Review comments
nhess-2021-196

Title: Idealized Simulations of Mei-yu Rainfall in Taiwan under Uniform Southwesterly Flow using A Could-Resolving Model

Authors: Wang et al.

Recommendation: major revisions

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.

Specific comments

1. experimental design:

   - Why do you restrict the flow direction to SW? Is the southwesterly flow 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.

   - Are the fine steps of 2.5 m/s and 15 degrees really necessary? Would not a larger range of flow 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. Kalthoff (2014): Impact of upstream flow 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

- 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?
- What boundary conditions are applied in the model? Open, periodic?
- Deep convection is considered to be resolved at 2-km grid spacing, but is shallow convection still parameterized? If yes, how? Please specify.

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 flow 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, ...).
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.
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 reponsible for the plateau?
5. L71: What are "unwanted features"? Please specify.
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.

Technical corrections

- L15: local afternoon ~~during daytime~~
- L17: This sentence needs to be rephrased. What is a "large angle"?
- L40: Blumen, 1990: Blumen is the book editor for Banta (1990). Do the authors mean the Banta article here?
- L45: orographic precipitation can often be resulted → please rephrase
- L60: Wang et al., 2002, 2003: For these years, there are only entries in the references for Wang and Chen (2002, 2003).
- L80: $Fr \rightarrow F_r$
- L144: Murakami et al. (1990, 1994) → Murakami (1990), Murakami et al. (1994)
- L147: Sagami → Segami
- L174: Chen and Lin (2005): Which entry is meant here, 2005a or 2005b?

- L184: The result~~s~~ of the CTL**-run** is ...
- L185: it ~~behaviors~~ **behaves** as designed.
- L235: regime~~s~~
- L330: ...this is resulted because... $\rightarrow$ please rephrase
- L565: Miguietta $\rightarrow$ Miglietta