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

Anonymous Referee #2

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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. 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 autocorrelation timescales, which I would think relevant to the idea that trends arise due to low-frequency natural variability.

I think this study could be published if revised substantially.

Comments
p 18066 ln 8: acronym ESM not defined.

p 18066 ln 22: Sentence beginning, "Reasons are large..." Awkward. Rephrase.

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

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.

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.

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