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Interactive comment on "Trends and regime shifts in climatic conditions and river runoff in Estonia during 1951–2015" *by* Jaak Jaagus et al.

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We thank both anonymous reviewers very much for their comments, suggestions and recommendations. We agree with them and we tried to improve our manuscript following their instructions. Here we answer to the questions and respond to every critical comment. We try to explain our preferences and improvements of the text.We also add the Word file of the improved manuscript where all our modifications are indicated in the Track Changes regime. The only change that we could not do up to now is the improving of Figure 1. It will be done late when Kiira Mõisja will return from her field works.

REVIEWER 1 General comments The authors have published several papers on the

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Discussion paper

same topic: Jaagus, J. 2006. Climatic changes in Estonia during the second half of the 20th century in relationship with changes in large-scale atmospheric circulation. TAC, 83, 77–88 Sepp, Mait, 2010. On regime shift in the general atmospheric circulation over the Baltic Sea region in winter. International BALTEX Secretariat Publication, 46, 46-47 Sepp, Mait, 2016. On Regime Shift in the General Atmospheric Circulation over the Baltic Sea Region. COST Action 733: Harmonization and Application of Weather Type Classifications for European Regions ; Final Scientific Report, 221ôĂĂĂ228. Jaagus, J., Briede, A., Rimkus, E., Sepp, M. 2016. Changes in precipitation regime in the Baltic countries in 1966–2015. TAC, DOI: 10.1007/s00704-016-1990-8. What is new in the present paper? 1. 15 years and 2 meteorological stations were added, but this did not change the conclusions drawn on temperature, precipitation and snow cover trends in (Jaagus 2006) 2. Trend analysis was complemented with regime shift analysis, but the conclusion that an abrupt change in the circulation over the Baltic Sea has taken place at the end of the 1980s only repeats the existing knowledge (Sepp 2010, Sepp 2016, Lehmann 2011, Soomere and Räämet 2014, Soomere et al 2015, etc) 3. The analysis of river runoff was added and this is new. Therefore, the authors should make clear difference between the well-known facts, their own earlier conclusions and new information.

Response. Many thanks to the reviewer for these detail critical comments. It is true that we should stress much more to the novelty of our results. We should compare our results with the previous ones and leave these parts, which are more or less repetitions. We modified the text in the sections of introduction, results and discussions. Our more detail response is the following. In the previous article (Jaagus, 2006) trend analysis for mean air temperature, precipitation and snow cover duration in Estonia was realised for the period 1951-2000. Results of trend analysis depend very much on the time frame, i.e. they are different for different periods. Therefore, it was important to look how much the results of trend analysis will change if we add data from 15 updated years. Continuous monitoring of climatic changes gives us valuable information about possible changes in the future. In the section of discussion we tried to put forward

similarities and differences between the results of trend analysis between these two periods. In the paper on precipitation (Jaagus et al., 2016) there is a comparison of results of trend and regime shift analyses in three Baltic countries. But the period was shorter (1966-2015) than in the current article (1951-2015). Of course, here is an overlapping of results and partly a repetition. The main idea and novelty of our study was to make an integrated analysis of trends and regime shifts in time series of many inter-related and indicative variables of the climate system starting from the indices of the large-scale atmospheric circulation and ending with specific runoff of rivers. Therefore, we did not leave out the precipitation data from our study. We tried to explain all these aspects in the improved version of our manuscript. Concerning the analysis of regime shifts we think that we have found new results and have not only repeated the existing knowledge. Mait Sepp (2010; 2016) has analysed time series of the frequency of different circulation types according to the classifications of large-scale atmospheric circulation, which are selected in the COST Action 733. He has not analysed the circulation indices, which were analysed in this study. The famous article by Lehmann et al. (2011) used a wide range of data sources for the analysis of climate variability in the whole Baltic Sea region. They used mostly reanalysis data and regression, correlation, wavelet and cluster analyses. The detection of regime shifts was not the main task. They discussed shifts in the locations of the NAO centres of action but they also demonstrated that since 1987 the winter season (DJFM) of the Baltic Sea area has tended to be warmer, with less ice coverage and warmer SST, especially pronounced in the northern parts of the Baltic Sea. We would like to emphasise that when Lehmann et al. (2011) analysed climate variability in the regional scale then we have analysed trends and regime shifts in local scale, i.e. in Estonia. We showed that the most significant regime shift in Estonia has occurred exactly since the winter 1988/1989. We are very sorry that we have not referred the publications by Soomere and Räämet (2014) and by Soomere et al. (2015). The first one analysed wave properties of the Baltic Sea, which were simulated using the WAM model. They were related to annual mean components of air flow of the adjusted geostrophic wind.

The second article also deals with wave and wind climatology of the Baltic Sea but it uses observation data at 8 stations in the eastern coast. A shift in simultaneity in annual mean wave length between the stations was detected since 1988. It was related to the observed shifts in the annual mean zonal and meridional components of air-flow of the adjusted geostrophic wind. In conclusion, we can state that the three mentioned articles used completely different data sources, study areas, variables and methods for the analysis of regime shifts in comparison with our research, but they resulted in a similar result pointing out a significant regime shift in the end of the 1980s. In this sense, the main result of our paper is really a repetition of the results of the previous studies. We think that it is important to emphasise that this shift was a big change in the whole climate system in this region. We hope that our article could contribute into this study field. We tried to improve our manuscript in this sense in the introduction and discussion.

References: Lehmann A., Getzlaff K., and Harlaß J. 2011. Detailed assessment of climate variability in the Baltic Sea area for the period 1958 to 2009. Clim. Res., 46, 185–196. Soomere, T. and Räämet, A. 2014. Decadal changes in the Baltic Sea wave heights. J. Mar. Syst., 129, 86–95. Soomere, T., Bishop, S.R., Viška, M., Räämet, A. 2015. An abrupt change in winds that may radically affect the coasts and deep sections of the Baltic Sea. Clim. Res., 62, 163–171

Response. These articles are referred in the improved version of the manuscript.

Specific comments An interesting part of the paper is regime shift analysis of large-scale circulation indices. Table 2 presents only trends and regime shifts are described very shortly in chapter 3.1. Figure 2 presents the time series of only one index. What about the others? What about return shifts?

Response. We agree that results of regime shifts in circulation indices are not well presented. Now, they are described more in details in the sections of results and discussion. We also added Table 3 and two figures (Figures 2b and 2c). Regime shifts

appeared, first of all, in case of winter NAO indices. Shifts for the other indices were quite rare and random. Return shifts were detected for the NAO indices in February since 2004 (Figure 2b).

Table 3 shows trend and shift values for the whole Estonia, but text in chapters 3.2, 3.3 and 3.4 describes regional differences. This is not acceptable. The spatial distribution of these values should be presented in a more convincing way. E.g., there is a sentence "Generally, the trends and regime shifts at the coastal stations in western Estonia are weaker than at the inland stations of eastern Estonia". There are no numbers to prove this. Therefore, this sounds like belief, not knowledge.

Response. This result is presented more exactly in the section of results. In fact, we obtained a huge number of numeric results that is difficult to present in the article. We made a choice to present general results for the whole Estonia in tables, gave some typical time series on figures and described spatial differences within Estonia in the text. We added into the improved version the following sentences. For example, the changes by trend of annual, winter and spring temperature during the 65 years were in coastal stations – Vilsandi 2.2, 2.4 and 3.0, Ristna 2.0, 2.1 and 2.5, Tallinn 1.9. 2.4. 3.2 – and in inland stations of eastern Estonia – Võru 2.4, 2.4 and 3.4, Tartu 2.5, 2.9 and 3.7, Tiirikoja 2.3, 3.1 and 3.7 K, correspondingly. The values of regime shift in winter mean temperature since 1989 were the following; Vilsandi 1.9, Ristna 1.9, Tallinn 2.2, Võru 2.4, Tartu 2.7, Tiirikoja 2.7 K. We do not agree with the assumption that this is a belief. This sentence mentioned by the reviewer is a generalisation of results confirmed by the concrete results of trend and regime shift analyses.

Trend values and regime shifts for precipitation shown in Table 3 do not coincide with the data in Tables 1 and 2 shown in (Jaagus et al 2016). This discrepancy should be clarified.

Response. The discrepancies were caused by different periods for the analysis: 1951-2015 in our case and 1966-2015 in case of the mentioned article (Jaagus et al. 2016).

Figures 3, 5, 6, 7 and 8 contain nearly no information, because they are drawn for some selected sites and simply illustrate how trends can be replaced by regime shifts. There is nothing new in comparison of these two methods.

Response. These figures are really illustrations of the text. I do not imagine how we can describe trends and regime shifts in various time series without showing their graphical form. We tried to select the most typical examples. For example, data from the Tartu and Türi stations are more or less representative for describing climate variability in the whole continental part of Estonia. In the improved version we tried to change figures more informative and general. We merged Figures 3 and 5 showing the variability, trends and regime shifts in winter temperature and snow cover duration together on one figure. We draw Figure 6 showing coherent regime shifts in winter specific runoff in the majority of station.

Figure 4 is drawn for only one station and only one index, therefore, its informative value is low. Besides, positive correlation between NAO indices and temperature in winter is a trivial fact.

Response. We omitted the figure.

REVIEWER 2 General comments In general the article is well prepared and worth to publish with minor revision. Most of introduction is devoted to regime shift, while not enough attention was paid to previous investigations on climate parameters and especially on river runoff change.

Response. The analysis of regime shifts is quite new method in climatology and, therefore, it is more widely described in the introduction. Here we described nearly all investigations on climate changes in Estonia and referred also studies in the wider context (BACC, 2008; 2015). We added some other publications. But, unfortunately, studies on climate changes in river runoff in Estonia are only few. All they are referred. Authors should to reveal novelty of their study too, i. e. to show if their study differs from the previously conducted in Estonia. This question is answered more exactly at the

same comment made by the reviewer 1. We improved the sections of introduction and discussion.

I disagree that correlation coefficients can be provided in the article without evaluation of statistical significance. You can easily do that.

Response. The significance of the correlation coefficients is now evaluated. We added the following text into the section of data and methods: "As the length of the time series in this case is 65 years then the critical value of statistical significance on $p<0.05$ level is $r = 0.245$ and on 0.01 level $r = 0.318$. All correlation coefficients presented in this study are statistically significant at least on the $p<0.05$ level".

The reasons why "after numerous tests" certain set of parameters for regime shift detection was chosen should be described more clearly.

Response. We added the following text. As in case of many statistical methods, the STARS method has no strict rules for the selection of input parameters. The choice depends on the purpose of the study. For example, if there is a need to detect short-term variations in the time series then loose parameters – high HWP, short l and low significance. Our task was to study general changes in atmospheric circulation, climatic and hydrological variables. Therefore we used a relatively conservative set of input parameters . . .

In some cases, very short periods (e. g. <10 years (line 234) or 6 years (line213)) were described as regime shift. It is not clear what is the difference between short-term fluctuations and regime shift.

Response. The text in the section of data and methods is improved and additional explanation is provided. We added the following text. One of the peculiarities of the STARS method is that it tends to find regime shifts at the rear years of time series. This characteristic of the method has been considered as an advantage that allows "to process data in real time, signalling the emergence of a potential shift and measuring

changing confidence in the evidence for a shift as new data arrive" (Rodionov and Overland, 2005). Also, at the formal point of view, those RSI shifts are statistically reliable. However, it is understandable that in reality those shifts in rear years are meaningless and do not represent any substantial changes. Thus we have ignored shifts that occurred in last 5 years of the analysed time series. Short-term fluctuations are typical for climatic time series. Regime shifts are more stable in time. But it is really difficult to define a clear difference between them.

The term "trend" can be used only in case of statistically significant changes. Otherwise increase, decrease, tendency or other terms should be used.

Response. We agree. We used this term only in these cases. We checked the text and made some corrections.

The purpose of section 3.1 remained unclear for me. There is a lot of different circulation indexes presented as well as a lot of regime shifts were found. However, it's difficult to understand significance of all this findings. For example, what mean negative trends in POL index values and how that relates to climate conditions in investigated area? I would suggest to remove this paragraph, while results of atmospheric circulation regime shift analysis can be used in other part of the text for explanation of tendencies of climate and runoff parameters.

Response. We added a sentence explaining the purpose of the section 3.1: As large-scale atmospheric circulation has close relationships with air temperature and precipitation in Estonia (Jaagus, 2006), time series of its parameters were analysed as the main factors inducing climate variability. The Reviewer 1 thought that "An interesting part of the paper is regime shift analysis of large-scale circulation indices". He asked more detail description of these results. Therefore, we do not want to remove the section 3.1 but we presented more detail analysis in the text.

Specific comments Page 1. Line 20-22. It is not clear for me if authors analyzed correlation between AO and NAO, while I didn't find such information in the following

text. Moreover, what is a purpose of such correlation? From my point of view, it is not related with tasks of research.

Response. We apologise for the incorrect wording. We did not analyse correlation between AO and NAO. We wanted to say the correlations between the NAO and AO indices (from one side) and the studied variables (from another side) were 0.5–0.8. We corrected the sentence accordingly: “Correlation coefficients between the circulation indices reflecting the intensity of westerlies and the studied variables were 0.5–0.8.

Page 4. Lines 106-110. I suppose, that such detailed information about relocation of two stations isn’t important. Only statement about data homogeneity can be left in the text.

Response. We omitted two sentences with detailed information.

Page 7. Line 221-223. Results presented in this part of text don’t fully meet data in Table 3 (e.g. up to 5 mm in monthly precipitation). Moreover monthly data about changes in stations aren’t presented in mentioned table.

Response. The text is modified significantly.

Page 10. Line 292. You mention that you detected regime shifts in NAO and AO indices, however in introduction you mentioned that such shifts were already discovered in other research.

Response. In the introduction, there were referred studies where regime shifts were detected in many parameters but not in NAO and AO indices.

Page 11. Line 321 and 337. Two statements about annual river runoff contradict each other.

Response. Unfortunately, there was a mistake in the first sentence. We replaced it with the following sentence: “Annual mean runoff has a statistically significant trend and regime shifts only in some stations”.

Page 11. Line 326-327. In this paragraph you talk about already observed changes. So you don't need to wait the end the 21st century and consequently the last sentence of this paragraph have no sense.

Response. The last sentence of this paragraph was omitted.

Page 11. Line 338-339. What are the specific features of the last two distinguished groups of rivers?

Response. We added the following text: The first one can be characterised by positive regime shifts in January and March and negative shift in April since the end of the 1980s. They have also significant negative trends in April and May. Rivers of southern Estonia have a general increase in specific runoff in January, February, March and June with positive shifts in the same months (except June) and negative shifts in April.

Page 12. Line 352. Do you think that March is winter month?

Response. In hydrology, March is an end of winter and the beginning of spring in Estonia. Due to the observed changes March has become more like a spring month with runoff maximum due to snowmelt.

Figure 1 should be improved with additional regional map where location of investigated area can be seen in more general context.

Response. We'll improve the map and add a small map with the regional location of Estonia.

Technical corrections Page 1. Line 19-20. The main idea of the sentence "All meteorological: : :" should be expressed more clearly.

Response. We replaced "All meteorological and hydrological variables" with "Air temperature, precipitation, snow cover duration and specific runoff of rivers".

Page 4. Line 106. I would propose to use "station location" instead of "the measuring sites of the stations".

Response. We agree and made this change.

Page 4. Line 112-113. The sentence “The wetting: : :” is difficult to understand.

Response. There was a term “wetting correction”. We rephrased the sentence writing “the correction for wetting of the walls of the gauge”. This correction was used all over the former USSR. We added the reference on Groisman et al. (1991).

Page 4. Line 95 and line 221. What mean “increasing trend”. Is it gradual change in trend values? I suppose that you should use the term “positive trend” or “significant increase of in river runoff” instead of.

Response. We replaced this term in the both cases.

Page 7. Line 195. The expression “had a jump by” isn’t usually used in English language.

Response. We replaced “jump” with “upward shift”.

Table 4. It isn’t clear if the regime shifts are upward or downward. Not always shifts sign correspond to general tendency.

Response. May-be, the presentation of the results in Table 4, now Table 5, is not the best. There have been up to three statistically significant regime shifts for some time series. Shift years in one column are related to shift values in the last column. For example, there were two shifts in the Tartu station: in 1964 there was a downward shift by 2.10 l/s per km2 and an upward shift in 1978 by 2.85 l/s per km2. We added the unit (l/s per km2) into the heading of the table.

Please also note the supplement to this comment:
http://www.earth-syst-dynam-discuss.net/esd-2017-24/esd-2017-24-AC1-supplement.pdf

Interactive comment on Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2017-24,

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