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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 CO_2 budget in South America" *by* D. S. Moreira et al.

Anonymous Referee #2

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I can summarize this paper: "we replaced LEAF with JULES and ran BRAMS." That's fine, but is it really worthy of publication? I'd rather see this new coupling used to explain something about South American meteorology and/or ecophysiology. The new coupling shows improvement over LEAF in the metrics shown for evaluation, but a lot of the model tendencies remain (increased daytime RMSE for dewpoint temperature, for example). Also, MODIS data from 2001-2002 is used to prescribe phenology, but cases from 2010 were simulated. I'm pretty sure 2010 MODIS data is available-why

wasn't it used? Furthermore, we know there was regional drought in 2010; The model may have non-drought 2001-2002 phenology being forced by drought conditions in 2010; this is not self-consistent.

I also wonder about the choice to aggregate all of the observation/model datapoints into a single plot. I realize this can save space, but we know there is large variability across Amazonia, as both da Rocha et al. (2009) and Costa et al. (2010) describe transitions from water-limited to light-limited regimes across vegetation and moisture gradients in the basin. Is the improvement shown by replacing LEAF with JULES consistent across these gradients, or is there regional variability across the cerrado to the transition forest to the interior forest?

Offhand, I can think of several interesting science questions that might have been addressed with this model. For example:

âĂć Wang et al. (2007) replaced LEAF with SiB in BRAMS and described CO2 organization along fronts and patterns associated with regional drought in North America. I'd be very interested to see something similar for South America. Does JULES-CCAT-BRAMS (JCB) capture wet-season squall lines? How is the 3-d CO2 field organized around these lines? How is vertical transport different in wet and dry seasons, and how is that reflected in the CO2 concentration field?

âĂć Rong Fu and Wenghong Li (2004,2006) have a body of work describing the transition between wet and dry seasons and have described several mechanisms whereby the land surface influences atmospheric behavior. Does JCB reproduce what they've described? How is it similar/different, and does this support/refute their findings?

âĂć Lu et al. (2005) parameterized CO2 flux in RAMS and evaluated circulation along the Tapajos river. I'm sure there are other case studies that where surface-atmosphere exchange would be relevant and important to the analysis.

âĂć There is an ongoing disagreement between those who say the region exhibits ‘green-up’ during dry periods (Saleska et al., 2007; Huete et al. 2006) and those who claim it doesn’t (Samanta et al., 2010). Can JCB shed any light on this issue?

Some specific comments:

âĂć Page 456, lines 7-8: “Amazonia has been one of the largest contributors to atmospheric CO2 removal.” Are you sure about that? Inversion studies show that whether tropical SA is a source or sink is not known (Stephens et al., 2007; Gurney et al., 2008), and uncertainty is large. I think that it is well-established that tropical SA has a strong influence on the global CO2 growth rate in a given year (Rayner and Lawy, 1999; Bousquet et al., 2000; Friedlingsteing et al., 2006; Baker et al., 2006). This statement is provocative, and needs more than a single reference to justify it.

âĂć Page 457, line 13: I would not call LEAF an “outdated surface model”, as that phrase is somewhat derogatory to the LEAF developers. LEAF was a fine landsurface module, but has been surpassed as our knowledge increases. I might suggest that the authors use wording that lauds JULES for more explicitly resolved features, and the fact that it reflects the advances in our understanding of terrestrial biophysics that have been made in the last decade or so: praise JULES, as opposed to bad-mouthing LEAF. There are several instance in the text where this change might be made.

âĂć Equation 1: multiple instances of an identical term (ds(bar)/dt) do not really assist the reader; I suggest a conceptual equation, where the terms are shown in a qualitative manner (mixing ratio = advection + PBL diff + deep conv. . .).

âĂć Page 460, lines 11-12: “as well as not does not cause. . .” Looks like a typo that needs to be fixed.

âĂć Page 460, lines 18-19: should be “Reid et al. (1998).”

âĂć Page 461, line 13: JULES is composed of MOSES and TRIFFID. MOSES has many characteristics in common with the models that Saleska et al. (2003) demonstrated were unable to capture the seasonal cycle of carbon flux at Tapajos (K67,

K83). Furthermore, Hadley Centre runs using JULES show a positive temperature bias in Amazonia (Huntingford et al., 2004). This suggests that JULES generates unrealistic vegetation stress during the dry season, which will influence the Bowen ratio, and therefore the weather in the model using JULES as a lower boundary. Has JULES been evaluated against eddy covariance flux towers in Brazil? Does it capture observed annual cycles of carbon flux, latent and sensible heat? If so, then citations to that effect would be appropriate. If not, that might be another science application that would make the paper much more interesting.

âĂć Page 462, lines 9-10: “where trees normally earn of grasses. . .” I don’t understand what that means.

âĂć Page 464, lines 5-15. Is NDVI from MODIS used, instead of MODIS LAI/fPAR products? If so, a reference to how NDVI is processed into model parameters would be appropriate (e.g. Sellers et al.,1996). Also, cloud masking of spectral indices is a real issue in this part of the world (Sellers et al., 1996, Los et al., 2000). A citation or short description of how this issue is dealt with would be appropriate.

âĂć Page 465, lines 14-16: I understand the difference between experiments, but I’m not sure I understand the difference between the 30/31 members.

âĂć Page 466, lines 20-22: This sentence is redundant.

âĂć Page 468, lines 4-18: A negative temperature bias suggests (perhaps) an anomalously small Bowen ratio. Is this true? If so, JULES is a departure from most other models, which are biased positive (hotter) when compared to obs. This is very interesting, and might be fleshed out a little bit. Possible reasons for bias would be radiative transfer or transpiration, and comparisons at flux towers (see Lu et al., 2005) might be instructive.

âĂć Page 475, lines 13-14: “the simulations of vertical profiles over 4 sites of Amazon basin and for wet and dry seasons did not show very accurate agreement. . .” It seems to me that in some cases the agreement is good, other cases not so. I'd like to see some discussion of this and analysis of where the disagreement might come from.

All in all, the paper is written well and describes the component models reasonably. But it doesn't address any science questions; after reading the paper I haven't learned anything new about Amazonia, and this is a missed opportunity. I believe that to merit publication, the authors need to do more than just explain how they coupled a new land module into BRAMS. For that reason, I recommend rejection of this manuscript, with the caveat that I strongly encourage the authors to resubmit once they've addressed a science question or two. There are plenty of topics to select...

References:

Baker, D.F., R.M. Law, K.R. Gurney, P. Rayner, P. Peylin, A.S. Denning, P Bousquet, L. Bruhwiler, Y.-H. Chen, P. Ciais, I.Y. Fung, M. Heimann, J. John, T. Maki, S. Maksyutov, K. Masarie. M. Prather, B. Pak, I. Taguchi and Z. Zhu (2006), TransCom 3 inversion intercomparison: Impact of transport model errors on the interannual variability of regions CO2 fluxes, 1988-2003. Global Biogeochem. Cy., 20, GB1002, doi: 10.1029/2004GB002439.

Bosquet, P., P. Peylin, P. Ciais, C. Le Quere, P. Friedlingstein, P.P. Tans (2000), Regional Changes in Carbon Dioxide Fluxes of Land and Oceans Since 1980. Science, 290, 1342-1346, 17 November 2000.

Costa, M.H., M.C. Biajoli, L. Sanches, A.C.M. Malhado, L.R. Hutyra, H.R. da Rocha, R.G. Aguiar, A.C. de Arau ÌAjo, 2010. Atmospheric versus vegetation controls of Amazonian tropical rain forest evapotranspiration: Are the wet and seasonally dry rain forests any different? J. Geophys. Res., 115, G04021, dpi:10.1029/2009JG001179.

da Rocha, H.R., A.O. Manzi, O.M. Cabral, S.D. Miller, M.L. Goulden, S.R. Saleska, N.R. Coupe, S.C. Wofsy, L.S. Borma, P. Artaxo, G. Vourli- tis, J.S. Noguiera, F.L. Cardoso, A.D. Nobre, B. Kruijt, H.C. Frietas, C. von Randow, R.G. Aguiar, J.F. Maia, 2009.

Patterns of water and heat flux across a biome gradient from tropical forest to savanna in Brazil. J. Geophys. Res., 114, G00B12, doi:10.1029/2007JG000640.

Friedlingstein, P.F., P. Cox, R. Betts, L. Bopp, W. von Bloh, V. Brovkin, P. Cadule, S. Doney, M. Eby. I. Fung, G. Bala, J. John, C. Jones, F. Joos, T. Kato, M. Kawamiya, W. Knoff, K. Lindsay, H.D. Matthews, T. Raddatz, P. Rayner, C. Reick, E. Roeckner, K.-G. Schnitzler, R. Schnur, K. Strassmann, A.J. Weaver, C. Yoshikawa, N. Zeng, (2006), Climate-carbon cycle feedback analysis: Results from the C4MIP model intercom- parison, J. Clim., 19, 3337-3353. Fu, R. and Li. W. (2004), The influence of the land surface on the transition from dry to wet season in Amazonia. Theor. Appl. Climatol., 78, 97-110, doi:10.1007/s00704-004-0046-7.

Gurney, K.R., D. Baker, P. Rayner and A.S. Denning (2008), In- terannual variations in continental-scale net carbon exchange and sensitivity to observing networks estimated from atmo- spheric CO2 inversions for the period 1980 to 2005. Glob. Biogeochem. Cyc., 22(3), doi:10.1029/2007GB003082.

Huete, A.R., K. Didan, Y.E. Shimabukuro, P. Ratana, S.R. Salexska, L.R. Hutyra, W. Yang, R.R. Nemani, R. Myneni (2006), Amazon Rainforests Green-up with Sun- light in Dry Season. Geophys. Res. Lett., 33, L06405, doi:10.1029/2005GL025583.

Huntingford, C., P.P. Harris, N. Gedney, P.M. Cox, R.A. Betts, J.A. Marengo, J.H.C. Gash (2004), Using a GCM ana- logue model to investigate the potential for Amazonian forest dieback. Theor. Appl. Climatol., 78(1-3): 177-185. Li, W. and Fu. R. (2004), Transition of the large-scale at- mospheric and land surface conditions from the dry to the wet season over Amazonia as diagnosed by the ECMWF re- analysis. J. Clim., 17(7), 2637-2651.

Los, S.O., G.J. Collatz, P.J. Sellers, C.M. Malmstrom, N.H. Pollack, R.S. DeFries, L. Bounoua, M.T. Parris, C.J. Tucker, D.A. Dazlich, 2000. A global 9-year biophysical land surface dataset from NOAA AVHRR data. J. Hydrometeor., 1, 183-199.

Lu, L.X., A.S. Denning, M.A. da Silva-Dias, P. da Silva-Dias, M. Longo, S.R. Freitas, S. Saatchi, 2005. Mesosscale circulations and atmospheric CO2 variations in the Tapajos region, Para, Brazil. J. Geophys. Res., 110(D21), D21101, doi:10.1029/2004JD005757.

Rayner, P.J. and Law, R.M. (1999), The relationship between tropical CO2 fluxes and the El Nin ÌČo-Southern Oscillation. Geophys. Res. Let, 26(4), 493-496.

Saleska, S.R., S.D. Miller, D.M. Matross, M.L. Goulden, S.C. Wofsy, H.R. da Rocha, P.B de Camargo, P. Crill, B.C. Dauge, H.C. de Frietas, L.R. Hutyra, M. Keller, V. Kirchoff, M. Men- ton, J.W. Munger, E.H. Pyle, A.H. Rice, H. Silva (2003), Carbon in Amazon Forests: Unexpected Seasonal Fluxes and Disturbance-Induced Losses. Science, 302, 1554-1557, 28 November 2003.

Saleska, S.R., K. Didan, A.R. Huete, H.R. da Rocha (2007), Amazon Forests Green-Up During 2005 Drought. Science, 318(5850), 612. (10.1126/science.1146663)

Samanta, A. S. Ganguly, H. Hashimoto, S. Devadiga, E. Vermote, Y. Knyazikhin, R. R. Nemani, R.B. Myneni (2010), Amazon forest did not green-up during the 2005 drought. Geophys. Res. Let., 37, L05401, doi:10.1029/2009GL042154.

Sellers, P.J. S.O. Los, C.J. Tucker, C.O. Justice, D.A. Dazlich, G.J. Collatz, D.A. Randall (1996b), A Revised Land Surface Parameterization (SiB2) for Atmospheric GCMs. Part II: The Generation of Global Fields of Terrestrial Biophysical Param- eters from Satellite Data. J. Climate, 9(4), 706-737.

Stephens, B.B., K.R. Gurney, P.P. Tans, C. Sweeney, W. Pe- ters, L. Bruhwiler, P. Ciais, M. Ramonet, P. Bousquet, T. Nakazawa, S. Aoki, T. Machida, G. Inoue, N. Vinnichenko, J. Lloyd, A. Jordin, M. Heimann, O. Shibistova,R.L. Langen- felds, L.P. Steele, R.J. Francey, A.S. Denning (2007), Weak Northern and Strong Tropical Land Carbon Uptake from Ver- tical Profiles of Atmospheric CO2, Science, 316, 22 June, 1732-1735.

Wang, J.-W., A. S. Denning, L. Lu, I. T. Baker, K. D. Corbin, and K. J. Davis, 2007:

Observations and simulations of synoptic, regional, and local variations in atmospheric CO2. J. Geophys. Res., 112, D04108,doi:10.1029/2006JD007410, 2007

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