Review ACPD-12-27891-2012
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Title: Impacts of the mountain-plains solenoid and cold pool dynamics on the diurnal variation of precipitation over Northern China

Authors: Bao, Xinghua, and Fuqing Zhang

Recommendation: Accept after moderate revision

General comments:

In this manuscript, the authors examine the diurnal cycle and the down-slope (downwind) propagation of warm-season rainfall signals (systems) through numerical simulation and sensitivity tests, following an earlier work using observational data (HZ10). In particular, the roles of cold pool, condensational heating, and evaporative cooling on the propagation of convection over northeastern China are examined. The manuscript is generally well written, except for a few issues that: (1) a proper distinction between the MPS circulation and the circulation (especially the updraft) associated with the propagating convective system needs to be made, (2) there should be analyses on stability in the CNTL and sensitivity tests, and (3) further discussion should be made to shed light on the behavior of (organized) convection without condensational heating (in Fake-dry) and evaporative cooling (in NOVAP) in the model as well as in the real atmosphere. I think that these issues can be surmounted by the authors without much problem, and therefore recommend acceptance after moderate revision.

Major comments:

1. In this manuscript, the authors discussed the mechanism through which the convection (or convective updraft) propagates southeastward from the leeside to the plain area, but not very consistent at different places (for example, in p.27903, L10-13; p.27905, L4-7; also see minor comments #4 and #5). The authors should revise the relevant material, and cite at least a few appropriate references relevant to organization and propagation of convective systems (such as the RKW theory, e.g., in p.27907, near the end of the second paragraph).

2. P.27903, L6-9: The issue of stability has never been brought up and discussed by the authors. It would be nice and worthwhile if the authors can examine at least briefly the mean thermodynamic conditions and the stability of the atmosphere over the plain area

before the arrival of the nighttime convection. This would also have implication in understanding why the model produces much more rain in NOVAP run compared to CNTL and Fake-dry experiments. Please also see major comment #4 below.

3. P.27906, L1-4, title and content of section 5: I am confused by the title and L3-7 (i.e., the first two sentences) of section 5. The authors need to make a distinction between the MPS (produced through solenoids from uneven heating/cooling) and the circulation induced by deep convection here. Being induced by topography, the former reverses in its pattern between day and night but stay more-or-less fixed in location, while the latter of course moves (propagates) with the convection. So, I am not sure how latent heating and/or evaporative cooling get the MPS to propagate (which is what the authors state here, see e.g., p.27908, L6-10; p.27909, L1-3; p.27910, L13-17). An example is Huang et al. (2010, already in reference list), where the MPS (between the eastern Tibetan Plateau and leeside lowlands) is shown to regulate (or modulate) the propagation of the convection and the convection (at the corresponding phase speed) acts to enhance the MPS locally. In my understanding, the content in section 5 discusses how the development and propagation of convection, not MPS, are affected by latent heating and cooling.

4. P.27906-27909, section 5: In this section, the authors discuss the impacts of latent heating and/or evaporative cooling on the behavior of convection development and propagation (not MPS, please also see major comment #3 above). In the Fake-dry run where latent heating/cooling is turned off, the rainfall is much reduced and almost out-of-phase from its normal diurnal cycle (Figs. 4c and 6c). However, when only the evaporative cooling is turned off (with latent heating kept on) in NOVAP run, the rainfall is increased dramatically and the convective system propagates at about 2/3 of its normal speed (Figs. 4d and 6d). Notice that in Fig. 6d, there seems to be a second propagation signal at the same phase as the main updraft in CNTL (appearing as light blue, from 07 UTC at 450 km to 14 UTC at 800 km). While I understand the scenario in NOVAP (and in Fake-dry) is hypothetical, the results are a bit surprising since the initial conditions (of each 1-day run) are taken from CNTL and are therefore the same. I think that further discussion on why the rainfall increases so much (e.g., reduction in surface cooling and thus less stabilization, and this aspect is related to the major comment #2) and what controls the system propagation (e.g., divergent outflow at surface without enhancement by cooling, or steering flow at certain level?) in this case can shed light on the understanding the behavior of convection at least in the model, and perhaps in the real world as well. To the very least, some plausible explanation needs to be offered. Currently, it is neither clear nor sufficient to me.

Minor comments:

1. P.27891, title: The data and results of this study are applicable to early summer and perhaps much of the warm season, but not the cold season. Thus, I think it is more appropriate to add “warm season” in the title.

2. P.27894, L2-3 and other places: Throughout the text, specific geographic features, mostly mountains and plains near and over Northern China, are mentioned quite frequently, such as the Great Khingan, Taihangshan Mountain, Wushan Mountain and Xufeng Mountain, as well as the Yanshan-Taihangshan Mountain ranges (e.g., p.27895, L7-8). I think that a figure showing these features early in the paper can assist the unfamiliar readers a great deal and help the authors convey their arguments better.

3. P.27903, L10 and other places: The cold pools in Fig. 8 and other similar figures (Figs. 10 and 11) are not shown clearly. Please consider some alternatives to enhance the readability, for example, highlight a certain potential temperature value (or plot a certain negative perturbation value) using a different color. Similarly, the reversal of horizontal temperature gradient (p.27903, L27-28) at nighttime is not clear in Fig. 8.

4. P.27903, L10-13: While reasonable and likely so, the existence of forward-directed horizontal pressure gradient force (PGF) is not demonstrated in Fig. 8, and I am not convinced that this is the primary mechanism by which the updraft propagated forward. An example of case study can be found in Wang et al. (2011), where the PGF caused acceleration further downstream near the surface and triggered new convection remotely, away from the old convection.

5. P.27904, L13-15 (also p.27905, L1-2): If the environment is conditionally or convectively unstable, the (convective) updrafts, once developed, are bound to produce precipitation, so I don’t think that they can be considered the “triggering mechanism” of rainfall (as they are associated with one another). Based on the authors’ discussion, the cold pools of the propagating MCSs act as the triggering mechanism of new convection. If the authors meant the upward branch of the MPS, the convection would be locally triggered. This is different from the propagating component and requires further clarification.

7. P.27907, L11-21: Note that the system in Fake-dry propagates (at about 1/2 speed) from about 250 to 600 km (Fig. 9c), where significant sloping terrain exists (cf. Fig. 3c). The

authors may want to stress this.

8. P.27908, L9: Producing much more rain, I am not convinced that the mean convective updraft (not that of MPS, again, see major comment #3) in NOVAP is “much weaker” than that in CNTL (cf. Figs. 8 and 11). Please revise.

Other comments:

1. P.27896, last paragraph, and p.27915, Table 1: Based on the description, CNTL is a 15-day simulation using mean diurnal cycle over 17-24 Jun 2004 as IC/BCs, while Fake-dry and NOVAP runs are 10 consecutive 1-day simulations initialized using the 0000 UTC forecasts for each of the last 10 days of CNTL. So, only in model physics are Fake-dry and NOVAP configured the same as CNTL (except of course in latent heating and evaporation of liquid water, respectively), and currently the relevant descriptions in the text are not very clear and a bit confusing. Also, the information for forecast lengths and number of runs (one 15-day continuous run for CNTL; but ten consecutive 1-day runs for Fake-dry and NOVAP) should be added in Table 1 to better clarify the differences among the experiments.

2. P.27897, L3: FNL should be defined near the beginning of section 2.

3. P.27897, L4-7: The author may want to elaborate a little more about how long the NECV dominated during the 8-day period, and how common such NW flow pattern is over the region during the warm season. This may have implication on the applicability of the results from this study on diurnal cycle of the precipitation in northern China.

4. P.27897, L14: It is a bit awkward toward the end of this sentence. Please revise.

5. P.27901, L18-19 (and p.27905, L1): If the propagation speeds of the primary and secondary updraft are both about 12 m s^{-1}, the 300 km distance will require at least 6 h to reach. In Fig. 9a, it is indeed the case at 700 hPa (see p.27904, L12) and the two exhibit the same speed. So, the authors may want to revise this and be consistent throughout the text.

6. P.27902, L5-6 and likely other places: I suggest that either “northern China” or “North China” should be used in a consistent manner throughout the text.

7. P.27904, L24: It is probably better to clarify that the authors mean the “strongest solar

heating” within the diurnal cycle.

Technical points:

1. P.27893, L6: The reference of Hirose et al. (2005) should be Hirose and Nakamura (2005).

2. P.27894, L10: The paper cited here is Yu et al. (2007a) or (2007b). Please clarify.

3. P.27895, L21: The operational center should be NCEP, not NOAA. Also, the full name should be spelt out in its first appearance in the text.

4. P.27899, L14: “BST” may need to be defined at its first usage.

5. P.27902, L10-11: It should be “Figs. 8b-i” (in plural form).

6. P.27911, L5: This is the first usage of “TACC”, so please provide its full name.

7. P.27912, L7: Based on the issue number (138) of MWR, the paper of He and Zhang should be in 2010, not 2011. Throughout the text, He and Zhang (2010), or HZ10, is also used (e.g., p.27893, L7).

8. P.27923, Fig. 8: It would be nice if the secondary updraft is also marked (using U1 and U2)? Also, currently Fig. 8 is a little too small and the vectors are not very clear (the caption is taking up too much space, also see minor comments #3 and #4). Please make some improvement on the readability if possible.

9. P.27924, caption of Fig. 9: The units of vertical velocity are missing in the caption.

References:

Wang, C.-C., G. T.-J. Chen, and S.-Y. Huang, 2011: Remote trigger of deep convection by cold outflow over the Taiwan Strait in the Mei-yu season: A modeling study of the 8 June 2007 case. Mon. Wea. Rev., 139, 2854-2875.