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Interactive comment on "Limited impact of El Niño – Southern Oscillation on the methane cycle" *by* Hinrich Schaefer et al.

Hinrich Schaefer et al.

h.schaefer@niwa.co.nz

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We thank Joe Melton for the constructive and helpful comments and suggestions. Below we address each criticism individually. Please note that some points were brought up by several referees and commenters; please see our other responses for additional information and changes to the manuscript. Referees' comments are bracketed as follows: <>. Our response is in regular font. Quotes from the manuscript are in quotation marks.

Response to main comments:

<1. I didn't see a discussion of significance level for the correlation coefficients. Without one I have trouble understanding if a value of 0.25 is significant or not. Is there a reason

that was not done? Otherwise, while all of the r2 are low, it might help understand what are just noise and what is representing a true signal.>

We have marked all correlation results for the Spearman ranks that are not statistically robust at a level of p>0.01 with grey backgrounds in the results tables. This applies only to results with very low r-squared values and therefore does not affect the interpretation. The exception are correlations of global δ13CH4 with ENSO, which all have p>0.05. These correlations did not reveal a strong ENSO impact either, so removing them from the interpretation does not alter the findings.

The resulting changes in the manuscript are as follows:

Section 5.2.: "P-values for the Spearman ranks indicate that all results for r2>0.1 are significant (p<0.001), with the exception of global δ13CH4 correlations, where no p-values below 0.05 occur."

Section 5.2.: "None of the global δ13CH4 series produced statistically robust correlations with ENSO; all p-values were higher than 0.05. The following findings are therefore not relevant for further interpretation. The highest correlation is between global detrended δ13CH4 and SOI monthly means with r2=0.37."

Section 5.2.: "The lack of statistical robustness for global δ13CH4-ENSO correlations may stem from the different resolution of the two sets of time series. In this case, the southern hemispheric record from BHD may represent the extra-tropical impact of ENSO on δ13CH4."

<2. There is relatively little discussion on possible changes in the main sink of methane - hydroxyl radicals. The dynamics of this sink has been highlighted in recent studies (McNorton et al. 2016, Turner et al. 2017, Rigby et al. 2017). Is it possible that ENSO would have an impact upon local concentrations of OH? Is it a safe assumption to assume a constant sink strength? Given the power of this sink, and its recent importance (at least in the studies just mentioned) perhaps it is worthwhile to give more consider-

ation to how a changing sink due to ENSO could impact upon the methane cycle. Or at least expand the discussion of the sink to demonstrate why a constant assumption is valid.>

A detailed discussion of OH-dynamics has been included in the revised manuscript, for details please see replies to SC1 by Alex Turner.

Response to minor comments:

<- What about using the GFED4(s) burned area product to investigate changes in burning? It would have the advantage that it is global and extends further back in time than the HCN record. However the caveat is that burned area is not the same as C emitted as CH4, however it might be a reasonable test since they would be closely related. A recent study highlighted the continual decrease in burnt area over the course of the record (Andela et al. 2017) which then should have some impact upon the methane cycle and perhaps can be used to tease out any ENSO influence.>

Evidence for an influence of ENSO on earlier versions of the GFED data set has been presented by Van der Werf et al. (2006). Although GFED now covers a longer period, we see limited value in repeating the analysis. The biomass burning proxy HCN as an atmospheric tracer provides a more direct comparison with the methane records (e.g., both are subject to atmospheric transport and mixing). Given that the focus of our study is the overall impact of ENSO on methane dynamics, with biomass burning emissions as a piece of the puzzle, we prefer to maintain a clear scope of the work.

<- Can we have a table with the various tests laid out (det-nom, det-gro, nominal mm, nominal run, etc.)? It is difficult to keep them all in the head and then interpret some very busy figures.>

We have included a new Table 1 that provides a list and description of all data sets.

<- p. 2 line 30 - What is aggregate source here?>

We have clarified that 'aggregate source' applies to the combined total of global emis-

sions. The relevant passage now reads: Introduction: "Biogenic methanogenesis in wetlands discriminates strongly against 13C and creates methane that is 13C-depleted (δ13C = -58‰ for tropical wetlands) relative to the plant precursor material (δ13C of -12‰ to -28‰ and to the combined total of global emissions (δ13C $\sim$ -53.5‰."

<- Author contributions - there is no E.D author.>

We have corrected the author contributions. Before the original submission we mutually agreed that Ed Dlugokencky's contribution would be more appropriately reflected in an acknowledgement rather than through co-authorship.

<- Data avail - raw data is little use. Best approach would be to make the actual analysis available.>

We are now stating that all data products and time series used in the study are available from the corresponding author.

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