

Interactive comment on “Soil moisture–precipitation coupling: observations from the Oklahoma Mesonet and underlying physical mechanisms” by T. W. Ford et al.

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Received and published: 29 March 2015

Review: HESSD 12, 3205-3243 The manuscript by Ford et al. 2015 discusses the feedback between soil moisture and precipitation using a long data set. I have enjoyed reading the manuscript. The analysis is clean and uses many observations. My main comments are mostly about one thing: placing the introduction and results in the context of recent results both in the region and in terms of physical understanding (especially the role of stability for dry vs. wet advantage, which should be stressed more)

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Here are my detailed comments:

p3208: lines 1->3: what is missing in this discussion is the stability and dryness of the atmosphere see (effect of surface vs entrainment) Ek, M. B., and A. Holtslag (2004), Influence of soil moisture on boundary layer cloud development, *Journal of hydrometeorology*, 5(1), 86–99. Westra, D., G. J. Steeneveld, and A. A. M. Holtslag (2012), Some Observational Evidence for Dry Soils Supporting Enhanced High Relative Humidity at the Convective Boundary Layer Top, *Journal of hydrometeorology*, 13, 1347–1358. (effect of stability) Huang, H.-Y., and S. A. Margulis (2011), Investigating the Impact of Soil Moisture and Atmospheric Stability on Cloud Development and Distribution Using a Coupled Large-Eddy Simulation and Land Surface Model, *Journal of hydrometeorology*, 12(5), 787–804, doi:10.1175/2011JHM1315.1. (stability and dynamic vs thermodynamic advantage for wet-dry advantage) Gentine, P., A. A. M. Holtslag, F. D’Andrea, and M. Ek (2013), Surface and atmospheric controls on the onset of moist convection over land, *Journal of hydrometeorology*, 13(2), 1113–1121, doi:10.1175/JHM-D-12-0137.1. - line 4: moist convection is generated not deep, you should mention shallow convection and cumuli - the sentence with less CAPE is not always true ex: Sahel - line 27: there are also observations of no feedback in the region Phillips, T. J., and S. A. Klein (2013), Land-Atmosphere Coupling Manifested in Warm-Season Observations on the U.S. Southern Great Plains, *J Geophys Res-Atmos*, doi:10.1002/2013JD020492. you might want to mention observations in the region and different results obtained: Findell, K., and E. Eltahir (2003), Atmospheric controls on soil moisture-boundary layer interactions. Part II: Feedbacks within the continental United States, *Journal of hydrometeorology*, 4(3), 570–583. Findell, K., P. Gentine, and B. Lintner (2011), Probability of afternoon precipitation in eastern United States and Mexico enhanced by high evaporation, *Nat Geosci*, 4(7), 434–439, doi:10.1038/NCEO1174. Aires, F., P. Gentine, K. L. Findell, B. R. Lintner, and C. Kerr (2013), Neural Network–Based Sensitivity Analysis of Summertime Convection over the Continental United States, *J Climate*, 27(5), 1311–1326, doi:10.1175/JCLI-D-13-00161.1. Guillod, B. P. et al. (2014), Land-surface controls on afternoon pre-

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precipitation diagnosed from observational data: uncertainties and confounding factors, Atmos. Chem. Phys, 14(16), 8343–8367, doi:10.5194/acp-14-8343-2014-supplement. - You mention the last one, which evaluated Findell et al. 2011, but the sentence says that soil moisture changes EF, isn't it obvious? Maybe this is not what you meant. I would reformulate. Also you should define EF." I would change this with a chronology of results (and mention the datasets since it is useful for your later points). - p3209 l8-10" you might want to talk about atmospheric stability in the region and role of low level jet. Also mention mesoscale convective systems and squall lines in the region. You talk about it later but I would change that. - p3210 l10: Mesonet are high quality: reformulate - 2.2 for the majority of rain you should mention MCS - p3211 l 5-6: you should definitely mention Philips and Klein - p3211 l21: might want to mention the work of Guillod et al. 2014 who also used NEXRAD in the US - p3214 l10: you should add a word on which parcel you use for CAPE and CIN definition - l 13-14: might want to talk about Findell and Eltahir 2003 analysis with radiosounding and CTP HI framework. You talk about it later. Also Aires et al. 2014 and Guillod et al. 2014 - p3215 l16-17: Could be interesting to compare with Findell et al. 2011, Guillod et al. 2014, 2015 (Nature communications) - l 23: Some of that is described in Guillod et al. 2014 - end of section 3.1: Would be interesting to link that to the stability and humidity of the atmosphere, as discussed in Huang and Margulis, and what was called thermodynamic vs dynamic advantage in Gentine et al, 2013 (BTW I actually noticed this paper is not in the references) - section 3.2: might want to cite Avissar and Pielke - p3220: line 9-11: yes see discussion in Gentine et al. 2013 and the role of slope of theta, actually in this framework you should be able to differentiate early morning and surface conditions. IN fact it would be really interesting to see if you fall in the same delimitations of regimes (positive and negative) as the one described in Gentine et al. 2013 in terms of atmospheric stability and soil moisture/EF. Given all the data you have that should be straightforward analysis but very interesting to see if there is clear delimitation of the phase space. - line 23-25: higher CTP is the same as lower stability in the free troposphere Huang and Margulis 2011, - p3222 line 5: yes described as well in Huang and

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Margulis 2011 and Gentine et al. 2013: thermodynamic effect for deep boundary layer which lowers the LCL and LFC compared to PBL growth thermodynamically (through changes in Clausius Clapeyron) line 11-12: over strong stability direct moistening by increased EF and ET is the reason for the increased MSE, might want to make that more explicit (dynamic effect in Gentine et al. 2013) -p3223 line 5 yes discussed in Ek and Holtslag 2004 and Gentine et al. 2013 - p3224 lines 4-5: with strong stability (Huang and Margulis 2011, Ek and Holtslag 2004 Gentine et al. 2013)

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 3205, 2015.

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