NHESS-2021-196
Authors' Responses to Reviewer 2 (RC2, anonymous)
Date: 22 Nov 2021

Title: Idealized Simulations of Mei-yu Rainfall in Taiwan under Uniform Southwesterly Flow using A Cloud-Resolving Model
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1. General comments:

This study presents findings from idealized simulations for the island of Taiwan in which a uniform southwesterly flow is prescribed at fixed directions/speed combinations to investigate rainfall characteristics in the absence of large-scale frontal systems. In addition, near-surface relative humidity is varied and a subset of the simulations has been compared to observational data. The authors identify three rainfall regimes that correspond to different ranges of the wet Froude number and possible mechanisms for the resulting precipitation location and intensity are hypothesized. Although the paper is mostly well written and the illustrations have a good quality, it is a bit hard to see the innovation of this paper. The main result regarding the dominant process for rainfall production through mechanical uplift or thermal forcing is pretty much expected and there is a lack of evidence for their hypotheses. Although I like the general concept of idealized simulations using a real topography, I find the implementation and the connection to previous work for other islands unsuccessful so far. I would welcome a revised paper that is more physics-based, but that would probably involve substantial additional work and rewriting of the paper.

Reply: The constructive comments from this reviewer (Reviewer 2) are deeply appreciated, and the paper has been revised accordingly. In the revision (color-coded version), the changes made in response to Reviewer 1, Reviewer 2, and by ourselves (mostly some corrections and minor changes in English) are marked in red, blue, and orange, respectively. A point-by-point response to each of the comments from this reviewer are given below following their order. In each point, how and where the revision is made in the text is also specified.

2. Specific comments:

1) Experimental design:

- Why do you restrict the ow direction to SW? Is the southwesterly ow during that time of the year dominant? How often are there situations without the Mei-yu front? This important information needs to be given to determine if the model setup is representative or not.

Reply: As reviewed in Section 1, heavy rainfall in Taiwan during the Mei-yu season occurs predominately under the southwesterly flow regime (tropical air mass with abundant moisture). Thus, we focus on these wind directions in the present study. In the revision, the above reasoning is better conveyed to the readers more clearly (L23-24, L27, L33, L36, L51, L111, L663-664) as suggested.

- Are the fine steps of 2.5 m/s and 15 degrees really necessary? Would not a larger range of ow direction with larger steps (e.g. 5 m/s and 30 degrees) be more informative? For example, in the study of Metzger et al. (2014), the incoming wind direction for the island of Corsica has been changed in steps of 30 degrees to cover the all possible wind directions. Although Corsica is a smaller island, these previous results should be cited in this study. Furthermore, within the framework of the HyMeX project, several other publications covering island convection and terrain effects were published.

  Metzger, J., C. Barthlott, N. Kaltho (2014): Impact of upstream ow conditions on the initiation of moist convection over the island of Corsica, Atmos. Res. 145-146, 279-296, DOI:10.1016/j.atmosres.2014.04.011

Reply: Thank you for this opinion. Since we focus on southwesterly flow directions in the present study, finer steps of 2.5 m s^{-1} and 15° are used, as described in Section 2.4. Please see our reply to the bullet point above. In the revision, the study of Metzger et al. (2014) and another relevant paper (Kirshbaum, 2011) are also cited for comparison (L103-104, L283, L285-286, L311, L586, L611-612), as suggested.

- Why do you use an integration time of 50 h? Would not 24 h be sufficient? Which day do you take for the analyses in Fig. 7 etc.? Day 1, day 2 or the mean of both?

Reply: As discussed in Section 2.4 and shown in Fig. 6 for a few examples, two similar diurnal cycles are produced in each run during t = 2-50 h (L181-182). While the differences may be small, it is most likely still a little more representative to use the averages of two days (2-50 h), compared to the accumulation over a single day (2-26 h). In the revision, it is also clarified that the averages over days 1-2 (or 2-50 h) are shown in Fig. 7, both in the caption and at the end of Section 2.4 (L181-182, L371, L758), as suggested.

- What boundary conditions are applied in the model? Open, periodic?

Reply: In the revision, it is clarified that open boundary conditions are used (L161), as suggested.

- Deep convection is considered to be resolved at 2-km grid spacing, but is shallow convection still parameterized? If yes, how? Please specify.

Reply: No, shallow convection is also handled by the 1.5-moment bulk cold-rain scheme and not parameterized in CReSS. This is clarified in the revision (L149), as suggested.

2) I do not understand why the Froude number changes with the wind direction. If the wind speed does not change and the mountain height is constant as well, the Froude number should be independent of the ow direction unless the stability is changed. The authors should make an effort to explain how they calculate their Froude number in detail (spatial average, at what time, ...).

Reply: The Froude number (F_r, $F_r = U/Nh_0$) changes with wind direction because U is the speed of wind component normal to the long axis of topography, and this is clarified in the revision (L42, L100, L189-190), as suggested. Thus, even with strong flow, the F_r would still be small if the wind is parallel to an elongated topography like Taiwan (e.g., 195° in Table 3). However, if the topography is bell-shaped and does not have a long axis (as adopted in many earlier studies), the wind direction then indeed does not affect F_r. In several places in the text, this is also made clearer to the readers (L46-47, L100, L229, L259-260, L416-417), along the lines as suggested.

3) The authors speculate about the involved processes, i.e. terrain uplift and/or sea breeze/thermal circulations. None of these are assessed or proven in a quantitative way. Only for the CTL-run presented in Fig. 5, there is some evidence by the streamlines. I suggest to include additional material, e.g. low-level moisture convergence for establishing the impact of sea breeze on island convection.

Reply: For the three cases shown in Fig. 6, a new figure (Fig. 8) is added in the revision to show the diurnal variations at the times of the peak amplitude in surface warming/cooling with discussion, including that on the daytime sea breeze (L243-245, L252-253, L285, p.33, L763-767), along the lines as suggested.

4) Fig. 6: Observed precipitation starts to increase at around 20 UTC and reaches a plateau between 22-05 UTC before it further rises to the maximum value at 07 UTC. What mechanisms are responsible for the plateau?

Reply: We have checked the rainfall data and radar/satellite loops on those dates used to construct the observed cycle in Fig. 6 (as also better clarified in the caption). In the revision, it is explained and clarified that the plateau structure (about 0.5 mm h^{-1}) was mainly from migratory rainfall systems from upstream on two of the days (29 May and 4 June) during 2200-0500 UTC (0600-1300 LST), and by design, such systems are largely absent in our idealized simulations with uniform flow and no disturbances (L220-226), as suggested.

5) L71: What are "unwanted features"? Please specify.

Reply: In the revision, "unwanted features" is rephrased to "undesirable features" to improve clarity of the sentence (L73), along the lines as suggested.

6) The intercomparison to observations mostly shows a bad agreement between simulations and observational data (Fig. 10, 11, 12). Either the environmental conditions in the dates chosen do not match the model settings or other processes are missing in the model. I suggest to run realistic simulations with initial and boundary conditions from an operational model or other global analyses for these cases.

Reply: Indeed, some processes other than those associated with Taiwan's topography must exist (and cannot be avoided) in real conditions, such as frontal forcing, various disturbances, and low-level convergence from non-uniform flow, and even deviations from the prescribed profile and state. All these differences are not included in our idealized simulations by design. In the revision, the above points are stressed as caveats in sections 3.1 (L220-226) and 5 (L358-360, L365-369, L395-396, L428-435), along the lines as suggested. In the comparison between idealized simulations with real events of choice (when the conditions are relatively pure) in Section 5 of the previous draft was not successful, mainly because we didn't include the right data for comparison. In the revision, satellite cloud imageries at selected times are also provided (together with radar composite and the derived rainfall estimate), and they are much better to validate the model simulations (L194-195, L371, L374-378, L380-396, L402-414, L416-435, L565, L569-571, Figs. 11-13, p.36-38, L779-786, L789-791, L795-797). The reasons why both the rain-gauge data (used in previous draft) and radar composites cannot capture the convection/rainfall along the eastern slopes of the CMR are provided (L383-387, L527-529, Fig. 2b, p.24, L707-711). At various places where needed, caveats are also added or stressed in the revision to clarify possible (or likely) differences between the model results and observations (L220-226, L365-369, L381, L395-396, L408-414, L424-426, L428-435), as suggested. For heavy-rainfall cases, modeling studies using gridded analyses and full physics have been carried out, and are also cited in the revision (L425-426, L431-433), as suggested.

3. Technical corrections:

1) L15: local afternoon ~~during daytime~~

Reply: Deleted as suggested (L15).

2) L17: This sentence needs to be rephrased. What is a "large angle"?

Reply: This sentence is broken down into two sentences to improve the readability, and it is also clarified that "large angle" means not parallel (L17-18), as suggested.

3) L40: Blumen, 1990: Blumen is the book editor for Banta (1990). Do the authors mean the Banta article here?

Reply: Yes, the reference is meant to be Banta (1990) here. It is now corrected in the revision (L41), as suggested.

4) L45: orographic precipitation can often be resulted → please rephrase

Reply: This sentence is rephrased to "… to climb over the terrain and orographic precipitation is often resulted…" to improve the readability (L46-47), as suggested.

5) L60: Wang et al., 2002, 2003: For these years, there are only entries in the references for Wang and Chen (2002, 2003).

Reply: Corrected to Wang and Chen (2002, 2003) here (L63), as suggested.

6) L80: $Fr \rightarrow F_r$

Reply: Corrected as suggested (L82).

7) L144: Murakami et al. (1990, 1994) → Murakami (1990), Murakami et al. (1994)

Reply: Corrected as suggested (L151).

8) L147: Sagami → Segami

Reply: Corrected as suggested (L155).

9) L174: Chen and Lin (2005): Which entry is meant here, 2005a or 2005b?

Reply: Corrected to Chen and Lin (2005b) here (L185-186), as suggested.

10) L184: The results of the CTL-run is ...

Reply: Revised as suggested (L199).

11) L185: it ~~behaviors~~ behaves as designed.

Reply: Revised as suggested (L200).

12) L235: regime~~s~~

Reply: Corrected as suggested (L257).

13) L330: ...this is resulted because... → please rephrase

Reply: This sentence is rephrased to "… Nevertheless, with a reduced RH, the convection becomes more difficult to be triggered and thus less active at the windward side, and thus a lowered peak amount and a shift in its sub-region are resulted" to improve the readability (L353-354), as suggested.

14) L565: Miguietta → Miglietta

Reply: Corrected as suggested (L613).