NHESS-2021-196
Authors' Responses to Reviewer 1 (RC1, anonymous)
Date: 22 Nov 2021

Title: Idealized Simulations of Mei-yu Rainfall in Taiwan under Uniform Southwesterly Flow using A Cloud-Resolving Model
Authors: C.-C. Wang, P.-Y. Chuang, S.-T. Chen, D.-I. Lee, and K. Tsuboki

1. General comments:

The authors used a cloud-resolving model to investigate the role of the complex topography in Taiwan on rainfall characteristics during the Mei-Yu season without the influence of fronts or disturbances. They initialize the model using horizontally uniform flow without vertical shear with different wind speeds and directions. They characterized their rainfall regimes based on the wet Froude number (*Frw*). For the low-*Frw* regime, rainfall production is dominated by thermal forcing from the surface, whereas for the high *Frw* regime, the mechanical uplift of unstable air becomes important. Between these two regimes, the mixed regime exists for intermediate *Frw* number. They also compare their model results with real cases in Section 5. The manuscript is fairly well written. However, there are significant major concerns the authors will need to address before this work can be accepted for publication.

Reply: The constructive comments from this reviewer (Reviewer 1) 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. Major comments:

1) This study is lacking well-defined scientific objectives. It is well-known that under weak wind conditions, thermally driven diurnal circulations are important, whereas under strong wind conditions with a large impinging angle, mechanical uplift becomes significant. What is new?

Reply: Thank you for the suggestion. In the revision, a well-defined scientific objectives is clearly stated in Section 1 (L97-99), as suggested. Compared to previous studies for Taiwan in the literature, the main differences in the present study are the use of 3D framework with different wind directions, real topography, and also the inclusion of (diurnal) thermodynamic as well as the Coriolis effects. Thus, different rainfall regimes and the range of Froude number (F_r) for each of them can be identified with a better agreement with real conditions. This has not been done before for Taiwan. In the revision, the above arguments are better and more clearly conveyed to the readers (L88, L102-104, L215, L298-299), also as suggested.

2) The Froude number has been used to classify flow regimes (flow over vs blocked flow regimes) for airflow over an isolated mountain by many authors. Compared with classical theoretical studies, that use a bell-shaped mountain, this study uses the realterrain of Taiwan. However, the results from a series of numerical experiments in this study simply confirm this well known fact.

Reply: Thank you for the suggestion. Compared to previous studies for Taiwan in the literature, the main differences in the present study are the use of 3D framework with different wind directions, real topography, and also the inclusion of thermodynamic (diurnal effects) as well as the Coriolis effects. Although the flow is still idealized, the results are more applicable and comparable to the observations at least to a reasonable extent (Section 5, please see our reply to major comment #4 below for detail description). In the revision, the above points are better clarified and conveyed to the readers (L88, L97-99, L102-104, L215, L298-299, L442), along the lines as suggested.

3) The authors fail to state the theoretical basis or hypothesis to invoke Froude number theory for the rainfall regimes.

Reply: As reviewed in Section 1, the Froude number theory has been applied and linked to different rainfall regimes in Taiwan in some earlier studies (e.g., Chen and Lin, 2005a,b), so the present study is not the first one to do so. However, in the revision, the general linkages between F_r and rainfall regimes in Taiwan (orographic precipitation in high-F_r regime and rainfall from island circulation and thermodynamic effects in low-F_r regime) is better clarified (L53-54, L58-60), as suggested.

4) In Section 5, the authors attempt to compare their model simulations initialized by a single upstream sounding to real cases of heavy rainfall events during the Mei-Yu season over Taiwan.

This is simple minded. Heavy rainfall events in many different parts of the world are related to synoptic and mesoscale processes in addition to orographic effects (G. Chen 1983, JMSJ; Doswell, 1987, WAF; Doswell et al., 1996, WAF; Maddox et al., 1979, BAMS; and many others). Without including these processes in the models, it

is unlikely that numerical simulations initialized by a single sounding will be able to simulate these events.

Reply: Thank you for this suggestion. The comparison between idealized simulations with real events of choice (when the conditions are relatively pure) in 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, L778-786, L789-791, L795-797), and 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, L365369, L395-396, L408-414, L418-419, L424-426, L428-435), along the lines as suggested. For heavy-rainfall cases, previous modeling studies using gridded analyses and full physics are also cited in the revision (L425-426, L431-433).

5) The authors state that the wet *Fr* number is very close to the *Fr* number. Thus, in terms of flow regimes, the moisture is not important. However, for heavy rainfall events, moisture availability is a significant parameter for rainfall production and may be more important than the variations in the Froude number for $Fr < 1$.

Reply: Thank you for this comment and we agree. In the revision, it is clarified that F_{rw} (and F_r) applies, strictly speaking, only to stable conditions with $N_w > 0$ (L190-191), and we also added that the moisture content near the surface affects the instability and rainfall production (L171), both as suggested.

6) The upstream sounding used is horizontally uniform with very little vertical wind shear below the 500-hPa level. Is this a typical sounding for heavy rainfall events over Taiwan? Do soundings

in the warm sector of Mei-Yu systems exhibit clockwise turning with respect to height due to warm advection?

Reply: As described in Section 2.1, the sounding profile used in CTL is the mean from seven profiles at 0000 UTC and thus is typical in the Mei-yu season, but not necessarily conducive to heavy rainfall (as heavy rainfall occurred only in some of the sampled days). The rainfall information on these seven occasions are added as suggested (L117-118). The profile indicates only weak veering with height from 950 to 500 hPa (Figs. 3a,c). In the revision, the above points are better described or clarified (L111, L119-121), also as suggested.

7) Except in the lowest levels, the thermodynamic profiles used seem rather dry. Is this typical for the heavy rainfall soundings during the Mei-Yu season over Taiwan?

Reply: As noted in the reply above (to point #6), the sounding profile used in CTL is the mean from seven profiles at 0000 UTC and thus is typical in the Mei-yu season but not necessarily conducive to heavy rainfall (L111). In the revision, rainfall information associated with the sampled days are added in Section 2.1 (L117-118) as suggested. For more moist conditions at low levels, the effects of changing near-surface RH to 92.5% and 100% are also tested in this study, and relevant results are discussed in Section 4.1 (L333-345).

8) The authors use observed SST as the lower boundary condition over the open ocean. The reviewer presumes there are spatial variations in SST in this region. How would the spatial variations in SST affect the horizontal distributions of thermodynamic fields in the mixed layer and the depth of the mixed layer? Are those being considered in the model initial conditions?

Reply: As described, the time-mean of NOAA analyzed SST (with spatial variations) for the period of May-June 2008 are provided at the lower boundary, coupled with a substrate model (down to a depth of 40 m). In the revision, the above configuration is better clarified (L144, L155-156) as suggested.

9) Each simulation was run for 50 hours and the first two hours are considered as the model spin up period. How is the initial spin up period determined?

Reply: In the revision, it is clarified that the spin-up period of 2 h is determined for the flow in the model to adjust to the topography in Section 2.4 (L180), as suggested.

10) To address the effects of thermal forcing from the land surface, it is imperative to describe the lower boundary conditions over land used in the model. The authors should also compare the simulated diurnal variations in temperature, winds and rainfall with observations very carefully. Fig. 4 shows simulated rainfall on the eastern leeside in the afternoon hours which is odd. It fails to show the effects of orographic uplift on rainfall production.

Reply: In the revision, the lower boundary conditions, including the SST and the substrate model (both over land and ocean) are better clarified (L144, L155-156) in Section 2, as suggested. In Section 5, examples of our model simulations and real events are compared (please also see our reply to major point #4 above), and they agree on the convection (confirmed in satellite imageries) along the eastern slopes of the CMR, when F_r is relatively small including the mixed regime (L311). The results of leeside convection are also consistent with Metzger et al. (2014), which is cited in the text in the revision (L103-104, L283, L311, 611). In addition, comparison of diurnal effects in the temperature simulations for the cases shown in Fig. 6 is added in Fig. 8 (a new figure, please also see our reply to the major comment #11 below) in the revision together with discussion (L243-245, L252-253, p.33, L763-767), along the lines as suggested.

11) Areal averaged rainfall shown in Fig. 6 is inadequate. There must be large spatial variations in rainfall throughout the diurnal cycle. What are the days used for observations (gray curve) in Fig. 6? How often do you observe southwesterly winds > 20 m s^{-1}?

Reply: The spatial variations of rainfall in the CTL experiment is shown in Fig. 4, while the other two model curves in Fig. 6 are shown as examples to demonstrate that large (little) diurnal variations exist under low-F_r (high-F_r) regime. For the three cases shown in Fig. 6, a new figure (Fig. 8) is added to show the diurnal variations at the times of the peak amplitude in surface warming/cooling with discussion (L243-245, L252-253, p.33, L763-767), along the lines as suggested. In the revision, the dates used for the observed diurnal cycle in Fig. 6 are clarified both in the text and caption (L220, L747), and the observed peak strength of the southwesterly LLJ in the Mei-yu season (rarely exceeds 22.5 m s^{-1}) are also provided with references (L165-166, L520-521), both as suggested.

3. Minor points:

1) The figure caption for Fig. 2b is very confusion.

Reply: The caption of Fig. 2b is revised and clarified (L709-711), as suggested.

2) Figure 4: Should provide information on local time. The authors should also adjust the color table.

Reply: The information of local standard time (LST) is added in the caption of Figs. 4 and 5 (L733-734, L739-740), as suggested. The color table of Fig. 4 is also revised and updated (p.27, L729-730) as suggested.

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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

Authors: C.-C. Wang, P.-Y. Chuang, S.-T. Chen, D.-I. Lee, and K. Tsuboki

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, L402414, 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).