ACP-2015-687
Authors' Responses to Reviewer 1 (anonymous)
Date: 26 February 2016

Title: A numerical study of back-building process in a quasi-stationary rainband with extreme rainfall over northern Taiwan during 11-12 June 2012
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1. Overall comments:

The authors present a case study on a heavy precipitation event over northern Taiwan. The case is investigated in reanalysis data, observational data and data from numerical model simulations. The underlying processes of the storm evolution are evaluated, with a special emphasis on the dynamics of the storm. In particular, the developing pressure perturbation is investigated, and its thermodynamic and dynamic contributions are separated. The case is well studied and documented. It could benefit from placing the studied case into a larger context, e.g. by giving an indication, how often these events occur over this area, or by describing if this is a typical or untypical event.

Reply:

The positive views and constructive comments from this reviewer (Reviewer 1), as well the comments from the second reviewer, are deeply appreciated, and the paper has been revised according to all these comments and suggestions. In the revised manuscript (color coded), the changes made in response to Reviewer 1, Reviewer 2, by ourselves (mostly minor changes in wording or to correct mistakes), and during the typesetting stage of ACPD (to comply with the journal format and style) are marked in red, blue, orange, and green, respectively. Thus, words in green are the same as seen by the reviewers earlier, and can be ignored. Our reply to the "overall comment" from this reviewer is given in the next paragraph below, while the point-by-point responses to each of the general, minor, and technical comments are listed further below. In each point, how and where the revision is made in the text is specified.

As suggested, the studied case is placed into a larger context, and more material is added to discuss the rarity or unusualness of this event (p.4, L7-8), as well as the background forcings from the (Taiwan) topography and the approaching mei-yu front at scales larger than the storm scale (e.g., p.1, L14-15; p.7, L20-27; p.10, L1-11; p.18, L22-24). For changes related to the latter point, please also see our reply to the major comment #1 below.

2. General comments:

1) In the area where the back-building process occurs, the flow undergoes channeling between the Taiwanese island and the Chinese main land. As described in the study the atmosphere is very moist, especially in the lower layers, thus the channeling leads to strong moisture-flux convergence. In addition, the flow undergoes some lifting over the Taiwanese mountain chain. The fact that LHR and buoyancy play the dominant role in the storm process hints at moisture-flux convergence as the driving mechanism. The process of moisture-flux convergence in a conditionally unstable environment, which naturally leads to the initiation of convection should be mentioned and discussed further.

Reply: At several places (mainly in section 3), the flow splitting due to terrain blocking of Taiwan and the subsequent channeling, convergence, and acceleration (as well as moisture flux convergence) at low levels over the northern Taiwan Strait is discussed with appropriate references (p.1, L14-15; p.7, L20-25; p.8, L4-6; p.10, L1-8; p.12, L14-15 and L18-25). In the revision, it is also stated that such conditions, together with the approaching mei-yu front, are particularly conducive to quasi-linear MCSs, and both the frontal forcing and the terrain-induced (moisture flux) convergence are driving mechanisms at meso-α and meso-β scales in the present case (p.7, L25-27; p.8, L12-15; p.10, L8-11; p.12, L9-10; p.14, L22-25; p.17, L13-15; p.18, L22-23), as suggested.

2) The numerical model seems to capture the storm evolution well. However, it appears to me from looking at the figures, that it overestimates the amount of the flow that goes over the mountain chain but underestimates the portion that is directed around the ridge/island and undergoes channeling. As a result, the simulated precipitation tends to occur more on top of the mountains and is underestimated at the tip. Please include some analysis into the manuscript.

Reply: Although our focus of the study is not in the details of flow behavior mentioned by the reviewer here (it would also be very difficult to verify due to sparse coverage of upper-level observations in the mountains and over the ocean around Taiwan), it is stated in the text that the model might somewhat over-estimate the flow over the mountain because it predicted more rainfall in the interior of southern Taiwan than observed, along the lines as suggested (p.9, L17-19, p.11, L19-21).

3) The formation mechanism for the layer with negative $\Delta p'$ and negative $\Delta p'_b$

around 5 km between 120.8 and 120.9° remains unclear to me. It coincides with a layer of negative buoyancy. There is no cloud visible, so evaporation of rain can be ruled out.

Reply: While the induced p' and p'_b are more subtle than $\nabla^2 p'_b$ (cf. e.g., Figs. 12g,h and 14d,e), the latter being positive near 120.8°-121°E at 5 km (associated with the development of B2 cell) is apparently due to $\partial B/\partial z > 0$ [or more precisely, $(\rho_0 B)$ becoming less negative with height, cf. e.g., Figs. 13g,h and Eq. (3)]. This is because the adiabatic cooling from rising motion (forced by near-surface convergence) diminishes above 5 km where the atmosphere is potentially stable. The above mechanism is better explained in the revision, as suggested (p.8, L6-8; p.15, L20-23).

4) Page 32682, second paragraph: Even though the low levels of the atmosphere are very moist and little rainfall evaporates, downdrafts can still be driven by melting of cold hydrometeors, or simply by buoyancy loading. The buoyancy loading mechanism is studied later in the study. How strong/important is the melting? Melting and evaporation may moreover be quite sensitive to the parametrization of microphysical processes. The study uses a single-moment scheme, which may underestimate the evaporation process, as it cannot reproduce a variable droplet-size distribution (see e.g. Morrison, 2009).

Reply: In the revision, the possible sensitivity of the strength of rear-flank downdraft of B1 on cloud microphysical scheme is noted and the work of Morrison et al. (2009) is cited, as suggested (p.15, L30-31; p.23, L11-13). Nonetheless, at least at later times when the updraft of B1 becomes more tilted and B2 grows higher and stronger (e.g., 2100 UTC, cf. Fig. 13j-l) it would become more difficult for the rear-flank downdraft to reach close to the surface, and this is also pointed out in the revision (p.15, L27-31).

5) Mention the term "case study“ somewhere.

Reply: While it is quite clear that the present study is on a single event, as indicated throughout the text in the revision (p.1, L19; p.4, L10; p.5, L24; p.7, L11; p.12, L1; p.14, L16; p.16, L23), the term “case study” is explicitly mentioned at several places in the revision (p.1, L16; p.10, L15-16; p.17, L29), as suggested.

6) Some of the figures are extremely small and hard to read, especially the labels and land-sea mask. This makes it very difficult to compare the different panels. Please make sure that they are printed larger.

Reply: The contour labels and most of the axis labels in Figs. 10b and 11-16 are all enlarged to improve their readability (p.44-51), and the land/sea boundary in Fig. 4 is also highlighted with thicker outlines (p.29, L16-19; p.36-37), as suggested. Since many figures are arranged (with their panels) to fit a portrait layout, which is used in Atmos. Chem. Phys. (ACP), some of them are not very suited for a landscape layout used during the discussion phase of ACPD. If accepted, the paper will be eventually printed using a portrait layout, and some figures can be printed larger and clearer.

3. Minor comments:

1) Page 32680, Line 9: mention already in the abstract which model you are employing.

Reply: The sentence is revised to "…the linear MCS and the BB process in this case are successfully reproduced, and are found to be …", along the lines as suggested (p.1, L19-20).

2) Page 32681, Line 12: clarify what is meant by "slow-moving surface boundary". Is it a front, or a convergence line?

Reply: It is clarified that the boundary can be a front or a convergence line, as suggested (p.2, L19).

3) Page 32682, first paragraph: how important are elevated mixed layers remanent from the previous convective events?

Reply: The role of the remnant of elevated mixed-layer is not discussed in these earlier studies, likely because the topography over southern China (hilly terrain) or near Taiwan (offshore with sparse data) is quite different from the Great Plains of the US (flat and quite homogeneous). In the present case, there was little rain over northern Taiwan preceding the current event (and on previous days), and this is noted in the text to indicate that there was no elevated mixed-layer (p.9, L17-20), as suggested.

4) Page 32686, line 13: illustrate the "relaxation method further". How is laplace p' inverted to retrieve p'? Which boundary conditions are used?

Reply: An appendix is added to explain the details and the boundary conditions used in the relaxation method, as suggested (p.6, L22; p.19, L19 to p.20, L23).

5) Page 32697: Which diabatic processes are active? Radiative cooling? Or something else?

Reply: The diabatic effects mentioned in section 6 are those associated with phase changes, i.e., condensational heating (from LHR) and evaporative cooling. This is better explained in the revision to avoid confusion, along the lines as suggested (p.16, L14-16).

4. Technical comments:

1) Abstract, line 9, replace reproduced with reproduces.

Reply: The sentence is revised to "…the linear MCS and the BB process in the present case are successfully reproduced, and are found to be …" in response to minor comment #1 (p.1, L19-20).

2) Abstract, line 18: replace "gain" with "gained".

Reply: Corrected as suggested (p.1, L28).

3) Page 32680, line 24, remove "the" in front of "squall lines".

Reply: Removed as suggested (p.2, L6).

4) Page 32681, line 6: replace "potential of" with "potential for".

Reply: Replaced as suggested (p.2, L13).

5) Page 32683, line 3: include "whether" or "if" before "some other processes".

Reply: A word "whether" is added, as suggested (p.3, L32).

6) Page 32684, line 7-8: Split the sentence up after "numerical simulation".

Reply: The sentence is split into two sentences, as suggested (p.5, L2-4).

7) Page 32684, line 23: include "grid points" after 1000 x 800 x 50 and include "as"

before "Already".

Reply: The sentence is revised to "… and a grid dimension (x, y, z) of 1000 × 800 × 50 points (cf. Fig. 1, Table 1). As already described, …" along the lines as suggested (p.5, L16-17).

8) Page 32685, line 6: replace "on" by "of".

Reply: Replaced as suggested (p.5, L26).

9) Page 32685, equation (1): introduce F_z (presumably turbulent mixing).

Reply: The term F_z is introduced explicitly, as suggested (p.6, L2-3).

10) Page 32688, line 6: include "the level of" before "free convection".

Reply: The sentence is revised to "… to reach the level of free convection at 789 hPa. …", along the lines as suggested (p.8, L10).

11) Page 32692, line 22: replace "resulted" by "resulting" and remove "the" before "strongest".

Reply: Revised as suggested (p.12, L26-27).

12) Page 32692, line 27: remove "the" before "strongest".

Reply: Removed as suggested (p.12, L31 to p.13, L1).

13) Page 32694, line 26-27: this sentence is confusing. Rewrite. The dominant contributor is p_b.

Reply: The sentence is rewritten to "…the total pattern of $\nabla^2 p$ is dominated by $\nabla^2 p_b'$ everywhere, except near the based of B1 (below 1.5 km) where $\nabla^2 p_d'$ contributes significantly (Fig. 12k and l). …" for better clarity, as suggested (p.14, L20-22).

References:

H. Morrison, G. Thompson, and V. Tatarskii, 2009: Impact of Cloud Microphysics on the Development of Trailing Stratiform Precipitation in a Simulated Squall Line:

Comparison of One- and Two-Moment Schemes. Mon. Wea. Rev., 137, 991-1007. doi: http://dx.doi.org/10.1175/2008MWR2556.1