Firstly we would like to thank Anonymous Referee #2 for the constructive review given to the paper in discussion. Addressing the comments will certainly help the manuscript increase its clarity making it easier for readers to understand.

1) First major comment

As Anonymous Referee #2 points out, the confusion between recurrence and seasonality has been highlighted in previous comments to the paper. In our response to Anonymous Referee #1 (AR1) and answer to the Short Comment by WR Berghuijs we tried to address the difference and relation among seasonality, variability and recurrence. The following is our discussion on the matter in the Response to Referee #1:

In general we refer to recurrence as a variable returning to the same state after a certain period of time, but particularly, in this study we define the recurrence term as "the degree to which a monthly hydrological variable returns to the same state in subsequent years."

The recurrence in this study measures something different from the seasonality. The seasonality pointed out by the reviewer may be defined as "the degree to which each monthly value of a regime curve deviates from the overall monthly mean" modified from Walsh and Lawler (1981). In this definition the regime curve refers to the long-term mean of a hydrological variable at each month of a year. On the other hand, with the recurrence as defined above, we try to measure how a monthly hydrological variable will return to the similar state in subsequent years.

A more detailed discussion on the difference between recurrence and seasonality stated in the response to AR1 will be included in the introduction of the revised paper with figures that help exemplify the concept.

In this study, recurrence measures something different from seasonality, and from our view-point it gives information that adds to other parameters such as seasonality and aridity. Our definition and quantification of recurrence intend to identify to what degree the yearly cycle of a particular variable in a particular basin follow the identical pattern every year. The idea, motivation and need to understand recurrence are that water managers will have an easier task in managing a basin with recurrent runoff than a basin that is not recurrent. The reason is simply because in a recurrent basin it will be easier to adapt because the pattern will always be consistent and the timing and amount of water should be consistent year after year. In a basin with low recurrence, the amount and timing of water can vary greatly each year compared with those in a recurrent basin. Additionally, we measure recurrence on the variables of precipitation, evaporation and storage to try to understand how the patterns of these variables affect the recurrence of runoff.

The reviewer also highlights that recurrence will be dependent on the time resolution. In our case, we used monthly data and computed correlations lagged by 12, 24, 36, 48 and 60 months. Results would change largely whether data are daily or yearly; however basins with a high monthly recurrence should also have a high daily recurrence in lags close to 365 days. We decided to use monthly data and to look at a yearly cycle (12, 24 etc.) because one year is a time unit in which most of human activities and natural cycles repeat themselves. In this study, we are interested not in how a variable repeats in a period of 2, 6 or any other arbitrary lag of months, but in the similarity of yearly patterns that selected variables follow every year. We indeed checked the whole autocorrelograms in the early stages of this research but decided to keep only the autocorrelation values of multiples of 12 month lags.

The last issue pointed out by the reviewer regarding the first major comment is an example of the use of the word "fluctuation" when comparing the Ob and Yenisei basins. We acknowledge that in several points in the manuscript, we mixed some statements by not using them consistently. We need to revise these points thoroughly throughout the main text in order not to mislead and confuse readers. For instance, in the example highlighted by the reviewer we use "fluctuation" to describe that the Ob basin has a larger difference in yearly pattern than the Yenisei basin, whose pattern repeats more similarly every year. We will be more careful with the use of fluctuation by using it only when we are speaking about intra annual increase and decrease of a particular variable.

2) Second major comment:

We realize that there is no explicit definition of storage in the current manuscript. We also realize that without a clear definition of the terminology, there are several parts of the manuscript that can result in confusion. We will add the following definition in the revised version of the paper:

Storage is defined as the total amount of water held in a basin system regardless of its physical state or location. Following this definition, we defined storage in each model as the total of the different storage components particular to the model presented in Table 1. Recurrence is calculated from the summation of the components for each model, and further analysis is conducted by using individual components to understand changes in which component is more influential on changes in the total storage.

According to the water balance concept and to some degree of consistency that exists within the models in our study, the total storage of all the models should behave similarly. However, the differences among storage tanks or modules of each model also exist as one of the main differences, and cause spreads in results highlighted by Haddeland et al. (2011);Gudmundsson et al. (2012a);Gudmundsson et al. (2012b). For this reason, we decided to use only one of the eight models to analyze the recurrence of the variables, and selected WaterGAP as it is the only one with a simple calibration module and has better agreement with observations (Haddeland et al., 2011). The other models were used to check if there was consistency in the calculations made on the WaterGAP model. All the models showed agreement with the results except for a few particular conditions that were addressed and discussed in section 5.3 Model Uncertainty. The differences among the model components are also the reason to choose the total storage of all the models as the classification variable, and individual storages were only analyzed to see if they have any relationships with changes in total storage.

Storage, as defined above includes Surface Storage (SS) in the WaterGAP and LPJmL models. The definition of storage used in both models includes Lakes, Wetlands and Rivers (Döll et al., 2003;Bondeau et al., 2007) in a similar way as Kim et al. (2009). As the reviewer pointed out, the inclusion of river storage may result in scale dependence due to the length of river channels. Nevertheless, we had to include surface storage as part of total storage due to the provided dataset from EUWATCH. The dataset only provides access to total rainfall, snowfall and evaporation including water that falls and evaporates directly from snow, bare soil and open water bodies. Since our analysis is performed from a water balance perspective water stored as surface water needed to be taken into account. We will add this fact as a limitation of our study and revise related statements carefully based on this understanding. For example, in the statement about filling and emptying that the reviewer highlights, we should state that we are referring strictly to the soil moisture component causing the largest impact on changes on total storage, include a reference to the supplement where the individual climatology of each component is shown.

The reviewer inquires about the existence of seasonality in certain storage patterns in Figure 6 and its absence in the classification shown in Figure 4. This is an excellent case for demonstrating that seasonality and recurrence are something different. Seasonality, as explained in the response to AR1, is mostly calculated from a long-term average monthly pattern and shows how the average from each month deviates from the overall monthly mean. The patterns shown in Figure 6 are only the monthly means which can display some degree of seasonality; however, this does not mean that the pattern of a variable will be almost the same every year. This difference in a yearly pattern is shown by autocorrelations lagged by 12, 24, 36 48 and 60 months and is what we defined as recurrence.

Another issue highlighted by the reviewer is the groundmoist component. Deep groundwater is not considered by most models and the only part of groundwater considered would be groundwater located in the active saturated zone. This is a limitation of the current study as it is also a limitation of current global models. In future research it is worth exploring the storage components independently, and as global models improve in the inclusion of deep groundwater representation, its effects over the runoff patterns can be further studied. For now, the fluctuation of total storage has been taken into account, and the only aspect analyzed individually was the relative change of each component in relation to changes in total storage. SWE in cold regions or soil moisture in some particular tropical and subtropical basins represent the largest change in total storage. We need to revise the explanation to Figure 8 in the main text to argue that by the largest volume, we mean the largest volume in storage change as pointed out by the reviewer.

3) Specific Comments

1. Both of the references recommended by the reviewer are highly relevant to this study and they can further enhance the presentation of results and discussion of the paper. We will include both of them in the revised paper.
2. Sections 1 and 2 will be revised to reduce redundancy in the presentation of data.
3. Regarding the highlighted issues about the FFT, we need to revise captions and the main text to refer consistently to this term as “FFT intensity” instead of only FFT to

avoid confusion.

4. Further explanation on AC will be provided, following previous comments by the reviewer to describe why we used only the lags of 12, 24, 36, 48 and 60 months, and the effects of a monthly dataset instead of a daily or yearly dataset. Further explanation on the inclusion of the Colwell's index will also be provided. In summary, we have decided to include Colwell's Contingency Index because its definition is "the degree to which time and state are dependent on each other" (Colwell, 1974), and also because it has been used to qualitatively analyze the number of times that a state repeats at a given time. We have decided to include it to check the consistency of our results by using other measures used to describe similar characteristics of time series. Another reason for our decision is that we tried to find a measure to better represent what we wanted to measure.
5. Together with improving the sets of wordings for clear definition and consistent use of recurrence throughout the main text to describe and discuss our findings, we will also improve the structure of sections 4 and 5 so that they have the same structure and the subject of each paragraph does not jump from one subject to another.
6. The tone of section 5.4 will be smoothed so that we introduce recurrence as an additional way to analyze climate change impact on hydrology that can add to our knowledge and understanding instead of giving the idea that it is the best measure. We acknowledge that there is not a single ideal way of analyzing climate change and that we are merely introducing a concept that can be useful together with other already existing measures and procedures.
7. The conclusions section will be revised to include our findings in a concrete and concise manner.
8. With regard to Figure 2, our intention to include a tree, with which we classified the basins, is to illustrate the order graphically, so that it is possible to show the existing and non-existing combinations and to identify the names of the classes by a color code. The intention of this tree is only graphical and is not a decision tree as it is used in other classifications. We think that this figure aids the reader in understanding the classification and the colors on maps such as in Figure 4.
9. Figures 9 and 10 will be revised regarding the graphical representation (numbers on the axis, start of the x axis, etc.) The captions for other figures will also be revised to include only what the figure is and how to read it. Discussion regarding Figures 9 and 10 will also be revised to include quantitative descriptors on the amounts of snow and SWE and their effect on recurrent storage and runoff.
10. Figure 12 will be revised to improve readability.

11. Section 5.4 and Figure 14 will be revised to use more specific words instead of “model uncertainty.” The idea of this section is to present consistency and discrepancies amongst different models and discuss the reasons why discrepancies between models exist. Instead of using model uncertainty we propose to use “Result dependency upon model structure”.

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