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Interactive comment on “Amplification and dampening of soil respiration by changes in temperature variability” by C. A. Sierra et al.

Anonymous Referee #1

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This paper is a technical note on the impact of temperature (T) variability to changes in respirations rates. The authors point out correctly that in most previous investigations, focus has been solely on the mean values of T, in particular increases due to climate change. They use a set of well-known models for respiration as a function of T, from extremely simple to more advanced, including one which is used in an established biogeochemical model (CENTURY). Their main line of argumentation centers around the observation that for many circumstances, these relationships have a positive second derivative, i.e. are convex. It is rather unfortunate that the Table 1 contains a number of mathematical errors affecting the convexity, thus part of the assumptions lack their basis. See detailed comments in the attached pdf. The analysis is straightforward and easy to follow. The results are intuitive and non-surprising. The paper is almost purely a theoretical study, and the obvious conclusion is that changes in variability will

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in general have an impact on respiration rates, as surely expected. The direction of change (increase or decrease with an increase in variability) is, however, different from model to model and also from location to location. This could have been concluded without any calculation, but this paper is quantifying the effect. In my opinion, the paper overstates the importance of the convexity of the relationship. This simplifies the analytical calculations, in particular the validity of the geometrical argument crucially depends on the assumption, but if one replaces the "larger than" sign in eq. (1) by a "non equal" sign, it becomes obvious that for any nonlinear relation between R and T, there will be an impact of changes in var T. Thus, var T is important in any case, and this should be noted in the paper as well since it almost has not been taken care of so far in previous publications. The authors actually use measured time series from three different locations, but nevertheless are very far away from a data analysis study since they construct artificial series from them - extending them to 10 years and changing them with respect to mean and variance. They only serve the purpose to illustrate the main finding with numerical examples. That the effect is different for boreal vs. tropical sites if one uses a model with an inflection point at $T=15.7$ deg C is hardly surprising. That the residuals of the original time series after 9-day low-pass filtering are mainly white noise I doubt very much. Thus, the simulated 10-year series are almost certainly different in their statistical properties (e.g. power spectrum) than measured ones would be. It is possible that the results are not affected qualitatively but only quantitatively. The idea of the paper is simple, performed straightforward, with obvious conclusions. It could qualify as a "technical note" if such a category would exist in Biogeosciences. At the same time, there are quite a bit of redundancies in the text, which thus could be shortened a good deal. All more detailed comments are provided in the attached pdf file. The mathematical errors and a few others have to be corrected, and the authors have to avoid the impression that it is the convexity of the relationship which determines the impact of var T on respiration. If all these changes, which I would summarize as "minor revisions", are taken into account, the paper is acceptable for publication in Biogeosciences.

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Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/7/C5200/2011/bgd-7-C5200-2011-supplement.pdf>

Interactive comment on Biogeosciences Discuss., 7, 8979, 2010.

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