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Paper Review: Tian, Y., E. R. Weeks, K. Ide, J. S. Urbach, C. N. Baroud, M. Ghil, and H. L. Swinney (2001), Experimental and numerical studies of an eastward jet over topography, J. Fluid. Mech., 438, 129-157.

Dear editor, bellow you will find my review of the Tian et al. manuscript. This document is organized in eight points: [1] Paper's overview, [2] Importance, [3] Methodology, [4] Originality, [5] Results summary, [6] Implications, [7] Critics, and [8] My recommendation.

[1] Basically, this study reports measurements from one laboratory experiment which simulated a barotropic flow in a rotating annulus, and results of numerical simulations designed to help better interpret these measurements. The author's primary objective is to investigate how this barotropic flow behaves over a topography in such controlled environment in order to understand what is the dynamics involved and draw a parallel with real zonal atmospheric flows (e. g. Earth's westerly winds and other planets atmospheric features). Both experiments, laboratory and numerical, qualitatively reproduced the characteristics and behavior of the sub-tropical large-scale circulation in the Northern Hemisphere and its low frequency variability.

[2] In the first and second paragraphs of the manuscript, the authors pointed out that the interactions between rotating flows and topography is a classic and important topic of geophysical fluid dynamics due to its applicability to the understanding of real atmospheric flows. Therefore, any new characteristic and insights about the dynamics of known features could lead to improvements of knowledge and forecast of the atmospheric general circulation (**although, the authors were not clear in saying that!**).

The non-linear interactions between the atmospheric Westerlies and land topography imposes two distinct basic states in the large-scale circulation of the Northern Hemisphere, also known as the high-index and low-index flows. In the past fifty years (e. g. Section 1), we acquired extensive knowledge about the behavior and characteristics of the low-frequency variability of these states. Although the authors emphasize that their experiments can help understand this phenomena, **they did not present specific gaps of knowledge addressed in the work**.

[3] As I mentioned in [1], this study consist of laboratory and numerical experiments. The laboratory experiment (referred as "experimental investigation") consist of measurements (i. e. particles tracking using a video camera) in a large rotating annulus. The experimental apparatus has a rigid and flat top, and a conical bottom to simulate the β-effect. Two symmetric mountains were placed over the conical bottom in order to simulate the topographic interactions in the low and high-index flows. Moreover, a pump system generated the barotropic currents by imposing inflows and outflows in the tank.

The numerical experiment consists of the numerical solution of the potential vorticity equation under the Quasi-geostrophic (QG) approximation. The model parameters and the boundary conditions were set to simulate the tank environment. This procedure allowed the authors to have access the variables in the tank without directly measuring it. I should mention that the significant quantitative differences between the models indicate the numerical model did not represent well the the experimental investigation. **Therefore, I think the applied methodology was conceptually adequate to reproduce and study the low and high-index flows in a simplified version of the Earth's atmosphere, however adjustments must be done**.

[4] Although the idea of reproducing the low and high-index flows with very simple models to capture the essential dynamics is good, the authors did not addressed any specific unanswered question or original hypothesis in the manuscript. In my point of view, this lack of specificity is reflected in the results presented, which were poorly explored and do not comprise new aspects of the phenomena characteristics or dynamics.

[5] In summary, the present manuscript shows that it is possible to obtain features that qualitative resembles atmospheric high-index and low-index flows using both models. The regime is determined by the combination of the Rossby and Ekman numbers, which are controlled by the rotation frequency and velocity scales, however when the parameters were gradually changed the laboratory and numerical models presented different behaviors. The tank experiment presented spontaneous abrupt changes in the regimes, while the numerical model allowed multiple states at the same time.

The numerical results also allowed the authors to observe that the topographic resonance affects the flows. The high-index regime appears to be super-resonant, and the low-index flow is subresonant. Besides the qualitative agreement between the models and the real atmosphere, no quantitative results were compared to the real world.

[6] Unfortunately, the authors did not explore new aspects of the phenomena of interested, therefore the implications of publishing this work is restricted to some of the experimental aspects, which is not enough for an article in the Journal of Fluid Mechanics. Additionally , similar experiments were already reported in the literature.

[7] In order to justify my recommendation given in [8], I will expose some general and specific comments about this study. Here I am going to focus on major aspects of the manuscript, however I found some parts confusing and repetitive.

- **General comments:** As I mentioned before the authors are motivated by an important topic of our atmospheric dynamics (i. e. better understanding of the high and low-index flows), however the article does not provide any original information about it. I believe they missed the essence of the scientific progress when they tried to get any new insight about the subject with their experiments, i. e. identify (formulate) specific open questions (hypothesis) and try to fill the gaps of the current knowledge. Since they did not presented such gaps, I think they were not able to properly explore their results.

  Moreover, this study does not accomplish the "implicit"[1] goals set by the authors. Because the numerical model failed in providing quantitative information about the laboratory experiment, and both models only reproduced flow regimes that qualitative resembles the real atmospheric features (e. g. Section 4. Summary and Discussion), the goals were not achieved.

- **Specific comments:**
  1. *Introduction:* Good literature review, however it is Missing focus (i. e. clear objectives and knowledge gaps) and more precision in using some of the citations (more details about references).
  2. *Experimental and numerical investigation:* Once the authors decide which specific problem solve they can design better experiments (**I should emphasize, the idea of performing laboratory experiments and confront the results with numerical modeling is valid and very powerful**). Additionally, some of the original results (e. g., the existence of spontaneous regime shifts in the tank, and multiple states in the numerical) demand detail explanation. These open questions could be the motivation for a solid work.

[1]"implicit": The authors did not clearly set goals for this study, therefore I deduced the primary goal is to get any new insight about the different zonal winds regimes in our atmosphere by performing laboratory and numerical experiments. The later is used to better interpret the first.

3. *Summary and discussion:* Even with all the quantitative differences of the models I would expect some comparison with the real atmospheric phenomena, which was not done.
4. *References:* It caught my attention the fact that 25 of the 79 reference materials (approximately 30%) have one of the co-authors involved, including the Master and Doctorate thesis of the first author. The usage of such references must be justified by adding relevant information to the sentence or paragraph, however some of them does not contribute to the text. Examples where the bold faced references are not essential to the content:

   Section 2.2.3, page 137 "... The low frequency oscillations in the blocked flow are of particular interest, since Ghil (1987) hypothesized that an oscillatory instability of blocked flow over topographic might cause the 40-50-day oscillations in the Northern Hemisphere's mid latitudes **(see also Ghil & Childress 1987; Jin & Ghil 1990). ...**";

   Section 4.2, page 153 "... **Cheng & Wallace (1993), Kimoto & Ghil (1993a), Smyth, Ide & Ghil (1999), and Corti, Molteni & Palmer (1999)** all essentially find two regimes, zonal and blocked, in each one of the Pacific/North American and Atlantic-European sectors of the hemisphere. .... ";

[8] Finally, based on the issues pointed out above I recommend to **reject** this manuscript. However, the authors must keep in mind they have some interesting results that could be better explored from the theoretical point of view that could lead to a future publication.