NHESS-2021-196
Authors' Responses to Reviewer 2 (RC2, anonymous)
Date: 30 Mar 2022

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 submitted version of the manuscript has been significantly improved over the first version. The authors have addressed all of the prior concerns to my satisfaction. I therefore believe that this article can be accepted for publication after very minor modifications which are outlined below:

Reply: The positive view and constructive comments from this reviewer (Reviewer 2) are deeply appreciated, and the paper has now been revised accordingly. In the revision (color-coded version), the changes made in response to Reviewer 2 and Reviewer 3 are marked in blue and green, 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.

Minor comments:

1. p13, L412: "when the convection is relatively clean" --> what is clean convection supposed to be, do the authors mean scattered convection?

Reply: In the revision, this sentence is clarified to "…especially on radar and satellite images at one selected time (when the convection is less widespread)" (L414-415), along the lines as suggested.

2. Fig. 12: last plot should be labeled (h) and not (g)

Reply: Thank you for point this out. In the revision, the label has been corrected (Fig. 12, panel h) as suggested.

NHESS-2021-196
Authors' Responses to Reviewer 3 (RC3, anonymous)
Date: 30 Mar 2022

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

General comments:

In this study, the authors have conducted idealized simulations using a cloud-resolving model with a horizontally uniform, southwesterly flow to investigate rainfall characteristics, moist flow regimes, and the role of the complex topography in Taiwan during the Mei-yu season in the absence of Mei-Yu fronts or other weather systems. The design of idealized simulations on testing different moist flow regimes is excellent and the paper is well-written. Thus, except for the minor comments described below, I would recommend this paper be accepted with minor revision.

Reply: The positive view and constructive comments from this reviewer (Reviewer 3) are deeply appreciated, and the paper has now been revised accordingly. In the revision (color-coded version), the changes made in response to Reviewer 2 and Reviewer 3 are marked in blue and green, 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.

Minor Comments:

Line 59: What kind of thermodynamic effects of the topography? The authors need to clarify it.

Reply: Here, we meant the thermodynamic effects just reviewed in this paragraph, and this is clarified in the revision (L59), as suggested.

Line 90: "The long-term climatology (1981-2010) reveals abundant Mei-yu 90 rainfall in the two-month period of May-June, with three maxima: two on the windward side of the Central Mountain Range (CMR) in southern and central Taiwan, respectively, and the third, less distinct center in northern Taiwan, roughly along the northern slope of the Snow Mountain Range (SMR)" – I would prefer to justify this sentence with the reference.

Reply: Here, we were referring to the climatology shown in Fig. 2a, but we forgot to cite the panel explicitly. In the revision, this is clarified and Fig. 2a is cited (L90, L93), along the lines as

suggested.

Line 118: “Shown in Figs. 3a-d, the averaged thermodynamic, moisture, and wind profiles in the vertical from these data indicate a rather uniform south-southwesterly flow (8-13 m s-1) that veers slightly with a height from the lower to middle troposphere” This sentence is not clear (see highlighted in blue), which needs to be reworded.

Reply: In the revision, it is clarified that the mean profiles here (and shown in Figs. 3a-d) are from the seven selected soundings as just described (L118-119), as suggested.

Line 164-165: What about high wind with speeds more than 22.5 ms^{-1}? Do you have any point/explanation about if there is a high wind speed, e.g., 25, 30, 35 ms-1…..etc.?

Reply: The observed limit in wind speed near Taiwan during the Mei-yu season is reviewed here, and the reason to set the highest wind speed to 22.5 m s^{-1} is clarified in the revision with reference to two more studies already included in the list (L167-168), along the lines as suggested.

Line 172: Why did the authors choose these specific wind directions (210º, 240º, and 270º) and wind speeds (10, 15, and 20 m s^{-1}) to examine the moisture effects? Why not rest of others' direction and speeds? Are there any specific reasons? If there are any, it is better to explain here.

Reply: In the revision, it is clarified that the additional tests on moisture are a subset of those designed to test wind direction/speed combinations, and it is so chosen without adding a large number of extra experiments (L172-175), as suggested.

Lines 219 to 222: Authors mentioned that the CTL case produced poor results compared to the observation. However, the c050_210 case produced better results than the CTL case when compared with Obs. Why does c050_210 produce better results? I tend to think CTL should produce better results than other cases.

Reply: In the manuscript, the reason for the difference in rainfall production in the CTL and the observation is explained (L220-227), and in the revision, it is also pointed out that the c050_210 case, with more daytime rainfall, produces a diurnal cycle in Fig. 6 that is more similar to the observation than the control run (L247-248), along the lines as suggested.

Table 1: Is there any specific reason to use Lambert conformal projection instead of Mercator projection, which is considered to be better for this region? Any reason needs to be mentioned/explained in the model and experimental part of the manuscript.

Reply: In the revision, it is clarified that the CReSS configuration given in Table 2 (including the projection) is similar to those adopted in previous studies with references cited (L163-164), along the lines as suggested.

Table 3: Why are the values of moist Froude number for the case with 195° direction almost constant (~0.01) for all varying wind speeds (5 to 22.5 m s^{-1}) cases?

Reply: At a fixed angle and the same stability (i.e., Brunt-Vaisala frequency N), the values of F_{rw} would be proportional to the wind speed. At a wind direction of 195°, the values increase from about 0.01 at 5 m s^{-1} (and 7.5 m s^{-1}) to 0.03 at 22.5 m s^{-1} in Table 3, so they are small but not constant (as the value is roughly tripled like the wind speed). The values are small because the wind direction is nearly parallel to the topography (so the normal component is nearly zero), and this is already explained in the manuscript (L230-231). In Table 3, the values (for 195°) only appear to change little because they are rounded to two places below the decimal (not more places to better tell the differences).

Table 4: Authors found the moist Froude number for the case with 195° direction almost constant (~0.01); however, the mean daily rainfall decreased; why?

Reply: As the flow is nearly parallel to the topography, all cases with 195° direction belong to low-F_{rw} regime and the relevant discussion on the reasons for the rainfall decrease with increasing wind speed is in the second paragraph of Section 3.2 (L238-248). In the revision, to better clarify, it is explicitly pointed out that all cases with 195° direction belong to this regime (L241), along the lines as suggested.

Table 6: What does S, P, and M stands for needs to be mentioned in the caption.

Reply: In the revision, it is clarified in the caption of Table 6 that the three letters stand for plain (P), slope (S), and mountain (M), respectively (L825), or M: mountain), as suggested. Earlier in the manuscript, the letters have been defined in the caption of Fig. 2.

Line 441: Do you think about the sensitivity of the terrain played on other factors? For example, what about removing the whole mountain and/or removing the mountain sequentially?

Reply: The terrain effect on other factors have been reviewed in Section 1 of the manuscript (L35-38, L39-50), including studies that employed sensitivity tests with terrain removal and/or terrain reduction (such as Wang et al., 2005). Since these studies have been reviewed, it is perhaps not

necessary (nor suitable) to mention them again in Section 6 (Conclusion and summary), as they are done with real events using a different approach (idealized simulations) as the present study.