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Interactive comment on "Improvement of Soil Respiration Parameterization in a Dynamic Global Vegetation Model and Its Impact on the Simulation of Terrestrial Carbon Fluxes" *by* Dongmin Kim et al.

Anonymous Referee #4

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I fear I cannot really write a more positive review for this very manuscript. I have to say that I am quite confused by this paper. In particular, I have two very fundamental concerns:

1. The authors write in the abstract "… (non-uniform spatial distribution of Q_{10}) … improves the simulation of gross primary production (GPP). It leads to a more realistic spatial distribution of GPP, particularly over high latitudes …". This statement suggests that GPP$= f(Q_{10,soil}, \ldots)$ which makes no sense at all to me. Or do I fundamentally misunderstand the model assumptions here? To me this sen-

tence suggests that either I don't understand the basic dynamics under scrutiny, or that the authors have been very sloppy in putting the manuscript together, or that there is indeed a very fundamental conceptual issue here. I fear that we are talking about the latter.

2. The authors write that "the Q_{10} value derived from soil respiration measurement tends to decrease with temperature because substrate availability decreases as temperature increases". To my mind this is rather reflecting that any regression model that considers abiotic drivers only, would be confounded by co-variations with e.g. substrate supply or other biotic drivers. This has been shown e.g. in Reichstein & Beer (2008), Mahecha et al. (2010), Wang et al. (2010), Graf et al. (2011), and we could cite more recent papers. Inferring from varying abiotic controls that Q_{10} should also vary across geographic locations is misleading. Non-constant parameters in biosphere models may actually reflect missing process detail: For instance, if one tries to subsume in Q_{10} variation of e.g. substrate supply then one is simply missing a good representation of the supply term. Various papers by e.g. Davidson (e.g. 2012 and more recent ones) explain this in a very didactic manner and should be studied before discussing this aspect further and tweaking models without actually developing the underlying model structures further.

To my mind, the authors first need to clarify these two aspects in a very convincing manner before discussing the "minor" issues of the manuscript. However, these other aspects that are in fact not so minor. For instance when the authors write that "the Q_{10} parametrization tends to enhance the relationship between Rs and soil temperature from CTL" they refer to Fig. 6 which show differences in respiration modelled with different runs and T_{soil}. But the scatters are all over the place (positive/negative) and it is unclear about what relationship we are talking here. And I find more examples of this kind in the text ….

So in the overall view, I would like to encourage the authors to carefully rethink what the focus of this study can be and what can be really learned with this experiments.

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