

The paper is focused much better now, but there are still serious problems.

RESPONSE OF THE AUTHORS

I definitely refuse to accept the sentence in the response of the authors: „Continuous monitoring of climatic changes gives us valuable information about possible changes in the future.“ It is absolutely impossible to predict anything in the future on the basis of the trends calculated from the data in the past. Fortunately, I did not find this sentence in the text.

COMMENTS ON THE PAPER

I do not agree with the authors that showing trends and regime shifts in their graphical form is necessary. I have seen such drawings for years. These pictures only demonstrate the differences in the two methods and have no substantial value.

The authors have added Table 3 that gives a comprehensive overview on the regime shifts in the time series of large-scale circulation indices. Therefore, Figure 2 is superfluous, as it does not contain additional information in comparison with Tables 2 and 3.

I strongly recommend to add a table showing trends and regime shifts for snow cover. Then also Figures 3 and 4 could be removed. The more that the principles of choosing the examples to demonstrate trends and shifts are not clear.

Figure 3 shows temperature and snow cover time series in Tartu. The text says: “The highest (temperature) shift was recorded in Tartu.” There is a contradiction between the sentence in the response: „We tried to select the most typical examples.“

Even worse is the situation for Figure 4 that shows an upward shift in precipitation at Türi in winter. According to Table 4, it is not typical for the whole Estonia. Thus, presenting something that is exceptional looks like an artificial attempt to add one more point to the demonstration of a general regime shift in 1989.

Figure 5 presents one case from those that are shown in Figure 6. Duplication should not be encouraged.

As a result, when the authors confess that “the main result of our paper is really a repetition of the results of the previous studies”, they should avoid putting stress on examples and should try to show the results in a wider picture.

The authors say in the response: “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.” In the discussion of the paper they write: “We suppose that our findings are relevant for much wider territory in the Baltic Sea region.” There is an evident contradiction in these statements that should be clarified. I recommend to stress the locality, as the paper by Lehmann *et al* is widely known and charismatic.

MINOR REMARKS

Page 2, line 8 – The North Atlantic Oscillation (NAO) strongly **affect** Northern Hemisphere... Should be **affects**.

Page 4, line 19 – The results indicated an increase... which could **leave to drought**... - Should be **lead**?

RECOMMENDATION

To better show the necessity to analyse a longer time series and soften critics on repetitions, one could add a clear statement that the method of Rodionov does not give reliable results for the end of the time series. Therefore, adding 15 years permits one to be sure that the regime shifts at the end of the 1980s have really taken place.