

Response to Referee 2:

This paper describes $\delta^{13}\text{C}$ 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 $\delta^{13}\text{C}$ 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 and will modify the text as follows:

Line 13-14... although their role as atmospheric carbon sources or sinks is poorly...

- 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 will revise the manuscript to ensure consistency in the reported time span (~160 years) throughout, including the abstract.

- 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 $d^{13}\text{C}$ of atm CO_2 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 CO_2 . That is, however for $d^{13}\text{C}$ rather not important. Air-sea gas exchanges happens in both directions throughout the year even if the fugacity of CO_2 in the surface ocean and the atmosphere are always in balance. The CO_2 imbalance (source or sink) is not so important for $d^{13}\text{C}$. 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 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.

- 4) l 21: Please change „Suess effect“ into „ $\delta^{13}\text{C}$ Suess effect“ throughout the draft. There is also a ^{14}C Suess effect.

We agree and will revise the manuscript to consistently refer to the “ $\delta^{13}\text{C}$ Suess effect” 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; we will replace the previous reference and update this statement as follows:

Over recent decades, the ocean has absorbed approximately $25\% \pm 0.05\%$ to $26\% \pm 0.06\%$ of anthropogenic CO_2 emissions from fossil fuel combustion, cement production, and land use changes, among others (Sabine et al. 2004; Friedlingstein et al., 2025).

- 6) l 42: delete „natural“. Citation for $d^{13}\text{C}_{\text{CO}_2}$ (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. which is a nice summary of existing atm $d^{13}\text{C}_{\text{CO}_2}$ data, but check if your given citation to Rubino et al. 2019 might be a better fit here.

We have removed the term “natural” as recommended and updated the reference for atmospheric $\delta^{13}\text{C}$ by replacing Baldwin et al. (2005) with a more recent study. We have selected Rubino et al., as it provides an updated and high-resolution reconstruction consistent with the temporal coverage of our record.

- 7) l 85 or so. A recent compilation of $d^{13}\text{C}_{\text{orgC}}$ 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 will include the recommended reference (Verwega et al. 2021) to complement Tagliabue and Bopp (2008) and support the use of $\delta^{13}\text{C}$ of organic carbon as a proxy for past surface ocean CO_2 conditions, as follows:

In addition, the organic carbon $\delta^{13}\text{C}_{\text{OC}}$ also serves as a valuable proxy, as it reflects the photosynthetic conditions under which carbon was fixed, offering insights into past surface CO_2 air-sea exchange processes (Tagliabue and Bopp, 2008; Verwega et al., 2021).

- 8) Figure 1: Frame around upper figure? The given $d^{13}\text{C}$ data (atm, ocean, orgC) need a reference in the caption. There is no „ $d^{13}\text{C}_{\text{vertical mixing}}$ “, This is maybe $d^{13}\text{C}_{\text{DIC}}$? Overall, I find the lower figure not very helpfull and I suggest you only keep the top map.

The lower panel will be removed.

9) 1118-119: *The description of where the image was taken is already given in the caption of Fig 1, so the sentence here can be deleted*

We will remove this sentence, as the information is already provided in the figure caption.

10) 1186-191: *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).*

We will correct the starting year from 1800 to 1850 to match the data shown in Fig. 3.

b. *Are you using the data to calculate a fractionation factor of -21 permil or are you assuming the factor? If I look at Fig 3 $d13C_{OC}$ is around -21 permil, but I am missing a calculation (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 pointing out this inconsistency. The reviewer is right the fractionation factor would be closer to -22.5‰, the only $\delta^{13}C$ of DIC record we have in this basin was collected by us in 2011, and surface waters show mean values of 1.5‰ and thus the fractionation factor would be closer to -22 to -23‰. What we did was to subtract from the individual data points their mean values for the time period between 1850 to 1950, which was -21‰, we are basically calculating the anomalies for the organic carbon record from the previous 100 years.

c. *Second, we used the $d13C_{calc}$ from *G. ruber* and *N. dutertrei*, assuming isotopic equilibrium fractionation at the depths where each species calcifies. We estimated the $d13C_{DIC}$ yearly by subtracting the organic carbon fractionation factor from the corresponding $d13C_{calc}$. This difference represents the inferred $d13C_{DIC}$ throughout the water column. “ I do not get it. I see that your first sentence says: $d13C_{calc} = d13C_{DIC}$, which makes sense to me, your data in Fig 3 agree with Gruber et al. 1999). Why all the rest?*

This was probably lost in our explanation, the idea behind showing the planktic foraminifera and the organic carbon $\delta^{13}C$ was to highlight the parallel behavior of both records in the inorganic and organic phases to record the $\delta^{13}C$ Suess effect in the surface waters of this basin, to better constrain the most likely control of the $\delta^{13}C$ of these surface waters.

The paragraph has been rewritten accordingly to reflect these clarifications.

We applied complementary approaches to evaluate changes in $\delta^{13}C$ in the surface ocean. First, we used the $\delta^{13}C_{C_{org}}$ record spanning 1850–1950 to define a baseline mean value (−21‰), which was used to calculate anomalies relative to pre-industrial conditions. This approach does not represent a biological fractionation factor but rather provides a reference for assessing temporal changes in the organic carbon pool.

Second, we used $\delta^{13}\text{C}_{\text{calc}}$ measured in planktic foraminifera (*G. ruber* and *N. dutertrei*), which approximate $\delta^{13}\text{C}_{\text{DIC}}$ at their respective calcification depths under near-equilibrium conditions. The comparison between $\delta^{13}\text{C}_{\text{calc}}$ and $\delta^{13}\text{C}_{\text{Corg}}$ allows us to assess the consistency between inorganic and organic carbon reservoirs in recording the $\delta^{13}\text{C}$ Suess effect. The coherent trends observed in both proxies may indicate that the $\delta^{13}\text{C}$ signal primarily reflects changes in surface ocean dissolved inorganic carbon rather than proxy-specific fractionation effects.

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 will be removed from the draft.

12) *Fig. 3: units are missing. Instrumental $d13\text{C}_{\text{CO}_2}$ 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 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 CO₂ Program for this dataset. We have also clarified that the atmospheric $\delta^{13}\text{C}_{\text{CO}_2}$ data were obtained from the Scripps CO₂ Program database (Mauna Loa Observatory), spanning 1978-2024. The revised caption now reads:

Figure 3. Time series of the carbon isotopic composition are presented in three sections: the upper panel illustrates the atmospheric reservoir from an ice core (Rubino et al., 2019) and instrumental data (Keeling et al., 2005; Scripps CO₂ Program database). The middle panel shows the organic carbon phase obtained from sediment cores, and in the lower panel, the calcite phase obtained from planktic foraminifera *N. dutertrei* and *G. ruber* from sediment cores.

13) *Fig. 4: units are missing. r^2 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 agree that using consistent time windows improves the comparability between figures. We have therefore revised Fig. 4 to show all records starting from 1950, consistent with Fig. 5. The atmospheric $\delta^{13}\text{C}_{\text{CO}_2}$ record is included from its available range (1978 onward), and this is now clearly indicated in the figure. We have also added the missing units (‰). In addition, following the suggestion of another reviewer, we now report the R^2 values for all regression lines, calculated over the overlapping time intervals between the records, to ensure a consistent comparison.

14) *Fig. 5: y-axis is not only $d13\text{C}_{\text{calcite}}$, but also $d13\text{C}_{\text{DIC}}$, maybe say only $d13\text{C}$. Units missing, r^2 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 $\delta^{13}\text{C}$ (‰) to correctly reflect the combined $\delta^{13}\text{C}_{\text{calc}}$ and $\delta^{13}\text{C}_{\text{DIC}}$ values. The regression equations have been reformatted to ensure consistency across figures.

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

We have already commented on this point in observation 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. To ensure consistency, we have revised the analysis so that both Fig. 4 and Fig. 5 use the same time interval (1950–2011).

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 agree that atmospheric CO₂ trends are not restricted to the Law Dome ice core record and are also documented in other ice cores and extended into the instrumental record over the last decades. We intended to highlight that the long-term trend toward lighter $\delta^{13}\text{C}$ values is already evident in ice core records before the onset of direct atmospheric measurements. We have revised the text to clarify this point and to better reflect the broader observational basis. The text has been modified as follows:

Our results emphasize the importance of the anthropogenic source of carbon in altering the oceanic carbon isotopic composition, and how this trend has significantly shifted in the CCS since the 1950s, mirroring the atmospheric CO₂ trend recorded in ice core records (e.g., Law Dome, Antarctica) and extended into the instrumental record.

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) and clarify that our results are consistent with their findings of a reduced oceanic $\delta^{13}\text{C}$ Suess effect in regions influenced by strong mixing and upwelling.

Consistent with this interpretation, global syntheses (Eide et al. 2017) indicate that the magnitude of the oceanic $\delta^{13}\text{C}$ 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 $\delta^{13}\text{C}$ 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.