Point-by-point response to the editor comments:

1.) Lines 333 – explicitly state the indicative meaning used for the marine terraces. Maybe include an additional table and reference that table here (I know you give a value on lines 726 but it isn't clear how much this value varies across your study area).

We calculated the indicative meaning for each marine terrace measurement. The associated values (upper and lower limits of modern analog, reference water level, and indicative range) can be found in the WALIS database, which we refer to in line 371. The variation of the reference water level along the WSAC is shown in Figure 11F.

In line 372 we added that we calculated the indicative meaning "for each marine terrace measurement" and we also added a new Table 2 with the median values representing the indicative meaning for the four sectors that we also used for the results section.

Table 2. Median values and standard deviations (2σ) that represent the indicative meaning along the WSAC. The four sectors were chosen based on their main geomorphic characteristics (see results section).

	Upper limit of modern analog [m]	Lower limit of modern analog [m]	Reference water level [m]	Indicative range [m]
Ecuador and northern Peru	2.89 ± 0.16	-1.78 ± 0.47	0.54 ± 0.21	4.66 ± 0.65
Central and southern Peru	2.98 ± 0.31	-3.05 ± 0.52	-0.03 ± 0.11	6.06 ± 0.90
Northern Chile	3.01 ± 0.15	-2.89 ± 0.30	0.06 ± 0.08	5.90 ± 0.51
Central Chile	3.21 ± 0.19	-3.03 ± 0.38	0.07 ± 0.11	6.25 ± 0.60

2.) I think GIA is currently not adequately discussed (as suggested in the earlier review by Paula Marques Figueiredo). I appreciate the efforts and the addition of Figure 3 (which definitely strengthens your argument) but some questions still remain that likely have bearing on your findings (and particularly your uplift rate calculations. I agree with you that the tectonic signal likely dominants the differences in marine terrace elevations, but as written, the manuscript seems to discount GIA, prematurely. For example, GIA can result in 10's of m in differences in sea level elevations across a N-S transect (e.g. Potter et al., 2003; Creveling et al., 2017; Simms et al., 2016). Figure 3 assumes a MIS5e RSL that is constant across the area, which it likely was not. Furthermore, what elevations did you use for "sea levels" during MIS5e for Figure 3? GIA-induced differences are even greater for MIS5c. This influence would also apply to your later estimates of uplift (line 630) and errors (line 420). The variability is likely 5-10 m for MIS5e but could be as much as 20-30 m across the WSAC during MIS5c. This should be acknowledged as a limitation of your uplift estimates (section 5.a).

I think much of this could be dealt with by adding to Lines 422-427. Maybe look at the global model of Creveling et al. (2017) to estimate how much variability would be expected across this margin and mention that this is still less than the X m of differential elevations found along the margin – you could even include that in your error terms as uncertainty for the sea level estimates.

We followed your suggestion by adding the information that GIA may cause local differences in sea level of up to 30 m (Simms et al., 2016; Creveling et al., 2017) [lines 211-212]. However, the results of Creveling et al. (2017) suggest differences in sea-level due to GIA along the WSAC north of 40°S range from -2 to +2 m, supporting our statement that "the amplitude and wavelength of GIA is mostly determined by the flexural rigidity of the lithosphere (Turcotte and Schubert, 1982) and should therefore not severely influence vertical deformation along non-glaciated coastal regions (Rabassa and Clapperton, 1990) that are located in the forearc of active subduction zones." [lines 429-431]

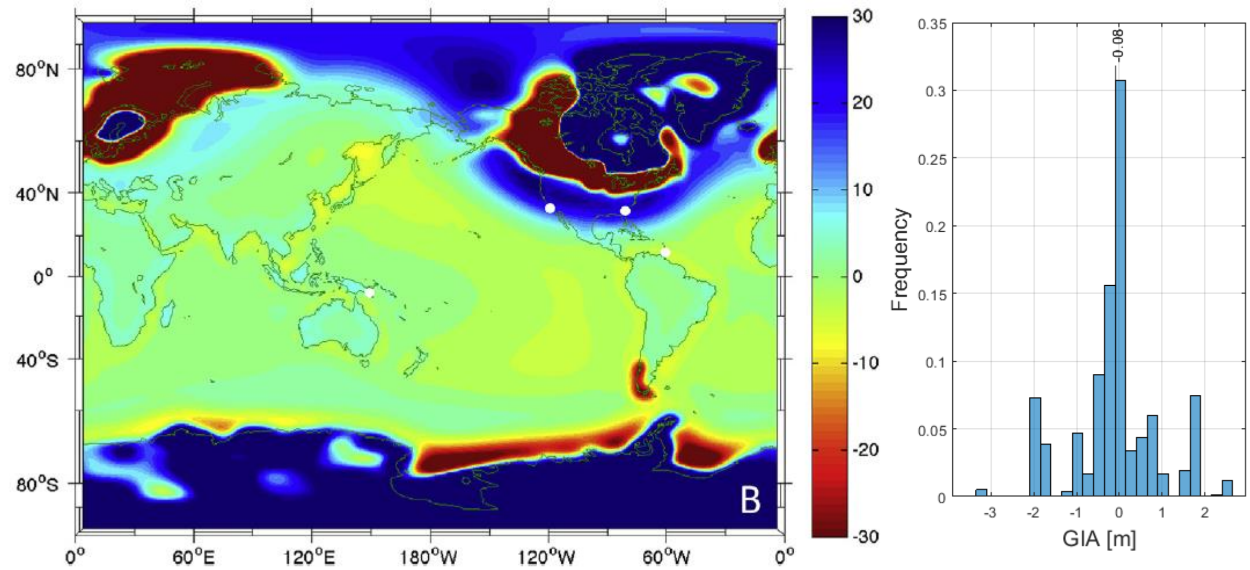

Figure 2B of Creveling et al. (2017) showing the predicted GIA since MIS-5a (left). Histogram of GIA estimates along the WSAC (1°N-40°S).

We caution that current GIA models use an oversimplified lithospheric structure defined by horizontal layers of homogeneous rheology, which might be appropriate for cratons and ocean basins, but not necessarily for the forearc regions of subduction zones. Therefore, assessing the contribution of GIA to RSL during MIS-5 is beyond the scope of this study as it would require a dedicated modeling experiment.

3.) The third is relatively minor, could you clarify the difference between your "inner edge" and the commonly used "shoreline angle" a little earlier in section 3.1. Maybe the description on line 315 of the inner edge could be moved to line 293, when it is first introduced.

We define the inner edge in lines 294-295 as the location at the foot of the paleo-cliff where significant changes in slope occur. A short version of this definition is used in lines 354-355 to explain the difference to the shoreline-angle elevation. We would like to point out that these definitions are described in great detail in the methodological papers on the TerraceM software (Jara-Muñoz et al., 2016; 2019). We cite both papers, which are easily accessible in the international literature and refrain from repeating this published material.