Review: Duarte et al., Evaluating the Community Land Model (CLM4.5) at a coniferous forest site in northwester United States using flux and carbon-isotope measurements.

This manuscript (MS) presented a difficult review. After the first couple of readings, I found the paper easy to read, with no obvious shortcomings. This should have prompted a favorable review, right? However, the MS does read somewhat like 'we ran a model, here is what happened' which is bothersome to me. The use of isotopes in a model is no longer novel; you need to go beyond just saying you put isotopes in a model and ran it, and say something about how this capability and our inclusion of it in models informs our understanding of natural systems. With more careful reading, more issues emerged for me.

1. The first paragraph talks about drought, and drought stress is mentioned throughout the MS. However, drought is an *anomalous* reduction in precipitation from climatological values. If, climatologically, 5% of annual precipitation occurs in JJA, then a dry summer is just…normal. Drought in this part of the country will likely manifest under climate change as a longer growing season and higher summertime temperatures and VPD when compared to the present-day. If meteorological forcing from the period 1998-2006 is cycled multiple times to simulate the period 1850-present, you're not going to capture any slow secular changes. Really, what's being done here is an evaluation of CLM's ability to capture a *mean seasonal cycle* that includes a dry summer. This site has a 50+ meter tall canopy, and fluxes (LE/H) are in phase with Boreal summer. LE fluxes appear to peak in June or July, and H peaks one month or so later, from a quick inspection of Figure 3. That suggests an ecosystem that may experience some stress in late summer, but is not water-limited on the whole. Costa et al. (2010) and da Rocha et al. (2009) discuss the notion of environmental and physiological stress, albeit in Amazonian forests. At Wind River, I suspect an ecosystem that is environmentally-limited for large parts of the year (winter, obviously) and may experience some physiological stress in late summer. There may be some value in studying interannual variability (IAV), as observed GPP in 2002 (a known western drought that year) and 2006 both taper off rather quickly following an early peak. CLM does not capture this behavior in 2002, but does in 2006. But the authors do not discuss IAV, but rather concentrate on the idea of annual drought stress. I'm not buying it; you're not going to have a 50-meter canopy in a region that is water stressed. Furthermore, βt drops below 1 in less than half of the years, and then only to a minimum of 0.6. That's not a whole lot of stress. I strenuously object to the use of the word drought for a dry summer that occurs every year.

2. As I think more and more about the paper, the notion of equifinality (multiple solutions in parameter or process space that result in a single

model outcome) keeps arising. A strength of CLM is that there is a large community of researchers banging on the model, and just about any process that one can imagine is included in the model and can be turned on or off (sunlit and shaded leaves; explicit nutrient cycles; hydraulic redistribution; multiple hydrology schemes; diagnostic canopy vs. DGVM; etc., etc.). This is also a CLM weakness. There are so many knobs to turn, how can you really be sure you are turning the right ones? With this in mind, I returned to Raczka et al. (2016, hereafter R16), which simulated Niwot Ridge (NR1, another site with similar vegetation type, but dissimilar climate). Since Raczka is the second author on this MS, and Duarte is the second author on R16, these papers offer an opportunity to present a body of work that compares and contrasts CLM behavior at two sites that have a similar dominant PFT but dissimilar climate (and soil, I presume). Imagine my surprise when I find that R16 claims that the nitrogen limitation scheme is a critical component, but is not considered in sensitivity tests in this MS! R16 is happy using CLM4.5 hydrology, while the current MS states that CLM4.0 hydrology was necessary to capture realistic behavior. I was encouraged to see that both papers suggest a change from standard BB C3 slope of 9 to 6 in the evergreen needleleaf PFT, and to see that both papers see excessive discrimination of ^{13}C. But I think there is an opportunity being missed here, to compare and contrast the results at two similar-but-not-identical sites. This is an opportunity to say something about how evergreen needleleaf forests behave, as informed by dissimilar CLM simulations. The first 2 authors are at the same institution, if not in the same department, and they need to be talking to each other. I think closer coordination/comparison/contrast of the results presented in R16 and here is required. Currently, I find myself more confused than anything. Can we trust CLM at all in evergreen needleleaf forest without extensive tuning from site-specific observations?

3. The authors question the veracity of the observations twice, with regard to H for 1998-2003, and SWC for 1998-2002. I urge extreme caution here, and sincerely hope the authors have corresponded with Dr. Wharton to express their concerns and make these statements with the understanding and approval of the site PI. This is the kind of situation that can foster distrust and animosity between the observational and modeling communities. I most strongly recommend that the authors verify that there may be uncertainty in these observational records, and the listing of Dr. Wharton as a coauthor would legitimize the claims as stated in the MS and confirm that she has participated in discussions of this issue.

4. Isotopes are difficult. They are difficult to explain, and difficult to understand for many (if not most) readers. The provenance of the treatment of carbon isotopes in CLM is poorly summarized in this MS. Oleson et al. (2013) does

not mention them; is Mao (2016) the seminal paper? What about Randerson et al. (2002) or Suits et al. (2005) which investigated isotopes using SiB, or the work of Van der Velde et al. (2013, 2014) which studies isotopes in SiB-CASA? Did isotopes in CLM build from that work, or from an independent source? R16 is cited, but elaboration is warranted, especially with regard to superposition of an annual cycle onto the larger trend in forcing data. The treatment of $\delta^{13}C_{ER}$ in models is extremely difficult to capture. Heterotrophic respiration is comprised of old, intermediate, and young components, and the $\delta^{13}C$ of each is difficult to constrain, as is the fractional contribution of each. The description of $\delta^{13}C_{ER}$ in section 3.3.1 is troubling; if I understand this section correctly, $\delta^{13}C_{ER}$ follows $\delta^{13}C_{GPP}$ in the daytime, then switches to follow $\delta^{13}C_{HR}$ at night. If the $\delta^{13}C$ of the C_{XS} pool has no sensitivity to recent discrimination, then I assume the pool is large enough that the $\delta^{13}C$ of this pool reflects some previous state. Is this true? What is that state? Is it realistic? The MS states that this behavior "aligns with expectations", but is it realistic? Is this behavior observed at sites with more detailed observations? If this behavior is not observed anywhere, how can you trust the model results? Similarly, the authors state that "Autotrophic respiration at Wind River is likely fueled by a mixture of stored and recently-fixed carbon, as indicated by ^{14}C measurements…(and) cannot be appropriately modeled by CLM with the current allocation scheme…" so if I understand correctly, we can't trust the $\delta^{13}C_{HR}$ because CLM doesn't consider different contributions from differently-aged dead pools, and we can't trust the $\delta^{13}C_{AR}$ either. Both $\delta^{13}C_{AR}$ and $\delta^{13}C_{HR}$ influence the $\delta^{13}C$ of the canopy air, which will in turn have a strong influence on $\delta^{13}C_{GPP}$! At this point I'm left thinking that we have no confidence in any of the discrimination values of this version of CLM, and any resemblance to observations is a happy accident.

I do not recommend rejection, but this paper requires major revisions to be acceptable for publication. The differences between the findings at Wind River and those of R16 must be reconciled, and the problems with the isotope treatment must be resolved. Furthermore, the characterization of climatological dry summer as 'drought' is unacceptable.

Specific Comments

- The plant wilting factor, w_i, can be expressed in a multitude of ways, and could have serious implications for this site. Entekhabi and Eagleson (1989) suggest a linear reduction in w_i from some point s* below field capacity (where w_i = 1) to a value of 0 at wilt point, while Sellers et al (1996) and Colello et al (1998) promote a nonlinear equation for w_i based on field data from FIFE. Baker et al (2008) demonstrate that in tropical forests, a direct linkage of w_i to the vertical profile of root density can be problematic. What form does w_i take in CLM? Neither R16 nor this manuscript discuss this; was it investigated? This suggests yet another path that can be taken to tune CLM. More equifinality.

- Discussing equation 5, the authors state that since $g_b >> g_s$, g_b can be neglected in the calculation of c_i/c_a, and therefore discrimination. In midday this is certainly true, but what about near sunrise/sunset, particularly sunrise? I can imagine that immediately after sunrise, the canopy is cool and the nocturnal inversion has not yet been broken. The canopy, however, is illuminated, and both temperature and humidity are favorable for stomatal conductance. In this situation I might expect that g_s could be larger than g_b. This might also have some bearing on the large excursions in $\delta^{13}C^{GPP}$ values seen early and late in the day. Was this investigated?
- Line 31, page 6; should be 'resulting', not 'resulted'
- Why is the observed δ13C of bottom canopy leaves so much lower than elsewhere in the canopy? Does this inform the CLM treatment of isotopes?
- ET will be composed of transpiration, leaf evaporation and ground evaporation components. In a dense canopy like Wind River, I would expect ground evaporation to be low, but excessive leaf evaporation could influence the amount of infiltration and therefore the amount of water available for transpiration later in the season. What is the partition of these components at Wind River? I know it is impossible to quantify these components with a single ET observation, but the model partition may give insight into behavior.
- The authors mention a "spring-to-summer decrease in the contribution of root respiration towards total soil respiration". That makes me think that the observed signal could come about from one of 2 ways. 1) there is a decrease in discrimination as WUE increases, or 2) there is an increase in the HR component towards older material with a lower $\delta^{13}C$. Is this possible?
- When discussing soil moisture, fraction of saturation may be more useful than volumetric water content. The volumetric content at various important points (wilt point, field capacity, saturation) can vary significantly depending on soil character.
- Page 13, lines 14-17: "Observed $\delta^{13}C_{ER}$ was found to have a low negative correlation with observed G_c, but not statistically significant. The low correlation was likely a result of $\delta^{13}C_{ER}$ reflecting constraints of prior environmental drivers in comparison with the more rapid response of G_c to more recent environmental drivers." This is not surprising, but doesn't it underscore the importance of getting a handle on all these drivers, in both your observations and model? Aren't you basically saying here that the model results are not to be trusted because the proper mechanistic pathways for the various isotopes are not simulated?

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