The motivation behind this study is excellent—recent empirical measurements have discovered a robust and interesting dynamic whereby plants can lose water at night. This has transformed our understanding of plant physiology coupled to the atmosphere. When such transformation of understanding occurs, models of these dynamics should also be updated, and this is what this study intends to do; and, in an important model—CLM—which has overly-simplified stomatal conductance parameterizations well-structured for updating.

I would have thought this study would be relatively straightforward, and I expected to review the paper without much comment, giving my stamp of approval for a good model update. However, there are potentially very serious critical flaws in the approach. This is evident in the methods and manifest in the shocking results (50% reduction in semi-arid soil moisture seems way too high and is rather questionable, given that nighttime transpiration in semi-arid regions should not be 50% of total daily transpiration—is this even physically possible, or is this substantiated by observations?). The authors equate nighttime conductance/transpiration with minimum conductance/transpiration (this is the fundamental change they implement). However, these are not the same thing. Nighttime transpiration does not occur all the time in the empirical literature within plants that it can occur. The minimum conductance can easily be 0, but when conditions are ripe—high VPD, etc.—nighttime transpiration kicks in. So, this could be a fatal flaw.

What should have been done is to make g0 a function of environmental variables (e.g., VPD) so that it can vary between 0 (or 10 or whatever baseline) and the observed gs,n data. This could probably help out the shocking semi-arid soil moisture 50% reduction results because VPD would probably be high only when the soil moisture was low anyway, so there wouldn't be much absolute loss. The authors kind of hint at this in Section 3 (which, noted, I enjoyed reading, though perhaps should have been part of the Introduction given that the authors are questioning themselves in it).

I also note that the authors seem to be missing a large piece of the empirical literature on nighttime transpiration, particularly from sap flow. Much of this was synthesized in a *Tree Physiology* special issue in 2007, and there have been many other papers since then using the technique for this analysis. These papers describe the environmental sensitivities of nocturnal transpiration, e.g., the mechanistic basis for updating the models as I suggested above.

Specific comments:

- Title: slightly awkward wording—it's not that observations cause an alteration of models.
- Title: I think that GMD has changed their policy and wants all papers to have model names and version numbers in the titles (e.g., CLM4.5SP).
- Abstract: here it would definitely be worth mentioning which land surface model.
- Abstract: since this is GMD, it would probably be worth noting in slightly more detail how nighttime conductance was "applied".
- Abstract/Results: while it is interesting to know that a change in modeled transpiration caused a change in simulated transpiration, and things linked to transpiration, it would be even more interesting to know how these modeled changes compare to the aforementioned empirical observations.

- There is some argument that says that nighttime transpiration could actually increase/improve carbon gain because of xylem refilling, i.e., there is water already ready to go once the first daylight hits. I would like to see this analysis in this paper.
- P10341L2: can you list which of the "many" models use only two g0 values?
- Methods: what does the "SP" in CLM4.5SP stand for? Satellite phenology? (I had to Google that).
- Methods: more description should be given to how the empirical values were collated for representativeness to PFTs, and the statistical implications and/or error propagation.
- Results: it is really hard to see the difference between the blue and green lines (the red line isn't great either) in Fig. 1. A re-draw of Fig.1 is in order.
- Results: "…(Fig. 5) illustrates that a minimum gs threshold improves transpiration estimates during the early part of the night…" This is really not illustrated. The color lines at night are barely distinguishable from one another, except maybe in July, but then the difference between simulated and observed is so different that it's hard to see the improvement. Fig. 5 does illustrate, however, that the model updates really actually didn't do much for total nighttime transpiration. This is both interesting and odd, given that I would expect some nighttime transpiration!

Looking forward to a revised paper (or a convincing rebuttal to my critiques, which were harsh, I know, sorry).

Best wishes,
Josh Fisher