

Interactive comment on “Impacts of inhomogeneous landscapes in oasis interior on the oasis self-maintaining mechanism by integrating numerical model with satellite data” by X. Meng et al.

Anonymous Referee #2

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This manuscript discusses a numerical modeling study on the impact of inhomogeneous landscapes in oasis interior on the mesoscale circulations around the oasis. The changes in mesoscale circulation can affect the energy and moisture transport between the oasis and the surrounding desert, thus affecting the self-maintaining mechanism of the oasis. I find the topic to be fascinating, and the research can potentially make significant contribution to our understanding about how oasis system works and sustains. This research can also have important implications on how human activities can affect the functioning of oasis and its ecosystems. However, the current manuscript is

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not good enough to be published in HESS, and the authors should consider a major revision to improve the research and the quality of the writing.

My major concern about the research is on its experiment design. To investigate how inhomogeneous landscapes affect regional climate by altering the mesoscale circulations, one should consider conducting multiple (ensemble) simulations under different synoptical weather conditions, and possibly include interannual variability. In this way, the differences between two sets of experiments can be tested for statistical significance, and conclusion can be more meaningful. However, this study ONLY conducted two simulations for a single case with two different landscapes. Although differences in circulation, humidity and other fields between the two can be calculated and analyzed, it is impossible to attribute the difference to the change in landscapes conclusively. Additionally, the manuscript ONLY examined the difference between two simulations at a single time (05UTC July 5, 2004), which makes it even less persuasive. The current study presets some necessary but not sufficient evidence to support the final conclusion, so the analysis should be considered incomplete, or fundamentally flawed. I recommend the authors to carry out additional simulations over a long period of time, preferably over multiple years, then compare these simulations. Without ensemble simulations, the work is not likely to be publishable because its analysis will be very inconclusive.

The writing needs to be improved. The authors didn't carefully proof-read the manuscript, so there are numerous grammatical errors, which makes it hard to read. I listed some of specific issues below.

Line 19 on page 1981: "play [an] important role" Line 9 on page 1982: "These processes as a whole are called . . ." Line 18-19 on page 1984: nonhydrostatic is repeated here. Line 21 on page 1984: Grell et al 1994 does not seem to be the right reference here, does it? Line 23 on page 1984: "this work is a [continuation] of previous work" Line 1 on page 1985: "[is] located at . . ." Line 16 on page 1986: what is a moisture inversion? Line 24 on page 1986: If the basin is located in the northwest of China, then

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5UTC isn't really local afternoon. It is in fact local morning, about 11 a.m. local time.
Line 19 on page 1987: "the contour lines are more density. . ." is a wrong expression.
Line 11 on page 1989: "a secondary circulation in in the lower atmosphere" Figure 1:
It will be useful to indicate where the basin is in China.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 1979, 2012.

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