Authors' Response to Anonymous Referee #1

We appreciate the comments by the referee #1. We respond to his/her comments below:

Reviewer #1: This manuscripts presents the coupling of a more complex land surface model to WRF and thoroughly evaluated the performance of the new coupled modeling framework against the observations and original WRF-NOAH framework. I appreciate the amount of efforts and the compelling motivation in the introduction and can see high chance of this manuscript eventually to be published. The demonstration of the scientific value of such a new modeling framework, however, deserves more attention and extra efforts. Given the significantly increased model complexity, it is not exciting to see that "Overall, when compared to the simple single layer WRF–NOAH model, the WRF–ACASA model has greater model complexity without decreasing the quality of the output". What's more exciting is to see the model simulated carbon dioxide fluxes, and if feasible, some evaluation on that.

We agree with the reviewer that we need to better identify the novelty of the WRF-ACASA model. In the revised manuscript we will make it clearer that this paper is the first of several evaluation papers. This paper focuses on the fundamental representation of surface meteorology, which is a necessary evaluation of a land surface model.

We will also add a discussion in the conclusion section on the comparison between lower and higher complexity model. The high complexity ACASA model properly accounts for important biological and physical processes between the ecosystem and the atmosphere, and the model performs well when compare to an extensive set of observation. It is true that ACASA did not outperform the NOAH scheme at this point. However, without tuning the ACASA model to any region, the model performs comparable to that highly tuned and lower complexity NOAH model. This should be considered as a good sign of the ACASA scheme.

Finally, we will extend the discussion of model capability that makes it novel and "exciting": simulation of carbon dioxide fluxes and water fluxes. While these are not evaluated in this paper, we are currently preparing a study on that exact topic. However, we feel that a more meteorologically focus evaluation study is necessary before looking into carbon fluxes (and water fluxes, which we are also considering).

Also, most of the model comparison essentially focuses on the local scale simulations. I am wondering whether extra spatial complexity of the atmosphere and land processes and their interactions can be revealed by the more physically based representation of the ecophysiological schemes, which is not extensively discussed in this manuscript.

The reviewer raises an interesting point about the complexity and spatial issues of the study. The sophisticated ecophysiological schemes of ACASA are not discussed in detail in this manuscript, because that work has already been fairly extensively published and is referenced in the manuscript. However, we understand the need to extend the discussion of this topic, which will be done in the revised manuscript.

Lastly, the figure quality can be improved. For example, the fonts in Fig. 5-13 are too small to read.

The quality of figures will be improved in the revised manuscript. We will pay particular attention to the visibility of figures including the fonts.

Figure 3 seems not necessary and can be easily combined with Fig. 2.

This is a great idea, and we will combine Figure 2 and 3 in the revised manuscript.

Authors' Response to Anonymous Referee #2

We appreciate the comments by the referee #2. We respond to his/her comments point by point.

Reviewer #2: Major Comment:

(1) I think, the overall advantages gained due to increased model complexity have been lost in presenting the results for all 700 sites together. Authors discuss some of the advantages particularly related to land cover type in the text e.g. Line 5 to 14, page 2843; however these advantages are not clearly visible to me in Figure 2 and other figures. Focusing the results for contrasting land cover regions, e.g. central valley regions would be helpful.

We realize that we have to improve the discussion on why WRF-ACASA is a novel and useful model in comparison to WRF-NOAH. The revised manuscript will include the following point:

Beyond the complexity of the land surface scheme used in ACASA, ACASA can simulate carbon dioxide fluxes and water fluxes using high complexity turbulent scheme. While this is not presented in this particular paper, which focuses on the more fundamental meteorological aspect of the land surface model, we are currently preparing new manuscripts on the evaluation of carbon dioxide and water fluxes in WRF-ACASA. However, we feel that an evaluation of surface meteorological variables (such as temperature, dew point temperature and relative humidity) is first necessary step.

In addition, we will try to better highlight the advantages of using WRF-ACASA compared to WRF-NOAH and we will focus the results for the contrasting land cover regions in the Central Valley of California.

We will also add a discussion in the conclusion section on the comparison between lower and higher complexity model. The high complexity ACASA model properly accounts for important biological and physical processes between the ecosystem and the atmosphere, and the model performs well when compare to an extensive set of observation. It is true that ACASA did not outperform the NOAH scheme at this point. However, without tuning the ACASA model to any region, the model performs well and quantitatively similar to that highly tuned and lower complexity NOAH model. This should be considered as a good sign of the ACASA scheme.

(2) Several figures e.g. Figures 6 and 7 are not legible, i.e. figure legends, x and y axis titles are not readable, mostly because authors present 16 plots in a single figure. Also, hourly data has been plotted (I think) in Figure 6, and 10 which may not be required because hourly composite (diurnal cycle) have been presented in the subsequent figures. Authors may want to synthesize the data and present in the figure only when it is necessary. For example, authors may want to present the figure only for JJA because land-atmosphere interaction is strong during JJA. Also,

plotting the difference plot from observation in Figures 6, and 10 may be helpful.

We realize that we need to improve the quality of the figures. The revised manuscript will include figures with better visibility and organization. We will follow the advice of the reviewer regarding figures 6 and 10 in the revision.

Minor Comments

(1) Page 2834, Line 5: 2.5 degree (equivalent to 250 km2) -> 2.5 degree (equivalent to 250 km at the equator)

This will be corrected in the revised manuscript.

(2) Page 2845, Line 10 to 19: This description seems to be based on Figure 7, MD JJA. Please check why there is sharp drop at the beginning of the diurnal pattern. Does this affect the simulation?

The sharp drop at the beginning of the diurnal pattern originates from the observational data and probably caused by instrumental error. This will be made clear in the revised manuscript.

(3) Basins and stations are confused some times. For example, page 2848 Line 28-29, says Figure 10 show results for four stations; whereas Figure 10 caption says results are for four basins. Since, a basin has several stations (Table 2), please check carefully.

This will be corrected in the revised manuscript.

(4) **First paragraph in section 4 describes differences between ACASA and NOAH LSM, which is rather long and may not be needed here. Such description can be a part of model description (Section 2.2)**

We agree with the reviewer and this will be changed in the revised manuscript.

(5) **Page 2854, Line 6: "… ecosystem responses to the atmospheric impacts…" -> "… ecosystem responses to the human and natural disturbances.." or something similar to this.**

We will modify the sentence according to the reviewer's comment.

(6) Page 2870: Figure 9: Legends and axis titles are not legible.

We will improve the quality of all figures.

Authors' Response to Anonymous Referee #3

We appreciate the comments by the referee #3. We respond to each of his/her comments below.

Reviewer #3: General Comments:

1. I suggest the Introduction be made more concise and include some more recent references. I am not sure if the necessity of land surface models should be so thoroughly discussed and defended, it makes the Introduction uncessesarily long in my opinion. For example, the majority of earth system models/coupled GCM now use land models with interactive carbon cycles (see for example Table 2 in Anav et al. 2013 which lists CMIP5 models and their relevant land components, attached as a supplement to this review). It should be clear that the limitations in the land surface models discussed in the Intro refer specifically to the land models of WRF, and not LSMs in general.

The revised manuscript will include a more focused introduction, and we will make clear of the limitation of LSMs refer specifically to the WRF model. In addition, we will acknowledge the importance of representing carbon fluxes in regional climate models and the gap in the complexity of the representation of the land surface between regional climate models and GCMs.

2. How are biophysical parameters set in each model (for example, land cover type, the LAI and canopy height?).

We will provide more details in the revised manuscript on these parameters.

3. In regards to the issue with the measurement heights and what "2m" temperature is in the model: Is it not possible to use above-canopy simulated temperatures, and would these be more analogous to the observed temperatures? Also, what were the measurement heights for the four stations and how do these influence the results?

The reviewer raises an interesting point. We do not believe it is possible to generally use the above-canopy simulated temperatures to emulate 2 m observed and interpolated (in reanalysis fields) temperatures that are generally measured above short grass canopies, with the 2 m temperatures representing measurements at 10-20 times the canopy height. The details of taller canopy turbulent transfer make such physical analogies to shorter canopies inaccurate, because 10 to 20 times the canopy height would frequently put the measurement and simulation heights above the surface layer and into the planetary boundary layer; or alternatively, using heights just above the taller canopy heights would also be problematic as they would be equivalent to meteorological measurements a few centimeters above a short grass canopy. We will give the measurement heights for the four stations as requested by the reviewer. We will discuss in the revised manuscript how these heights will influence the results.

4. At some points the text in this section is repetitive, or else it does not follow a logical order. I suggest breaking up the results section to help the reader. Either divide it by the meteorological variable discussed (e.g. 3.1 Temperature; 3.2 Dew point temperature, etc), or by the regions (e.g. 3.1 Northeast Plateau; 3.2 Mojave desert, etc.). Another suggestion is to segregate all discussion of reasons for model-obs mismatch from the results – either separately for each variable or together at the end of this section. This would reduce the repetition.

We will take the suggestion of the reviewer and will better organize the result and discussion sections. In particular, we will keep in mind the need to reduce the repetition and thus simplify the paper.

5. Since relative humidity is a function of the temperature and Td, it makes sense to me to combine the Td and RH results/discussion. This is another place where repetition could be reduced.

We agree with the reviewer and realize the need to combine the results and discussion.

Specific comments

Page 2834, Lines 13-16: It is not clear to me how the study addressed objective 1, since model parameters are barely covered in this paper.

We will rephrase the objectives of this study to "evaluate the newly coupled WRF-ACASA model simulate surface meteorology from the diurnal to seasonal cycle over California, a region with a complex terrain and heterogeneous ecosystems".

Pg. 2834, Lines 1-2: "The mass-based terrain following coordinate in WRF improves the surface processes." This sentence is vague, which surface processes are improved with the terrain-following coordinate?

We will clarify and provide reference to this statement.

Page 2840, Line 28: Precipitation is not included in the results/discussion.

We agree with the reviewer and we will remove the mention of precipitation.

Page 2841, Lines 16-22: Figure 4 is not entirely necessary. The reasoning behind only using days with 24 hours of data is well explained and well justified without these sentences and the figure.

We will remove Figure 4 as reviewer suggests.

Page 2842: For the reader unaccustomed to maps of California, it would be helpful to explain where the Central Valley is – for example by stating that it is seen as the oval region of relatively warm temperatures (if true . . .). Otherwise, if Fig. 2 included a topographic map it would probably be more clear where the valley is.

We will make clear in Figure 2 the different regions of California in the revised manuscript.

Page 2843 Line 12-14: Related to the above, the meaning of this sentence is not entirely clear if you don't know exactly where the Central Valley is. The LAI is highest in the middle of the Central Valley, so ACASA simulates a higher latent heat flux and cooler temperatures than NOAH. Even though NOAH is a big-leaf model, does it scale the fluxes to get canopy level fluxes (i.e. by leaf area index or absorbed PAR)? And are the LAIs the same for NOAH and ACASA?

We will clarify this sentence by providing the following details: both NOAH and ACASA use the same set of LAI values from the WRF model, however, ACASA distributes the LAI values into vertical layers according to vegetation types.

Page 2843, Lines 27-29: How are LAI values in ACASA determined?

See statement above.

Page 2844, Line 24 – Page 2845, Line 2: What is the height of the lowest sigma layer? Also can it be shown that the turbulent mixing is lower in ACASA or is this just conjecture? Do the two models have similar night-time sensible heat fluxes?

The first half sigma height is about 30 m, and the first full sigma height is about 60-100m. We will remove the issue regarding the PBL height and we will explain in more detail the night-time temperature bias in the revised manuscript.

Page 2845, Line 24-25: Is this a typo, it seems visible in Fig. 6 that the diurnal range is smaller in WRF-ACASA.

This analysis of the diurnal range will be assessed using Fig. 7. In addition, we will reconstruct Figure 6 to reflect daily variability instead of hourly temperature variation.

Page 2845, Line 29: I do not see a warm bias in NOAH during these times at the MC site.

We will improve the analysis of Fig. 6, based on daily means.

Page 2848: I am not sure what Table 2/Figure 9 add. Through the rest of the paper, there are 4 basins discussed and now there are 13 – how do these relate? It seems

this figure just confirms the previous analysis. If it's retained, the authors should show the equation used for the Degree of Agreement statistic. Also, in Fig. 9 the convention used in the other figures of red for WRF-NOAH and blue for WRF-ACASA is reversed.

We will move Table 2 and Figure 9 as supplemental documents and correct the colors on the figure. We will provide the statistical equation as suggested by the reviewer. And we will add the following statement to clarify the issue regarding the air basins: "At the time of the study, there are 13 air basins over California designated by the California Air Resources Board to represent regions of similar meteorological and geographic conditions. In this study, 4 basins are selected for more detailed analysis due to their distinct meteorological, geographic, as well as ecological attributes."

Page 2850, Line 5: The choice of land surface model clearly does affect the simulation (as is shown in Fig. 10-11), but maybe not the overall basin-wide biases.

We agree with the reviewer that the choice of land surface model does affect the simulation on certain stations but maybe not the overall basin-wide biases. We will clarify this in the revised manuscript.

Page 2850, Line 8: What is meant by "This" at the beginning of the sentence? Do you mean the poor model performance in both NOAH and ACASA? It is a little unclear since the previous sentence addresses atmospheric processes, but this sentence refers to surface properties. Also the following sentence is hard to understand as its written.

"This" refers to the poor model performance in both NOAH and ACASA over the Mojave Desert. We will take the suggestion of the reviewer to rewrite this section to make it easier to understand.

Figure/Table Comments:

1. Fig. 2: Replace the numbers in Fig. 2a with labels for each vegetation type.

We will add a legend describing the vegetation types.

2. Figures 6 and 10 would be easier to interpret as difference plots (ie: Show the Model-Observations for each model). Or, plot the daily averages in Figures 6 and 10 since the diurnal cycle is examined in Figures 7 and 11.

We will plot the daily averages of Figure 6 and 10 as suggested by the reviewer.

3. It would be useful for the four basins to be shown in Fig. 2 or 3.

We will merge Figs. 2 and 3 and add a panel showing the location of the 4 basins.

4. Fig. 3: Show the 4 stations used in the analysis in a different color/symbol.

We will highlight the 4 stations in Figure 3.

5. Table 1: Remove the column for "Vegetation" since these numbers have little meaning to non-ACASA users.

We agree with the reviewer and will remove the "Vegetation" column from the revised manuscript.

Technical comments:

Page 2850, Line 13: Typo ("pervious") Page 2849, Line 28:

We will correct the typo on this line.

Remove the first part of this sentence ("Figure 12 shows . . . surface temperature,").

We will remove the part of sentence as reviewer pointes out.