

## ***Interactive comment on “Carbon cycle uncertainty in the Alaskan Arctic” by J. B. Fisher et al.***

**A. Das**

dasananta1133@gmail.com

Received and published: 27 March 2014

Overall Comments: The analysis of carbon cycle uncertainty for Alaska is based on the outputs of 40 models. It brings together multiple models which were a part of NACP, TRENDY, and WETCHIMP analyses. One of the fundamental flaws of this study is, it attempts to compare different carbon flux variables that were generated from heterogeneous data sources. The forcing datasets were different for different models and this will be reflected in the carbon flux variables. Model intercomparison is valid when all the models are simulated with a homogeneous set of data and at the same spatial and temporal resolutions. Abstract: The authors mentioned autotrophic respiration and ecosystem respiration separately. To my knowledge, ecosystem respiration covers both autotrophic and heterotrophic respirations. This is confusing. Either authors need to mention about the results of heterotrophic respiration and autotrophic respiration separately or it should be combined into ecosystem respiration. The authors did

C604

not mention any detail on the spatial resolution of the individual models and what kind of forcing data were used to run those models. Despite the authors used the model outputs only, but this is important for the readers to know at least the basic information on what are the spatial resolution of the individual models and what forcing datasets were used to generate the carbon fluxes. Otherwise it appears extremely vague study. The uncertainty analysis is not clear. Only simulated outputs from different sources were used. How the authors introduce uncertainty? This is misleading. In section 2.1 it is mentioned that the model outputs were downloaded from some sources, but in page 2896 (line 15 – 20), it is mentioned that the TRENDY models are forced with CO<sub>2</sub> alone and forced with varying CO<sub>2</sub> plus climate. There is a mismatch between section 2.1 and here. Section 2.2 (Page 2897, line 1 – 5). Contradictory. Here it is said that the model outputs were downloaded. Here again it says that the models were forced with in-situ measurements. Given the nature of analysis it appears that the CO<sub>2</sub> flux outputs were used from different sources. How the models were run with the in-situ data in that case? Section 3.5 (Site level evaluation). Here you said that the subsets of models were run using in-situ forcing data. This implies you have actually run all the models. But in section 2.1 you said that you used model outputs and the outputs were at different spatial resolutions. This is contradictory. Figure 2. Very poor spatial scaling. It does not make any sense. How did you scale the CO<sub>2</sub> flux outputs to 0.5 degree from different resolutions? Downscaling is a different technique. From the Figure, it appears that a linear scaling was done. As a result the appearance of the spatial distribution does not agree among models. Some models show CO<sub>2</sub> sink where as some shows CO<sub>2</sub> source over the same areas. This is not appropriate. The reasons behind showing 2003 results in Figure 3 and 5 are not understood. Figure 4. Very poor. Figure 6. How many years of mean were taken? Spatial standard deviation does not make any sense here. In my opinion this paper lacks the quality that is required for Biogeosciences.