Author response bg-2013-533

We thank the referee for the careful review and the constructive comments. Please find the answers to all specific comments below. Author comments are given in turquoise, extracts from the paper in blue.

Both reviewers criticized that the definition of Time of Emergence (ToE) in the submitted manuscript is flawed. Unfortunately, there is an error in the definition given in the submitted manuscript. This has now been corrected. We emphasize that all calculations were done using the correct definition and results presented in this revised version remain unchanged compared to the first version.

Review #2

This paper attempts to quantify "time of emergence" (TOE) for select ocean biogeochemical variables. The TOE concept in general is a good one, and something in which a broad community would have keen interest. However, this study falls short, in that the criteria used to compute TOE is not clearly explained.

We realize and acknowledge that the ToE method is not sufficiently explained and that the ToE formula has to be rearranged to provide the correct unit, i.e. time. The according part of the method section was rewritten and extended:
"ToE is defined as ToE = (2xN)/S where S is the trend and N a measure for variability.
For each grid cell, S is defined as the linear trend (per year) over the period 1970–1999. [...] For N, the standard deviation (sdv) over the entire simulation, 1870–1999, is used. Prior to this last step, the data is detrended via a spline approach (cut-off period: 40 yr; Enting, 1987).
For illustration purposes, we calculate ToE for DIC at a location in the subtropical North Pacific (see also Fig. 1). By inserting the respective values for S (0.94 mmol m-3/yr) and N (7.24 mmol m-3), we obtain (2x7.24)/0.94 = 15.4 yr, i.e., a (rounded up) ToE of 16 yrs."

Moreover, I suspect that better, more rigorous TOE criteria could be defined. The basic notion is whether observed trends could be driven by natural variability. There is no mention of autodecorrelation timescales, which I would think relevant to the idea that trends arise due to low-frequency natural variability.

ToE is widely used by the physical community (see papers cited in the introduction). A common approach to estimate ToE is the comparison of modeled noise (usually the standard deviation of an unforced control simulation) and observed (Karoly and Wu, 2005) or modeled (Mahlstein et al.,2011; Hawkins and

Sutton, 2012) trends. Here we follow this approach and we see no reason to revise this method. A paragraph was added to the method section:
"ToE is a measure for the point in time when the trend signal (S x ToE) exceeds two times the background variability N, i.e., the approximate 95% confidence interval of the background variability. The choice of the detection threshold differs between studies, other approaches are e.g. one sdv of seasonal or annual means (Hawkins and Sutton, 2012), observation-based thresholds (Ilyina et al., 2009; Ilyina and Zeebe, 2012) or the range of the pre-industrial annual cycle (Friedrich et al., 2012). Here, we use the rather conservative value of two sdv of interannual variability. For a threshold of one sdv ToE would be half, accordingly."

Concerning autodecorrelation timescales: This study aims at the time period necessary to detect a trend signal with a magnitude comparable to recent observations. The long-term century scale trends in the model simulation are mostly driven by anthropogenic forcing as revealed by corresponding control simulations. Strictly speaking, however, we do not specifically focus on the question whether the simulated trends are caused by natural variability or not. We fully agree with the reviewer that the quantification of the influence of natural climate variability on carbon cycle and climate variables is an important area of research (see e.g. earlier work by our group: Frölicher et al, 2013; Keller et al. 2012). Here, we assess interannual variability for a range of variables and from results of 17 Earth System models.
In addition, we have now tested the significance of the linear trends determined from the model output over a 30-yr period and thus to which extent the data are correlated over the 30-year period. The ToE introduction part of the method section was extended and reads now:
"By calculating S over a time period of 30 years, we can to a certain degree rule out interference of low-frequency variability in the detection of the trend (see e.g., McKinley et al., 2011). A ToE of only a few decades, as we find it especially for the three carbon cycle variables (see Sec. 3.1), is thus a strong indicator for the significance of the respective trend. This is confirmed by a significance test (t test, 5% level) of the trend of the underlying 30 year time series (not shown): For all 17 models, all trends in pH are significant. The trends in pCO2 are also significant, yet with localized insignificant exceptions in the Southern Ocean (BCCR BCMC, IPSL-CM5A-MR) and the upwelling region off Peru and Chile (CanESM2). Trends in DIC are significant in large parts of the global oceans, exceptions are the high latitudes and the equatorial Pacific. Statistically significant trends in SST are less widespread and corresponding regional results are highly model-dependent."

We provide now (also in response to a comment by reviewer 1) a discussion of the role of natural variability in pCO2 as linked to climate modes in section 3.1:
"This issue is addressed in a recent study by Fay and McKinley (2013). These authors investigated trends in surface ocean pCO2 measurements between 1981 and 2010 for periods of 4 years to up to 30 years. They found that, on shorter timescales, trends of surface pCO2 are sensitive to variability presumably linked

to climatic oscillations and, consequently, may vary between different periods. Consequently, this caveat has to be taken into account when comparing modeled and observed trends over relatively short time periods. Fay and McKinley also find that the influence of climatic oscillations fades when analysis periods are between 25 to 30 years, e.g. as used in this study to determine trends. We note that a direct comparison of the trend signals computed by Fay and McKinley with our trend signal is hampered by the fact that Fay and McKinley use relatively sparse observational data to determine trends."

I think this study could be published if revised substantially.

[Comments]
p 18066 ln 8: acronym ESM not defined.
DONE. Rewritten:
"We investigate the ToE of trend signals in different biogeochemical and physical surface variables utilizing a multi-model ensemble comprising simulations of 17 Earth System Models (ESMs).

p 18066 ln 22: Sentence beginning, "Reasons are large..." Awkward. Rephrase.
Rewritten to:
"Responsible for these changes is the CO2 emitted by mankind through combustion of fossil fuels, land-use change and industrial processes (e.g., Hegerl et al. , 2007) which have brought the global carbon cycle out of steady state."

p 18068 ln 4: "In ocean biogeochemistry, the [TOE] method..."
DONE

p 18069 ln 3: Gettelmann et al. reference is weird, talks about feedbacks; it is not a general CESM1 reference and says nothing about the ocean carbon cycle model.
Replaced with: "Moore, J. K., Lindsay, K., Doney, S. C., Long, M. C., and Misumi, K.: Marine Ecosystem Dynamics and Biogeochemical Cycling in the Community Earth System Model [CESM1(BGC)]: Comparison of the 1990s with the 2090s under the RCP4.5 and RCP8.5 Scenarios, Journal of Climate, 26, 9291–9312, 2013. "

p 18069 ln 14: why is S/N > 2 used as the threshold? Presumably there are statistical arguments to be developed that would yield confidence level estimates.
+
p 18069-18070: this definition of S/N does not make sense to me. As described, S has units of quantity/time, i.e. °C/yr, whereas N has units of °C. So S/N has dimensions of 1/time. Furthermore, the standard deviation of annual means doesn't really seem to be the relevant metric against with to evaluate trends. It

might be a good metric to evaluate extreme values, and these might manifest as a product of trends. But the language here is very imprecise. Fig. 1 doesn't help allay the confusion. It seems like by "trend" the authors mean a trend times a time period, yielding a projection.

We clarified the definition of ToE: see above.

References:

Frölicher, T. L., Joos, F., Raible, C. C., and Sarmiento, J. L.: Atmospheric CO2 response to volcanic eruptions: The role of ENSO, season, and variability, Global Biogeochemical Cycles, 27, 239–251, 2013.

Keller, K., Joos, F., Raible, C., Cocco, V., Frölicher, T., Dunne, J., Gehlen, M., Bopp, L., Orr, J., Tjiputra, J., Heinze, C., Segschneider, J., Roy, T., and Metzl, N.: Variability of the ocean carbon cycle in response to the North Atlantic Oscillation, Tellus B, 64, 18738, doi:10.3402/tellusb.v64i0.18738, 2012.