Response to Referee 1:

"Timing of the anthropogenic carbon invasion in the Southern California Current"

In their manuscript "Timing of the anthropogenic carbon invasion in the Southern California Current," Contreras-Pacheco et al. (2025) address a critical gap in our understanding of the global carbon cycle: quantifying how and when anthropogenic CO_2 has penetrated the surface waters of Eastern Boundary Upwelling Systems, regions that remain poorly constrained in global carbon budgets despite playing a disproportionately important role as seasonal sources and sinks of atmospheric carbon. The authors address this gap through stable carbon isotope records spanning approximately 150 years, derived from organic carbon and shells from two foraminifera species in laminated sediments. These sediments offer an unusual high resolution archive for the southern domain of the California current, a region that is at the boundary between cool productive waters and warm subtropical waters and therefore sensitive to changes in both upwelling intensity and atmospheric carbon loading.

However, I have a few comments that I think will make the manuscript more robust. My main concerns revolve around quantitative analysis, lack of statistical treatment, and uncertainty quantification.

1) *My first and most important concern relates to the chronological framework of the study. For a manuscript whose title explicitly addresses the timing of anthropogenic carbon invasion, the age model is surprisingly underdeveloped and, as presented, cannot be independently evaluated by the reader. In the main text, the age model receives only a single short paragraph (Section 2.4), with no figure, no sedimentation rates, and no uncertainty quantification. The supplementary material, while providing Figure S1, does little to resolve this concern. The entire chronology of core SaLa11-E19-MC1 rests on a single-proxy stratigraphic correlation based on calcium carbonate content matched against core BAP96-6C. This approach raises several methodological concerns because the visual correlation between the two carbonates is ambiguous, multiple peaks could be matched in different ways, resulting in a potentially non-unique stratigraphic alignment. There is also the underlying assumption that carbonate fluctuations are synchronous across the basin, which is stated, but it is not demonstrated, which makes the reader incapable of evaluating the reliability of the chronology on which everything else depends. The age uncertainty is never quantified. I suggest explicitly mark and justify the tie points used in the carbonate correlation, quantify the age uncertainty propagated into SaLa11-E19-MC1, and discuss how the 15-year gap between core collection dates (1996 versus 2011) was handled in the stratigraphic alignment.*

We thank the reviewer for highlighting the need for a clearer and more transparent presentation of the chronological framework. We agree that the previous version of the manuscript did not provide sufficient detail to allow readers to independently evaluate the age model.

In response to this comment, we have substantially revised Section 2.4 (Age model). The revised section now provides a more detailed description of the chronostratigraphic framework used in this study, including:

- The regional chronology previously developed for San Lázaro Basin cores
- The sedimentation rates derived from excess ^{210}Pb measurements

- The procedure used to align the newly collected cores (SALA11 and E19MC-1) with the published composite stratigraphy

To address the reviewer's concerns about the 15-year difference between core collection dates (1996 vs. 2011), we now explicitly describe how the expected stratigraphic offset was estimated using the basin-wide sedimentation rate (~2.5 mm yr^{-1}).

In addition, we now explicitly mark and justify the tie points used in the carbonate correlation in the revised Supplementary Figures based on a published multicore stratigraphic alignment. We also compare downcore variations in the Planktic/ (Planktic + Benthic) foraminifera ratio across the correlated cores.

Although precise annual age equivalence between independently collected cores cannot be guaranteed, the relatively small temporal offset between cores and the consistent correlation observed among multiple variables suggest that any age uncertainty associated with the correlation is limited and does not affect the interpretation of our results at least on decadal timescales.

The revised text of "Section 2.4" is provided below:

2.4 Age model

Excess ^{210}Pb derived sedimentation rates in core BAP96-6C are approximately 2.5 mm yr^{-1} downcore (Esparza-Alvarez et al., 2007). Similar sedimentation rates have been reported for another box core from the same basin (PCM-78C), described in Abella-Gutierrez et al. (2020, supplementary information).

A multicore chronostratigraphic framework for the SLB was developed by Abella-Gutierrez et al. (2020) using X-ray lamination patterns. This framework includes several sediment cores from the basin, including the bow core BAP96-6C and the Kasten core PCM00-78K. In that study, the age model was constrained using ^{210}Pb and ^{137}Cs measurements in box cores together with radiocarbon measurements from Kasten cores. The results of this work showed that changes in sediment density reflect productivity cycles with lower (higher) carbonate content (Abella-Gutierrez et al., 2020). These relationships allow carbonate minima (and C_{org} maxima) to be used as stratigraphic markers across cores from the basin.

To integrate the new cores SALA11-E19MC-1 into the published BAP96-6C – PCM00-78K composite, we first applied the basin-wide sedimentation rate of ~2.5 mm yr^{-1} to account for the 15-year difference between the collection of BAP96-6C (1996) and the SALA11-E19MC-1 cores (2011). The age difference between core BAP96-6C and SALA11-E19MC-1 is 15 years which corresponds to an expected stratigraphic depth offset of approximately 37.5 mm between BAP96-6C and SALA11-E19MC-1. Following this initial adjustment, the alignment was refined by correlating carbonate minima between the cores.

As an independent verification of the stratigraphic alignment, we also compared variations in the Planktic foraminifera / (Planktic + Benthic foraminifera) ratio between the cores (Supplementary Figura x). The consistency of these patterns across the correlation intervals supports the robustness of the stratigraphic correlation and suggests that any residual age uncertainty is unlikely to affect the interpretation of our results, specifically on the decadal timescales which are relevant for this paper.

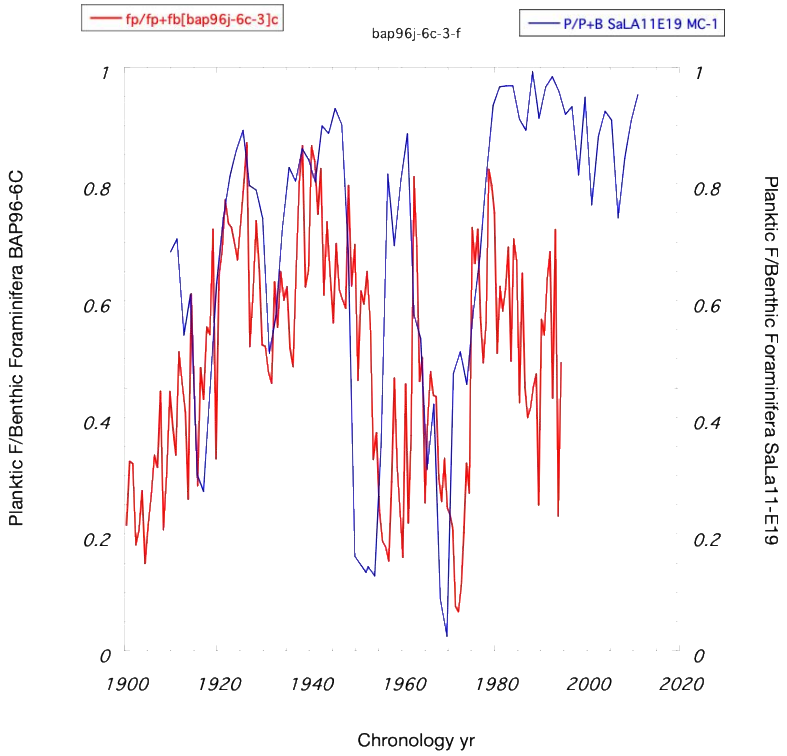

Figure. Downcore profiles of the Planktic / (Planktic + Benthic) foraminifera ratio in cores BAP96-6C (red) and SALA11-E19MC-1 (blue) show similar variability patterns and coherent maxima and minima over the correlated interval.

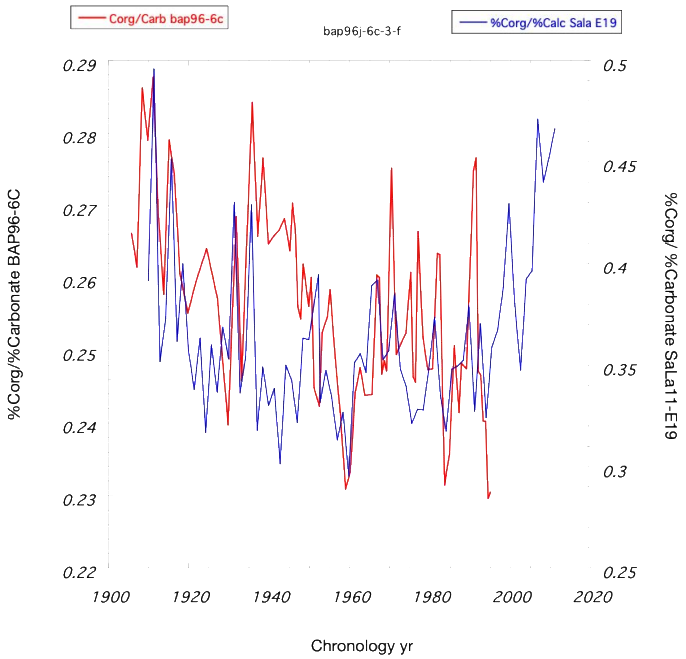

Figure. Carbonate-related downcore records from cores BAP96-6C (red) and SALA11-E19MC-1 (blue) display comparable fluctuations, with matching low-carbonate intervals across the correlation interval.

2) *A second important gap concerns the statistical treatment of the isotopic trends, which are the paper's primary quantitative contribution. The authors present slope comparisons between atmospheric and marine $\delta^{13}C$ records reporting values of −0.12‰ per decade for foraminiferal calcite and −0.15‰ per decade for organic carbon, against −0.27‰ per decade for the atmosphere as central findings of the study, yet none of these trends are accompanied by any formal statistical analysis. There are no confidence intervals, no R^2 values, no p-values, and no uncertainty propagation reported anywhere in the manuscript, given the substantial variability evident in Figures 3–5. Without knowing whether these slopes are statistically distinguishable from one another, or whether the trends themselves are statistically significant, the paper's main interpretive argument that the marine records show a systematically lower slope than the atmospheric trend due to upwelling buffering cannot be evaluated. I suggest a test of significance to determine whether the marine slopes are distinguishable from the atmospheric slopes.*

We thank the reviewer for the comment. We agree that a formal statistical evaluation of the reported trends will strongly support our interpretation. Below, we describe our approach, and it will be included in the reviewed manuscript:

Linear trends in $\delta^{13}C$ were first evaluated for each record using ordinary least squares regression. To test differences among atmospheric and marine records, we applied an analysis of covariance (ANCOVA) including time, group, and their interaction.

Estimated slopes indicate that the atmospheric record exhibits a significantly steeper decline (-0.261 ± 0.011 ‰ decade^{-1}; 95 % CI: -0.283 to -0.239) compared to both calcite (-0.120 ± 0.020 ‰ decade^{-1}; 95% CI: -0.160 to -0.081) and organic carbon (-0.134 ± 0.013 ‰ decade^{-1}; 95 % CI: -0.159 to -0.109).

The interaction between time and groups was statistically significant ($F=36.72$, $p= 1.38 \times 10^{-11}$), demonstrating that the temporal slopes differ among the atmospheric and marine records. The main effect of time was significant ($F=29738.78$, $p<2.2 \times 10^{-16}$), reflecting differences in baseline $\delta^{13}C$ values among the atmospheric, calcite, and organic carbon records.

Pairwise comparisons of slopes with Tukey adjustment confirm that the atmospheric trend differs significantly from calcite ($p < 0.0001$) and organic carbon ($p < 0.0001$), whereas no significant difference is observed between the two marine proxies ($p = 0.839$).

3) *Another aspect I am concerned about relates to the assumption of a constant phytoplankton fractionation factor used to reconstruct $\delta^{13}C\_DIC$ from the organic carbon record. The authors derive a fixed value of $\varepsilon = -21$‰ from the preindustrial period (1800–1940) and apply it uniformly across the entire 150-year record. The authors cite Young et al. (2013) whom in their paper, showed that phytoplankton fractionation change measurably between 1960 and 2010 in response to rising atmospheric CO_2, using a fixed preindustrial fractionation factor to the post 1950 introduces a systematic bias into the*

reconstructed $\delta^{13}C$_DIC values which are then used to validate the foraminiferal calcite in Figure 5. The authors should explicitly acknowledge this limitation in the manuscript.

We thank the reviewer for this comment. We have now expanded the discussion in the revised manuscript to explicitly address the assumption of a constant phytoplankton fractionation factor and its potential implications for the reconstruction of $\delta^{13}C_{DIC}$. The revised text reads as follows:

Sea surface temperatures have been shown to be related to a suite of environmental variables in our study region, such as upper ocean mixing, nutrients, and primary productivity (Abella-Gutierrez and Herguera, 2016; Durazo, 2015). It also has a well-known relationship with CO_2 concentrations in the water column (Weiss, 1974) and with the fractionation of carbon isotopes between the DIC in the water column and phytoplankton biomass. This fractionation, known as the biological fractionation factor (Freeman and Hayes, 1992), has been reported to change by phytoplankton in the surface ocean in a well-resolved time series study of phytoplankton $\delta^{13}C$ in the subtropical Atlantic, showing how the isotopic composition of the POC trended towards lighter values than the atmospheric CO_2 trend (Young et al., 2013). They hypothesized that the anthropogenic CO_2 emissions and the consequent increase of the availability of inorganic carbon in the water column is changing the fractionation towards lighter values. Although they cannot rule out the importance of other factors such as carbon fixation, CO_2-specific uptake, and HCO_3 use.

They further state that this biological fractionation is controlled by intracellular Rayleigh fractionation, and that the sensitivity of this fractionation could decrease with increasing CO_2 concentrations, which they use to explain the differences in the fractionation trends between the subtropics and the tropics. There is clear evidence of higher CO_2 uptake in the CCS (Wolfe et al., 2023; Frazão et al., 2025), and how these eastern boundary current waters do have already relatively high DIC values brought by subsurface carbon and nutrient rich old waters, and consequently CO_2, which may push biological fractionation to the minimum values observed for the tropics of 0.01‰ $year^{-1}$ (Young et al., 2013). Although we have no time series of the DIC now alkalinity values in SLB, if we would take this change in fractionation for the past decades, this would mean a change in the slope of 0.1‰ per decade towards lighter values on top of the atmospheric $\delta^{13}C_{CO2}$. This value would mean a steeper slope of 0.36 ‰, much lighter than what we observe, which suggests that the observed slope in our study may be compensated by another process.

4) *In Figure 5, the authors shows the presence of an $\delta^{13}C$ offset between G. ruber and N. dutertrei explained in the text purely in terms of the different depth habitats of the species but do not discuss how the depths of the calcification may shift seasonally and interannually in response to changes in upwelling intensity, thermocline depth and chlorophyll maximum position especially for N. dutertrei whose preferred habitat is tied to the chlorophyll max. if the habitats have changed over the 150 year record, in response for example to the upwelling intensity this could introduce a non atmospheric component into the isotopic trends that are currently attributed entirely to the Suess effect, the author should address the potential sources of bias explicitly in the manuscript.*

We thank the reviewer, and we agree that the depth of calcification for *N. dutertrei* changes on interannual timescales in response to upwelling changes during El Niño and La Niña events, as well as on decadal to interdecadal timescales driven by the PDO, in contrast to *G.ruber* that stays within the upper tens of meters of the water column due to its symbiosis with some phytoplankton

species. Numerous observations show how *N. dutertrei* dwells usually close to the chlorophyll maximum depth (CMD), which in this region has been reported to be between 30 and 40 m depth. The CMD is usually found deeper during El Niño and warm years and shallower during La Niña events and cool conditions and therefore changing the carbon isotopic composition gradient with depth in the water column. On interdecadal timescales, deeper during warm phases (positive PDO) with weaker alongshore winds and decreased coastal upwelling, and shallower during cool phases (negative PDO) associated with the opposite conditions in the CCS.

Furthermore, long-term historical observations in the northern CCS show a trend towards a more stratified upper ocean column over the last half of the twentieth century, associated with warming sea surface temperature, deepening of the thermocline depth, and declining zooplankton biomass (McGowan et al., 2003 and Di Lorenzo et al., 2005).

To infer the stability of the mixed layer, we developed the $\Delta\delta^{18}O$-water column structure proxy by subtracting $\delta^{18}O$ composition from the surface-mixed-layer species (*G.ruber*) with the thermocline to deep-dwelling foraminifera species (*N. dutertrei*) ($\delta^{18}O_{G.ruber}$ - $\delta^{18}O_{N.dutertrei}$). The $\Delta\delta^{18}O$ record shows a mean offset between the respective oxygen isotopic compositions for the past 60 years of 1.88 ± 0.39 ‰ in the SALA11-E19 and of 1.81 ± 0.37 ‰ in the BAP96-6C cores, equivalent to a 7°C to 9°C temperature difference between both habitats, and a very low interdecadal trend towards a higher $\Delta\delta^{18}O$ most likely due to decreased upwelling rates and an increase in the upper water column stratification. These ocean conditions would most likely result in a change in a change of the slope for the $\delta^{13}C$ of *G.ruber* towards heavier values, but it would be the opposite for *N.dutertrei*, since the chlorophyll maximum would deepen and consequently would record lighter values, which we do not observe.

Therefore, although we agree with the reviewer that in the short term there might be some changes in calcification depth, our results with the $\Delta\delta^{18}O$ record suggest it does not play a role in the last half of the past century trend towards lighter values, which is the focus of this study.

5) *I also have a few comments related to the structure and depth of the discussion section, which is largely qualitative and somewhat circular. In the paper, the author shows that the upwelling of older, isotopically heavier subsurface waters partially buffers the atmospheric Suess effect signal, explaining the shallower slope observed in the marine records relative to the atmosphere. This is invoked repeatedly but never quantitatively, is it possible to estimate what fraction of the observed slope attenuation can be explained by subsurface water mixing alone?*

We thank the reviewer for the comment, and we appreciate the concern regarding the qualitative nature of the discussion and the need for a more quantitative assessment of the processes involved. We have revised the manuscript to improve clarity and to better describe the mechanisms underlying the observed $\delta^{13}C$ trends. We expanded the discussion of subsurface water contributions and their interaction with atmospheric signals. Although a quantitative partitioning of these effects would be valuable, it remains challenging given the proxy-based nature of our data and the tight coupling between physical transport and biogeochemical processes.

A recent publication from Frazão et al., (2025), not cited in the initial manuscript, helps understand how local changes in the oceanic conditions can drive an average regional signal that basins like LSB record, but also how challenging would be to calculate the components quantitatively from a

proxy-based analysis. We don't have enough elements to separate quantitatively the components that change $\delta^{13}C$ as air-sea exchange and upwelling waters processes are mixed through the ecosystem respiration, not just as a physical phenomenon. We have updated parts of the discussion for clarification, adding the reference mentioned above (Frazão et al., 2025).

EBUS are characterized by intense upper ocean mixing driven by favorable alongshore winds, which promote the upwelling of nutrients, DIC, and relatively $\delta^{13}C$-depleted, oxygen-deficient subsurface waters. These conditions sustain high primary productivity and support major fisheries in these regions. In this context, $\delta^{13}C$ values in surface waters are primarily controlled by (i) air-sea CO_2 exchange, (ii) biological processes such as primary production and respiration, and (iii) the DIC content of upwelled waters. Primary productivity consumes nutrients and carbon in surface waters, driving $\delta^{13}C$ values toward heavier signatures and establishing a strong vertical gradient within the upper ~100 m (see Figure of $\delta^{13}C_{DIC}$ profiles from the surface to ~550 m in SLB). Below this depth, subsurface waters reflect older DIC signatures due to their limited interaction with the atmosphere. A time series analysis by Frazão et al., (2025) at a near-shore site in the CCS shows net positive fluxes of CO_2, meaning it is acting as a source of carbon to the atmosphere due to the strength of upwelling of DIC richer subsurface waters. Although the observed long-term trend for the near-shore site follows the atmospheric CO_2 increase, they interpret this result from changing water mass composition.

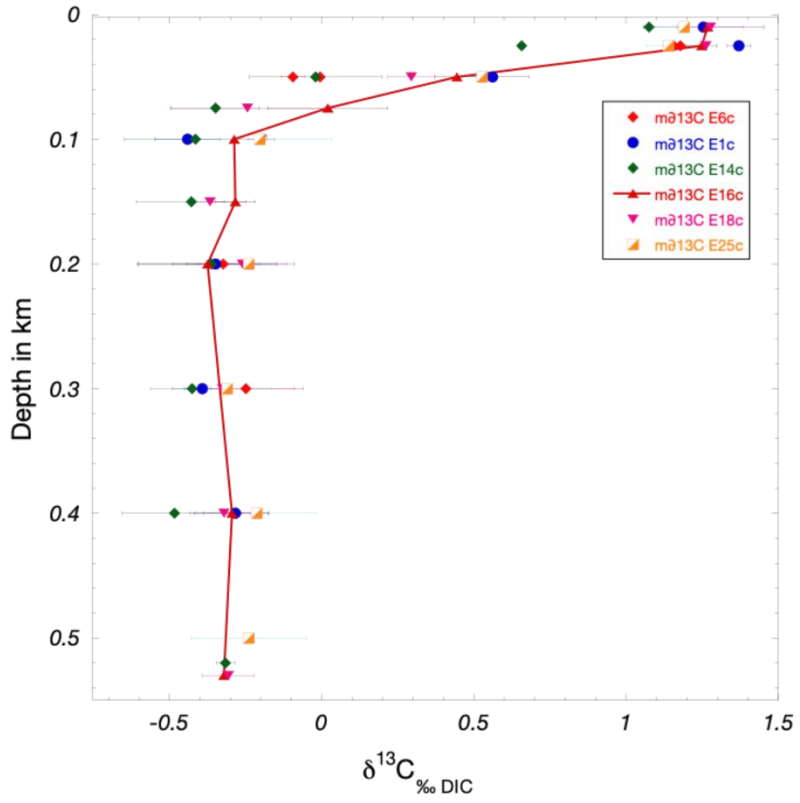

Figure. Vertical profiles of mean $\delta^{13}C$ values show a strong near-surface enrichment, a pronounced decrease within the upper ~100 m, and relatively uniform, lower $\delta^{13}C$ signatures at greater depths across the sampled stations.

6) *I think it would be better to compare the other EBUS records from different ocean basins in the discussion such as Humboldt Current off Peru and Chile, or the Benguela Current off southwest Africa. This would substantially strengthen the paper's contribution to understanding how the Suess effect propagates through EBUS regions globally. If similar slope attenuations are observed in other EBUS sedimentary records, this would powerfully support the authors' mechanistic interpretation and elevate the paper's findings from a regional observation to a globally relevant result. However, if the attenuation observed in their system is stronger or weaker than in other EBUS regions, this would open an important discussion about how additional factors can control the regional expression of the Suess effect. I suggest that the authors add a dedicated paragraph in the discussion addressing this broader EBUS context.*

We thank the reviewer for the comment. We agree that placing our results within a broader EBUS framework strengthens the interpretation and potential relevance of the study. As far as we are aware, long-term, high-resolution $\delta^{13}C$ records from other EBUS regions (e.g., Humboldt and Benguela systems) remain relatively scarce, which limits the extent of direct comparisons. We have nonetheless expanded the discussion to incorporate this broader context as follows:

The attenuation of the $\delta^{13}C$ trend observed in our sedimentary record is consistent with processes documented in the CCS and other Eastern Boundary Upwelling Systems. A recent reconstruction of surface ocean acidification in the CCS for the past century Osborne et al. (2020) estimated a reduction of 35% in the carbonate ion concentration and an ensuing decline of 0.21 in pH, which doubles the global average decline. Nevertheless, their $\delta^{13}C$ records of three planktic foraminifera show a reduced slope for the past century; the steepest slope is exhibited by *G. bulloides* of 0.07‰ per decade, which is even less than the one observed in this paper. Interestingly, their $[CO_3^{-2}]$ reconstruction shows a marked shift towards lower values in the late 1940s and 50s, which is not reflected in none of their $\delta^{13}C$ records. They suggest the undersaturation of $[CO_3^{-2}]$, shown in culture studies by Spero et al., (1997), could have made their $\delta^{13}C$ heavier and thus masked the shifting $\delta^{13}C$ of DIC due to the $\delta^{13}C$ Suess effect.

Our records indicate that the increasing surface ocean inventory of anthropogenic CO_2 is the dominant long-term control on $\delta^{13}C$**,** however, the reduced slope observed in our $\delta^{13}C$ records may reflect this undersaturation effect and alternatively the modulation of the $\delta^{13}C$ Suess effect in upwelling-dominated systems in a similar fashion to the observations by Frazao et al. (2025) on the most coastal monitoring site in the CCS**.** In their study, they observe how the upwelling of old, carbon-rich subsurface waters acts as a net source of CO_2 to the atmosphere. They show how in their near-shore site, the upwelling of CO_2-rich waters controls the pCO_2 and DIC in the surface waters, which leads to a net source of CO_2 to the atmosphere. Although on interannual scales, such as during El Niño events when upwelling is hindered, this site reverses to become a net sink. A phenomenon further observed during the arrival of marine heat events that are associated with a deepening of the thermocline impeding upwelling, causing abnormally low pCO_2 anomalies in surface waters, turning them into a net sink. Therefore, while normal years and La Niña events promote a net CO_2 outgassing from the ocean, El Niño events and Marine Heatwaves favor a net sink of CO_2, and it is most likely that a combination of these outgassing and intake of carbon to the atmosphere controls the $\delta^{13}C$ of the DIC in these surface waters.

Recently published research has proposed the importance of the thermodynamic buffering effects acting as amplifiers of the remineralization processes in the subsurface ocean dissolved inorganic carbon (Stoll et al 2025). The reduction in buffering capacity underscores a greater effect on pCO_2 and consequently acidification processes and brings forth the importance of water mass age when attributing biogeochemical change to anthropogenic carbon. This buffering effect is a nonlinear effect that may not be captured by the $\delta^{13}C$ record and opens questions on the reliability of this record as an acidification proxy, but rather as an indicator of the propagation of the $\delta^{13}C$ Suess effect in surface waters.

Minor comments:

We thank the referee for the comments:

7) *Abstract: Line 12 should mention the abbreviation CCS used in line 14*

We will revise the abstract to introduce the abbreviation (CCS) at its first occurrence (Line 12)

8) *Lines 12-13 needs to be rewritten. How is the role of EBUS a well known high productivity region? Maybe remove 'the role of'*

We will remove "the role" in line 12

9) *Lines 28-29 need to be rewritten or removed. it is better to start the introduction by line 32: 'over the past century, the ocean's carbon uptake… then talk about the processes.*

We have revised the introduction to begin with the statement, followed by a more focused discussion of the processes as follows:

Over the past century, the ocean has absorbed approximately 25-35% of anthropogenic CO_2 emissions from fossil fuel combustion, cement production, and land use changes, among others (Sabine et al., 2004). This uptake plays a critical role in regulating atmospheric CO_2 concentrations and climate. The CO_2 exchange between the ocean and atmosphere is driven by a combination of physical and biogeochemical processes….

10) *Lines 37-40: There is no obvious link between this part and the previous part.*

We agree that the connection between these lines was not sufficiently clear. We have revised the text to improve the transition by explicitly linking the role of EBUS to the broader context of ocean carbon uptake and air-sea CO_2 exchange as follows:

Line 37 (Feely et al., 2024)… Within this context, Eastern Boundary Currents (EBUS) play a key role in modulating regional air-sea CO_2 exchange. Studies have shown that these systems can act as either carbon sources or sinks, depending on season, latitude, El Niño and La Niña events, and the arrival of marine heatwaves (Brady et al., 2019; Chikamoto and DiNezio, 2021; Feely et al., 2008; Lachkar et al., 2024; Lefèvre et al., 2023; Osborne et al. 2020; Frazao et al. 2025), making their representation in global carbon models particularly challenging.

11) *The caption for Figure 1 appears to be duplicated in the text at lines 117–119 of the manuscript.*

We will remove the duplicated figure caption from lines 117-119 in the revised manuscript.

12) *Figure 3 is particularly overcrowded, with multiple cores, proxies, and reference datasets sharing overlapping symbol styles. I suggest at least separating the 3 figures, so the separation is visual.*

While the original layout aimed to facilitate visual comparison of the trends, we agree that it may reduce clarity. We are happy to separate the figures if considered necessary to improve readability.

13) *The conclusions section introduces some new interpretive content that does not appear in the discussion, particularly regarding the 1950s shift mirroring the Law Dome ice core record, which should either be developed properly in the discussion, or removed from the conclusions or mentioned in some way as a perspective.*

We agree that this interpretation should not be introduced in the conclusions. It has now been moved to the discussion section, where it is developed in the context of atmospheric and regional records:

Our results indicate a marked shift in $\delta^{13}C$ trends beginning in the mid-20th century, particularly around the 1950s, which coincides with the accelerated rise in atmospheric CO_2 recorded in the Law Dome ice core record. This timing is consistent with the well-documented post World War II increase in fossil fuel burning, suggesting that the onset of a stronger anthropogenic carbon signal is captured in our record.

In contrast, this shift is less clearly expressed in planktic foraminifera $\delta^{13}C$ records from the CCS (Osborne et al. 2020), despite clear evidence of declining carbonate ion concentration beginning in the late 1940s and 1950s. This discrepancy has been attributed to carbonate chemistry effects that can bias foraminiferal $\delta^{13}C$ toward heavier values under undersaturated conditions (Spero et al. 1997), potentially masking the atmospheric $\delta^{13}C$ signal.

However, the presence of a similar mid-century shift in our organic carbon $\delta^{13}C$ record, which is not subject to these carbonate-related biases, supports the interpretation that this signal reflects a surface ocean response to the increasing atmospheric CO_2 burden rather than an artifact of foraminiferal calcification processes. This suggests that the propagation of the $\delta^{13}C$ Suess effect into surface waters is detectable in the CCS but modulated by regional oceanographic processes.

Consistent with this interpretation, global syntheses (Eide et al. 2017) indicate that the magnitude of the oceanic $\delta^{13}C$ Suess effect is reduced in upwelling and strong mixed regions relative to the atmosphere, reflecting the influence of aged, carbon-rich subsurface waters. These results are consistent with those observations and support the interpretation that the attenuated $\delta^{13}C$ trend observed in our record arises from the interaction between anthropogenic forcing and regional

upwelling dynamics, highlighting the role of oceanographic processes in modulating the atmospheric signal.