

Interactive comment on “Coupling between the JULES land-surface scheme and the CCATT-BRAMS atmospheric chemistry model (JULES-CCATT-BRAMS1.0): applications to numerical weather forecasting and the CO₂ budget in South America” by D. S. Moreira et al.

Anonymous Referee #3

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The authors develop the regional atmospheric modeling system CCARTT-BRAMS continuously by implementing the soil/vegetation model JULES. They evaluate the new model approach in terms of key meteorology parameters and the green house gas CO₂ using in-situ observations, and discuss its potential improvement in relevance to previous model approach. The study is interesting and valid since this kind of regional atmospheric modeling system, coupling processes in atmospheric dynamics and chemistry with land ecosystem, is necessary for air quality and climate study. However, the paper

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in its current presentation format needs major revision to gain the value of the work.

General remarks:

The paper needs to be more quantitative. Authors point out repeatedly that the new model system improves (significantly) simulation without giving details of statistical confidence, particularly in abstract and conclusion. It would be helpful if the authors could summarize the statistics used to evaluate the various model approaches in a table so that readers can easily recognize the degree of improvement of the new model approach.

The paper needs to be more concise. For example, in the abstract, the authors describe the evaluation of CO₂ with observation repeatedly in lines 3-8, 12-14, and 17-19. Furthermore, it should not be necessary to discuss CO₂ separately from other meteorology parameters unless the authors discuss other distinctive performances for it. Please go through the text to delete redundant phrases/sentences, some of which are pointed out in the specific comments.

The paper needs more clarification. There should be a brief description and discussion of the databases that are used for the model evaluation and the models that are used to provide initial and boundary conditions. Also, acronyms should be described the first time they appear, such as CPTEx/INPE, ECMWF, ERA, and NEE.

Specific comments: 1. Abstract: It is vague to just mention that the new model provides a significant gain in its performance. Please also make the abstract in more concise and organized. 2. Page 455 line 5: What does “pressure reduced to mean sea level” mean? 3. Page 455 lines 9-11: It is not necessary to mention this specifically in the abstract. 4. Page 456 Lines 22-26: repeat of lines 11-15 5. Page 457 Lines 1-15: This is a general discussion fitting in for all modeling work and not necessary to be presented in this paper. 6. Page 458 lines 6-14: Add a description for the measurements that are used in model evaluation somewhere probably in section 2 after the description of the models. 7. Page 460 lines 7-12: Rephrase this sentence.

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8. Page 460 line 19: Elaborate on size distribution and complex refractive index used in this work. 9. Page 464 lines 6-10: Please explain “the land use map” and “the soil type” used in this work. 10. Page 466 lines 2-8: Move the description of the observation data to section 2. 11. Page 468 lines 7-14: It may be misleading to give the scores on a daily basis since the bias during day and night may have an opposite sign. For example, the performance of ECMWF in comparing temperature at 2m is worst among the four approaches if the evaluation is separated over day and night. However, if the overall bias of ECMWF would be better than that of LEAF if the evaluation was conducted on a daily mean basis. 12. Pages 468-470: for the discussion of Figure 4 and Figure 6, please add tables to list the bias/RMSR during day/night for the discussed parameters. 13. Page 471 lines 4-21: How sensitive is the JULES-CCATT-BRAMS simulation to NCEP bias? Does the simulation of JULES-CCATT-BRAMS enlarge or shrink the input initial and boundary errors? 14. Page 474 lines 13-17: How do you know the interpolation delivers a lower value? Could you justify this conclusion with any theory or example? 15. Page 474 lines 14-15: Briefly describe “to be quite complicated”. 16. Page 474 lines 12-14: Discuss why the surface CO₂ simulated by the model agrees with observation at tower km-67, but not at the four sites of Amazon basin. 17. Page 474 lines 16-17: Is it possible to obtain any optimal setting constrained by local observations over Amazon basin?

Technique correction: 1. page 455 line 24: Add “to be” before “able to produce ...”. 2. Page 458 line 3: Change the sentence to “... was able to simulate most of the ...”. 3. Page 458 line 6-14: You sometimes use “section 2”, sometimes “sect. 4”. Please be consistent. 4. Page 458 line 17: BRAMS has been spelled out on page 457 line 15-16 already. 5. Page 459 line 20: Is “tracers em form of” a typo? 6. Page 460 line 28: Change “PREP-CHEMSRC” to “PREP-CHEM-SRC”. 7. Page 461 line 17: Change “schematic form the processes” to “schematic form of the processes”. 8. Page 465 line 3-4: Change the sentence to “The time in processing JULE-CCATT-BRAMS in relation to CCATT-BRAMS was increased around 17%.” 9. Page 469 line 16: Delete “as well”. 10. Page 470 line 18-19: Change the sentence to “The monthly mean column amount

of particulate matter less than 2.5 μm (PM_{2.5}) in the area showing in Fig.7a ...” 11. Page 472 line 5: Change the first “observed” to “observation”. 12. Page 472 line 20: Change “(Gatti et al., 2010)” to “Gatti et al., (2010)”. 13. Page 474 line 8: Change “Fig. 10” to “Fig. 9”. 14. Table 1: Please check whether “No-hydrostatic” should be “Non-hydrostatic”. 15. Table 2: Why does rootd_ft have 6 instead of 7 values.

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