Biogeosciences Discuss., 9, C2724–C2728, 2012
www.biogeosciences-discuss.net/9/C2724/2012/

Biogeosciences Discussions

Interactive comment on "Alaskan soil carbon stocks: spatial variability and dependence on environmental factors" *by* U. Mishra and W. J. Riley

K. Johnson (Referee)

kristoferdjohnson@fs.fed.us

Received and published: 25 July 2012

General Comments The discussion paper offers interesting results and a welcome alternative to assessing the soil carbon stocks and changes in Alaska. The equilibrium model approach is particularly helpful since the only other alternative to addressing the question of SOC response to warming seems to be process model outputs (i.e. "ESM's" in the paper). The map is probably an improvement over other soil carbon maps and, as the authors point out, there is a need for spatial datasets to help improve and test model outputs. A couple of results that I found most revealing were: 1) that soil carbon stocks respond to warming; specifically, that there is a net loss of SOC

as temperatures increase and permafrost decreases, but that some stocks actually increase (e.g. the active layer in this analysis), and 2) confirmation that soil drainage (i.e. "soil wetness") is a strong control on soil carbon storage in northern latitudes, which must be reconciled by ESM's. Nonetheless, we should recognize that there are still significant data and knowledge gaps to overcome in addressing the question of SOC change in Alaska and it is possible that the available data is simply not large enough for us to address it. The purpose of the following comments is to point out that we should be uncomfortable in presenting magnitude of SOC change as a salient result, because it is also the most uncertain. It is easier to determine the direction of SOC change than the magnitude. The problem is that it is already difficult to predict relative changes in SOC pools in space in Alaska where there are large and spatially complex areas that have few or no observations (especially wetlands). Although this study's SOC map is probably an improvement over previous maps (this is actually a very low bar to clear!) it may still have serious unknown errors that could propagate into modeled responses to warming. The lack of data in this situation offers little constraint on model parameters as the observation data itself may not detect SOC change, or may erroneously indicate change. This will probably be true for even very sophisticated spatial modeling approaches that try to minimize these types of errors. Before reporting such a change estimate, I would ask the authors this: how confident are we that the equilibrium model would produce the same magnitudes of change if a perfectly unbiased dataset was available for the same analysis? A discussion on this point would be very welcome. I want to be sure to not downplay the value of the study as a whole. As mentioned, the map is a welcome contribution and I can also appreciate the effort to model SOC changes as a useful exercise. It is worth noting, too, that the authors already appear to have tempered the significance of their results somewhat by listing assumptions and limitations (although note below other uncertainties that I believe were not discussed). However, might there be a way to present the estimate of SOC response to temperature in an even more conservative tone if at all?

Specific Comments Another way to explain why the lack of data is prohibitive to making SOC change estimates is because the models must extrapolate beyond the environmental bounds of the observations, as opposed to extrapolating within its bounds where data is adequately sampled. Further, validation techniques are probably not accurate for the domain outside the sampled environmental conditions. One of the most problematic areas is Western Interior Alaska where there are simply not enough data to cover the east-west gradients in precipitation and temperature in this portion of the state. Northern Interior Alaska is also sparsely sampled although it is thought that permafrost occurrence is the highest in this part of the region. Other gradients, in contrast, may be adequately covered, and therefore modeled, such as the east-west direction along the northern coast. The authors do not mention the uncertainty associated with sampled depth. As mentioned, previous estimates have been limited to 1-m, but there were good reasons for this. Those studies' authors may not have felt comfortable that the bottom of the soil profile was reached in the NCCS dataset. My understanding is that often field crews will sample profiles to the top of the C horizon OR about a 1-m depth. Therefore, it is difficult to know whether the whole soil profile to the C horizon was sampled. Another possibility is that field crews may have stopped when permafrost was reached because of the obvious difficulty in excavating further, regardless of whether or not they reached the C horizon. These sampled depth issues potentially add another dimension to the measurement uncertainty. A comment about how these issues were dealt with would be appreciated. There is very little data on the bulk density of frozen horizons. How did the authors address this problem? Were the equations mentioned from Calhoun et al (2001) and Adams (1973) developed to include frozen soils? Is it possible that your estimates were higher because of the method of predicting bulk density? As mentioned, a likely reason for the larger SOC estimates in this study is that they go past 1-m. Many of these soils will likely be wetland soils with very deep organic horizons. Wetlands are generally considered to be poorly sampled, especially in the Interior. For example, Johnson et al., (2011) found only 6 profiles in the Boreal region sampled past 1-m, and none in Southeast Alaska, whereas there were 25 in the Polar region (using the same NCCS dataset). Was any

special consideration made to address this gap? My concern is that even though SWI shows a relationship with SOC pools, very deep wetland soils may still be missed. This is important because although wetlands make up a smaller area (and there is more carbon in them), these soils may not respond as strongly to climate change as upland soils. If they are not adequately weighted into the model, then the modeled change could be inflated. I would not expect a novel treatment of this issue, again because of the lack of data, but the author's thoughts about it would be appreciated. The comparisons made to the Johnson et al. (2011) should be taken out or modified. It is not true that estimated SOC stocks of the current study for Boreal Alaska are 5.8X higher than the stocks estimated in Johnson et al. The authors took only the estimates made for the Upland conditions in Table 1 of that study, leaving about Lowland, Sandy Lowland, Silty Lowland, and Wetland. If any comparison is to be made, it would have to use an area weighted average, which would be 16.6 kg m2, or 3X difference. The same applies to the Arctic region, but in this case the mistake was even more obvious because Johnson et al. includes the area weighted calculation and compared it to the Ping et al., (2008) paper. The correct difference between the current study's estimate and that of Johnson et al. for the Arctic is 1.9X, not 1.3X. There was no discussion about the importance of bedrock as a predictor variable. This is very coarsely mapped in Alaska, but can the author's comment on why it was significant in their model?

Technical Comments Is there any reason that there is no discussion about or comparison with the Bliss et al. (2010) paper, which also reports SOC stocks for Alaska? What was the spatial dataset used to delineate continuous, discontinuous, etc.? Was anything done to account for Geolocation error, i.e. misclassification error from extracting GIS data to the profile locations? Page 5704, line 15. Johnson et al. actually used 500-600 samples. I only point this out so that the authors know that most of this data is freely available for anyone to use via the NSCN website, should they want it for future analyses. In particular, there is a USGS dataset that has added significantly to the number of deep organic soil profiles. The website is designed to build a research community and if the authors have data of their own to contribute it would be most welcome. A new study might be of interest to the authors for comparison purposes: Yuan, Fengming, Shuhua Yi, A. David McGuire, Kristofer D. Johnson, Jingjing Liang, Jennifer Harden, Eric S. Kasischke, and Werner Kurz. In press. Assessment of Historical Boreal Forest C Dynamics in Yukon River Basin: Relative Roles of Warming and Fire Regime Change. Ecological Applications. http://dx.doi.org/10.1890/11-1957.1

Interactive comment on Biogeosciences Discuss., 9, 5695, 2012.