Manuscript hess-2015-414 entitled "Modelling evapotranspiration during precipitation deficits: identifying critical processes in a land surface model"

We would like to thank the reviewer for their constructive comments on our manuscript. This document outlines our point-by-point responses to the reviewer comments and the improvements made to the manuscript.

General comments

This paper addresses the important issue of land surface model behaviour during lack of rainfall. Its plots are clear and the statistics appear sound, albeit somewhat basic. However, I felt somewhat empty-handed at the end. Very little, process understanding was gained. Why are the key equations not provided? There are various points in the paper where I get the impression the authors have used the model like a black-box without truly understanding the equations within the model. This is also evident from their description of the model physics, soil physics in particular. This is a missed opportunity and leaves the reader somewhat frustrated. I guess most of the Conclusions could have been drawn without having gone through this considerable modelling exercise. More in-depth explanation of the findings is required using equations presented and explored explicitly, not tentatively (using words such as 'likely', 'multiple explanations are possible', etc.

We have addressed the reviewer's concerns by including key equations in the paper to support the conclusions reached in the paper. We have also added more in-depth discussion to attach broader relevance to the paper's findings, including more detailed examination of each of the model process analysed here (soil, LAI, hydrology and stomatal conductance). We have provided additional supplementary figures to illustrate these points. However, we would like to point out that some explanations remain necessarily tentative due to a lack of observations to test specific model processes but the proposed mechanisms firmly rely on existing CABLE parameterisations.

Specific comments

Page 10792: I think the statements in lines 3-8 are somewhat naïve.

“We use *QE* because it is the variable that links the land surface energy, water and carbon budgets. It is also one of the variables supplied by the land surface to the atmosphere and is therefore important to a climate model. We do not use soil moisture as evaluating soil moisture from LSMs directly is problematic (Koster et al., 2009) due to different soil structures assumptions, storage capacity and timescales inherent in how LSMs represent this variable”.

Soil moisture, and the models underlying hydraulic properties and soil water transfer and root water uptake equations, ultimately determines the latent heat flux; via transpiration and direct soil evaporation. If a model gets QE right, but soil moisture content (considerably) wrong, this is a sign of poor process presentation particularly with regards to soil hydrology and plant water stress parameterisations.

Also, what exactly is meant by ‘soil structure assumptions’. This is unclear terminology.

We agree that evaluating LSMs against soil moisture would be valuable for better understanding hydrological processes in models and acknowledge this in the revised manuscript (section 2.1). However, many discrepancies exist between a real soil column and a model’s representation that make direct comparison extremely difficult, even if appropriate observations existed. Examples in this case include homogeneous soil properties with depth (there are no soil horizons), bedrock distribution and the commensurability of layer discretisation and measurement depths. In addition, few long-term in-situ observations are available for these flux tower sites that record soil moisture changes at appropriate depths (for example CABLE divides the soil into multiple layers with a total depth of 4.6 metres). Furthermore, *in-situ* measurements are highly localized in nature and strongly depend on local soil properties, making direct comparison to larger-scale models difficult (Koster et al., 2009). Remotely-sensed soil moisture products only record the top few centimetres of soil moisture (with the exact depth dependant on vegetation and soil moisture conditions) and typically from a coarser scale than the flux tower point fetch. Thus, these are not helpful for evaluating soil moisture outputs from LSMs in the context of our study. Given the discrepancies between modelled soil moisture and currently available observations, we feel that the uncertainty associated with evaluation against soil moisture means it is of limited value and flux tower measurements of Q_E remain a valuable alternative.

Line 25: soil texture is generally not a model parameter. It is used to derive other parameters from, such as hydraulic conductivity or the water retention curve. On the next page line 13 you use the term soil properties, which would be more appropriate.

The reviewer is right to highlight that in models soil texture is often used to derive other properties, such as hydraulic conductivity or the decomposition rate in CENTURY-type models (and thermal conductivity in CABLE). Nevertheless, it is correct to describe soil texture, the fraction of sand, silt and clay as a model parameter in the CABLE (and other) models. We have replaced soil "texture" with "properties".

Page 10792:

Line 15-17 You say "Where the LSM cannot capture the observations, despite variations in LAI and soil parameters, points to systematic errors in the model's representation of physical processes"

I guess we could consider this roughly to be the case, but this ignores errors in driving variables and energy balance closure errors, or the fact that your parameter range was possibly unsuitable.

We agree with the reviewer that flux tower measurements themselves contain errors, notably in energy balance closure. We have acknowledged this in the Discussion of the revised manuscript (Section 4.1). However, it appears highly unlikely that the CABLE biases identified here are solely an artefact of erroneous driving/evaluation data due to the large and strongly seasonal nature of Q_E biases. In fact, Haughton et al. (in review) showed based on the PLUMBER results (Best et al., 2015) that problems with energy balance closure in flux tower data do not account for the poor performance of LSMs (including CABLE) when compared against simple benchmarks. We have employed the forcing data and Q_E observations from the PLUMBER study.

We have tested a very large range in soil parameters by varying soil properties from sandy to clay soil type. These represent the extreme soil types available in the standard CABLE soil input dataset but it is not possible to quantify if this soil dataset reflects the full range of soil properties at the flux tower sites. LAI was varied by site depending on the remotely sensed MODIS data. It is possible that the range in LAI does not capture site LAI in some cases. However, this and previous studies (Kala et al., 2014) have pointed to a limited sensitivity of CABLE-simulated total Q_E to LAI. It is thus unlikely that LAI errors could account for the high seasonal biases in Q_E.

Also, it is not sure that the error in QE was related to soil hydrological parameterisations. It could just as well have to do with soil thermal and land surface radiative parameterisations, affecting sensible and soil heat flux.

The nature of the errors in Q_E and the responses of those errors to the parameters and parameterizations strongly points to errors being related to the hydrology. This is supported by Haughton et al. (in review), who found that errors in the partitioning between latent and sensible heat, rather than in the calculation of net radiation and ground heat flux, accounted for the poor performance of LSMs when evaluated against simple benchmarks. Without independent observations of all elements of the surface radiation balance it is of course impossible to be certain but it is a reasonable conclusion based on results across multiple sites that the hydrology is the problem.

Page 10795:

Lines 8-9: "The soil module simulates the transfer of heat and water within the soil and snowpack following the Richards equation".

This is incorrect: soil heat transfer cannot be determined with the Richards equation. It is generally determined with the Fourier's law.

The reviewer is of course correct and we have removed this reference to heat transfer.

Page 10796:

Line 4-5: "It does not distinguish between saturated and un-saturated top soil fractions or simulate groundwater dynamics"

This sentence needs elaborating. Distinguish in what way? In the context of surface run-off? At the moment it reads as if model soil moisture plays no role in any soil hydrological process.

We have clarified that the old scheme does not simulate saturated and unsaturated top soil fractions separately but rather treats the top soil layer as one entity, which is either fully saturated or unsaturated. Neither does it take account of water storage in groundwater aquifers, or recharge from those water stores. These are two key differences between the new and default schemes and are therefore highlighted in the text.

Page 10796: Line 24: ..... Table S2.

Table S2 in Supplementary material contains soil physically incorrect terminology. It should be “Soil dry bulk density” not simply “Soil density”.

Also, “suction at saturation” is per definition equal to zero. What you mean is “suction at air entry point”. Furthermore, suctions always have positive values. If you use negative values, as in Table S2, it should be referred to as “matric potential” or rather “matric head” as you are working in length units.

Finally what is meant exactly by soil heat capacity? At air-dry or saturated moisture content? Why are these values the same for all soil texture types?

We have corrected the terminology in Table S2 as per reviewer comments. The dry soil heat capacity has an identical value across the three soil classes as 850 (J/kg/C) was the value provided for the sandy and clay soil types in the standard CABLE soil input data set used in this study (provided with the standard CABLE distribution). The “medium” soil class uses the median value of the sandy and clay soils and thus also uses the same value.

Page 10798:

Lines 1-4: “The default hydrological scheme uses these three soil parameter sets directly, whereas the new scheme employs an empirical approach to calculate the parameters governing water holding and thermal capacities from sand, silt and clay fractions”

I am not sure what is meant here? The default scheme also has values for wilting point moisture content etc. They must also have been derived from texture?

The default scheme uses all soil parameter values in Table S2 as inputted, whereas the new scheme only uses inputted sand, silt and clay fractions and calculates the other eight parameters from these texture parameters using pedotransfer functions (see comment immediately below). We have clarified this in the text.

In both cases using so called 'pedotransfer functions'? Use this word instead of "empirical approach". Which ones were used? By the looks of it Cosby et al., seeing you are using the Clapp and Hornberger B parameter? You need to state this explicitly.

Also, these soil hydraulic parameters govern more than just water holding capacity. They govern soil water transfer via Darcy's law and Richard's equation (with Ks embedded in them).

Finally: you are using heat capacity, but is thermal conductivity not required in Fourier's law?

The new and old schemes use Clapp and Hornberger (1978) relations to relate soil moisture, soil matric potential, and hydraulic conductivity. The old scheme uses a look up table to relate the inputted soil class (e.g. sand or clay) to the parameters in the Clapp and Hurnberger formulation. The new scheme, however, follows the work of Cosby et al. (1984) and uses pedotransfer functions to relate the sand, silt and clay content to the parameters in the Clapp and Hornberger equations, thereby negating the need for a look up table. We have clarified this in the text and replaced "water holding capacities" with "hydraulic properties".

CABLE calculates thermal conductivity from soil texture parameters (sand, clay and silt fractions) and thermal conductivity is thus not an input parameter.

Line 6-7:" Leaf area index (LAI) plays an important role in the surface energy balance in CA- BLE (Kala et al., 2014)".

Can this sentence be elaborated upon by 1-2 follow-on sentences? In what way? By upscaling from leaf to canopy scale conductance? In light interception?

We have added further details on the use of LAI in CABLE. It is used to calculate aerodynamic resistances, partitioning the absorbed radiation flux between sunlit and shaded leaves and to scale fluxes from the leaf to the canopy.

Page 10799:

Line 1-3: "...... the dry periods were defined based on precipitation as this allowed the use of available observations, but we note the simulated fluxes will also depend on other processes such as soil moisture availability".

The simulated fluxes will also very much depend on the driving variables that determine evaporative demand. That has been overlooked in this definition of 'dry periods'.

As we point out in the text, there are various ways to define a "dry" period. We have added evaporative demand as one such definition. We have chosen to use precipitation as this relies directly on available observations, but acknowledge that many other indices could have been chosen. As the dry periods used in this study coincide with the hottest part of the year in most cases, an alternative definition of a dry period taking into account evaporative demand should broadly coincide with the time periods used here. Given the large biases in CABLE that clearly coincide with periods of low rainfall (Fig. S6), we do not believe that the choice of drought definition compromises the results.

Line 8-10. "The dry-down period generally coincides with the maximum and the following minimum observed latent heat flux during the one-year period but has been adjusted for some sites to best capture typical model behaviour (Fig. S6)".

What is meant by this exactly?

We mean that usually the periods to be analysed are chosen via an automated procedure but on occasions we adjust the period chosen where there is a better example that is not identified automatically. We have modified the text to elaborate this.

I feel what is missing from the paper is a basic description of plant water stress, e.g. along the lines of the beta function in the Jules model (see Egea et al. 2011). I understand CABLE uses the same approach?

We have added a description of the plant water stress function in CABLE in the Methods. CABLE uses a similar beta function to JULES and a number of other LSMs.

Page 10800 Lines 23-24: "This is likely due to overly rapid drying of top soil layers, which strongly control *QE* in CABLE (De Kauwe et al., 2015c)" But which process is being affected (mostly) here? Transpiration or evaporation?

In both cases soil moisture content is a key variable, yet we do not get any insight into how well this variable is predicted by the model.

We have clarified in the text that transpiration is affected and elaborated on the reasons behind the rapid drying. We have also added a supplementary figure showing the individual components of evapotranspiration (soil evaporation and transpiration) during the dry-down periods.

In addition, we have added a supplementary figure showing soil moisture variations for the new and default hydrological schemes for each of the six soil layers during the one-year periods. Unfortunately soil moisture observations do not exist for soil depths used in CABLE (4.6 m) or are not made freely available at these sites. We agree that the model should ideally be evaluated against soil moisture as an additional constraint but this was not possible and, in common with many other studies (e.g. De Kauwe et al., 2015; Li et al., 2012; Whitley et al., 2015), we have instead relied on Q_E measurements.

Lines 23-24: "This is particularly evident during warmer summer months when fluxes are more strongly moisture-limited"

One may assume that this is indeed the case, but with no information on soil moisture content, nor how SMC affects evapotranspiration, this remains speculative.

We have added a supplementary figure showing soil moisture variations (see comment immediately above). We have also added the plant water stress and soil evaporation functions in the Methods to show how soil moisture limits both components of ET.

Lines 27-28: “While encouraging, this is likely due to compensating errors, such that early season overestimations in *QE* are counteracted by underestimations during the dry-down periods.“ Remove the word likely. It either is or it isn’t. You have the data in front of you.

We have removed ‘likely’.

Page 10801 Line 8: “The model dries down too quickly”. The model itself is not drying...

We have rephrased the sentence.

Line 12-14: “These characteristics of CABLE are not dependent on the choice of LAI, *g*s, or soil parameters; the range in *QE* fails to overlap the observations irrespective of how these properties are varied.”

I have not been able to find anywhere in the paper between what values these variables have been ranged. Nor am I sure what is meant by soil variables. Are these the soil hydraulic variables? Or texture percentages?

The range in LAI is shown in Figure S5 and referenced clearly in section 2.2.5, which also details how the LAI time series were generated and varied between model runs. Soil parameters are discussed in detail in section 2.2.4 and the parameter values of each of the three soil classes that were used as CABLE inputs are displayed in Table S2. The soil parameters were varied between the values of three soil classes. We have reworded the sentence to state that the results are not dependent on the choice of LAI or soil inputs or g_s parameterisations.

Page 10802

Line 19-23: “Both hydrological schemes are sensitive to soil parameters during the dry- down period but show smaller variations due to soil during other parts of the year (see Amplero, Blodgett, Howard Springs and Palang in Figs. 6 and 7). This transition from low to high sensitivity occurs as soil moisture stores begin to deplete and *QE* becomes increasingly limited by moisture supply”.

This last sentence seems a very obvious statement. Does it need stating? How meaningful is it anyway, if we are not told (at least approximately) how CABLE deals with plant water stress?

As stated in a previous comment, we have added an equation for plant water stress and a figure showing soil moisture variations during the dry-down period.

Line 23-25: “The new hydrological scheme uses a narrower range of parameter values for water holding capacity and conductivity (Table S2) and thus results in a smaller range of uncertainty due to soil parameters.”

As far as I can see Table S2 does not give a range per soil parameter. It gives one value only for each soil texture type. Also, what is meant by water holding capacity? Water holding capacity is defined as the total amount of water a soil can hold at field capacity. Did you mean ‘water retention curve’. Furthermore, ‘conductivity’ needs to read ‘hydraulic conductivity’

The model was ran using three alternative soil classes as per Table S2, generating a range of 3 parameter values in simulations. Table S2 details the parameter values for each soil class. The sentence in question was removed from the text.

Page 10803

“The slope parameter affects the rate of subsurface drainage and represents a key difference between the new and default schemes”

If this is the case, then why the emphasis on water retention? These parameters are generally related to plant water availability.

We are not sure what the Reviewer means here. We hope that the additional edits we have implemented will satisfy the Reviewer but are open to further changes if their comment can be clarified.

Why is the slope parameter not in Table S2?

The slope parameter is not a soil parameter but was varied separately in additional simulations between 0 and 5 degrees using the “medium” soil class and new hydrology only, as detailed in Section 2.2.2.

What meaning does this parameter have anyway at the site scale?

The slope parameter can be obtained from high-resolution elevation data at scales relevant for flux tower sites. In this case, the slope reflects the average slope of a 3x3 km area centred around the tower. The surface slope derived with this method only reflects the slope at a scale larger than the footprint of the site and, as we acknowledge in the manuscript, it is a first order approximation to the features affecting subsurface drainage such as bedrock slope and lateral heterogeneity in subsurface properties. However, as the model was shown to be sensitive to the choice of slope, we believe it is important to discuss the effect of the slope parameter on Q_E simulations.

Page 10804

Seems this section makes a lot of rather obvious statements that could have been made without the work conducted in this paper.

The Reviewer demonstrably has a deep understanding of the areas covered by this paper. We suggest that other readers may not see all the statements as obvious and in any case, demonstrating statements supported by evidence is useful. We do agree that some of our discussion and conclusion is fairly straightforward for the expert, but we are also writing for readers who may not have the awareness of the Reviewer.

Page 10805

Lines 5-7: “The reason for the overestimation of peak fluxes is not clear but is not resolved by the new hydrological scheme despite this parameterising many of the relevant processes differently”.

This sentences underlines the frustrating nature of this paper: CABLE is used like a black-box without the authors truly having investigated the inner workings of the model. I doubt this is acceptable to the journal and its readers.

Why/how would changes in a slope parameter affect soil evaporation?

We have added equations to show how soil evaporation is treated in each hydrological scheme. We also demonstrate that these changes do not resolve the biases. As several of the processes that can be responsible for the excessive soil evaporation cannot be constrained from observations at these flux sites, it is not possible to present definitive reasons behind these biases. Such reasons include errors in LAI. This is a systematic bias in CABLE and warrants a separate body of work to fully understand the model errors relying on alternative data streams. We simply highlight this excessive spring Q_E as a possible mechanism for depleting soil moisture stores earlier than observed but without an alternative scheme available to test, it is not possible to fully quantify this in the present study.

In terms of the slope parameter, if slope increases such that runoff increases soil evaporation is affected by water balance constraints.

Line 9: “..multiple potential causes to this excessive QE??” There are only a limited number of causes and they are all embedded in the equations that determine soil evaporation.

Multiple causes was used to highlight that there are a number of processes affecting simulation of soil evaporation, which the section goes on to discuss. We have reworded this sentence. However, soil evaporation can be affected by causes unrelated to the equations that describe soil evaporation via changes in state variables.

Page 10806

Line 9-11 “Other model processes, particularly vegetation response to drought, have been identified as critical for capturing drought processes and shown to improve CABLE performance during droughts but were not explored here.”

I do not understand this? Wilting point and field capacity are two key parameters in the CABLE model plant water stress factor (empirical scalar beta, see De Kauwe et al. 2015).

So implicitly you have explored vegetation response to drought?!

The Reviewer is correct that we vary the wilting point and field capacity parameters, which are part of the water stress function. However, we do not contrast alternative formulations for plant water stress as this has been done elsewhere (De Kauwe et al., 2015b; Li et al., 2012; Egea et al. 2011). Instead, we have explicitly explored hydrological processes and errors in model inputs as alternative explanations for poor simulation of Q_E during dry-down, which has not been fully resolved in previous studies.

One can also argue that the water stress function in its current form does not reflect vegetation drought responses as we understand them from experiments, as it takes no account of plant adaptations to drought in dry environments (De Kauwe et al., 2015). It mostly reflects properties of soil, and has little empirical support (Medlyn et al., in review). This section discusses the limitations of this approach and discusses alternatives presented in other studies as a means to improve drought simulations. We have reworded parts of this section to clarify this.

References:

Best, M. J., Abramowitz, G., Johnson, H. R., Pitman, A. J., Balsamo, G., Boone, A., Cuntz, M., Decharme, B., Dirmeyer, P. a., Dong, J., Ek, M., Guo, Z., Haverd, V., van den Hurk, B. J. ., Nearing, G. S., Pak, B., Peters-Lidard, C., Santanello, J. A., Stevens, L. and Vuichard, N.: The plumbing of land surface models: benchmarking model performance, J. Hydrometeorol., 16, 1425–1442, doi:10.1175/JHM-D-14-0158.1, 2015.

Clapp, R. B. and Hornberger, G. M.: Empirical equations for some soil hydraulic properties, Water Resour. Res., 14, 601–604, 1978.

De Kauwe, M. G., Zhou, S.-X., Medlyn, B. E., Pitman, A. J., Wang, Y. P., Duursma, R. A. and Prentice, I. C.: Do land surface models need to include differential plant species responses to drought? Examining model predictions across a latitudinal gradient in Europe, Biogeosciences, 12, 12349–12393, doi:10.5194/bgd-12-12349-2015, 2015.

Haughton, N., Abramowitz, G., Pitman, A. J., Or, D., Best, M., Johnson, H. R., Balsamo, G., Boone, A., Cuntz, M., Decharme, B., Dirmeyer, P., Dong, Z., Haverd, V., van der Hurk, B., Nearing, G., Pak, B., Santanello, J. A., Stevens, L. E. and Vuichard, N.: The plumbing of land surface models: is poor performance a result of methodology or data quality?, J. Hydrometeorol., in review.

Kala, J., Decker, M., Exbrayat, J.-F., Pitman, A. J., Carouge, C., Evans, J. P., Abramowitz, G. and Mocko, D.: Influence of Leaf Area Index Prescriptions on Simulations of Heat, Moisture, and Carbon Fluxes, J. Hydrometeorol., 15, 489–503, doi:10.1175/JHM-D-13-063.1, 2014.

Koster, R. D., Guo, Z., Yang, R., Dirmeyer, P. A., Mitchell, K. and Puma, M. J.: On the nature of soil moisture in land surface models, J. Clim., 22, 4322–4335, doi:10.1175/2009JCLI2832.1, 2009.

Li, L., Wang, Y.-P., Yu, Q., Pak, B., Eamus, D., Yan, J., van Gorsel, E. and Baker, I. T.: Improving the responses of the Australian community land surface model (CABLE) to seasonal drought, J. Geophys. Res., 117, G04002, doi:10.1029/2012JG002038, 2012.

Medlyn, B., De Kauwe, M., Walker, A., Duursma, R., Luus, K., Mishurov, M., Pak, B., Smith, B., Wang, Y.-P., Yang, X., Crous, K., Drake, J., Macdonald, C., Norby, R., Power, S., Tjoelker, M. and Ellsworth, D. S.: Using models to guide experiments: a priori predictions for the CO2 response of a nutrient- and water-limited mature Eucalypt woodland, Glob. Chang. Biol., in review.

Whitley, R., Beringer, J., Hutley, L., Abramowitz, G., De Kauwe, M. G., Duursma, R., Evans, B., Haverd, V., Li, L., Ryu, Y., Smith, B., Wang, Y.-P., Williams, M. and Yu, Q.: A model inter-comparison study to examine limiting factors in modelling

Australian tropical savannas, Biogeosciences Discuss., 12, 18999–19041, doi:10.5194/bgd-12-18999-2015, 2015.