Biogeosciences Discuss., 9, C5115–C5118, 2012
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Biogeosciences Discussions

Interactive comment on "The climate dependence of the terrestrial carbon cycle; including parameter and structural uncertainties" *by* M. J. Smith et al.

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Received and published: 24 October 2012

Dear Bjorn Brooks,

Thank you for posting this. Your comments and questions imply to us that some of the relevant pieces of our Discussion section are not clear enough. To respond properly to your question here we will begin by stating clearly what our study does and does not assume, both for the full manuscript and for the additional analysis presented in the Discussion.

Scope of results. You are absolutely correct that all of the results presented in the Results section relate to regions of the Earth in which the carbon cycle is close to a state of dynamical equilibrium. Aiming to fully data constrain a global terrestrial carbon

model, assess the distribution of uncertainty, and identify a generalizable framework to manage future refinements was ambitious and to make it manageable for this first analysis it made sense to begin with data putatively at equilibrium, fit the model to subsets of that data, and compare the held out data with the predicted equilibrium states. This alone yielded sufficient insights to warrant publication. We then predicted the potential equilibrium state over a fine grid covering the world (Fig. 5) to assess the predicted geographical distribution of the carbon cycle at equilibrium, including uncertainty – although acknowledge that much of the terrestrial land surface is in a state of disequilibrium. Work to fully data-constrain the temporally dynamic components of a terrestrial carbon model is now underway and will be reported in a future publication.

Scope of the supplementary analysis in the Discussion.

Here is what we did for the supplementary item in the Discussion:

1. For the global land surface at 0.5 degree resolution initialise the vegetation and soil carbon pools (all pools in our model) to their equilibrium values for the mean climate variables in our gridded climate datasets. Assume this is the state in the year 2000 (we absolutely know this is not the case but below we will explain why we made this assumption)

2. Then for each location on earth (all land cells at 0.5 degree resolution) solve the DYNAMICAL model (differential equations 1) forward in time, using the gridded climate datasets for the year 2000 modified by the climate anomalies from the A1F1 or B1 scenarios for 200 years. Thus, the carbon cycle is only in equilibrium in the year 2000 (that defines the initial condition for each land cell) – after that it is changing continuously in response to the changing climate conditions according to the differential equations.

3. We ran item 2 above for every set of parameter values from our Markov Chain (i.e. propagated forward the joint posterior parameter distributions)

BGD

9, C5115–C5118, 2012

Your statement “So it seems we have a comparison of validation data representing near equilibrium rates of carbon exchange against model data from very non-equilibrium simulations (at corresponding location)” is not the case for our study. In the RESULTS we evaluated the model predictions of the equilibrium carbon cycle against the data that was from locations that were apparently close to equilibrium. In the DISCUSSION we do not compare our equilibrium predictions to non-equilibrium data.

Your comment that “this study does not appear to be telling me much about the envelope of uncertainty between A1F1 and B1” – is absolutely right in terms of real predictions of what will happen to the carbon cycle under these different scenarios. This supplementary investigation was never intended to be informative about actual uncertainty in carbon cycle projections under different forcing scenarios. If it were it would have been in Results and would have been a different paper. It is not in Results – it is intended to support a simple Discussion point. Our model has not been rigorously data-constrained and assessed in relation to temporally dynamic aspects of the carbon cycle. These occur in model simulations but to data-constrain the model we assumed that the input rates equal the output rates for all carbon pools. The reliable projections will therefore come after we have data constrained the model to data on temporally dynamic components (though our preliminary investigations indicate that the temporal dynamics of carbon predicted by the current model for different sites around the globe appear quite sensible). In addition, we initialised the simulations with the carbon cycle at equilibrium – whereas informative projections of how the actual carbon cycle is likely to change in future would take into account the current disequilibrium (e.g. by initialising vegetation with the IPCC Tier I vegetation database and/or doing standard spin-up simulations) AND future anthropogenic influences and climate feedbacks.

So what is the purpose of our illustration in the Discussion? We simply wanted to illustrate the relative magnitudes of parameter and structural uncertainty under two different climate forcing scenarios. We aimed to get across the simple illustration that by data constraining models in the way we have done we can investigate the magnitude

BGD

9, C5115–C5118, 2012

of parameter and structural uncertainty for future projections and identify where that uncertainty comes from. We did not choose a middle of the road scenario because we wanted two extremes for comparison - because it was straightforward to simulate the model under these contrasting scenarios. This illustrates that the relative importance of different sources of uncertainty may depend on the climate change scenario simulated.

In relation to "I could be wrong but I think by this work we are seeing how well the model performs in places where there is not substantial environmental degradation or post disturbance regrowth". For the RESULTS you are not seeing how well the model performs for places where there is not substantial environmental degradation or post disturbance regrowth – although in Fig. 5 you are seeing what it predicts for equilibrium carbon for many locations where carbon is far from equilibrium (under constant climate this is a prediction of the carbon values the vegetation would return to given sufficient time). For the DISCUSSION the global land surface is initialised at equilibrium (but doesn't stay there under climate change simulations). Do not believe the projections in the Discussion are actually our estimates of future changes in the carbon cycle under different anthropogenic scenarios. They are not.

Thank you again for getting back to us about this. We were aware that including projections anywhere in the paper would attract interest. We were always asking ourselves, and were continually asked, that even given the constraints of how we fit the model, how would uncertainty propagate into the future? We partly included the discussion about that because it was such a natural question to come up. We will revisit the wording this Discussion point to see how we can avoid any confusion.

Sincerely, The Authors.

Interactive comment on Biogeosciences Discuss., 9, 13439, 2012.

BGD

9, C5115–C5118, 2012