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" (Lines 172-197).

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 this comment. We agree that a formal statistical evaluation of the reported trends will strongly support our interpretation. We describe our approach, and it was included in the reviewed manuscript (Lines 233 – 240).

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 (Manuscript lines 320-347):

Against this background of interacting controls, biological fractionation may also contribute to the observed differences between atmospheric and marine $\delta^{13}C$ trends. 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-Gutiérrez 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 in the availability of inorganic carbon in the water column are changing the fractionation toward lighter values (Young et al., 2013). However, they cannot rule out the importance of other factors such as carbon fixation, CO_2-specific uptake, and HCO_3 use (Young et al., 2013).

Young et al., (2013) 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 explains the differences in the fractionation trends between the subtropics and the tropics. There is clear evidence of higher CO_2 uptake in the CCS (Frazão et al., 2025; Wolfe et al., 2023), and that these eastern boundary current waters already have relatively high DIC values brought by enriched subsurface carbon, nutrient, and CO_2 old waters, which may push biological fractionation to the minimum values observed for the tropics of 0.01‰ $year^{-1}$ (Young et al., 2013). Although long-term DIC and alkalinity records are not available for SLB, a simple sensitivity estimate can be made using the fractionation changes proposed by Young et al., (2013). Applying an additional isotopic fractionation of approximately 0.10‰ $decade^{-1}$ to the atmospheric $\delta^{13}C_{CO2}$ trend would result in an expected decline of about -0.36‰ $decade^{-1}$ (-0.26‰ $decade^{-1}$ from the atmosphere plus -0.10‰ $decade^{-1}$ from enhanced biological fractionation). This predicted trend is substantially steeper than the declines observed in either $\delta^{13}C_{Corg}$ or $\delta^{13}C_{calc}$. Therefore, changes in biological fractionation alone cannot explain the isotopic trends observed in our records, indicating that multiple processes influence the propagation of the atmospheric $\delta^{13}C$ signal into the surface ocean.

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.

We add a paragraph in manuscript lines 275– 284.

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.

We add a paragraph in manuscript lines 309 – 347

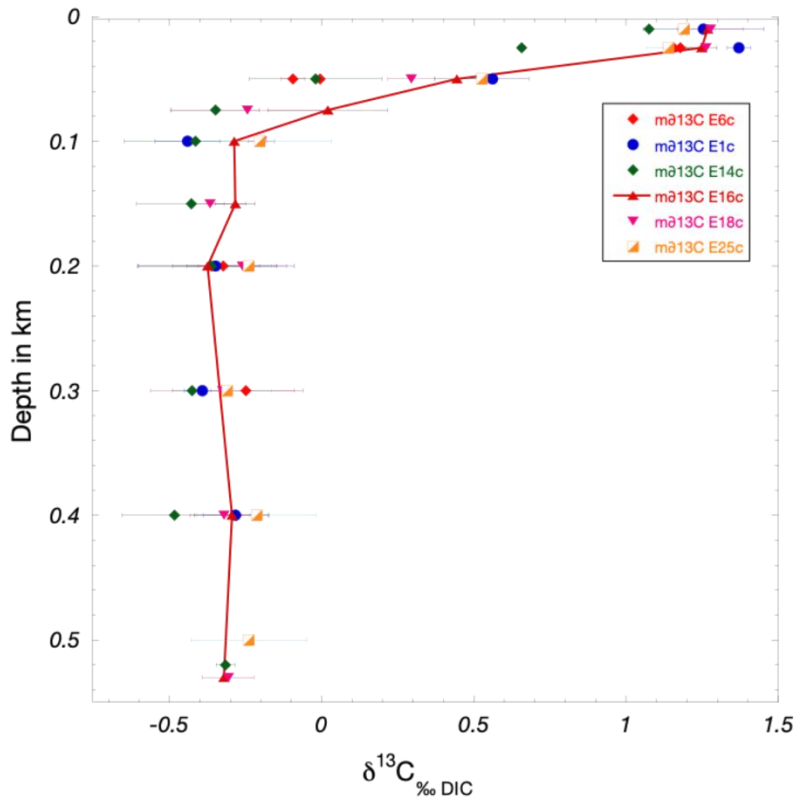


Figure A. Vertical profiles of mean $\delta^{13}C_{DIC}$ values show a strong near-surface enrichment, a pronounced decrease within the upper ~100 m, and relatively uniform, lower $\delta^{13}C$ signatures at greater depths across the sampled stations.

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:

Similar behavior has been reported in other studies of the CCS, highlighting the complexity of interpreting $\delta^{13}C$ trends in upwelling-dominated environments. Osborne et al., (2020)

reconstructed surface-ocean acidification over the last century and documented substantial declines in carbonate ion concentration and pH, yet observed relatively weak $\delta^{13}C$ trends in planktic foraminifera. The steepest decline reported in their study, recorded by *G. bulloides*, was approximately −0.07‰ decade^{-1}, lower than the trends observed here. They proposed that carbonate undersaturation may bias foraminiferal $\delta^{13}C$ toward heavier values, partially masking the atmospheric $\delta^{13}C$ signal. In our records, however, the similar mid-century transition observed in both carbonate and organic carbon phases suggests that the long-term trend cannot be explained solely by carbonate chemistry effects (Osborne et al., 2020).

Additional insight comes from recent observations of carbon cycling in the CCS. Frazão et al., (2025) showed that nearshore waters can alternate between acting as a source and a sink of atmospheric CO_2 depending on upwelling intensity, thermocline depth, and marine heatwave occurrence. During periods of strong upwelling, old carbon-rich subsurface waters dominate surface-water DIC and pCO_2, promoting CO_2 outgassing, whereas reduced upwelling during El Niño events and marine heatwaves can temporarily reverse this balance (Frazão et al., 2025). These observations emphasize that the isotopic evolution of surface-water DIC reflects the combined effects of anthropogenic carbon uptake, air-sea exchange, biological cycling, and the recurrent introduction of subsurface waters.

A recent study has also highlighted the importance of thermodynamic buffering in amplifying the effects of remineralization on subsurface dissolved inorganic carbon inventories (Stoll et al., 2025). Such nonlinear processes further complicate the relationship between ocean acidification and carbon isotope variability and underscore the importance of water-mass history when interpreting long-term biogeochemical change. Within this framework, sedimentary $\delta^{13}C$ records may be particularly valuable as indicators of the propagation of the $\delta^{13}C$ Suess effect through surface waters, even when multiple regional processes modulate the magnitude of the observed signal. Upwelling regions like the Southern California Current play a central role in shaping the marine isotopic carbon signal. The persistence of seasonal source/sink behavior in surface waters likely contributes to the lower slope observed in sediment $\delta^{13}C$ values. Although the ^{13}C Suess effect dominates the long-term trend, its regional expression depends on the balance between atmospheric CO_2 uptake and the delivery of older DIC from depth. As Le Quéré et al. (2018) emphasize regional systems with dynamic vertical and horizontal transport processes are key components of the global carbon cycle and crucial to understanding the propagation of anthropogenic CO_2 through the ocean-atmosphere system.

Lines 369 – 399.

Minor comments:

We thank the referee for the comments:

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

We revised 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 removed "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 recent decades, the ocean has absorbed approximately 25% ± 0.05% to 26% ± 0.06% of anthropogenic CO_2 emissions from fossil fuel combustion, cement production, and land use changes, among others (Friedlingstein et al., 2025). This uptake has slowed down the increase of atmospheric CO_2 concentrations, reducing the magnitude of the anthropogenically driven climate warming (Fung et al., 2005), and lowered the pH in surface waters with important implications for ocean acidification and marine ecosystems (Feely et al., 2004).

Done in manuscript lines 27 - 32

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:

A better understanding of these processes is necessary to improve our knowledge of the biogeochemical cycles, notably the carbon cycle, which has implications for climate change and ocean acidification (Wanninkhof et al., 2013). In this context, studies of 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 the 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; Frazão et al., 2025; Lachkar et al., 2024; Lefèvre et al., 2023; Osborne et al., 2020), making their representation in global carbon models particularly challenging (Lines 33 – 41).

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

We removed the duplicated figure caption from lines 117-119.

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.

Done in Figure 2 (shown in line 232).

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:

In addition to the long-term decline, our records indicate a marked shift in $\delta^{13}C$ trends beginning in the mid-twentieth century, particularly around the 1950s, coinciding with the accelerated rise in atmospheric CO_2 documented in the Law Dome ice-core record. This timing is consistent with the well-documented post-World War II increase in fossil-fuel emissions and suggests that the onset of a stronger anthropogenic carbon signal is recorded in both the organic and carbonate phases. A similar transition is less clearly expressed in planktic foraminiferal $\delta^{13}C$ records from the CCS (Osborne et al., 2020), despite independent evidence for declining carbonate ion concentrations beginning in the late 1940s and 1950s. This discrepancy has been attributed to carbonate chemistry effects that may bias foraminiferal $\delta^{13}C$ toward heavier values under undersaturated conditions (Spero et al., 1997), potentially masking part of the atmospheric signal. In contrast, the presence of a comparable mid-century shift in our organic carbon $\delta^{13}C$ record, which is not affected by carbonate-related fractionation during calcification, supports the interpretation that this feature reflects a surface-ocean response to the increasing atmospheric CO_2 burden rather than an artifact of foraminiferal calcification processes. The emergence of this signal in both proxies suggests that the propagation of the $\delta^{13}C$ Suess effect into surface waters was already detectable in the CCS by the mid-twentieth century, although subsequently modulated by regional oceanographic processes.

We add a paragraph in manuscript lines 252 – 266

Response to Referee 2:

This paper describes $\delta^{13}C$ data since 1850 from sediment cores in the California Current System. The data show that the Suess Effect is a major component of the time series, but also that this fossil fuel induced trend is smaller than in the atmosphere and therefore upwelling processes which bring waters heavier in $\delta^{13}C$ of DIC might also play a role here.

I cannot say anything on the methods, so I focus on presentation and interpretation.

That said I find this study worth publishing, but the figures and the result sections needs in my view some revision to sharpen them and to bring them up to standards. Thus, all comments below (in chronological order) are a lot a small issues which should be considered during revision:

We thank the referee for all the comments:

1) *line 13-14: CCS is an oceanic sink or source to atmospheric CO_2, not an „atmospheric carbon source or sink"*

We agree with the reviewer and have revised the sentence to clarify that the CCS functions as an oceanic source and sink of atmospheric CO_2 (Line 13-14).

2) *If I got it right, you have new data from ~1850 to 2011, so about 160 years. Please correct throughout (eg abstract says 150 years).*

We agree and have revised the manuscript to ensure consistency in the reported time span (~160 years) throughout (Line 18).

3) *Seasonal context (first mentioned in l 19, section 3.1 incl Fig 2): The importance of this seasonal context is not clear to me. First, if you keep it in, you need to clearly state, that this is not your data, but from SOCAT. This appear in the in caption to Fig 2, but not at all in the text. Second, Fig 2A mixes anthropogenic rise with seasonality. You would need to detrend them first and one might calculate a mean seasonal cycle to be of use here. However, your data have no seasonal resolution at all and you also discuss d13C of atm CO2 not on a seasonal timescale (those data would be available, eg. https://scrippsco2.ucsd.edu/graphics_gallery/isotopic_data/mauna_loa_and_south_pole_isotopic_c13_ratio.html). I think you use this seasonality to argue which time of the year your area might be a sink or a source for CO2. That is, however for d13C rather not important. Air-sea gas exchanges happens in both directions throughout the year even if the fugacitiy of CO2 in the surface ocean and the atmosphere are always in balance. The CO2 imbalance (source or sink) is not so important for d13C. I suggest to completely drop this section.*

We agree that the inclusion of the seasonal context is confounding the arguments raise by our paper, since these are data that come from different transects, mostly offshore from the San Lazaro basin, these are not representative of the air-sea dominant transfer processes of CO_2 in our study

location and we will therefore remove this section and the corresponding figure from the manuscript to improve clarity and focus (Deleted).

4) *l 21: Please change „Suess effect“ into „13C Suess effect“ throughout the draft. There is also a 14C Suess effect.*

We agree and have revised the manuscript. The term "^{13}C Suess effect" is now used consistently throughout the draft.

5) *l 34: The reference to Sabine et al., 2004 on the oceanic uptake of anthropogenic emissions is a bit outdated. Maybe use a recent paper from the Global Carbon Project (Friedlingstein et al.,: Global Carbon Budget 2024, Earth Syst. Sci. Data, 17, 965–1039, https://doi.org/10.5194/essd-17-965-2025, 2025) and check their Table 8 for recent numbers on relative oceanic uptake.*

We thank the referee for this helpful suggestion. The statement has been updated using estimates reported in the Global Carbon Budget (Friedlingstein et al., 2025), and the previous reference has been replaced. The revised text appears in lines 27- 29.

6) *l 42: delete „natural“. Citiation for d13CO2 (Baldwin et al. 2005), again is a bit outdated. Since your data go until 2011 your need a newer references than this, I suggest Graven et al. https://doi.org/10.5194/gmd-10-4405-2017, 2017. whic is a nice summary of existing atm d13CO2 data, but check if your given citation to Rubino et al. 2019 might be a better fit here.*

The term "natural" has been removed, and the reference for atmospheric δ^{13}C has been updated by replacing Baldwin et al. (2005) with Rubino et al., 2019, as it provides a high-resolution reconstruction consistent with the temporal coverage of our study (Line 43-44).

7) *l 85 or so. A recent compilation of d13C_orgC data is contained in Verwega, M.-T., Somes, C. J., Schartau, M., Tuerena, R. E., Lorrain, A., Oschlies, A., and Slawig, T.: Description of a global marine particulate organic carbon-13 isotope data set, Earth Syst. Sci. Data, 13, 4861–4880, https://doi.org/10.5194/essd-13-4861-2021, 2021.*

We thank the referee for this suggestion. The recommended reference has been incorporated (Line 87).

8) *Figure 1: Frame around upper fiugre ? The given d13C data (atm, ocean, orgC) need a reference in the caption. There is no „d13C_vertical mixing“, This is maybe d13C_DIC? Overall, I find the lower figure not very helpfull and I suggest you only keep the top map.*

The lower panel has been removed as suggested, and Figure 1 now includes only the study area map (Figure 1).

9) *l 118-119: The description of where the image was taken is already given in the caption of Fig 1, so the sentence here can be deleted*

Deleted

10) *l 186-191: This paragraph does not make sense to me:*

a. *In Fig 3 your d13C_OC starts at 1850, here you say, it starts at 1800 (if so, show data).*

The text has been revised to clarify that the pre-industrial baseline is based on the 1850-1950 interval, which is consistent with the record shown in Fig. 3 (Lines 212 and 218).

b. *Are you using the data to calculate a fractionation factor of -21 permill or are you assuming the factor? If I look at Fig 3 d13C_OC is around -21 permil, but I am missing a calcuation (mean +. error). Furthermore, this is NOT your fractionation factor. For that you need to know / assume d13C_DIC, which in surface waters is around +1.5 to +2.0 permil (Gruber et al., 1999) which together gives you a fractionation of about -22 to -23 permil.*

We thank the reviewer for this important comment. The manuscript has been revised to clarify that the mean δ^{13}Corg value of approximately −21‰ does not represent the isotopic fractionation factor itself. The reconstruction of $\delta^{13}C_{DIC}$ is based on an assumed constant isotopic fractionation factor of 22.6‰ between $\delta^{13}C_{org}$ and surface-water $\delta^{13}C_{DIC}$, following the range reported by Gruber et al. (1999). Lines 217-222.

c. *Second, we used the d13Ccalc from G. ruber and N. dutertrei, assuming isotopic equilibrium fractionation at the depths where each species calcifies. We estimated the d13CDIC yearly by subtracting the organic carbon fractionation factor from the corresponding d13Ccalc. This difference represents the inferred d13CDIC throughout the water column." I do not get it. I see that your first sentence says: d13Ccalc = d13C_DIC, which makes sense to me, your data in Fig 3 agree with Gruber et al. 1999). Why all the rest?*

We thank the referee for the comment. Our intention was to show the planktic foraminifera and the organic carbon δ^{13}C records to highlight the parallel behavior of the inorganic and organic carbon phases to record the δ^{13}C Suess effect in the surface waters of this basin, to better constrain the most likely control of the δ^{13}C of these surface waters (Lines 222-225).

11) *Fig. 2 (which hopefully is deleted or heavily revised, see above): Why 1993-2013 on the right hand side, but 1957-2013 on the left hand side?*

Figure 2 has been removed.

12) *Fig. 3: units are missing. Instrumental d13C_CO2 data go until 2010 or so, therefore the given reference to Keeling et al. 2001 cannot be correct.*

We have added the missing units (‰) to Fig. 3 (now Figure 2) in both the axis labels and the figure caption. We also agree that the reference to Keeling et al. (2001) is not appropriate for data extending beyond 2000. We have therefore updated the figure caption by replacing this reference with Keeling et al. (2005), which is the peer-reviewed citation recommended by the Scripps Institution of Oceanography CO_2 Program for this dataset. We have also clarified that the atmospheric $\delta^{13}C_{CO2}$ data were obtained from the Scripps CO_2 Program database (Mauna Loa Observatory) - Figure 2 (line 232).

13) *Fig. 4: units are missing. r2 of the regression lines are missing. Why do you use only data from 1975 here and from 1950 in Fig. 5? Give arguments and/or at best use the same time windows for analysis.*

We have added the missing units (‰) and included the R^2 values for the regression analyses (now Figure 3 – line 241). Regarding the different time windows, we agree that their selection should be justified, and a paragraph has been added to clarify this point (lines 211-216).

14) *Fig. 5: y-axis is not only d13C_calcite, but also d13C_DIC, maybe say only d13C. Units missing, r2 of regression lines missing. Why do you have a different version of the equation here and in Fig. 4 (here: y=a+bx; Fig 4: y=bx-a). None is wrong, but comparing both figures is thus more difficult than necessary.*

We have revised the y-axis label to $\delta^{13}C$ (‰) to correctly reflect the combined $\delta^{13}C_{calc}$ and $\delta^{13}C_{DIC}$ values. The R^2 values have been added, and the regression equations have been reformatted to ensure consistency across figures (now Figure 4).

15) *l 227: See my comments above on the fractionation factor.*

This point has already been addressed in our response to comment 10.b.

16) *l 247: The slope per decade depends on your window of analysis. It is different on Fig 4 and 5. Give arguments for your choices.*

We agree that the slope per decade depends on the selected time window. But we add a paragraph to justify our choice (Lines 207-216).

17) *l 309: „atmospheric CO2 trends". They are not restricted to Law Dome, but also in other ice cores, and, more important, for the last 40 years or so, also contained in instrumental measurements.*

We have revised the text to clarify that the atmospheric CO_2 trend is recorded in multiple ice core records (including Law Dome) and extends into instrumental measurements (Lines 416- 419).

18) *Overall: You should put your finding into perspective of other studies on the 13C Suess effect in the ocean, eg Eide et al. (2017), which is cited, but I do not think the results presented there are widely discussed here. Do you (dis)agree with Eide?*

We agree that placing our results in the context of previous studies strengthens the interpretation. We have revised the manuscript to more explicitly address Eide et al. (2017), whose results are consistent with ours regarding a reduced oceanic $\delta^{13}C$ Suess effect in regions influenced by strong mixing and upwelling (Lines 302- 308).