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Interactive
Comment

Interactive comment on "Coupled carbon-water exchange of the Amazon rain forest, II. Comparison of predicted and observed seasonal exchange of energy, CO_2, isoprene and ozone at a remote site in Rondônia" *by* E. Simon et al.

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This manuscript evaluates the 1-D multi-layer canopy model described in the companion paper by comparing model predictions with field observations. Although the model is somewhat unstable during periods when environmental conditions are changing rapidly, in general, model predictions of diel behavior of energy, CO2, H2O, isoprene and O3 fluxes agree quite well with observations. In my review of the companion paper describing the model, I asked the authors to include a paragraph describing the potential uses of the model. Is it just a description of a complex system, summarizing in mathematical form our understanding of how the system operates, or does it also

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have explanatory and predictive capabilities? These questions are addressed in the current manuscript, in which the model is used to help understand the workings of the system and also to predict system behavior under different scenarios, but I found the results somewhat disappointing.

The third stated goal of the manuscript (p. 402), “Diagnostic model application” gets to the heart of my concerns. “To what extent,” the authors ask, “does the model explain observed variabilities of net fluxes and concentration profiles?” and “. . . how does the model contribute to our understanding of the processes involved?” When seasonal flux variations or concentration profiles through the canopy were not well simulated by the “reference” parameterization of the model, various model parameters were modified in an attempt to improve the fits. And, sure enough, the fits improved. Does this “explain” the variabilities? Well, perhaps, but I’m not convinced. In my review of the companion model description, I took mild exception to the choices made to capture the seasonal changes in photosynthetic rate. The light use efficiency term (alpha) and the temperature response of electron transport were adjusted, but with no experimental or observational justification. The model behavior improved, but I didn’t feel that these changes “contributed to our understanding of the processes involved.” Likewise with respect to ozone deposition. Seasonal variations in deposition were well simulated only by imposing an arbitrary change in cuticular conductance, unsupported by any evidence. As the authors admit (p. 423) “strong seasonal variability of cuticular resistance to O_3 is unlikely.” So what exactly does this model simulation teach us?

The model is also used to generate the de rigueur doubled CO_2 scenario. And while the results suggest a number of potentially important impacts, the authors are quick to admit that “the results should be interpreted with caution,” since none of the additional potential effects of long-term exposure to elevated CO_2 are considered, including changes in canopy structure, soil nutrient status, changes in leaf biochemistry, climate, etc., etc.–the list is almost endless. Given all this, I’m not sure I see the point of the simulation. I recommend removing it from the manuscript.

But maybe I'm being a little unfair. . .Nah! Models such as this that allow us to scale up observations at the small scale to the canopy scale and beyond do serve a crucial purpose. Were a follow-up study to occur at one of the tower sites in Amazônia, a model such as that presented here would I think serve the very useful purpose of focusing the research on areas that could directly address some of the issues left unresolved in the current manuscript. Is O3 deposition to wetted surfaces a significant sink in wet tropical forests? Is chemistry a possibly significant O3 sink? What photosynthetic submodel parameters really do change seasonally to explain the observations? How is the sink for isoprene in the lower canopy best explained? Etc. One of the major outcomes of a field campaign such as EUSTACH is the development/parameterization of a model such as this, which is a huge amount of effort and summarizes in a very useful and (hopefully) more or less mechanistic way our understanding of how the system functions. This is great as far as it goes, but all too often this is the end of the story. Now that the model is operational, it should serve as a tool, directing future research, and future observations should be used to improve model performance. Anyone want to go back to Rondônia?

p. 406, l. 6 The model assumes no soil moisture dependency on soil respiration; is there any data demonstrating that a change from 25% to 15% soil water content has no affect on respiration?

p. 407, l. 12 suggest "negligible" for "neglected", although recent evidence (Goldstein et al., GRL, 2004) suggests that chemical losses within/above forest canopies may not be negligible. I'm not suggesting the authors include a chemical sink in their model, but given the considerable interest (controversy?) surrounding the Goldstein results, I think they merit at least a mention.

p. 408, l. 23 justification for neglecting any chemical ozone sinks is based solely on NO-NO2-O3 reactions; possibility that unmeasured, highly reactive VOC (some monoterpenes and sesquiterpenes with lifetime vis-à-vis O3 on the order of a very few minutes) should at least be mentioned

p. 409, l. 19 It's not immediately clear that "surface" in this context (and Ts) refer to the leaf surface

p. 410 (Fig. 3) Why, mechanistically speaking, should shaded leaves in the upper half of the canopy be significantly warmer ($>$2C) than air temperature?

p. 415, l. 9 "too weak" and "too small"

p. 417 (Fig. 9) units in panel 'c' should be mg m-2 h-1

p. 417, l. 14 same technique? Rinne et al. used eddy covariance and eddy accumulation (also, the 4th author was left off the citation–a shocking omission!)

p. 418, l. 28 Given the preponderance of solid evidence (references cited plus several others) that isoprene emission capacities decline substantially in response to decreasing light as one moves down through a forest canopy, I was a little surprised that the "reference" case assumes a constant emission capacity. Guenther et al. (1999; JGR 104:30625) which should probably be cited, presents a modeling scheme in which emission capacity is reduced as a function of cumulative LAI, which results in about a 69% reduction in emission capacity at the bottom of a dense canopy. This was also the scheme adopted by Harley et al. (2004). Not that there is anything special about that formulation; the one chosen in this manuscript gives very similar results; I just don't understand why the demonstrably false assumption of constant emission capacity is used as the reference case, and subsequently used to estimate a global total for isoprene emissions from tropical forests. In addition, the "isothermal surface" and "isothermal canopy layer" approaches (p. 421, l. 7) are (apparently?) superimposed on the unrealistic reference scenario, rendering them equally unrealistic.

p. 421, l. 6 I also find it a little strange that agreement with the G95 global estimate almost seems to be used as a criterion for validating the model results. The G95 estimates are, of course, highly uncertain (+/- 40%?) and while it's certainly valid to compare results form different models, I think the authors should choose what they consider their most realistic set of assumptions, run the model and present their results. Any model assuming constant isoprene emission capacity does not fit this criterion.

p. 420, l. 8 Fig. 10

p. 422, l. 11 suggest “twice as high”

p. 423, l. 3 I don’t really see any justification for doubling modeled fluxes by reducing the cuticular resistance for O3 by a factor of 5. Given the lack of measurements of this parameter, and the high uncertainty in assigning any value, adjusting the model output by varying this parameter becomes little more than a curve fitting exercise. I.e., there seems to be no physiological justification for doing so. And in any case, one obtains better fits during one season only at the expense of poorer fits for the other season. [Although my inclination, also unsupported by any evidence whatsoever, is to increase chemical losses within the canopy (and increase photochemical production of O3 above the canopy), invoking such a process would likewise improve fits for one season while fits during the other season would deteriorate.]

p. 424, l. 17 also changes in leaf physiology, i.e., “acclimation” to doubled CO2

Interactive comment on Biogeosciences Discussions, 2, 399, 2005.