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Earth System Dynamics Discussions

Interactive comment on "Downslope windstorm study in the Isthmus of Tehuantepec using WRF high-resolution simulations" *by* Miguel A. Prósper et al.

Anonymous Referee #3

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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.

I have tw0 main concerns: 1)he analysis if the model results is rather superficial and make 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.

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.

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

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

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

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

P3, l25: What do you mean by microscale?

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

P6, l3: How much of a spinup is required before the model physics is realistic?

P6, l18: 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.

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

P8, l8: What is the Rossby radius of deformation here.

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.

P9, l2: Here you argue that this is a "flow thinning" event and not a gravity-wave breaking downs-slope flow event, but later tit is the opposite.

P9, l4: 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

P9, l5&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 lwoer one looks more like an isothermal layer while the upper (subsidence) may be a weak one.

P9, l19: Awkward; what do you mean by "far reaching"?

Figure 4: Show the modeled wind speeds already here and save a plot later.

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

P10, l67; Awkward English; what is a "wind path"?

P10, line 19; "steeper" than what?

P10, l23-24 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.

P10, l28: What do you mean by "bounded by turbulence"?

P10, l31 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.

Figure 5: To much information in too 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.

P11, l1: 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.

P12, line 1 Use "indicating" instead of "signalling".

P12, l5; "further high" is awkward.

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.

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

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

Conclusions contain far reaching statements that cannot be substantiated by one cas study.

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