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Interactive comment on "Natural variability in the surface ocean carbonate ion concentration" *by* N. S. Lovenduski et al.

Anonymous Referee #2

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The manuscript 'Natural variability in the surface ocean carbonate ion concentration' by Lovenduski et al. investigates the natural variability of the carbonate ion from years to decades using a long preindustrial run from an IPCC-class Earth system model. Findings provide a useful background to further our knowledge of the ocean acidification. The manuscript is clearly structured and reads well. Nevertheless, I think this paper needs some clarification that have to be addressed first, and which prevent me of accepting this paper in its present form. Therefore, I recommend acceptance of this manuscript after some minor revisions, considering that major points (methodology, detailed below) can be addressed easily.

Minor comments: In absence of external forcings, the internal variability equals the natural variability. That said, the main message of the study might be clearer if the

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authors state that they analyze the internal variability (which is the unforced natural variability). Besides, It might be relevant to evaluate whether an interative atm CO2 will impact their findings or not (using esmControl simulation for example).

Major comments:

(1) While I acknowledge that several studies have relied on the Weatherhead et al formalism, we shall keep in mind that this formalism has been developed using an AR1-process assumption. This hypothesis is clearly relevant for a large number of physical or dynamical fields but shall be assessed for biogeochemical fields which present are inner ocean fields (evolving slowly). In order to evaluate an AR1-process for ocean pH or carbonate ion, I recommend to employ partial autocorrelation (pacf) which will give hints on the order of the auto-regressive memory of the fields. Eventually a map of the greater significant AR for CO32- might be of interest for the analysis. It might be helpful for me to see timeseries, acf and pacf at some oceanic domains.

(2) It is unclear to me how the authors have performed their analysis explained in Equation 3 and 4. Do they have set the temporal values of some parameters to zero or not. If not, how large are the crossing terms of the taylor expansion ?

Specific comments: P 13124

L2 fully coupled is not appropriate here since a time-invariant atm CO2 is used

L15 and lower autocorrelation in pH = few words on IAV might be helpful

P 13125

L19, "large internal climate variability " = natural because breaking down the contribution of the forced vs unforced variability in the observations is hard task regarding the few years of data we have. . .

P 13126

L28 I agree but they capture the chronology of events which is as important as the IAV

to apprehend the detection of anthropogenic signal

P 13127

L4 several others study have investigate this question: (Friedrich et al., 2012; Keller et al., 2014; Lehner et al., 2015; Séférian et al., 2014)

P 13128

L1 intrinsic ? see (Penduff et al., 2011)

P 13138

Large variability in the Tropical Pacific. This might be discussed a bit regarding the study of Resplandy et al which analyze the variability of fgCO2 anf fgO2 using an ensemble of Earth system model. It seems that the agreement between models in term of variability is poor. Few words on this point might be helpful and provide lead for future investigation (ensemble of control run, perturbed simulation or a greater ensemble of Earth system model).

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