Reply to reviewer #1

We would like to thank the reviewer #1 for the time dedicated for the review of our work, and for the several ideas that allowed to improve the clarity of the manuscript. Our replies are in bold.

The authors apply two dimensional signal processing to time/longitude model outputof ssh variability. They identify westward propagation of features with a phase speedof 4.17 cm/s. The Rossby wave dispersion relation, taking a reasonable value for thebaroclinic deformation radius, predicts a phase speed of 1.55 cm/s. The difference is consistent with previous obervations that Rossby waves tend to propagate faster than free wave theory predicts. The authors then go on to try to relate the NAO to the Gulf Stream position and the excitation of Rossby waves.

The application of the 2DRT seems useful and valid. The problem of how mid-latitude Rossby waves are excited and influence the Gulf Stream is important. However, while I do not find any technical problems with this analysis, I also do not find it particularly enlightening. There just isn't enough new here, my recommendation is to reject the paper.

The authors cite several previous works that have identified Rossby waves north of 35N, so the point in the Abstract that this is the first time Rossby waves have been found that far north for such an extended period is not that novel.

Regarding the previous works identifying Rossby waves north of 35°N, we cited Osychny and Cornillon (2004) and Lecointre et al. (2008). Lecointre et al. (2008) used a numerical simulation at a spatial resolution of 1/6° between 1993-2000. Osychny and Cornillon (2004) used a TOPEX/Poseidon product at a spatial resolution of 1° between 1992-1998.

In this study, we use an eddy-resolving numerical simulation at a spatial resolution of 1/12° between 1970-2015. Therefore, we definitely use a much longer period (46 years against 6 to 8 years) and a higher spatial resolution (1/12° against 1/6° to 1°) than the two previous studies to detect Rossby waves that far north.

We suggest to rephrase "this is the first time Rossby waves have been found that far north for such an extended period" into "this study extends the period over which Rossby waves have been found that far north to a much longer period, which reinforces the findings of previous works".

In addition, we fully described in Section 2 the advantages of working with a higher spatial resolution to better detect the Rossby waves.

There is little mention of the influence of the Gulf Stream on the wave propagation. Even though the meridional wavelength is likely large compared to the Gulf Stream width, the authors apply their analysis right in the latitude band of the strong eastwardflow. What happens if the analysis is applied a little further to the south?

In this study, we focus on the delayed impact of the NAO on the Gulf Stream. Applying such an analysis to a latitude band that would not correspond to the Gulf Stream latitude is thus outside the scope of the paper.

We added a corresponding comment in the paper "Since this study focus on the delayed impact of the NAO on the GS, other latitude bands are considered outside the scope of this paper."

Analysis demonstrating a lagged correlation between GSNW and NAO is not new and the present analysis, while consistent with previous work, does not add much to the paper.

The correlations we present here between the NAO and the GSNW are simply recomputed on a monthly basis from the indices of Watelet et al. (2017). Looking at delays at a resolution of 1 month instead of 1 year is relevant in order to allow a comparison with the Rossby wave speeds. The main goal of this paper is not to demonstrate that lagged correlations between NAO and GSNW exist. The purpose of our study is to detect Rossby waves at the latitude of the GS, compute the speed of these waves, and analyse the consistency between these waves and the lagged correlations that were found previously. The fact that we refined the NAO-GSNW correlations to a monthly basis is thus a minor part of the paper, but still a necessary improvement to reach our main goal.

In addition, the exact time lag between NAO and GSNW is still discussed, as stated in Section 4. There is thus room for further research there as well.

In order to improve the clarity of the manuscript, we added the following comment: "Looking at delays at a resolution of 1 month instead of 1 year is necessary in order to allow an accurate comparison with the Rossby wave speeds."

It was unclear to me how RT is used to generate a time series. Please expand on the discussion around lines 185.

We added the following sentence to the manuscript: "In other words, we projected the axis x' on the original time axis x to get a time series from the RT at phi=21°." For clarity, we also added: "As a reminder, the RT computation is based on the sum of the SSH of a Hovmöller diagram along the spatial axis progressively tilted to the left as the angle phi increases."

The results in Figs 8 and 9 are interesting but unfortunately the lack of statistical significance makes this a less than compelling argument.

We agree that the Fig 8 and 9 do not show statistically significant correlations, which is described in the manuscript. We also think, similarly to the reviewer #1, that these figures are nevertheless interesting. These weak correlations are an argument to encourage further research in the physical processes linking NAO and Rossby waves.

We adapted the text by adding "While the correlations are not significant, the figures are nevertheless interesting. These weak correlations are an argument to encourage further research in the physical processes linking NAO and Rossby waves."