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Earth System Dynamics
Discussions

Interactive comment on "Temporal variations in atmospheric CO_2 on Rishiri Island in 2006–2013: responses of the interannual variation in amplitude to climate and the terrestrial sink in East Asia" *by* C. Zhu and H. Yoshikawa-Inoue

Anonymous Referee #2

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General comments: Observation of atmospheric carbon dioxide plays a critical role in assessing carbon budget in atmospheric inversion analysis. The authors present an analysis of temporal variations of observed atmospheric CO2 mixing ratio at multiple time scales on a recently set-up monitoring site in eastern Asia and their response to climate change. The detailed patterns of the diurnal, seasonal and interannual variations of the observed CO2 are investigated at the monitoring site of Rishiri Island (RIO). In considering the high-resolution measurement of atmospheric CO2 at this site, this study could be contribute to the studies for better understanding carbon-climate sys-

tem, especially those on atmospheric inversion analysis.

The authors try to investigate the observed signals by linking local meteorology, atmospheric circulation, index of climate variation, terrestrial biospheric fluxes and etc. However, I am somewhat unconvinced by the authors in their discussion about which factors control/impact the specific CO2 patterns. Also, the author could improve the paper by drawing attention to the most important results and the physical meaning behind these results instead of presenting them like a list. Overall, I am unconvinced of any further consideration of possible publication before a heavy revision and significant improvement.

Major comments:

- The authors simply regress the atmospheric CO2 signals on multiple factors such as climate index (ENSO, NPI), regional climate variable and regional biospheric fluxes but not tell us about mechanisms link atmospheric CO2 to these factors. Alternatively, the authors cite so many previous literatures in order to indirectly link the mechanism to the observational patterns – which cannot support their arguments.

- Experiments using numerical simulation driven with different components (atmospheric transport, bisopheric fluxes, and oceanic fluxes) would be more helpful for mechanical understanding of their potential impact on the observation CO2 signal.

- Page 815, L24-L26: It is not clear why the author compare observations at remote sites: MLO and SHM to the investigated site: RIO. I am confused of the motivation of the comparison, given the fact the differences in regional atmospheric transport and contributing sources/sinks between RIO and the two sites.

- Page 815, L26- Page 816 L3: Are the temperature and precipitation data or surface? At 2m high? The authors should state the property clearly? Also, the authors should give the link to the temperature data.

- Page 816, L13-L17: The authors claim they would examine the influence of fossil fuel

emissions on growth rate of the observed CO2, but I cannot find the discussion about it.

- Page 817, L3- L5: This statement should be put in section 2.1.

- Page 817, L14-L17: Similar to the above, the statement here should be put in method section.

- Page 817, L17-Page 818, L2: The author should state why they look at the diurnal variation by month. I noticed that the diurnal amplitudes are smaller in cold months than the warm months. It is clearer to compare the two patterns than to show the curves for all months.

- Page 818, L3-L9: The explanation of the mechanism is limited. Actually, diurnal rectification – the diurnal variation of biospheric carbon exchange and diurnal profile of planetary boundary layer interact together to dominate the diurnal cycle atmospheric CO2. The authors should address this point.

-Page 819, L8-L10: Daily timeseries shown in Fig. 4a actually is combined by the long-term trend contributed by fossil fuel CO2 emission and seasonal cycle signal. The so-called "de-trended seasonal cycle" is not correct.

- Page 821 to Page 825: I am not convinced by the discussion in this Section that can supports the climate variables, circulation and ENSO are controlling factors of the associated CO2 signals. At most, they show a correlation between these factors. Also, I am not convinced that the authors need to specifically investigate the impact of atmospheric circulation on the seasonal CO2 signal. A set of experiments using atmospheric tracer transport model driven by the contributing sources would be more plausible to explore the contribution. Also, an approach by linking 222-Rn to CO2 signal just because 222-Rn has reported to be impact by circulation here doesn't support the argument strongly.

-Page 825, L10: I cannot understand why the authors use the phrase "the long-term

trend" for an actually interannul variation of the CO2 signal here. Normally, a "long-term trend" means a trend dominated by anthropogenic CO2 emission.

I have to say the manuscript is hard to understand. The main result of the manuscript is not clear. The authors should tell the reader the scientific important of the results. The English should be edited by native speaker – I had tried four times before I could force myself to read it through.

Interactive comment on Earth Syst. Dynam. Discuss., 5, 809, 2014.