Review: The biophysics, ecology and biogeochemistry of functionally diverse, vertically- and horizontally-heterogeneous ecosystems. The Ecosystem Demography Model, version 2.2- Part 1: Model description

By Longo et al.

Larger Impressions

This paper describes the code used in ED2.2. That's pretty much it. There isn't really any 'new science' here, and in fact any text not devoted to explaining equations is just showing that the model gives reasonable results and conserves energy (there are a few paragraphs showing differences in size/age distributions for two tropical sites with different disturbance regimes). It can be hard to get a paper like this through review, but the authors are lucky to have me for a reviewer. I know the value of papers like this (e.g. the BATS NCAR Technical Manual by Dickinson et al., the LSM Technical Note by Bonan, and the Sellers SiB papers from 1986 and 1996). However, the people reviewing the methods paper want to see results, and vice versa, and they want the paper to be short. But these 'code papers' have value for the people who use models, and I appreciate that because I am one of them.

I understand there is no way to combine the Parts 1 and 2 of the ED2.2 paper. This weighty tome already comes in at over 100 pages (paper plus supplements for Part 1), yet it is critical to make the code information available for people who will use the model. Some might suggest a technical manual (like is done for CLM), and the authors have in fact done this; I took a quick look at the wiki, and I think it has very useful information for users, but it doesn't really lay out the rational for the code. Also, the authors want to get journal citations and credit for the work they've done, and I don't blame them one bit.

I'm not going to download the code and study it line by line to see if the explanations make sense. There is no way I could do that and get a review back in under a year. Therefore, it is incumbent upon the authors to *very carefully* go over the manuscript and check for typos in the equations as they appear in the paper.

Initially, I thought that perhaps I was the wrong person to review this paper. I have years of experience with SiB and CLM, but none with ED. But then I realized that makes me the perfect person to review; someone familiar with ED will already know much of the material. But if ***I*** can understand how ED2.2 works after reading the paper, then the authors have done their job. And I think they've succeeded. I feel fairly comfortable, for the most part, about the ED2.2 framework after reading the paper (multiple times). This paper will be useful for researchers learning or developing ED, and other models, in the future.

My formal recommendation is to accept the paper for publication, with minor revisions. I don't need to see it again.

I really like the use of enthalpy; that is an innovative way to demonstrate conservation of energy, and I'm not sure it has been used before.

I'd like to see more emphasis on what is new in ED2.2 (final paragraph in the Introduction). A bullet list might draw the reader's eye to the new features in this version of the model.

Specific Comments

(many of these are suggestions for grammar, and in some cases need not be implemented exactly as I suggest. They are just places where I noted typos and grammar issues. I also apologize for location indicators; in my copy there were new line numbers on each page, and after about page 26 I found a line numbered 5 at the bottom of the page sometimes.)

- Abstract, line 11: "out and is presented"
- Page 2, lines 5-15: This description of generational advances in model development does not align exactly with Sellers et al. (1997). I think it would be helpful to acknowledge the Sellers paper and put the descriptions here in that context.
- Page 3, line 7: SiB does not have an explicitly layered canopy or sunlit/shaded leaves separately treated.
- Page 3, lines 12-13: I'm confused here. I thought models were transitioning from broadly-defined 'biomes' to a PFT-based mosaic structure. This sentence says the opposite.
- Page 6: The full set of PFTs is not listed. In table S5 we're shown parameter values for the tropical grasses and trees used here, but if this paper is going to be the 'go to' manual for ED2.2, all PFTs should be listed in a table somewhere. Don't worry about the extra length-this paper is already incredibly long.
- Page 7, line 15: I'd like to see the index k introduced here. I had to wade through a bit of text in the supplements before I realized that k addressed cohorts (this might also have to do with the fact that I had a hard time seeing k in the lettering in Figure 1. It might be helpful to have a small table showing the indexes used to address sites, patches, and cohorts. By the time I had read the paper several times I think I had it figured out, but a more explicit explanation might be helpful.
- Page 9, lines 31-32: How do you specify CO2 mole fraction on the timescale of the model? I'm not aware of CarbonTracker or GlobalView products that give that kind of resolution, and products with temporal averaging will cause issues with your carbon exchange during diurnal cycles (I think Jih-Wang Wang et al.,2007, talks about this). I don't see any mention of CO2 drivers in the wiki either. We've always calculated atmosphere-CAS CO2 exchange using a constant atmospheric value, and the flux can be easily scaled during a mesoscale- GCM- or transport-model application when a time-varying atmospheric CO2 value is available in the lowest atmospheric level. This may be a recommendation more appropriate for the github wiki, but I think the authors need to explain to the user how to deal with it.

- Page 10, lines 12-13: “aboveground part each cohort” Huh? I think there is some re-wording needed here.
- Page 11, line 11: “components on the right-hand side”
- Page 15, line 1: I’m not sure I understand exactly what j-prime means. I think I know, as in there is no sub-surface runoff from any soil level above the bottom one, and no ground evaporation from layers below the surface, but this is not made explicit to the reader.
- Page 10, line 3: I’m not sure that holding energy, enthalpy, and water fluxes to zero is consistent with the explanation given on page 9, lines 24-26. If free drainage is allowed out of the bottom of the soil column, won’t W-dot g0,g1 be nonzero? This needs to be made more clear.
- Page 16, lines 1-2: If layer Ns+1 does not exist, why mention it at all? Does it exist in the code as a placeholder? If so, that should be stated.
- Page 17, line 3 or so “that changes we obtain” could be “then we obtain”
- Page 21, line 3: “because the enthalpy” could be “due to the enthalpy”
- Page 28, line 22: “surface x is at temperature T with a liquid”
- Page 28, line 5 (bottom of page): “and Leuning” could be “and the Leuning”
- Page 31, lines 9-15: This temperature restriction is similar to what we’ve used in SiB for years. We also have a frost ‘delay’ term, where plants do not rebound immediately to photosynthesize during periods where temperatures may go below freezing (think spring in higher latitudes). I’d be happy to share it with you. Also, we have a humidity restriction term.
- Page 33, water extraction by roots: OK, so plants can extract water from all layers “to which they have access” (which, in a 3-layer soil I imagine is all of them), but roots have a uniform mass distribution. I think I might know why this is done. In the real world, I would expect a shorter/younger cohort to be less deeply rooted than an older/taller cohort, and grasses to be shallower rooted still. I also imagine that when this was done in ED, the short/young/grass cohorts might have died due to lack of water because the old/tall trees took it all. This is fine, but you can’t have it both ways. In Section 6.2, “Heterogeneity of ecosystems” the authors claim ED 2.2 “…improves the characterization of heterogeneity…by the number of individuals, their height and rooting depth, and their traits and trade-offs that determine their ability to extract soil moisture…” which contradicts what is described in Section 4.6. These stories need to be made consistent.
- Page 37: “nonexistent”
- Page 37: “stand-level” is not defined in the paper. Does this mean polygon, site, or something else? Also, I’m not sure the significance of the paragraph comparing *stand variability* to *patch variability*. What does it mean?
- Page 38: “density in the canopy air space”
- Page 39: SiB has had a prognostic CAS since 2003 (Baker et al), based on Vidale and Stockli (2005). Just sayin’.
- Page 40, line 12: “access to and competition for”
- Page 40, lines 29-30: “is fundamental to explaining”

- Page 41, lines ?: "degradation is pervasive"
- Page 42, line 29: "has excellent conservation"

On to the Supplements! (I do not have specific line numbers in my supplements file; I'll just have to do my best with explaining where the comments address)

- Table S2: might help to add bulk specific enthalpy, and Temporary Surface Water.
- S2: What is a "leaf elongation factor", and how is it determined? There is a long equation to describe s_{lk}, but we aren't told what it *means*.
- S7: is the 'b' term the Clapp and Hornberger b? If not C+H, where does the value come from?
- S7, field capacity: I've seen several definitions for determining field capacity from things like moisture potential. Is there a reference for what is being used here?
- S9: "contains contributions from reflectance and transmittance"
- S12.1: Are you really able to avoid the 'material surface at the top of the CAS' problem under stable conditions? This has been a problem for years, and may be worth a publication of its own. If you've already written it, advertise it here.
- S15: soil moisture limitation on photosynthesis. There's been a lot of work done on this with regard to the fact that individual plants maintain photosynthesis as soil dries down from wilt point, until suddenly closing stomates (Colello et al., 1998; Kim et al., 2010). This behavior, while well-known on the plant scale, is problematic when imposed on the ecosystem scale, as it frequently results in binary, or 'on-off' behavior. Many methods have been utilized to deal with it (e.g. Laio et al., 2001; Porporato et al., 2001, 2002: Rodriguez-Iturbe 2000; Baker et al., 2008, 2013; Wood et al., 1992, to name just a few). I'd like to see more explanation of what you're doing. A graph showing how stress is imposed, from field capacity to wilt point, would be helpful. Is stress imposed in a linear fashion, or does it behave like the *btran* function in CLM? Is this function based on previous research (which should be cited), or something incorporated specially for ED2.2? If so, why?

Figures

- Figure 1: White text was difficult for me to read. It might be worth sacrificing the pretty clouds/sky background for something more simple. Or maybe just use red lettering.
- Figure 2: caption should say "dashed yellow arrows"
- Figure 3: caption should say there are 3 cohorts shown.

Nice paper, people. Good work.

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References

Baker, I.T., A.S. Denning, N. Hanan, L. Prihodko, P.-L. Vidale, K. Davis and P. Bakwin (2003), Simulated and observed fluxes of sensible and latent heat and CO2 at the WLEF-TV Tower using SiB2.5. Glob. Change Biol., 9, 1262-1277.

Baker, I.T., L. Prihodko, A.S. Denning, M. Goulden, S. Milller, H. da Rocha (2008), Sea- sonal Drought Stress in the Amazon: Reconciling Models and Observations. J. Geophys. Res., 113, G00B01, doi:10.1029/2007JG000644.

Baker, I.T., H.R. da Rocha, N. Restrepo-Coupe, R. Sẗockli, L.S. Borma, O.M. Cabral, A.O. Manzi, A.D. Nobre, S.C. Wofsy, S.R. Saleska, M.L. Goulden, S.D. Miller, F.L. Cardoso, A.S. Denning (2013), Surface ecophysiological behavior across vegetation and moisture gradients in Amazonia. Agr. Forest Meteorol., 182-183, 177-188, doi: http://dx.doi.org/10.1016/j.agformet.2012.11.015.

Bonan, G.B, 1996: A land surface model (LSM version 1.0) for ecological, hydrological, and atmospheric studies: Technical description and user's guide. NCAR/TN-417+STR, 150pp.

Colello, G.D. and Grivet, C., P.J. Sellers, J.A. Berry (1998), Modeling of Energy, Water and CO2 Flux in a Temperate Grassland Ecosystem with SiB2: May-October 1987. J. Atmos. Sci., 55, 1141-1169, 01 April 1998.

Clapp, R.B. and Hornberger, G.M. (1978), Empirical equations for some soil hydraulic properties, Water Resour. Res., 14(4), 601-604.

Dickinson, R.E., A. Henderson-Sellers, P.J. Kennedy, 1993: Biosphere-Atmosphere Transfer Scheme (BATS) Version 1e as coupled to the NCAR Community Climate Model. NCAR/TN-38+STR, 72pp.

Haynes, K.D., I.T. Baker, A.S. Denning, R. Sẗockli, K. Schaefer, E.Y. Lokupitiya, J.M. Haynes (2019). Representing ecosystems using dynamic prognostic phenology based on biological growth stages: Part 1. Implementation in the Simple Biosphere Model (SiB4). Accepted for Pulication in J. Adv. Mod. Earth Sy.

Laio, F., A. Porporato, L. Ridolfi, I. Rodriguez-Iturbe (2001), Plants in water-controlled ecosystems: active role in hydrologic processes and resppnse to water stress II. Proba- bilistic soil moisture dynamics. Adv. Water Resour., 24, 707-723.

Porporato, A., F. Laio, L. Ridolfi, I. Rodriguez-Iturbe (2001), Plants in water-controlled ecosystems: active role in hydrologic processes and response to water stress III. Vegetation water stress. Adv. Water Resour., 24, 725-744.

Porporato, A., P. D'Odorico, F. Laio, L. Ridolfi, I. Rodriguez-Iturbe (2002), Ecohydrology of water-controlled ecosystems. Adv. Water Resour., 25, 1335-1348.

Rodriguez-Iturbe, I. (2000), Ecohydrology: A hydrologic perspective of climate-soil- vegetation dynamics. Water Resour. Res., 36 (1), 3-9.

Sellers, P.J., R.E. Dickinson, D.A. Randall, A.K. Betts, F.J. Hall, J.A. Berry, G.J. Collatz, A.S. Denning, H.A. Mooney, C.A. Nobre, N. Sato, C.B. Field, A. Henderson-Sellers (1997), Modeling the Exchanges of Energy, Water, and Carbon Between Continents and the Atmosphere. Science, 275, 502-509.

Sellers, P.J. and Y Mintz, Y.C. Sud and A. Dalcher, 1986: A Simple Biosphere Model (SiB) for Use within General Circulation Models. Journal of the Atmospheric Sciences, 43(6), 505-531.

Sellers, P.J., D.A. Randall, G.J. Collatz, J.A. Berry, C.B. Field, D.A. Da- zlich, C. Zhang, G.D. Collelo, and L. Bounoua,1996: A Revised Land Surface Parameteriztion (SiB2) for Atmospheric GCMs. Part I: Model Formulation. Journal of Climate, 9(4), 676-705

Vidale, P.L. and St¨ockli, R., 2005: Prognostic Canopy Air Space solutions for Land Surface Exchanges. Theoretical and Applied Climatology, 80, 245- 257, doi:10.1007/s00704-004-0103-2

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

Wood, E.F., D.P. Lettenmeier, V.G. Zartarian (1992), A land-surface hydrology param-eterization with subgrid variability for general circulation models, J. Geophys. Res., 97(D3), 2717-2728.