plotting with a thicker line the horizontal bar indicating the 0 mm/day level would help to notice which sources increase/decrease their MTP contribution. Finally, no information about the statistical significance of the differences in total MTP is provided anywhere (again, it could be indicated by using a continuous or discontinuous line)

It would be easier to follow the comments in the text if Figure S1 and figure 9 included some labeled meridians (or at least, some longitudes in the outer area of the maps).

P213: 'ong these effects. . .' I guess it should be 'Among these effects. . .'

C3

P3114 (p3116): 'vs' -> '-'

P3126: delete 'uses' or 'employs'

P5112: '...using an applied methodology. . .' -> '...using a methodology. . .'

P617: delete '(Greenland)' in '...to fall and winter (Greenland)'

P618: '...with the a dominating closest source' -> '...with the closest source dominating'

P6115: 'represent' -> 'represents'

P6121: lesser

P7120: '...MTP could not be homogeneous...' -> '...MTP could be non-homogeneous...'

P9129: Again, I do not know why this is a 'perfect pattern of MTP for Arctic sea ice melting'. It is shown that it favors the melting, but. . .

P1012: 'it is clear beyond doubt. . .' Is it?

Table S2: Are changes in the frequency of each class statistically significant?

---

Interactive comment on Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2017-122>, 2017.

C4