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Interactive comment on "Detection of land surface induced atmospheric water vapor patterns" *by* Tobias Marke et al.

Anonymous Referee #3

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I have reviewed "Detection of land surface induced atmospheric water vapor patterns" by Marke et al. This study is focused on the analysis of observations and large-eddy simulations of water vapor over a relatively small area (approximately 12 by 13 km) near Jülich ObservatorY of Cloud Evolution (JOYCE). Overall, the paper provides an interesting look at features of the flow near the JOYCE site, and it will be relevant and of interest to scientists studying land-atmosphere interactions. While the presentation is generally clear and the reasoning is sound, there are some issues that should be addressed befor the manuscript is suitable for publication in the journal Atmospheric Chemistry and Physics.

Specific comments include: Page 1, Lines 15 and 22: Is "compartments" the correct word? Components might be better. The the word could be removed from Line 15. In

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Discussion paper

line 22 it could be replaced with “the surface” or something similar. Page 2, Lines 1-2: I am not familiar with the Transregional Collaborative Research Center 32 (TR32) Patterns in Soil-Vegetation-Atmosphere Systems, or how it might be important be in the context of this study? Some additional explanation would be helpful. Page 2, Lines 5-6: What is meant by high-resolution? Isn’t LES by definition high-resolution compared to the scales of the flow? Page 3, Line 31: Do the years used in this study include a mix of relatively dry and wet years? Page 4, Line 1: The text states that linear interpolation is used for missing values. Is this treatment an issue if you are concerned about the details of the spatial pattern in the boundary layer? Page 4, Lines 18 and 19: The text states that the TKE dissipation rate is based on variance of the mean Doppler velocity. It would be helpful to have a few additional words about how the threshold is applied. Is it a threshold of dissipation, variance, or something different? Page 5, Line 14: It feels like there are some key aspects of the model configuration that are not covered in this section. For example, what data set is used for initial and boundary conditions or are they assumed to be periodic? These simulations could also be sensitive to the treatment of the land surface. It would be helpful to the reader to have some discussion of these important aspects. Figure 1: How does this domain compare to domain used by the ICON-LEM? Page 7, Lines 4-5: The text states “The main crop types between April and September are. . .”. Isn’t it important to differentiate between these various plant types that could have very different transpiration rates and how they are represented in the land-use/land-cover data set?? Page 7, Line 20: The sentence “The first and last scan of each sequence is neglected. . .” seems to contradict the previous one. Page 7, Line 26-34: The scan frequency of the MWR is likely much slower than the time scales of the turbulence. Thus, the data that is shown won’t be able to resolve individual thermals. Is this a potential shortcoming, or does is the point to the need look at features with longer temporal scales? Page 7, Line 29: I do not entirely understand the sentence “. . .was found as mean value for the zenith MWR measurements. . .”. It seems to indicate that the hourly mean value of IWV was determined based on 1 scan, but it seems that there should be additional scans in each hour-interval based on the text in section 2.1. Page 10, Line 11: Some additional detail of how the virtual MWR scan is derived would be helpful as the details are not clear to me. Page 10, Lines 13-14: Why not use the boundary-layer height derived from the Doppler lidar? Page 11, Line 1: I believe that this is the first time that irrigation is mentioned in the manuscript. Is this a regular occurrence? Should it be mentioned earlier? Page 11, Lines 14-15: The text highlights differences in the observed and simulated boundary-layer depth. Why not just normalize the results using common boundary-layer scales? Maybe there isn't sufficient data? Figure 5: Could more tick marks be added to the horizontal axis of Figure 5? Maybe one every 45 or 90°? Page 12, Lines 6-9: The text states "Also a more dominate large scale humidity....". This sentence argues for some additional discussion of the boundary-conditions use to drive the model. Figure 6: Over what depth was the vertical averaging applied? Page 13, Lines 4-6: Are the roles really a secondary circulation associated with the different amounts of moisture or are they simply a response to overall forcing in this particular case study? Back-of-the envelop calculations could be completed and compared to thresholds that have appeared in the literature. Page 14, Line 1: Is the change mentioned here associated with the intensity of the roles, or is it related to some other aspect of the flow? Page 14, Line 8: It would be clearer to use "larger" rather than "higher" in this sentence. Page 14 Line 10: Does the sum of the sensible and latent heat fluxes differ in the two simulations? It's hard to tell from Figure 7, and it could impact the interpretation of the results. Page 15, Line 3: Should "in" be "over" or some other word? Page 15, Line 6: Should "also" be added between "is" and "attributed"? Page 15, Line 16: "Are" should be "were". Page 15, Line 19: Is this really a mesoscale circulation, or is it smaller scale? Note that there was also a comment in section 3.3 regarding the changes in the winds and the nature of the changes in the boundary-layer flow. Would it be more accurate to simply say that there are changes in the boundary-layer flow structures?

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