

Anonymous Referee #3 Received and published: 19 March 2019

General: This paper reports on simulations of terrain induced wind systems in Mexico, and is novel in the respect that I have not seen much work in this particular region. The question that I have been wrestling with is however: "Is this novel science?"

The simulations are carried out for one single events, and while the authors may feels this was a typical event, the reader is not given much information other than some verbal arguments. So this remains a case study, and does not even claim to be general. The type of flows that are described (mainly downs-slope wind-storms and hydraulic jumps) occur at many places around the world and has been studies extensively both from observations and with models. It is well known that numerical models perform well in these type of "hydraulic flows"; hence it does not reveal anything new about the flow features other than such that are directly related to specific local features. The authors do not even try to show that this type of flows have special characters because of the geographic location, which closer to the equator than in most other studies.

Hence, there is nothing new here and from that respect I should have recommended "reject". However, there is nothing fundamentally wrong with the study as such, so maybe that would be unfair. So I will leave to the editor to determine if the world needs one more paper about these events. I would tend to say "no", but in the case the editor say "yes" the paper still needs major revision.

We thank the reviewer for accepting the evaluation of this manuscript; we appreciate the effort of his/her thorough review. The reviewer's general concern about the significance of our research to the existent science in the field is well taken; perhaps we have not made our contribution apparent enough in the text. We consider that the manuscript has several outstanding points that make it a noteworthy publishable work for this journal:

1) It is true that Tehuantepecers are orographically induced flows, of the kind occurring elsewhere on the planet. They are, however, of the very few with a global significance. Winds funneled through the Isthmus of Tehuantepec, are so intense and extend so far out to sea, that they are clearly visible from space, in scatterometer data, microwave derived total precipitable water, and even forming large rope clouds in their outflow/frontal boundary. Furthermore, Tehuantepecers produce strong upwelling in the Gulf of Tehuantepec resulting in large chlorophyll blooms that are critical for the food chain and marine life in the eastern Tropical Pacific. So, they are not just one more of this type of flows.

2) The scope of the paper is not to demonstrate anything particularly special about these orographic flows in terms of physics. Instead, our research's goal is to show that intense flow acceleration in the Isthmus of Tehuantepec during Tehuantepecer events is not restricted to the well-known mountain gap wind jet off Chivela pass, but occurs in the neighbouring Sierras as well, in a stretch of over 100 km and for different dynamical mechanisms, forming downslope windstorms and hydraulic jumps. Our research objective is scientifically relevant because, while much attention has been given to the gap winds through Chivela pass, very little is known about the flow structure and associated extreme phenomena developing elsewhere across the Isthmus. Our work is the first, as far as we know, to analyze these extreme winds and discuss their driving dynamics.

3) Knowledge about these phenomena is also important for social and economic reasons because of the major impact that they have on the region. These extreme events cause, every year, problems and accidents involving population as well as infrastructure (please, see the references in the Introduction section), and a better understanding about them can help to mitigate the damages that they produce. Moreover, the region is the most important for wind energy generation in all of Latin America. These events directly affect the production of wind turbines and could undermine their performance if not considered in wind farm operations.

In the new version of the manuscript we will bring out more clearly the goal of our study and why it is novel science (point 2 above), building upon previous studies on Tehuantepecers.

Tehuantepecers are a recurrent feature of the circulation in the area, particularly in winter months, as we explicitly mention and support with references in the Introduction section. We simulated other events during late 2013 and also the previous winter season in early 2013, and we saw that downslope windstorms and hydraulic jumps also develop in the flow across the Isthmus. The case we present in the paper is the most intense and long lasting in the period, but we are very certain that the occurrence of these extreme phenomena is commonly associated with Tehuantepecer events in general. Evidences come not just from our model data, but also from wind company reports and from the damages they cause.

With regard to the use of a model for the study, it is of course not a novelty and we do not claim it to be so at all in the paper. It is precisely because models perform well in simulating these orographic flows, as the reviewer mentions, that we use one to analyze the flow structure and explain the particular mechanisms producing downslope windstorms and hydraulic jumps in the area. Unfortunately, there are no direct observations to help with the task, so a model is the best approach we can find.

I have two main concerns:

1) The analysis of the model results is rather superficial and makes many claims that are hard to substantiate from the graphic material presented. I'm sure this could be turned into a nice paper if the authors try to think a bit out of the box and perform an actual analysis of the model results, rather than just produce a few standard plots direct from the model data. Much of the theoretical background includes the planetary rotation; this is not commented on at all and here may be an opportunity to take a novel angle, comparing these results to higher-latitude cases. Just in general I would like to see a more in-depth analysis.

The Rossby number for the flows in our study is high (above 13), given the small width of the mountains (about 35km in both cases), the strong wind speed of the order of 20m/s and the low latitude (f is about 4×10^{-5} rad/sec, less than half the value in midlatitudes). The role of planetary rotation is therefore very minor when compared with the effect of inertia, pressure and gravity forces. Mountain waves and related orographic flows such as those producing downslope windstorms and hydraulic jumps are in general high Rossby number motions everywhere, even at much higher latitudes; thus, in all related theoretical studies we know of (certainly in all those cited in the paper), the Coriolis force is neglected.

However, the effect of planetary rotation, or rather the lack of it, is indeed very relevant to explain the large extent over the ocean of the accelerated flow exiting the Isthmus. As mentioned above, the latter is instrumental to make Tehuantepecers stand out for their size and impacts among other orographic flows. Is it, perhaps, this effect what the reviewer's concern is about?

Gap winds and downslope windstorms in mid and high-latitudes do not usually extend too far downstream from the mountains where they develop; the exiting accelerated flow adjusts quickly to the general geostrophic synoptic flow in the region. Even in gap winds through marine straits, where the exit region is free from topographic obstacles as in Tehuantepecers, the outflow weakens and merges with ambient circulations in relatively short distances. The article by Steenburgh et al (1998), cited several times in the text, discusses the mechanisms behind the large downstream extension of Tehuantepecers over the Gulf of Tehuantepec arguing that it is the combined effect of the lack of obstacles on the marine surface and the low f parameter of the tropical latitude of the area. The small Coriolis force results in very weak synoptic circulation (small pressure gradients and low wind speeds) and hence the gap wind does not encounter any ambient large-scale flow to merge with, describing a very-close-to-inertial trajectory. Furthermore, a small Coriolis force produces weak wind deflection and thus the anticyclonic curvature of the gap jet as it moves over the ocean is not very pronounced. These authors perform numerical experiments for the Tehuantepecer case they studied with the f of 45° N and show that in mid latitudes the gap wind outflow would curve much more westward, thereby not reaching as far south as in the actual situation.

Another result in the numerical experiments in Steenburgh et al. (1998) with different f values, is that the gap winds and flow in the Isthmus itself are largely unaffected by changes in the magnitude of the Coriolis parameter. Therefore, as theory predicts, planetary rotation plays only a minor or negligible role in the development and dynamics of these orographic circulations.

As per the reviewer's suggestion, we will comment on planetary rotation and the impact of the low latitude in Tehuantepecer's structure (mostly on the extent of the outflow in Gulf of Tehuantepec) in the Introduction section of the revised manuscript, and also when discussing the large reach of the accelerated flow resulting from the downslope windstorm in the lowest mountain in our analysis.

2) *The main results are all together expected and reveals nothing about this flow that couldn't have guessed without the model simulations. Sure there are details here that no one could guess, but much of that detail is not harvested.*

We disagree. We haven't seen any mention of downslope windstorms or hydraulic jumps developing in the Isthmus in any study related to Tehuantepecers, not even in the highly referenced article by Steenburgh et al. (1998), discussed above, detailing the dynamics of the gap winds and their outflow over the Gulf of Tehuantepec with the use of numerical simulations. If it was so evident that the flow shows the behavior we analyze in our work, it would have been reported in some of the several studies dealing with Tehuantepecers. The focus is always on the gap wind jet off Chivela pass. In Steenburgh et al. (1998) there is only a very brief comment about mountain waves and flow acceleration occurring also on the lee slope of the mountains east of Chivela pass, where their trajectory analysis shows that the cold air is also able to cross over to the Pacific side of the Isthmus, but with no further elaboration. The reason is likely related to the low resolution (6.67 km) or to the now outdated MM5 model used in this early study, which were not capable of simulating the downslope windstorm and hydraulic jump phenomena. Thus, this provides evidence that high resolution simulations with an adequate tool (the WRF model) are indeed necessary to reveal these flow features. They are certainly not captured by global analysis and there are virtually no station observations or observational campaigns that can disclose their existence and structure.

Finally, the language would benefit from editing by a native english speaker.

We will ask a native speaker to edit the manuscript, as per the reviewer's suggestion.

Specific:

1- *Make sure you define abbreviations the first time you use it, and stick with abbreviation afterwards. HJ is not explained, and is used interchanging with "hydraulic jump".*

This is true, we will correct this in the revised version of the manuscript.

2- *Drop the entire section 1.1; this is textbook stuff and just take up space.*

Section 1.1 aims to put in context the hydraulic analog theory applying to the studied flows. We considered it important because it helps to better understand the discussions, as we often refer to this theory throughout the article. However, we understand the reviewer's concern, and in the revised manuscript we will reduce this section.

3- *If you feel a need to validate the model, this should come before results, not after. Moreover, the cloud evaluation is superficial to the point of being useless. Either drop it or develop it.*

The validation we are performing is not a general validation of the model, since there are very few observations (just two sites) and not exactly in the best locations in relation with the phenomena we are studying. To begin by validating d04 and d05 even before explaining what we are analyzing, could confuse the reader and make explanations and discussions difficult to follow. It seems more appropriate and natural to us to first analyze the model results and then verify that they indeed agree with point observations.

With regard to the cloud evaluation, we do not think it is useless at all. That lee wave clouds exist in the same location and with the same extent as in the model results, strongly suggests that the model solution is accurate in simulating trapped lee waves precisely in the focus area, and therefore realistic. In the revised version of the manuscript, we will show the cloud image and model results over the same exact domain, to make comparisons easier and highlight the value of this piece of evidence.

4- P3, I25: *What do you mean by microscale?*

We mean atmospheric motions of spatial scales less than 2km, following the definition of microscale in the American Meteorological Society (AMS) glossary of meteorology. The resolution of the innermost grid is 444m, sufficient to resolve some of these motions, including rotor circulations and even the hydraulic jump itself. However, since we are really referring to details of the downslope windstorms, while microscale meteorology is most often dealing with turbulence and other truly small-scale processes, we are going to change the word microscale and rephrase the line:

"In section III, the primary results obtained are shown, divided by the synoptic-mesoscale situation, the upstream-downstream structure of the phenomena and the microscale situation"

By:

"In section III, the primary results obtained are shown, divided into synoptic-mesoscale situation and upstream-downstream structure of the phenomena"

The word microscale in the line the reviewer is referring to is actually correct, since the WRF model can run in L.E.S (large eddy simulation) mode resolving turbulent eddies, which are indeed microscale motions.

5- Figure 1: *Why are the two most high-resolution domain off center wrt to the gap?*

Precisely because the focus of our study is not the flow through the gap (Chivela pass) but in the mountains around, especially those to the east. Perhaps we didn't make it apparent enough in the article. We will modify the Introduction section, as mentioned above, to highlight more clearly the goal of our work, and what sets it apart from previous literature on Tehuantepecer winds.

6- P6, I3: *How much of a spinup is required before the model physics is realistic?*

From 3 to 6 hours is usually recommended. This is a standard practice in WRF simulation with this resolution [1]. Downslope windstorms and hydraulic jumps form around 12 UTC December 23, which is when we start the simulations. We show results from 3h into the simulation in Figure 5 only, to illustrate how the phenomena that we want to study develop. Most of the analysis is from data with a spinup time of 15h and more, when the flow features are fully mature.

[1] Warner, T. T. (2011). Quality assurance in atmospheric modeling. *Bulletin of the American Meteorological Society*, 92(12), 1601-1610.

7- P6, I18: *This is incorrect; U^* is not a wind speed; its a scaling parameter that depends on the vertical turbulent momentum flux. Also explain how the logarithmic wind-law is applied; with this formula, the wind just increases with height, so you need an anchor point.*

We agree with the reviewer in that U^* is not a real velocity but a parameter related to the vertical turbulent momentum flux. It is a reference velocity or a velocity scale, whose square value yields the magnitude of the vertical turbulent momentum flux near the surface, where it is assumed to be independent of height and nearly constant. It has dimensions of velocity (units are m/s) and it is called friction velocity in all text books and literature we know of. Perhaps the reviewer concern is related to the w_s^* symbol we used in the text, which can be confused with a real wind speed, such as w_{s_z} . We will change the w_s^* naming for this parameter in Equation 3 to the more standard U^* symbol, so that no confusion can be made with an actual windspeed.

8- Figure 2: *What is the height of the 850 hPa wrt mountain crest? Why pressure levels at all, and not model levels? Note the warmest temperatures hanging on the southward facing lee slope; DSWs already happen here? Maybe the D01 domain is a bit small, or could have been located farther north, since there's a lot of uninteresting ocean south of the coast.*

Figure 2 displays the synoptic setting for the Tehuantepecer event in our study. It is not depicting model results, but global analysis data at 25km resolution. The use of pressure levels is a standard practice for this type of plots. We show 850 hPa temperatures and surface pressure because we are interested in the situation at low levels. The purpose of the figure is to illustrate how the low-level cold air driven by cold air damming in the Rockies continuing in the Sierra Madre Oriental in Mexico, moves fast southward, reaching the Bay of Campeche. The 850hPa surface is around 1600m in Mexico, within the cold air mass, which tops at about 2500m (see the sounding in Fig 3d). The tallest mountain crests in the Isthmus are around 2000m.

We do not see any sign of DSWs in these images; there is not enough detail, and as the reviewer suggests, a map in pressure levels might not be the most appropriate for the task.

The D01 domain is centered in the Isthmus. It includes a significant marine portion in the south because of large extent of the Tehuantepecer outflow into the ocean, as the reviewer can observe in Figure 3a. It is convenient to set the boundary relatively far downstream from the area of interest to avoid numerical problems related to the imposed lateral boundary conditions, such as wave reflections that can perturb the solution within the domain.

9- Figure 3: *Results are impressive but not unexpected. Moreover, legend for panel (d) is missing.*

We are glad that the reviewer finds these results impressive. They might be expected for someone with the level of expertise of the reviewer, but we believe that the general audience of the journal and especially those with interests in the area will find them revealing.

The missing legend for panel (d) will be included in the revised version. Thank you for noticing.

10- P8, I8: *What is the Rossby radius of deformation here.*

If we consider the depth of the surge to be around 2500m and the Brunt Väisälä frequency 0.012 s^{-1} , the Rossby radius of deformation is approximately 750 km. For comparison, the Isthmus is about 200 km across.

Clarke (1988) argued that since pressure gradients are so weak over the Gulf of Tehuantepec, the wind should follow a close to inertial trajectory. The radius of this inertial path for an outflow velocity of 25 m/s and 15N latitude is 662 km, which appears to be in the range of what we see in our simulations, at least along the main outflow axis, where the cross-flow pressure gradient is very small. Steenburgh et al (1988) found this to also be the case, as they discuss in detail the balance of forces for the gap outflow over the Gulf of Tehuantepec, explaining its fan like structure and curvature. In our work, as noted before, the focus is not on the gap outflow, but on the smaller scale extreme wind phenomena occurring in the Isthmus. We will, nevertheless, mention the reasons for the outflow's shape in the introduction section when briefly commenting on the effect of planetary rotation (see response to main concern 1, above).

Clarke, A. J., 1988: Inertial wind path and sea surface temperature patterns near the Gulf of Tehuantepec and Gulf of Papagayo. *J. Geophys. Res.*, 93, 15 491–15 501

11- P8, L9: *Are you here referring to the cross-over in the T-shaped structure? Might this is the DSWS? There seems to be a tremendous along-flow convergence/divergence here.*

Yes, exactly, we are referring to the strong flow acceleration represented in Figure 3b, with a T-shaped structure. As we show in detail later on, this is where the DSW occurs.

12- P9, I2: *Here you argue that this is a "flow thinning" event and not a gravity-wave breaking downs-slope flow event, but later it is the opposite.*

The flow thinning we are referring to here is that occurring under the dividing streamline generated by gravity-wave breaking to the lee of the mountain. We will remove "flow thinning" from this sentence to avoid confusion with the one happening for example in the case where there is an inversion layer close to the mountain top.

13- P9, I4: *And where is NP? Moreover, this analysis (d-panel) would have been much more informative if you had analysed the depth of the cold air and plotted it as a contour plot, showing its geographical distribution*

We agree with the reviewer's suggestion, in the revised version we correct Figure 3, better explaining where NP point is and introducing the caption for Figure 3d. Figure 3d contains more

information than just the depth of the cold pool, which is relatively homogenous in the area, as seen for example in Figures 6b and 6c. It shows the stability of the column and the vertical wind profile. We prefer to maintain the current panels in Figure 3, but we will add a comment about the depth of the cold air in position NP being similar to that closer to the mountains.

14- P9, I5&6: *If you want to use this type of sounding plot, you need to tell the reader what's on the axes. If the gray transverse lines are isotherms, there is hardly any inversions at all; to me the lower one looks more like an isothermal layer while the upper (subsidence) may be a weak one.*

The plot is a skew-T log-P diagram, very commonly used to represent upper level soundings and quite standard in weather analysis. Perhaps it may be confusing that it is lacking wet and dry adiabats? We will add the dry ones, which are relevant for the discussions. We will specify it is one of such diagrams in the text and add further details about the barbs in the wind profile and their scale, which is missing in the caption.

The reviewer is right in that the inversions are fairly weak, especially the lower one. We will replace the wording “shows the depth of the cold air pool, defined by the inversion existent at about 800hPa or 2500m in the temperature profile” by “shows in the temperature profile a stable lower boundary layer capped by a very stable isothermal layer from 850hPa up to about 800hPa, or 2500m, defining the depth of the cold air pool”

We will also change the word “inversion” by “very stable isothermal layer” in other instances of the text discussing the depth of the cold air pool.

15- P9, I19: *Awkward; what do you mean by "far reaching"?*

We mean “reaching out as far as the mountain gap winds do”. In the revised version of the manuscript we will rewrite this sentence to make it clearer for the reader.

16- Figure 4: *Show the modeled wind speeds already here and save a plot later*

With this figure we want to show the whole Tehuantepecer event duration (from the 23rd of the December to the 29th), as reflected by the observations. We also show some days before and after to contrast Tehuantepecer winds with the typical flow regime in the area. Furthermore, the figure provides justification on the period chosen for analysis as the 36h interval of maximum intensity (highlighted in both observational timeseries). We consider that including model results here for the purpose of validation would distract the reader by making the figure rather messy, so we prefer to leave it as it is, and keep the validation in a separate figure for a shorter interval allowing much more detail.

17- Section 3.2: *drop first sentence; we just read about that, no need to repeat.*

We agree we reviewer's comment, in the revised version of the article we will drop this sentence.

18- P10, I67; *Awkward English; what is a "wind path"?*

We mean “encompassing the flow path before and after crossing the mountains”, we will correct this in the revised manuscript.

19- P10, line 19; *"steeper" than what?*

We wanted to say: ‘asymmetric mountain with steeper lee than windward side’, we will change this in the revised version.

20- P10, I23-24 and elsewhere: *I don't dispute the wave-breaking argument, but how can you see this here? There are no temperature-gradient reversals that I can see, nor is there any TKE aloft that would result from it. I would have expected to see at least truly vertical isotherms and elevated layers of TKE, or gradient reversals and no TKE.*

Vertical isentropes are more clearly seen at early stages and in the case of the higher mountain (see Fig 5d), but they are also present in Fig 5a for the lower mountain. They occur in the area that appears dark blue later on (Fig 5g and 5j) showing very low or close to zero windspeeds, and where isentropes are split apart from each other, indicating a well-mixed region. Overturning isentropes and temperature-gradient reversals are observable in the case of the higher mountain only, in Fig 5d. We will include as supplementary material an animation of figures like those in Fig 5 to show the process of development of the DSWs. In the animated sequence wave breaking is more apparent.

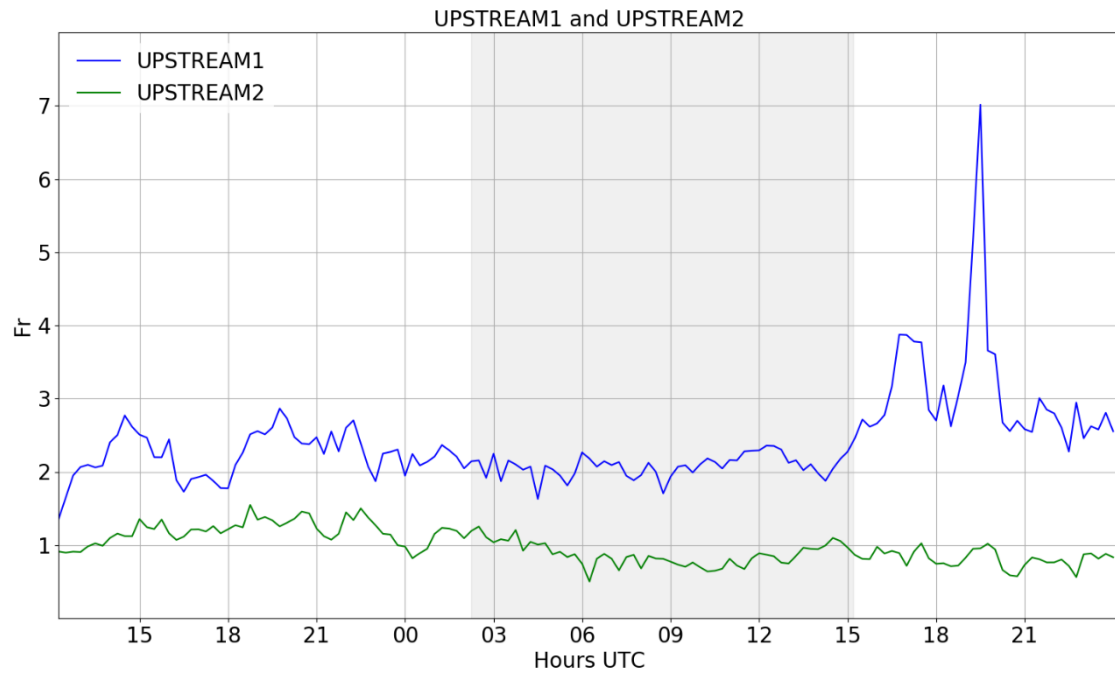
We agree with the reviewer in that there should be higher TKE values in the same layers where wave breaking occurs, but this variable is the result of the PBL parameterization in the model and thus it only considers subgrid scale variations in wind speed due to turbulent eddies, as represented in the scheme. The turbulence due to wind shear in the column is well captured; however, the turbulence associated with gravity waves, due to rotors and non-local turbulence advection is not represented and accounted for, because a much higher horizontal resolution of the order of tens of meters would be needed. This problem in numerical models is well known (see for example the review paper by Vosper et al. 2018) and we will introduce a comment about it in the revised version of the manuscript.

Simon B. Vosper, Andrew N. Ross, Ian A. Renfrew, Peter Sheridan, Andrew D. Elvidge and Vanda Grubišić, 2018: Current Challenges in Orographic Flow Dynamics: Turbulent Exchange Due to Low-Level Gravity-Wave Processes. *Atmosphere*, 9, 361. doi:10.3390/atmos9090361

21- P10, I28: *What do you mean by "bounded by turbulence"?*

We are describing Figure 5i disposition of TKE highest values, in shaded green. We mean "confined by layers of strong wind shear and turbulence", we will reword the sentence in the revised text.

22- P10, I31 *The use of Fr is a powerful but yet blunt instrument to analyze these flows. I have two concerns here: 1) As the air has propagated up til crest of the terrain, Fr is already modified. The classical analysis by Durran, cited earlier, also uses the upstream Fr before the flow has hit the terrain; not that at the top of the hill. Hence I would have liked to see the truly upstream Fr instead; not the value that has already been modified. 2) Is it certain that the air reaching the observation stations actually comes from the point directly north of the station? A trajectory analysis would clarify the 3D dynamics of the flow.*



The figure above shows the Froude number at upstream points 1 (low mountain) and 2 (high mountain). The values are not so different from those shown in the paper, with Fr close to 1 upstream of the high mountain where the strong HJ forms and around 2 in the case of the low mountain. We will now include the calculation of Fr upstream instead of at the top of the mountain as per the reviewer's suggestion.

With regard to the question about the upstream trajectory of the air reaching the observation stations, we do not fully understand the reviewer's concern. The wind is from the north and quite steadily at low levels, so we consider that the cross sections in the north-south direction serve very well the purpose of showing the vertical structure of the flow as it crosses the mountains.

23- Figure 5: *To much information in too many too small panels. In fact, you could easily get rid of one third, by plotting the TKE and w in the same panels, as you do with temperature and winds.*

Because of the relation between TKE and wind shear we would prefer to keep the panels showing both TKE and wind speed contours. Adding vertical velocity as an extra layer would make them very difficult to read. We think that is best to keep Fig 5 as it is, unless the reviewer has a very strong objection. With its high resolution, the figure can be readily expanded to reveal all fine scale details very clearly.

24- P11, 11: *The position of the hydraulic jump, which is far from very distinct to begin with, hardly makes any propagation clear. Instead, analyze the position of the jump at different times and plot the position, and maybe also its strength, as a function of time. Maybe also as a function of Fr. From this the reader cannot really see any propagation.*

The small hydraulic jump lies around latitude 16.40 in Fig5a, but it is only easy to spot if one sees the sequence in motion. We will now point at the animation in the supplementary material (see response to comment 20 above) instead of Fig5a alone when commenting on this modest hydraulic jump.

25- P12, line 1 Use "indicating" instead of "signalling"

We agree with reviewer's suggestion. We will use "indicating" in the revised version of the article.

26- P12, l5; "further high" is awkward.

The sentence "Stability is much reduced further high,..." will be rewritten as "Stability is sharply reduced aloft..."

27- P12, l9: Can't see any wave overturning in these plots. This may be because mixing erodes overturning isotherms before they can be seen in the model output. In that case there should be TKE there, which I can't see either

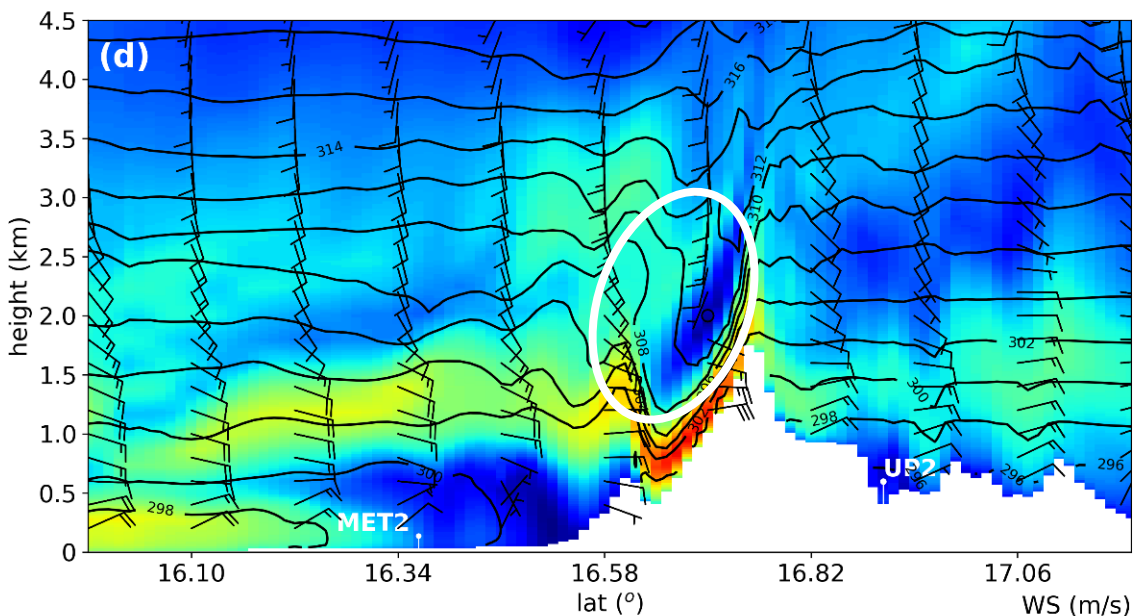


Fig 5 d from the paper is shown above with a white oval highlighting isotherm overturning, clearly visible for $\theta = 308$ K and $\theta = 310$ K to the lee of the mountain. We will add a similar marking in the revised manuscript.

The reviewer is right in that mixing erodes isotherm overturning very quickly, both with resolved vertical motions responding to the created instability and by subgrid turbulent eddies from the PBL scheme. The TKE variable is only reflecting the latter contribution; therefore, it has a smaller value than it should. In addition, the PBL scheme is more designed to account for convective instability and turbulent eddies rising from the surface due to heating, than to deal with instability resulting from gravity wave overturning, like what we see here. Thus, it is also underestimating TKE in this case. As mentioned in the response to comment 20, this is still an open issue in numerical modeling. We will add a comment about it in the revised version of the manuscript.

28- P12, l30: Awkward; "slightly ... d05".

We agree with reviewer. In the revised version, we replace the sentence "...slightly better so those from d05,..." by "...slightly better in d05 case ..."

29- P12, l34: Also awkward; "which situations".

We will replace "which registers HJ situations" by "downwind from the strong HJ"

30- *Conclusions contain far reaching statements that cannot be substantiated by one case study*

We will tone down some of the concluding statements. In particular, we will remove the ending sentence "It is likely that the depth of the cold pool and how it compares with topographic barrier height, is a key factor determining the extent, location and intensity of these lee wave phenomena and whether they take place at all", which we agree it is unsubstantiated from just one case study.