Review of the manuscript: "Documenting the 2015-2017 freshening of the eastern Eurasian Basin of the Arctic Ocean and evaluating its drivers and consequences".

The manuscript presents a combination of hydrographic data from moorings and from model outputs to investigate the origin and fate of a freshwater anomaly on the Siberian slope. The in-situ observations (form the NABOS moorings) are unique and highly valuable, as they witness the fate of the Atlantic water in the Arctic Ocean, and capture the interannual variability of the Atlantic Water Boundary Current over the last 10 years at least. The processes leading to the presence of the freshwater anomaly in the observations is analyzed using model outputs. The authors suggest that this anomaly is driven by the increased river discharge that year, while the wind pattern help maintaining this anomaly concentrated on the shelf. The results of this manuscript are for sure valuable and give important insight on the interannual variability of the upper water column in the Arctic. The manuscript is overall well written but is quite lengthy, and I think some figures could be merged/could be made smaller and should be edited for sake of clarity. See my comments below. This manuscript should be accepted for publication after some revisions.

I do agree with the 2 previous reviews that the link between the river discharge and the increase freshening is not so strong for me with what is shown in this manuscript, see some of the 'minor comments' below, especially regarding the one on how the wind is discarded very quickly as a driver of the freshwater anomaly.

My other main comment regarding the manuscript concerns the use of the model. The model is used to define the processes behind the freshwater anomaly. However, it is not mentioned if the model even represents appropriately the ocean in the region. It seems hard to explain an anomaly with the model outputs without some explanation on how it performs. Some kind of model evaluation should be added to the manuscript to give some weight to its use.

Minor comments:
L.131: It is in this section that a model evaluation should be added.

l.145-146: How reliable are the wind field reanalysis reliable in this region?

l.162: the first measurements of salinity are around 60m if I am not mistaking (Table 1). Which means that a significant part of the surface waters is not observed in the moorings. While I don't think this is changing the content of the analysis, I think this limitation should be acknowledged, especially that it means that neither the mixed layer nor the depth of the Transpolar Drift is actually covered by the mooring array. There is a full sensitivity study done on the definition of the surface layer for compute the freshwater content, but nothing is mentioned about this lack of data at the surface.

l.171: I was surprised by this value of 77m for the first measurement of salinity, as I thought that the shallowest one was 54m according to table 1. Please see my comment below regarding the clarity of the table.

l.200-201: When reading the manuscript, it was not clear for me why the cross-slope shifts of the AWBC core can cause freshening. The explanation does come later in the text, but it would be valuable to have it already here.

l.201: What do you mean by 'advective terms'? please define?

l.202: how many pairs of mooring do you define then?
l.202: please precise that you use meridional velocities as the slope is oriented East-West (or refer to figure 1 at minima).

l.210: what does 'deseasoned' mean? Does it mean that the seasonal average has been removed?

l.210: 'surface salinity time series': is that from in situ observations or from the mooring observations? Please clarify. This is a comment valid for the entire manuscript. It is not always clear if the data presented/used are from the moorings or from the reanalysis of from the model outputs.

l.230: as mentioned before, it is a bit hard to follow the depths of the different instruments. Please clarify the text and table 1 (see comment below).

l.234-236: I know it is explained later, but at first glance it is very clear that the freshwater anomaly is divided in 2 events. This should be mentioned already here.

l.287-287: I was first confused reading this sentence: 'the freshwater content and the ocean heat content showed a strong opposite tendency'. I understand now (if I am right) that this sentence refers to the average per year, and not to the actual trend of the curve. Please clarify in the text.

l.288: there is no section 6.

l.321: the meteoric water fraction is mentioned, while the upper water column is not sampled by the moorings. How is it possible to conclude something about the sea ice melt without the upper water column being sampled?

Section 3.2.2: There is no mention of advection patterns on the shelf, while one could explain that they could explain at least part of the freshwater anomaly, and not only the river discharge.

l.344: '4-month seasons': isn't it rather 3-months seasons?

l.345-346: What about the DJF&MAM 2016? Again, it seems that it could be differentiated in two different events, and this anomaly within the anomaly is only mentioned later while I think it should be mentioned earlier.

l.346-347: the last sentence: 'this pattern… easter Eurasian Basin'. I understood this sentence after reading the full manuscript, but here it seems too early to have this claim. Please remove.

l.377: is the model representing correctly the river discharges? Does the model actually show the freshwater anomaly?

l.387-389: I was a bit lost with the argumentation about the wind not having a significant impact on the freshwater anomaly, as the section before concludes that the wind is important. Please clarify in the text.

l.494-496: these sentences should be earlier in the manuscript, in the method section to motivate the use of this model compared to another one.

l.499-503: it would be interesting to investigate if this event of larger discharge was a one-time event, or if it is occurring frequently in the region. This will put this study in a larger temporal context and will provide a larger impact to this study. One question I was also asking myself was: what trigger this increased release of freshwater? Even though not fully detailed in this manuscript, some hypothesis could be advanced.

Table 1: all in all, I find table 1 hard to follow, to know at which depth are which variables. Perhaps add also the variables measured by each instrument. It will help non expert reader to find out what it available where.

Figure 3 and figures thereafter: I suggest using g/kg and absolute salinity instead of psu for the Salinity units, following the latest recommendations from McDougall et al., 2012.

Figure 3: the colorbar chosen is not colorblind-friendly. I suggest to rather a colorblind friendly one, as 'balance' form the cmocean toolbox on Matlab for example.

Figure 4 and 5: maybe these 2 figures can be merge by using a left and a right axis. It will also be easier to compare the different variables.

Figure 6: as the text mainly follow the chronological order for the description of this figure, I suggest to merge the winter and the summer panel of this figure, so that the bars are chronologically order, but to keep the color code (orange for summer and blue for winter) to help the comparison in between the same season.

Figure 10: same as for figure 3, the colorbar should be changed to something colorblind-friendly. Add also a label for the x-axis. This applies to figure 8 and in general to all figures where there is no label to the axis.

Figure 11: In the left panels in a) one curve has the lag applied to it if I understood correctly. However, the x-axis is the date. I would suggest showing the curves without adding/ subtracting the lag for sake of clarity and matching the x-axis label.

Figure 12: the labels are not shown correctly. I would suggest having windroses instead, so that we could see also the variability and not only the average direction.