Biogeosciences Discuss., 9, C4959–C4962, 2012
www.biogeosciences-discuss.net/9/C4959/2012/
© Author(s) 2012. This work is distributed under
the Creative Commons Attribute 3.0 License.

Biogeosciences Discussions

Interactive comment on "A model-based constraint of CO_2 fertilisation" *by* P. B. Holden et al.

P. B. Holden et al.

p.b.holden@open.ac.uk

Received and published: 19 October 2012

We are grateful for the helpful comments of both referees. All of the replies below will be incorporated into our revised manuscript. A supplementary document is attached. This details the LUC validation and implications for revisions to our analysis (most notably a change of the KC prior assumption).

Note abbreviated versions of the referee's questions/comments are included below to make the answers easier to follow (please see original for full detail of these questions)

Anonymous Referee #2

Q1: The GPP is expressed as a percentage change. While this is interesting it would be good to see what absolute values are inferred for GPP.

We will generate a pdf of absolute global GPP as suggested. This is a nice idea. The main limitation is that ENTSmL does not calculate GPP in cultivated regions (it assumes instantaneous harvest/grazing). The pdf will thus be limited to global GPP in regions unaffected by LUC, However, supplementary calculations with LPJmL (now underway), together with ecosystem-specific data-driven estimates of GPP (Beer et 10, 2010), will enable us to quantify this neglect.

Q2: The marginal probability distributions show a very spikey surface. This I find highly surprising. Is this due to an under-estimate of the structural uncertainty that leads to this?

These pdfs are derived by probability-weighting each of the 670 simulations according to their respective values of KC and VPC. The structural error assumption is applied in the calculation of these probabilities (i.e. it reduces the penalty for parameter values which are less likely to produce observational DeltaCO2). The spikiness arises because we do not apply a structural error assumption to the predicted quantities (land-atm flux, ocn-atm flux etc). Each simulation is regarded as a point prediction, rather than being associated with a probability distribution that reflects structural uncertainty in that output.

We agree that the spikes in the resulting pdf are unlikely to be meaningful. We will address this, either by applying separate structural error assumptions to the predicted quantities (a minimum value to smooth the pdf might be reasonable), or by clarifying the existing calculation in the text. Note that some level of structural error is implicit in the existing plots – the plotted data are binned averages and the degree of spikiness depends strongly upon the bin size. Simply broadening the bins to remove the spikiness may suffice here. Apologies, this aspect of the analysis should have been made clear in the original manuscript.

Q3: Does the addition of 4 new parameters (VPC, ALUA, etc) impact spin up states, or are they only influential in the response? Has any control drift in the spinup states been accounted for?

VPC and OL1 only affect simulations that are not in the preindustrial state (they are functions of the CO2 and global SAT anomalies respectively). ALUA and KC will have a small effect on these spin-ups due to LUC at 850AD (the spin-ups applied no LUC forcing). However, all simulations were run for 850 years with AD 850 boundary conditions to allow the equilibration of climate, vegetation and surface ocean, though clearly there will be remaining drift, especially from the sediments. The ensemble-averaged CO2 drift of 3ppm over the 850 year spin-on suggests that any residual drift (1850-2000) is very unlikely to be significant (cf 1-sigma structural error assumption of +/-17ppm). We will address this more thoroughly in the revised manuscript.

Q4: A table summarising the 4 experiments would be useful. Page 9434, line 10: Which comparison suggests that parametric is greater than scenario? Clarification needed in text.

Revised manuscript will clarify.

Q5: Language: discussion of what KC is would help the reader understand what is being discussed. And linking back to the process, rather than an obscure parameter name with the important discussions would help.

KC is the parameter in equations 3, 4, 5, discussed on p9430 l17-23. We agree it would be an improvement to name the parameter with more accessible language. We agree that a discussion of “what KC uncertainty represents” would be very useful and will be added (see text relating to Fig 8 of the supplementary material).

Q6: How sensitivity are the results to assumptions of the prior?

As discussed in the supplementary material, KC can be constrained by independent data and used to refine the validation of the model, an approach that will be followed in the revised manuscript. Vegetation carbon is well represented by ENTS, and a wide range of LUC vegetation uncertainty is encompassed. However, the response of soil

carbon to LUC is governed not only by ENTS uncertainty, but also KC uncertainty. We now consider that the prior for KC should be chosen to ensure that a reasonable range of historical emissions is encompassed. The supplementary material shows the calibrations with KC prior centered on 0.2 (existing manuscript), centered on 0.45 (our new base case, from comparison with Houghton, 2008), centered on 0.54 (from comparison with an LPJmL simulation) and with no prior. These sensitivities will be included in the revised manuscript.

Q7: The inclusion of structural error differs from that taken in Murphy et al. Would the structural error diagnosed in the Murphy method lead to a different estimate? A related question, would differences in the structural error term influence the results?

We will expand this discussion and the sensitivity of the results to the structural error assumption. Note that some level of sensitivity has been provided through the on/off analysis

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/9/C4959/2012/bgd-9-C4959-2012-supplement.pdf

Interactive comment on Biogeosciences Discuss., 9, 9425, 2012.