

## ***Interactive comment on “Time of Emergence of trends in ocean biogeochemistry” by K. M. Keller et al.***

### **Anonymous Referee #1**

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The study investigates the minimum length of the time period where trends in physical or biogeochemical ocean variables become detectable against the natural variability background. It uses a "Time of Emergence" concept applied to modelled fields of SST, DIC, pCO<sub>2</sub>, and pH.

This is a very relevant topic in the context of anthropogenic climate change. Nevertheless, as detectability is a statistical question, its investigation needs a clear statistical context, including tests of statistical significance of the trend with respect to a defined significance level. Such context or tests are completely absent from the manuscript. Rather, the "ToE" concept is introduced vaguely (and wrongly, see below). The threshold "S/N>2" is given without any motivating rationale, such that it remains completely open what a certain ToE value would tell in practice. I agree that the relative statements

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(such as "ToE of SST longer than that of DIC") are less affected by the exact definition, but still think that a clean statistical treatment is a necessity for meaningful results.

I also miss any process-related explanation, or implications for biogeochemistry, from the numerical findings. Why are the different variables behaving so differently? What do we learn from the numbers? Is there any particular advice for observations (other than just collecting more data)? Why are the results different from previous studies (e.g. the cited ones by Mc Kinley et al, Fay et al)?

I would like to encourage the authors to continue studying detection of trends, but feel that the clean statistical treatment is needed for publication.

Minor comments:

Sect 2: In the description of the ToE concept, it is unclear how time actually enters ToE, since trends are only calculated over a fixed period. Presumably S should not be trend but "trend times length of period" (otherwise "S/N" also would not be dimensionless).

p 18070 line 8: "well approximated" is vague and needs explanation

p 18070 lines 9-12: I expect that standard statistical tests exist for this question, that should be used.

p 18071 line 1: Delete sentence as this has been said in Methods.

p 18072 line 10: "reasonably" is vague.

p 18073 lines 2,8: Putting ranges by ":" is unusual in texts and hard to read, rather use "-" or "...".

p 18074 lines 15-16: As far as I see, these two points are actually the same.

p 18075 line 8: missing "of"

p 18076: I agree that these cases are revealing. Concerning the "Monthly" case, wouldn't it be a practically relevant option to deseasonalize?

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p18077 line 9-17: I agree that the "aliasing" of seasonality into yearly values is the major problem here. As far as I see, similarity of July and January unfortunately does not mean that there is no such "aliasing".

Fig 1: Hard to believe that the grey band gives the standard deviation (rather than e.g.  $\pm 2$  sigma). Further, the agreement of spline and linear trend is seen to only apply to the specific period.

Fig 3: Numbers are printed very small and not readable (probably even in full-width print). Variables should be arranged in the same order as in text and Tab 1 (also in Fig 2).

Fig 4: For the January and July cases, N is larger than for Annual, so why is ToE smaller rather than larger?

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Interactive comment on Biogeosciences Discuss., 10, 18065, 2013.