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Earth System Dynamics
Discussions

Interactive comment on "Explaining the seasonal cycle of the globally averaged CO_2 with a carbon cycle model" *by* G. A. Alexandrov

Anonymous Referee #2

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"Explaining the seasonal cycle of the globally averaged CO2 with a carbon cycle model" by Alexandrov describes the deficiency of the mean annual cycle in simulated CO2 relative to the globally averaged CO2 product calculated by NOAA ESRL from individual sampling locations in the marine boundary layer. The author suggests that he can rectify the mismatch simply by simulating a fast- and slow-decomposing pool in the soil and by limiting input of litter to the fast pool to a pulse at the end of the growing season.

While it is refreshing to see a paper attempt to bring a model into agreement with observations by making a simplification, rather than by adding more complexity, the current manuscript does not put its major result (that limiting the input of quickly decaying litter to the end of the growing season, limiting its availability during the warm summer) in the context of other work on soil decomposition processes. The author contrasts the

number of soil pools against other carbon cycle models (p 71), but does not contrast their treatment of substrate availability. Commonly used soil carbon models such as CENTURY (Parton et al., 1987), which provided the backbone for many CMIP5 soil carbon models were not mentioned in this paper, nor are recent results that suggest that complex Earth system models cannot reproduce soil carbon stocks (Todd-Brown et al., 2013). Some discussion of why these models' more mechanistic descriptions of litterfall and turnover fail to reproduce the mean annual cycle in CO2 is needed in order to convince me of the value of the simple adjustment presented in this manuscript. Without this discussion, it is not clear that the partitioning among the slow- and fast-decomposing pools is at all realistic and that it is the choice of (unrealistic) parameter values that enables the better comparison in CO2 seasonality.

The author states that the motivation from this work was the "lack of research articles proving that the seasonal cycle of the globally averaged monthly atmospheric concentration of CO2 could be explained by the net exchange between the atmosphere and the terrestrial part of the biosphere" (p 72). This paper does not provide such proof (not to mention that seasonal variations in 13CO2 and seasonally resolved flux observations do support this conclusion) but rather demonstrates that tuning a knob in a conceptual model can likewise result in a seasonal oscillation. Since the manuscript only compares results against the global-mean CO2 growth rate, there is no spatially resolved information to determine whether limiting substrate availability moves individual sites in the right direction with respect to the seasonal cycle amplitude goes.

I also question the virtue of using a globally averaged CO2 timeseries, rather than considering more spatially explicit surface CO2 data. Previous work has suggested that most of the seasonality in atmospheric CO2 is driven by high northern latitude ecosystems (Randerson et al„ 1997). Could the authors get the same result if their modification were made only in the northern hemisphere? If so, does this mean the seasonal behavior in temperate and tropical ecosystems is unconstrained without considering more spatially explicit data?

The author raises questions as to whether the mismatch between models and observations could arise from limitations in the observational network, but this analysis should be more fully developed or removed from the paper. The author did not propagate surface fluxes through an atmospheric transport model to simulate CO2 at actual observation sites or at additional synthetic observational sites, and therefore cannot comment on the suitability of the current observing network. Moreover, if the author is suggesting that the fact that many CO2 observations are limited to the boundary layer, while terrestrial exchange impacts the whole atmosphere, this limitation to the surface network has been explored by the use of remote sensing instrumentation that samples the whole column (Keppel-Aleks et al., 2012) and aircraft data with sensitivity to free-tropospheric CO2 (Graven et al., 2013). These datasets suggest a similar bias in the ability of models to capture the seasonal cycle as do the surface network.

References

Graven, H.D. et al: Enhanced Seasonal Exchange of CO2 by Northern Ecosystems Since 1960, Science, 341, 1085-1089, doi:10.1126/science.1239207, 2013.

Keppel-Aleks, G., et al: The imprint of surface fluxes and transport on variations in total column carbon dioxide, Biogeosciences, 9, 875– 891, doi:10.5194/bg-9-875-2012, 2012.

Parton, W. J., et al: Analysis of factors controlling soil organic matter levels in great plains grasslands, Soil Sci. Soc. Am. J., 51, 1173–1179, doi:10.2136/sssaj1987.03615995005100050015x, 1987.

Randerson, J.T. et al: The contribution of terrestrial sources and sinks to trends in the seasonal cycle of atmospheric carbon dioxide, Global Biogeochem. Cy., 11, 535-560, 1997.

Todd-Brown, K., et al.,: Causes of variation in soil carbon simulations from CMIP5 Earth system models and comparison with observations, Biogeosciences, 10, 1717–

1736, 2013.

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