NPG-2021-19 : Reply to comments from Referee #1

*This study concerns the modeling, through interpretation of laboratory experiments, of the dynamics of the very large features of the Earth system on climate time-scales. The manuscript (presumably) covers the Lecture for the Lewis Fry Richardson (EGU) medal of Berengère Dubrulle, with several of her co-authors. It is entitled "*How many modes are needed to predict climate bifurcations? Lessons from an experiment"*. The paper is placed in the broad framework of the question of whether a scientifically acceptable answer can be given to society concerning the evolution of climate, and what effort (theoretical, modeling, experimental and numerical) is needed in order to be able to do so reliably. Two examples are specifically examined: the switching in the atmosphere between convection and blocking, and the Niño-Niña transition, a prediction of which would be extremely useful to society (and to modelers of course). The principal tool is the experimental facility used by this group in Saclay (France), together with sophisticated mathematical tools. Specifically, the study is focusing on spontaneous transitions of the von Kármán flow between two counter-rotating cylinders under different types of forcing. Reynolds numbers are of the order of 10^{13} (or less, when using glycerol instead of water), basically too small compared with geophysical flows, but still very enlightening.*

We thank the Referee for her/his/their positive comment.

In the last sentence of the abstract and in the conclusion, the authors stress the importance of describing very small scales to capture fluctuations of correct intensity and scale. Of course, one thinks of the 50-year old Donnella Meadows et al. project at MIT that predicted a collapse of the world climate and economic system in or around 2020 with a model having a small number of relevant variables and different complex (feedback) interactions. Are these models enough? With such drastic truncations, how are the all-essential small-scales treated or incorporated? One could argue that the equivalence between Earth systems and the von Kármán (VK) flow is not quite as much as stated in the paper; in particular, what of the role of stratification, which is strong at large scale? What is the effect of shear? Can it be quantified? Do they play any role in these transitions? What about chemistry, moisture and the like?

Thank you for this interesting comment pointing at the rich range of scales and phenomena embedded in the climate system. Regarding the first question raised by the reviewer, about the possibility of representing collapse and bifurcation with small number of degrees of freedom, we do believe that the best approach to complexity is to use both simple conceptual models (as the one presented in Faranda et al. 2017, PRL for the VK flow or Faranda et al. 2019 ESD, for the jet stream) as well as complex models (e.g. DNS or LES for the VK and full numerical integration of primitive equations for the climate). While simple models can quickly provide an overview of the bifurcation landscape of a system, fine models can be used to explore targeted regions of the phase space where interesting phenomena produce e.g., extreme dissipation events in the VK flow or convective events in climate simulations. Furthermore, we would be very careful in the new version of the manuscript with suggesting "equivalences" between the VK and the climate system: our idea is rather to show that there is some universality in the behavior of highly turbulent flows in confined geometries. On one hand, the role of shear and stratification mentioned by the reviewer can indeed be observed and quantified in the VK experiment by varying the geometry of the impellers and, as in geophysical flows, those variations can trigger bifurcations. On the other hand, phenomena driven by biogeochemical reactions are beyond the analogy with the von Karman flow. We will make this explicitly clear in the new version of the manuscript in the discussion section.

Also, concerning the computational nightmare, sometimes called the curse of dimensionality, there are several papers claiming that neural networks can lead to avoiding that curse. Do the authors have a comment on that?

Indeed, in recent years there have been developments in understanding that the computational nightmare can be partially solved by applying neural networks or, more generally, machine learning approaches (see, e.g. Pathak et al. for the artificial intelligence applied to the behavior of chaotic systems). We would like to stress that these approaches are never holistic and often target a specific subset of spatial and temporal scales of the climate systems, e.g. the prediction of geophysical data (Wu et al., 2018), the parameterizations of subgrid processes in climate models (Krasnopolsky et al., 2005; Krasnopolsky and Fox-Rabinovitz, 2006; Rasp et al., 2018; Gentine et al., 2018; Brenowitz and Bretherton, 2018, 2019; Yuval and O'Gorman, 2020; Gettelman et al., 2020; Krasnopolsky et al., 2013), the forecasting (Liu et al., 2015; Grover et al., 2015; Haupt et al., 2018; Weynet al., 2019) and nowcasting (i.e. extremely short-term forecasting) of weather variables (Xingjian et al., 2015; Shi et al., 2017;Sprenger et al., 2017), the quantification of the uncertainty of deterministic weather prediction (Scher and Messori, 2018). The greatest challenge of entirely replacing the equations of climate models with neural networks capable of producing reliable long and short-term forecasts of meteorological variables is, to the best of our knowledge, not yet achieved with these methods. This has been added to the manuscript.

This paper is very pleasant to read. It describes the thinking going-on at the intersection of several fields of physics and the creativity in the development of new data and in interpreting it. I recommend publication.

We thank the Referee for his/her/their positive comment

Details:

1. *In the very first sentence, it is a bit odd to see the first author (BD) talk of herself in the third person*

We understand the Referee's comment. The manuscript was initially written in the first person, but we had to change it following another comment from the Editor. As a compromise, we have named the first paragraph "context" so that it is clear that it is just the story behind the review and we have written it so that it shows how it lead to the present paper co-signed with 4 other persons.

2. *In the title, fry and ?: might have to be changed*

We have removed the Lewis Fry Richardson Medal Lecture in the title.

3. *I have several problems with Figure 1 (l. 55+). What is a Dansgaard-Oeschger event? What is ka? What are M12345? What do the 8 12 14 18 etc refer to? Also, the insets are way too small, I suggest making the figure substantially larger. A reference (to Didier Paillard or other) would be necessary as well.*

Figure 1 has been reworked for improved readability, and the unnecessary information on the graph has been removed. A reference to the original source of the data [K. K. Andersen et al., High resolution record of Northern Hemisphere climate extending into the last interglacial period, Nature 431, 147-151 (2004)] has been added to the text.

4. line 92+*, perhaps one could cite and discuss a bit the drastic reduction of modes (down to 3) done by Ed Lorenz and which led to many investigations of lack of predictability of atmospheric flows. For example, why is 3 enough (when, as pointed out by the authors, we need a priori 1024 modes)? Why not 5 or 13? And what do larger systems bring compared to the small (and successful) system? When do we know that we have enough small scales? What criteria can be applied?*

We thank the Referee for this suggestion. Indeed, the work of Edward Lorenz provides the lower bound for the minimum number of modes to both obtain chaotic behaviour and reproduce several salient features of Rayleigh-Bénard convection dynamics. We have mentioned this point as well as a few References in Section 2 -- around line 100 in the new version of the manuscript.

5. Figure 2: *give the unit of sizes (0.925, 1, ...)*

The sizes are provided in units of the cylinder radius R, but we have changed them anyway for more clarity.

6. line 164*, ITCZ is not defined*

We have added a mention of the full name of the Intertropical Convergence Zone (ICTZ) to the manuscript, and removed the acronym since it is only used once.

7. Figure 3: *Is there a significance in the fact that the amplitudes in flow (a) are smaller than for (b) and (c)?*

The very strong shear layer produced in the experiments is located away from the impellers in the symmetrical state. In contrast, in bifurcated states, the whole flow rotates in the direction of the dominant impeller, whereas the shear layer is moved towards the slave impeller. As a result, both the angular velocity and the (absolute) angular momentum are significantly higher in bifurcated states than in the symmetrical state. We have mentioned this briefly in the caption of Figure 3.

8. Figure 5: *Spring and Summer states are undefined here. One could at least refer to Figure 3, or perhaps add a line in Table 1 defining the 3 (or more) states (Spring or autumn, Austral and Boreal Summers, and Transitional) since they are key ingredients of the analysis and the paper refers to them often.*

We have provided a definition of spring/autumn and austral/boreal summer branches in the caption of Figure 5.

9. Figure 11: Multifractal spectrum of what?

We plot here the multifractal spectrum exponent of the wavelet velocity increments. We have clarified this point in the caption of Figure 11.

10. line 345: *Are there not strong velocity gradients in the Planetary Boundary Layer as well as the extreme events mentioned here? Do they not translate into large-scale extremes?*

We agree with the referee. We have rephrased the sentence to make that clearer.

11. line 351 and beyond:

The comment from the Referee seems to be missing here.

12. line 365: *By few modes, do you mean 3 as in the Lorenz model, or 10^4 as in the glycerol experiment described in the paper? Again, how little can it be? How does one determine this threshold? Is not the computational nightmare mentioned in Section 5.4, *also* an experimental nightmare?*

We mean 10^4. This has been clarified. Regarding the sequel, we have answered to it previously, see above (comment 4).