Comments to 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"

Before any comments about the text or the figures, I have to say that I do not feel very comfortable with the title of the paper. I would suggest to change the title.

The reviewer is right. We´ll change the title to something less confusing such as "The pattern of long-term changes in the moisture transport for precipitation with Arctic sea ice melting"

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'.

The reviewer is right. This is one of the limitations of the approach when analyzing contribution of remote sources. Particles can gain moisture in the regions placed between the defined moisture source and the target region, even in the target region. We have used extensively the same approach in many papers (see Gimeno et al, 2010 or Gimeno et al, 2013 as examples) and not always put in evidence this limitation in the text. However as our defined moisture regions were identified as the ***major*** *moisture sources in the backward analysis (Vazquez et al, 2016) the contribution of the intermediate regions is much lower. We include figure 2 from Vazquez et al. 2006) that shows that intermediate regions are not net sources (particles reaching the Arctic region lost (not gained) moisture in these regions.*

In any case we will include this limitation in the text of the revised version of the manuscript.

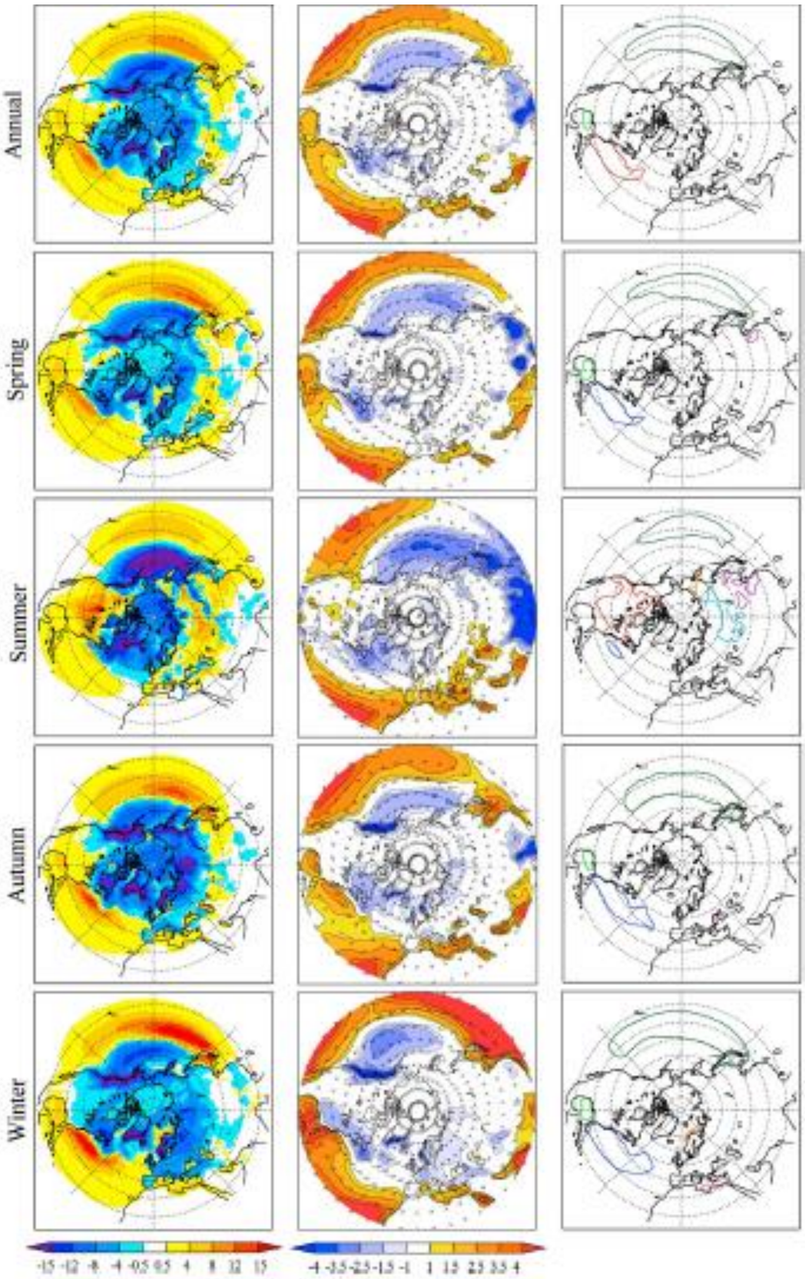

Figure 2. (left column) Climatological annual and seasonal 10 day integrated ($E - P$) values observed for the period 1979–2012, for all the particles bound for the Arctic domain, determined from backward tracking. Red (blue) colors represent moisture sources (sinks). Units are in mm d^{-1}. (middle column) Climatological annual and seasonal vertically integrated moisture flux values (vectors; measured in kg m^{-1} s^{-1}) and respective divergence (shade; measured in mm d^{-1}). Data are from ERA-Interim. (right column) Annual and seasonal moisture sources delimited only for those values of 10 day integrated ($E - P$) greater than 0.4 mm/d. Each contour color represents one source: the dark blue line represents the Atlantic source, light blue the Siberian one, dark green the Pacific, light green the Gulf of Mexico, dark pink the Black Sea, light pink the Caspian Sea, light orange the Eastern Russia source, dark orange the Norwegian and Barents Seas, purple the China source, red the North American source, and garnet the Mediterranean source.

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?

We don´t estimate precipitation in the Arctic but contribution of the major sources providing moisture for precipitation. Of course the rest of the particles are responsible for the rest of precipitation

We´ll include a comment on it in the revised version of the manuscript

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)?

The size of the target regions are bigger than many of the regions where the same methodology was used in previous studies. We calculated the average number of particle by source that reach daily the target regions (table below). The number is big enough.

	Central Arctic	*CCA*	*Beaufort*	*Chuckchi*	*E. Siberian*	*Laptev*	*Kara*	*Barents*	*E. Greenland*	*Baffin Bay*	*Hudson Bay*	*St. Law*	*Bering Sea*	*Sea of Okhotsk*
ATL	*4716*	*421,*	*543,*	*572,*	*1102*	*1300*	*2733*	*9256*	*36900*	*40613*	*2961*	*17441*	*1829*	*4157*
PAC	*11059*	*4662*	*11023*	*10358*	*9275*	*2938*	*1698*	*3902*	*17773*	*32785*	*18675*	*15147*	*60348*	*33112*
NA	*21129*	*10426*	*9249*	*3415*	*3421*	*3510*	*4706*	*15336*	*69134*	*141067*	*59233*	*58688*	*6659*	*4794*
SIB	*22220*	*2829*	*9831*	*12078*	*21880*	*17030*	*16358*	*17235*	*7211*	*6184*	*4303*	*1090*	*41223*	*79120*

We´ll write a sentence in the revised paper to address this comment and the table will be included in the supplementary material

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 (nut 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.

The size of the sections was 70 ºlatitude x 90ºlongitude. The analysis of changes in circulation types is complementary to the Lagrangian approach to check the coherence of the results. It is obvious that changes in the size of the sections can vary lightly the circulation types (Huth et al., 2008) but probably results of changes in the new types after/before the change point continue to be coherent with Lagrangian approach. We include in this comment a sample of this for the Atlantic section in fall by moving the domain 10º eastward and westward and by extending the domain 10º eastward (similar results, the patterns are very coherent)

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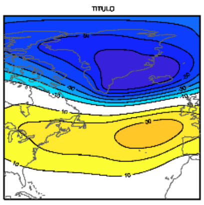
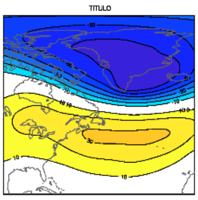
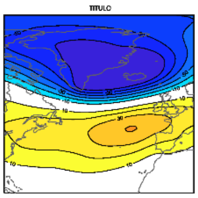
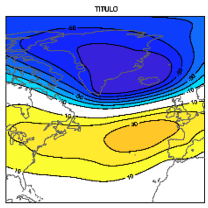
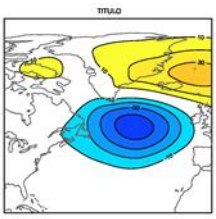
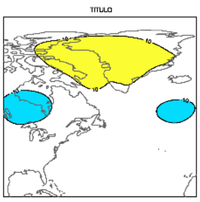
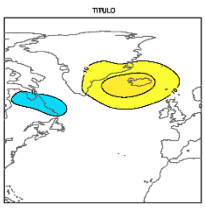
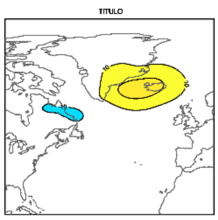
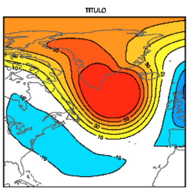
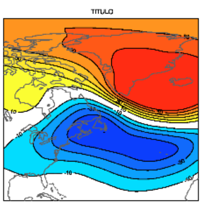
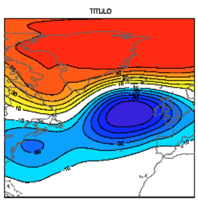
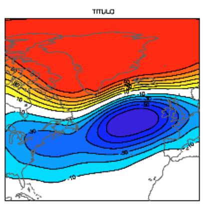
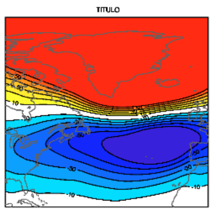
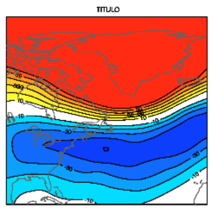
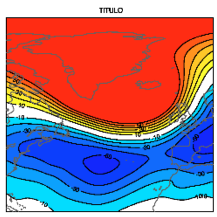
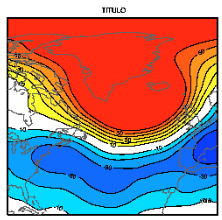
CTC1				
CTC2				
CTC3				

CTC4				

In any case we have taken a domain higher than the used by Fettweiss et al. (2011) (the higher they used was 30ºx30º), who showed no significant differences in the circulation types for 4 different domain sizes. The use of a regional domain centered in the moisture source is justified to account for regional modes instead of annular ones which could not catch details in regional circulation.

We´ll include a discussion on this in the revised version and will change figure 1c as requested

Huth, R. et al. (2008): Classifications of Atmospheric Circulation Patterns – Recent Advances and Applications. Annals of the New York Academy of Sciences: Trends and Directions in Climate Research, 1146, p. 105–152

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.

We include the same analysis for changing 2003 by 2004. As you can see in the figure R2 (the equivalent to figure 7 in the text) results are quite similar. A comment on this will be included in the revised version of the manuscript

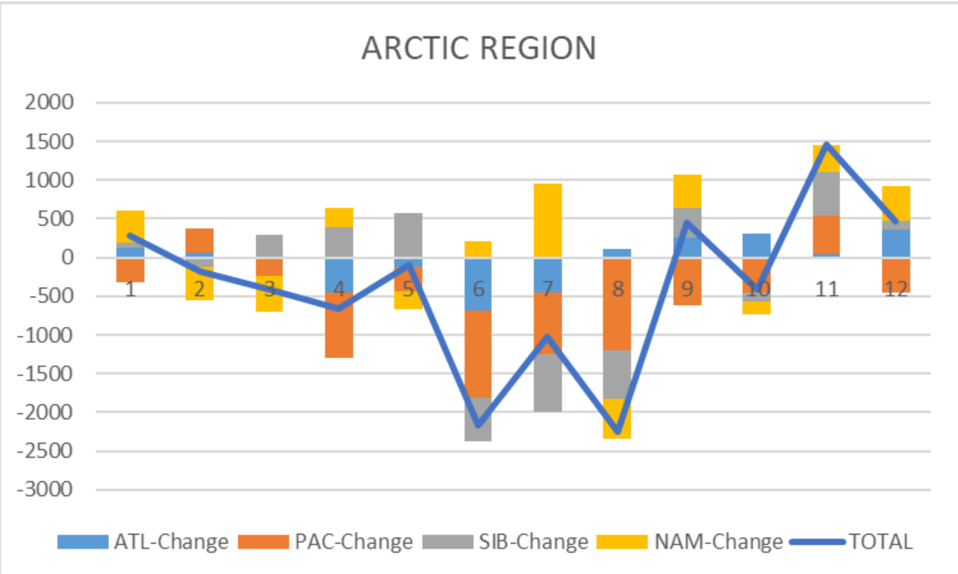


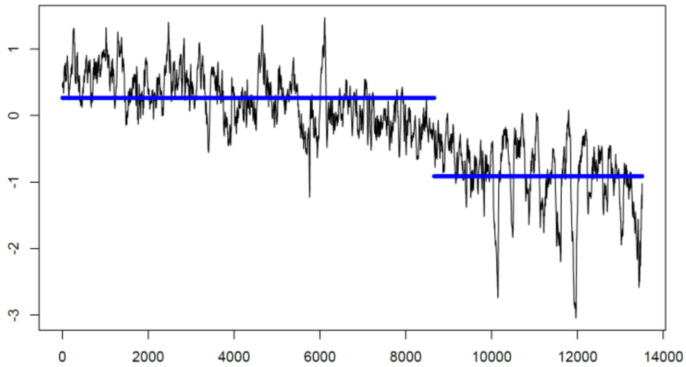
Figure R2 As Figure 7 in the manuscript but with 2004 as change point

In the description of figure 7 we commented the coincidence of change points found by AMOC (only one in the series) with any of the change points found by BinSeg and PELT (multiple change points). These two last approaches identify multiple change points (see the plots for the ADS series as an example of the series with more multiple changes or the plot for the 21 September DS series as an example of only one change in the three approaches, what is the most frequent case in DS and MS series). The idea of the paper is to identify the main change point to compare two periods (one with low ASI and the other with low). It is possible that any of the multiple subperiods identified by the other approaches merits analysis but it is out of the scope of this paper.

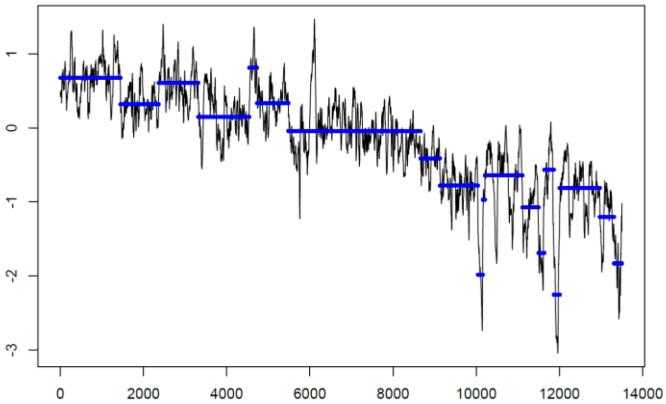
In any case we will include a comment in the revised version to suggest this for future work

<u>*ADS series*</u>

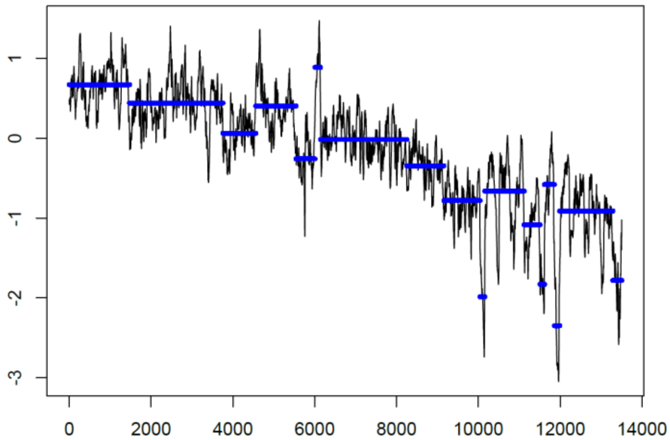
AMOC



BinSeg

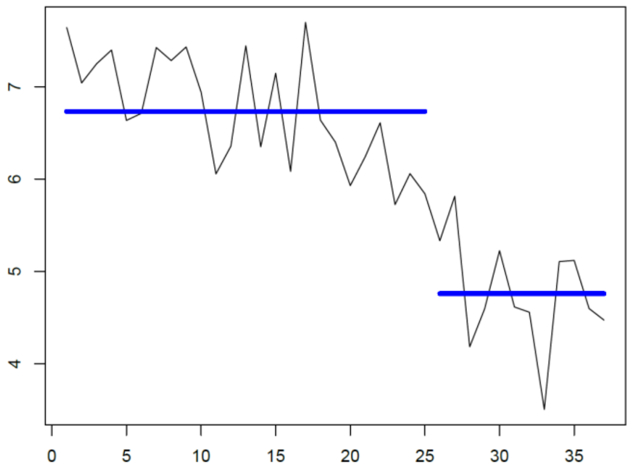


Pelt

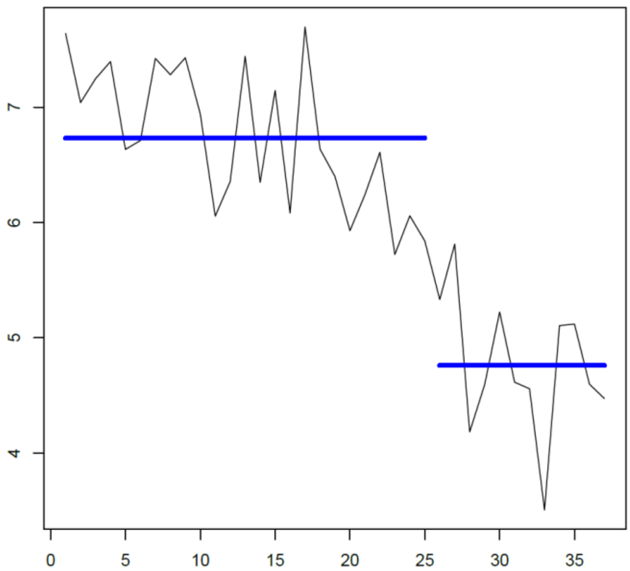


<u>*21 September DS series*</u>

AMOC



BinSeg



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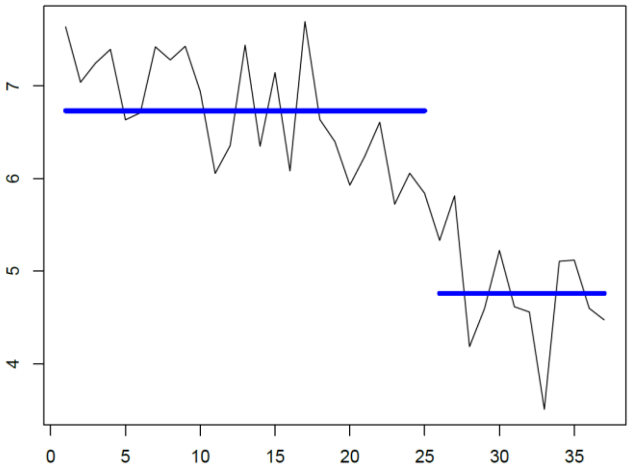

Some additional comments and typos: P7, paragraph describing figure 7: It is not explained anywhere that figure 7 includes the differences between mean values of MTP until 2003 and mean values after 2003 for every source region (this is my interpretation of what is represented in figure 7). The caption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

We´ll do in the revised version of the manuscript

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.

We´ll do in the revised version of the manuscript for Figure 7, not possible for figure 8 (we keep the significativity table) because of the small size of the component figures

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)

Indicated with a red asterisk in the new figure 7

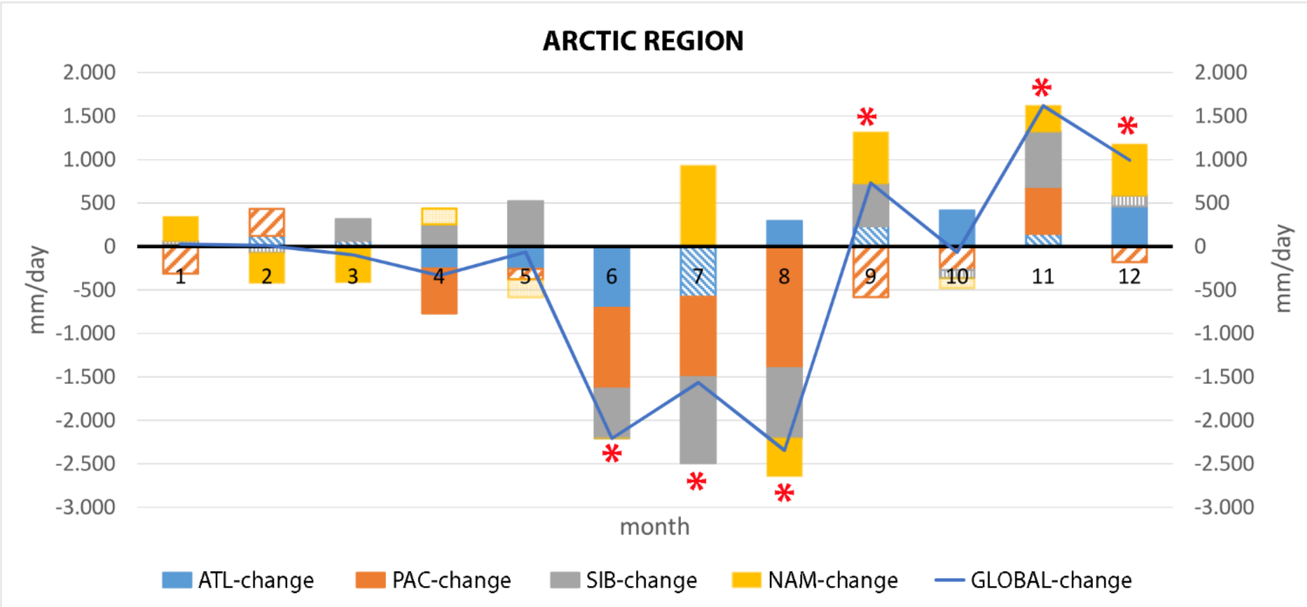

Figure 7. Differences between mean values of Moisture transport for precipitation (MTP) until 2003 and mean values after 2003 for every source region. Filled bars show those differences that are statistically significant at the 95% confidence level for decreases after the CP. Statistical significance of the differences in total MTP (sum of the four sources) is displayed with a red cross

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).

We´ll do in the revised version of the manuscript

TYPOS

We´ll correct the typos and minor changes in the revised version of the manuscript

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

We have used a z-test to compare two sample proportions (Sprinthall, 2011). Statistical significant changes has been now included in Table S2 using asterisks

- *Sprinthall, R. C. (2011). Basic Statistical Analysis (9th ed.). Pearson Education. ISBN 978-0-205-05217-2.*