

Response to Referee #1 comments

The authors would like to thank anonymous Referee #1 for her/his detailed revision and valuable comments and suggestions, which have greatly helped improve the manuscript and have provided interesting food for thought. A detailed point-by-point reply to the general and detailed comments follows below. In the following, referee comments are **slanted and bold**, while author comments are highlighted in blue.

General comments:

I have enjoyed reading this paper though it took me some time given the length. It is undoubtedly well-written and comprehensive, a very good example of using a numerical model to investigate the still controversial issue about the role of coastal systems in the global carbon balance. This manuscript investigates the processes at play in the California Current System building on previous works that have first described the hydrodynamics and the bulk biogeochemical dynamics. It is therefore a robust approach which may serve as example for other systems. This is why I would suggest the authors to make an additional effort and elaborate more on some of their findings while at the same time shortening some parts that cannot thoroughly be investigated with their specific setup. The paper should be accepted with minor revisions. I would like to point out that this is a personal view point as a peer scientist working in the same field and therefore I'll understand if the authors or editors have arguments against the suggested rearrangement.

- *The feeling I'm left with at the end of the paper is that, despite the accurate analysis, this work does not add much to the carbon balance in the CalCS. The explicit aims of the work were to quantify the mean CO₂ fluxes of the system and to assess the spatio-temporal variability of the driving processes, separating the contributions of solubility dynamics, air-sea exchange, biological through-flow and physical transport. I think the authors are doing a good job and should streamline a bit more the conclusion that coastal regions are likely to be much more compensated in terms of carbon fluxes than conventionally thought (in the limit of the Redfield assumptions, see my specific comment below).*

In Section 7, we discuss the near complete spatial compensation of air-sea CO₂ fluxes in the CalCS being caused by biological productivity very closely compensating the effect of ocean circulation. Further, we put this into the global context and discuss the importance of the efficiency of the biological pump. Based on our knowledge about nutrient utilization and limitation in the different upwelling systems, we attempted to compare the CalCS also to the Canary and Humboldt Current Systems. However, we cannot make any sound statements about the nature of air-sea CO₂ flux compensation for all coastal regions, based on our knowledge of the CalCS.

To discuss the effect of using a limited C:N ratio on the compensation of CO₂ flux, we have added the following text to the Discussion (Section 7):

“These arguments depend, of course, critically on the near constancy of the stoichiometric C:N ratio of phytoplankton growth. Any carbon over- or underconsumption relative to our assumed Redfield ratio of 106:16 would permit the biologically-driven component of the air–sea CO₂ fluxes to decouple from the efficiency of the biological pump. But as we argued above, we expect the potentially systematic tendencies of this ratio to have a relatively small effect on the whole domain air–sea CO₂ fluxes. While the nearshore carbon underconsumption makes the biological pump less efficient there, the tendency for carbon overconsumption in the offshore, which enhances the efficiency, may largely compensate for it, resulting in little overall change. This is rather speculative, and a more thorough assessment of the effect of systematic variations in the stoichiometric ratios on the air–sea CO₂ fluxes is clearly needed. But our current understanding of the underlying processes controlling these ratios is poor, preventing us from following this path.”

- ***If the authors have arguments to show (and I think they do) that the current observational network is inadequate to carry on estimates of carbon fluxes, I think they should state this clearly. The discrepancy with the CalCoFI line presented in Fig. 4 is rather remarkable and should be discussed more.***

The discrepancy with the CalCOFI line is most likely due to our spatially and temporally coarse wind forcing, which would favor an overestimation of pCO₂ in the first 50–100 km. More specifically, in our model forcing the typical nearshore wind speed drop-off is likely underestimated, which has been investigated before by Capet et al. (2004). This underestimation of wind-speed drop-off would favor more intense coastal upwelling and elevate nearshore pCO₂ levels. This issue is explained as well in the last paragraph of Section 3 of the revised manuscript.

We have additionally included in Section 6.2 suggestions on how the current observational network could potentially be strengthened:

“Without a full Observing System Simulation Experiment (OSSE), we are not in the position to make accurate recommendations with regard to how the current network would have to be expanded to capture the mean flux and its variability with good confidence. Nevertheless, we can make some qualitative, general statements, based on our model-based experience. First, the presently available observations are likely sufficient to estimate the domain-wide climatological annual mean air–sea CO₂ flux, as indicated by the relatively good agreement between the most recent estimates. Second, the current network is with good confidence insufficient to determine variability in time and space around this mean flux. In order to achieve this, the network would mainly need to be expanded in the first 100 km, where the short temporal and spatial decorrelation length scales require a denser coverage of pCO₂ and air–sea CO₂ flux measurements. It would furthermore be highly desirable to have a more complete latitudinal coverage of the nearshore area of the entire US West Coast, whose current observational coverage is at best fragmentary. To this end, alongshore underway cruises, rather than moored stations, may provide the most adequate means of measuring pCO₂ within this extended area of interest.”

- *The mesoscale analysis presented in Sec. 4.6 appears marginal and not as focused as the other sections. I would suggest the authors to reconsider the inclusion of this part or to make it more functional to the aim of the manuscript. As suggested by the authors in the conclusions, the study of mesoscale should be done in combination with other variables and to understand their spatial correlation.*

(The referee is referring here to Section 6.2.) Thank you for this suggestion. After a discussion of the relevance of Section 6.2 on mesoscale analysis for the whole manuscript, we have decided to keep it. Firstly, by including this analysis of mesoscale or “non-seasonal” variability – which can be seen as the residual variability after removing the seasonal from the total sub-annual variability – the analysis of seasonal variability in Section 6.1 can be put in a bigger context of the total sub-annual variability. Secondly, it gives an initial impression of the importance of mesoscale variability in the CalCS in the first 0–100 km, even if we cannot yet estimate the full scope of this variability with our current climatologically forced simulation.

- *The caveats of the sensitivity experiments for process understanding should be carefully outlined before being applied (see for instance the notes of caution given by Lovenduski et al in their 2007 paper). This issue is not only related to the numerics of the flux reconstruction, but also to the design of the experiments. Biology is responsible for the vertical gradient in DIC and therefore once biology is removed, it is obvious that circulation acts to restore the gradient found in the initial conditions leading to a surface ocean $p\text{CO}_2$ that is temporarily higher than the atmospheric value. In the longer term, without the mediating role of biological uptake, it is to be expected that DIC would equilibrate. It is trivial to consider that if the simulation would start from an homogeneous value of DIC no such effect would be seen. Disentangling the specific magnitude of each process by successive removal of the terms may lead to misleading considerations. It is an exploratory experiment but only by storing and analyzing the single terms of the dynamical equation we may hope to fully understand their dominance.*

We acknowledge the fact that the approach of separating the individual processes by sequential removal is a semi-quantitative, approximate approach. Nevertheless, we believe that the values of such an approach outweigh the caveats and that we can moreover gain valuable insight from this approach into the drivers and processes determining surface ocean $p\text{CO}_2$ in the CalCS.

We have added some sentences to discuss the limitations of this approach at the end of Section 2.3:

“In this second approach, we implicitly make the assumption that the contributions of the different processes are linearly additive. Given the non-linearities of the ocean carbonate system (Sarmiento and Gruber 2006), this is strictly speaking not the case.

This sequential removal of processes is at best an approximate method which allows the estimation of the magnitude of each term in Eq. 3. However, our experience with a permuted sequence where we first inhibited biological production and then set the air-sea CO₂ flux to zero, showed little difference, indicating that these non-linearities are not substantial enough to alter our results. Moreover, this kind of approach has previously been used to great effect to investigate similar questions (e.g., Murnane et al. 1999; Schmittner et al. 2013).”

Detailed comments:

abstract: The abstract is too long. It should be more focused on the major methodological aspects and findings. As it stands, it looks more like an extended abstract of a thesis work.

Done. The abstract has been shortened accordingly.

P14049_L11: The NPZD model in ROMS is not an ecosystem model. It is a biomass-based biogeochemical model where plankton functional groups are treated as clouds of unicellular organisms (even in the case of metazoans) represented by their nitrogen content. It is just a portion of the ecosystem.

We agree that the NPZD model is not a fully-comprehensive ecosystem model, but rather represents only a simplified part of the entire ecosystem. The term “ecosystem” has been removed, so that we now talk about a “physical-biogeochemical model”.

P14049_L21: Given the importance of the biological loop in controlling the carbon fluxes, I think the authors should consider in their discussion the limitation of using fixed stoichiometry in biogeochemical plankton dynamics (e.g. Thomas et al., 1999; Flynn, 2010). Especially in coastal systems, the decoupling of carbon and nutrient utilization may lead to a much larger carbon uptake than the one derived just by nitrogen drawdown, which is the relationship used in this model.

We have included a discussion of the limitation of using a fixed C:N ratio in our model and the potential impact on our air-sea CO₂ flux estimates towards the end of Section 4:

“Our uncertainty estimate also does not include the potential impact of variable stoichiometric C:N ratios for phytoplankton growth. Martiny et al. (2013) showed recently that these ratios may vary systematically with oligotrophic gyres having larger than Redfield ratios and nutrient-replete systems having lower than Redfield ratios. While we do not expect a substantial effect of such systematic variations in the C:N ratios on the overall budget of the CalCS, they will quite certainly affect the local fluxes nevertheless. We would expect a larger outgassing in the nearshore regions, as the tendency for lower than Redfield C:N ratio in such nutrient replete systems would cause a lower carbon drawdown, permitting a larger fraction of the upwelled carbon to escape to the atmosphere. In contrast, in the oligotrophic offshore regions, we would expect a stronger uptake of CO₂ from the atmosphere, as the higher than Redfield C:N ratio would tend to lead to lower pCO₂. Overall, we would expect a stronger

onshore-offshore gradient, but not a large change in the net flux over the entire study region. A more quantitative assessment of the effect of using a variable C:N ratio would require a more detailed, separate analysis with additional sensitivity simulations.”

P14051_L4: I have gone through the whole manuscript to find a reference on the type of forcing functions. Since I cannot believe that authors can produce any mesoscale dynamics with mean monthly forcings, I presume that the climatology has a higher temporal frequency. This is indeed described in previous works with the same model, but it should be written here as well as the period over which the climatology was derived.

The climatologies used to force the model at the atmospheric and lateral boundaries indeed only have a monthly temporal resolution, as described in Section 2.2. The spatial and temporal mesoscale dynamics which are discussed in Section 6.2 arise from the non-linearity of the physical transport within the model and exist independently of the temporal forcing frequency. More specifically, they arise from the baroclinic instabilities due to changes in the density gradients associated with coastal upwelling.

Thanks to this comment, we realized that there was a clear need of a better explanation of the temporal model output frequency used for our different analyses. From our 5km-resolution simulations, we saved the model output at 2 different temporal frequencies:

1. At monthly frequency: this is the output which we then average over the last 7 of a total of 12 analysis years and which is used for our annual mean and seasonal analyses in Sections 3, 4, 5 and 6.1.
2. At 2-day frequency: as we were interested in investigating the mesoscale variability, this output was not averaged over the 7 analysis years, but rather the whole span of the 7 analysis years was used for the mesoscale analyses in Section 6.2 (and in Figs. 8 and 11).

We have rewritten this in the first paragraph of Section 2.2 to state:

“The model was started from rest and run for 12 years with monthly climatological forcing. As our model simulations require about 5 years for the spinup, we use model years 6 through 12 for analysis. For our annual mean and seasonal analyses in Sections 3, 4, 5 and 6.1, we used model output at monthly resolution and averaged this to obtain a climatology over 7 years. For the analysis of mesoscale processes in Section 6.2, we used 2-day model output and looked at all analysis years without averaging.”

P14052_L23: It is not clear how the perturbation was done. Was it done on the model domain (that is, with the whole model starting from a perturbed state) or using just the carbonate equilibrium dynamics and taking the numerical derivatives?

The perturbation of DIC, Alk, T and S was done by adding a small amount (2 mmol C m⁻³ for DIC and Alk; 0.5°C for T; 0.1 for S) to the model output DIC, Alk, T and S at every grid box in our model domain. We then used an offline carbonate chemistry computing

tool which employs the OCMIP routines for the recalculation of pCO₂, using each perturbed variable in turn, which then gave us four different results for pCO₂ (plus the original “control” pCO₂).

We have rewritten this in Section 2.3 to make it clearer: “These partial derivatives were determined by adding a small perturbation to each driver and recalculating pCO₂ four times with these new values with an offline carbonate chemistry calculating tool based on the OCMIP routines.”

P14053_L10: Please consider the following questions and include relevant information in the text: 1) are the major features well represented by the degradation in resolution? 2) How long did you run these experiments? 3) Starting from the same initial conditions?

1. Given the fact that the 15km-resolution simulations can still be characterized as eddy-resolving, this means the most important features can still be well represented at this resolution. The absolute values of the variables of interest might differ slightly between the 5km- and the 15km-resolution simulations, due to the fact that with the 5km-resolution simulation we can resolve processes bordering on the sub-mesoscale, while with the 15km-resolution simulation we are limited to resolving coarser mesoscale processes. However, we strongly believe that the overall qualitative picture remains the same for both simulations and that our analysis of the processes driving pCO₂ remains unaffected by this degradation in resolution.
2. They were run for the same length as the 5km-resolution simulations, i.e. for 12 years, averaging over and analyzing only the last seven years (i.e., years 6-12).
3. Yes, these simulations were started from the same initial conditions as our 5km-resolution simulations.

We have changed the text in Section 2.3 accordingly, to read:

“Due to computational resource limitation, we undertook these simulations at a slightly coarser horizontal resolution of 15 km, using the same initial conditions and running them for the same length as the full-resolution simulations. Despite the degradation in resolution, the model still manages to well-represent the major mesoscale features.”

P14054_L1-5: This explanation should be expanded because it is crucial for understanding a large part of the manuscript. These kind of experiments are always intriguing because separating transport terms from with sequential exclusion necessarily modifies the concentration. Biology creates gradients and the circulation tends to restore them, therefore the order of permutations should count. Indeed, since the two major terms are biology and transport (and transport cannot be removed!), it is understandable that it makes not much of a difference. Also, permuted sequence means that, for instance, you also tested experiment S2 composed of no biology and constant solubility?

As circulation cannot be sequentially removed, it is necessarily what remains after the other processes have been removed. However, we performed one sensitivity study where we first removed biological production and then inhibited the CO₂ gas exchange and set CO₂ solubility to a constant value. This permuted sequence showed little difference to the sequence which we chose for the final version of the manuscript. We also performed another sensitivity study where we did not remove each process sequentially, but rather modified each process separately in 4 different sensitivity studies, always starting from unaltered initial conditions. In this case, the effects of the individual processes on pCO₂ were not additive, and hence the interpretation of each sensitivity study was rendered much more difficult.

We have added some sentences to explain our procedures better at the end of Section 2.3:

“In this second approach, we implicitly make the assumption that the contributions of the different processes are linearly additive. Given the non-linearities of the ocean carbonate system (Sarmiento and Gruber 2006), this is strictly speaking not the case. This sequential removal of processes is at best an approximate method which allows the estimation of the magnitude of each term in Eq. 3. However, our experience with a permuted sequence where we first inhibited biological production and then set the air–sea CO₂ flux to zero, showed little difference, indicating that these non-linearities are not substantial enough to alter our results. Moreover, this kind of approach has previously been used to great effect to investigate similar questions (e.g., Murnane et al. 1999; Schmittner et al. 2013).”

P14054_L12: If you run the experiment till adjustment than I guess that mortality terms consume all initial biomass within the first biomass.

The purpose of the sensitivity study where we switched off incoming solar radiation (S2), was to investigate the effect of inhibiting phytoplankton growth – and hence the production of organic matter and the biological drawdown of CO₂ – on pCO₂. At the beginning of our simulation, all initial biomass is rapidly removed, due to the fact that the temporal scale of mortality (i.e., 10 days) is much smaller than the length of the simulation (i.e., 12 years, of which we analyze years 6 through 12).

P14054_L17: Usually salinity has no unit, but maybe the journal accepts this.

We have removed all occurrences of “PSU” so that salinity is now unitless.

P14056_L5: I think it should be mentioned the large overestimation in the northern coastal region, particularly in spring, where the data show a clear low pCO₂ while the model does not. This should be introduced in view of the analysis done in the next section on the process assessment. It is interesting that the Taylor diagram reports a weak overestimation in this season where it does not look like in the map. Is this related to the underestimation in primary production reported by Gruber et al. (2011).

The reason for this discrepancy between what Fig. 2 shows and the numbers reported in

the Taylor diagrams in Fig. 3 is that fewer observations were used for the analysis shown in the Taylor diagrams than are visible in Fig. 2. Specifically, we only used grid boxes with at least two observations taken in two different months within a season for the seasonal analysis and grid boxes with at least 2 observations from opposite seasons (DJF and JJA, MAM and SON) for the annual mean analysis. For the annual mean analysis, these elimination criteria reduced the number of available grid boxes with observations by about 27%. Please also note that the Taylor diagrams show pCO₂ biases averaged over the whole nearshore domain (0–100km), which would include any pCO₂ overestimation by the model occurring in the central and southern nearshore domains.

We have added a sentence describing the model’s overestimation of pCO₂ in the northern and central nearshore domains in spring and summer:

“The model also captures the north-south gradients and its seasonal progression, particularly in the offshore regions (Fig. 2a, b and d). However, it does have a tendency to overestimate pCO₂ in the nearshore regions, which is especially noticeable in the northern and central subdomains in spring and summer (Fig. 2b and c).”

P14059_L27-: These comments are probably more pertinent to the final discussion. See my general comment above.

Thank you for this suggestion. After some discussion about the structure of the manuscript, we have decided to keep this part on the comparison of our air–sea CO₂ flux estimates with other studies at the end of Section 4 rather than moving it to the final discussion, as we believe that the readability of the manuscript is improved this way.

Sec4.2: This section seems like a repetition of the one before. It essentially describes Fig. 5 that has been previously discussed. What is the added value? By moving the paragraph from line 5 to 9 at page 14061 to the previous section and paragraph from 10 to 14 to the next one the paper would be streamlined and easier to read.

Done. Sections 4.1 and 4.2 have been merged into Section 4: “Sources and sinks for atmospheric CO₂”.

P14062_L4-6: See my final general comment above. Also, add a reference to Table 1 after the sentence “This process-based separation...”.

Please refer to the answer to the final general comment above. A reference to Table 2 of the revised manuscript was added to this sentence so that it now reads:

“This process-based separation based on the sensitivity studies (Table 2) reveals that the most important contributions to the spatial gradients of annual mean pCO₂ are circulation and biological production (Fig. 7a and b), both of which act upon DIC and Alk.”

P14062_L17: Please add “(not shown)” when describing alkalinity as it is not

in the figure.

Done. This sentence now states:

“The upwelled waters are also enriched in Alk, which acts to reduce the impact of the upwelling of DIC on surface ocean pCO₂, but this effect is substantially smaller (not shown).”

P14063_L1-8: The biological loop described by the authors has to be necessarily linked to the decoupling between nutrients (N in this case) and carbon uptake.

The “biological loop” as described by us refers to the combined effects of circulation and biological production, i.e., the ongoing supply of DIC and nutrients to the surface and concurrent reduction of these through biology. As the upward component tends to control also the supply of the limiting nutrient to the near surface ocean, and hence also determines to a large degree the magnitude of biological productivity, the upward and downward components of the biological loop are strongly coupled with each other. The nutrient use efficiency is ultimately what determines whether there is a spatial decoupling between the area of maximum outgassing and the area of maximum biological production. Of course, the tightness of the coupling between the nutrient supply and the strength of the biological pump (measured in terms of carbon fluxes) depends on the flexibility and variability of the C:N ratio (and those of C:P and C:Fe as well). Recognizing this, we have added some comments about this possibility of a decoupling. Please refer to the answer to the third detailed comment above.

P14064_L6-8: Why not using the standard deviation in the figure plots as well? It would be easier to understand the magnitudes of the processes. A possible alternative would be to use the coefficient of variation that gives an idea of the relationship with the mean.

Done. We have changed Fig. 8 to show the total and seasonal standard deviations of pCO₂ instead of the variance. Panel 8c still shows the fraction of the total pCO₂ variance (square of the standard deviation) attributable to non-seasonal variability, i.e. the difference between total and seasonal pCO₂ variance divided by the total variance. The phrasing in the caption of Fig. 8 has been changed accordingly.

P14064_L13: Fig 8c is in percentage while the others are absolute values. Please make this clear in the text as well.

Done. We have modified the first sentence in Section 6.2 to read:

“Figure 8 highlights that although the seasonal component accounts for most of the total pCO₂ variability in the offshore regions, a substantial fraction of the total variability in the nearshore regions is driven by the non-seasonal component (Fig. 8c, shown in percent).”

P14065_L20-23: This sentence seems to imply that upwelling decreases the pCO₂ value during wintertime, which is not physically possible, and it is just an appar-

ent effect of removing the annual mean from each experiment (dominated in this case by a large summertime upwelling). This is why I believe this kind of analysis should be explained with more details as the means from each process-driven experiment are sensibly different, and comparing the relative results may not be completely correct.

The analysis of the temporal contributions of the 4 mechanisms – shown in Fig. 10 and discussed at the end of Section 6.1 – is focused on the contributions of these 4 processes to the seasonal anomalies of pCO₂. To this end, we removed the annual mean pCO₂ from its seasonal cycle. Hence positive values in Fig. 10 mean that pCO₂ is higher than normal, relative to the annual mean (and does not mean undersaturation) and negative values indicate that pCO₂ is reduced compared to the annual mean.

P14066_L6: *Please specify the meaning of “somewhat different”.*

Thank you for pointing this out. This sentence has been removed as it contained no critical information.

Section4.6: *This is the weakest part of the manuscript. I would suggest the author to reconsider this section and maybe include it in a future work where the mesoscale aspects are more central. I thought the authors used a perpetual year simulation and therefore it is important that they explain what do they mean with nonseasonal component. There may be some mesoscale variability that is seasonal. The methodology described in the figure caption is not clear, and I do not understand why the authors need a smoothing of the anomalies. Also, Fig. 8c is expressed in percentage while the analysis in Fig, 11 is given with anomalies and therefore it is difficult to appreciate the magnitude of the variability associated to the mesoscale in the spatial domain.*

(The referee is referring here to Section 6.2.) Please see our answer to the third general comment above, where we state that we have decided to keep Section 6.2. However, we realized that there was a clear need of a better explanation of the temporal model output frequency used for our different analyses, in particular the analysis of mesoscale processes. This issue has been addressed in the answer to the fourth detailed comment above. The smoothing in Fig. 11 had only been added for visual purposes, but as this at the same time prevents an accurate determination of the non-seasonal pCO₂ anomalies, it has been removed to show the unsmoothed data.

P14068_L1-3: *This remark is exactly my last point in the general comments above. I think the authors should make clear from the beginning that their exercise of sequential removal of processes is only an approximate method to estimates the magnitude of each term in the dynamical equation.*

We have included some sentences explaining the approximate nature of this approach in

the revised manuscript. Please refer to our answer to the last general comment above.

P14068_L19: I guess the authors mean “loop” and not “pump”. The biological pump must be (partly) increasing as well because of enhanced nutrient availability, but the DIC upwelling is dominant.

We indeed meant “biological pump” and not “biological loop”, when talking about the efficiency. We were using this term “efficiency of biological pump” as used by Lachkar and Gruber (2013), i.e., the “relative balance between the nutrients and carbon that are transported and mixed upward into the euphotic zone and the nutrients and carbon that are fixed into organic matter and exported downward again”. Or more specifically, when talking about the supply of limiting nutrients, the efficiency of the biological pump refers to the efficiency with which the upward supplied limiting nutrient is biologically taken up and exported downward again.

P14071_L8-10: I don’t understand this sentence. Anthropogenic emissions are independent of atmospheric CO₂ concentration (unless the authors refer to mitigation policies based on threshold-control emission reductions, but it would be a bit out of context here).

We were referring here to the verification of CO₂ emissions using inversion methods based on the measurements of atmospheric CO₂ concentrations. The sentence has been altered to read: “Furthermore, accurate quantification of the net air–sea CO₂ fluxes in the CalCS is also becoming increasingly important in the context of CO₂ inversion studies that aim to verify the emissions of anthropogenic CO₂ in California through measurements of atmospheric CO₂.”

Fig.3: Taylor diagrams use the Pearson correlation because they require a correlation defined in terms of variance for the geometric relationship to hold. The Spearman correlation is non-parametric and based on rank correlation. I guess this is just a typo and the Pearson correlation was used.

Thank you for pointing this out. Indeed, we had mistakenly used the Spearman instead of the Pearson correlation coefficient. Please note that we completely redid the Taylor diagrams in Fig. 3 to account for this mistake and because of an additional error in the calculation of the subdomain areas. The values have been adapted accordingly in the text on model evaluation in Section 3.

References

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