Response to the reviewer comments

Please find in the following a point-to-point response to the reviewers' comments and the corresponding relevant changes in the manuscript.
All major changes in the text are marked in blue in the revised manuscript version.

Review #1
My only very minor comment is about the new paragraph on line 304-311. I found this paragraph a little confusing. Firstly, I think the authors meant "The diatom assemblages… show a strong association with…", rather than "strong association of…". Secondly, if there is a strong association between these parameters, then it's a little confusing that in the next sentence that they go on to say that not every change in d30SibSi "is mirrored by a change in diatom assemblage data and vice versa". I think it would be more appropriate to remove the word "strong" from the first sentence, to reduce the risk of misleading the reader.
The sentence has been changed according to the reviewer's suggestion and the word "strong" has been removed.

Review #2 (Patrick Rafter)
Summary
This is an improved manuscript and because I better understand the arguments and assumptions being made, I now have more comments to improve the manuscript.

First, I would like to see more elaboration on the origins of Fig. 2, especially since it includes new information calculations.
Regarding the confusion about in fig. 2: All data have been published already (as referenced throughout the text and as now highlighted in the figure caption). It is in fact only the calculations of the accumulation rates for BSi and TN that are new, which has now been emphasized more in the revised version of the manuscript.
Lines 156-174.

But most importantly the Discussion section needs a complete overhaul. Unfortunately, the new Discussion section is not significantly changed from the earlier manuscript. I think that much of the current Discussion text can be moved or in many cases removed entirely. To state this differently, the data and the application of the data is interesting and worthwhile, but the Discussion of the data can be much improved by discussing the results in the Discussion section. First, I would categorize most of the Discussion as unnecessary and / or Results section related text. Second, I would suggest how the interpretation of these results is consistent with theories and previous datasets about changes in ENSO variability over the last 600 years. I suggest a complete rewrite of this section with an emphasis on: (1) why these changes are consistent with ENSO and (2) the consistency of the implied changes in ENSO with other

We agree that the discussion of the changes in the source signatures, nutrient utilization and supply has indeed in parts been too descriptive and has now been significantly shortened. The description of "every wiggle" has been removed and only the important new observations are now discussed.

Regarding the second mayor point of the reviewer we added a new second sub-section to the discussion, in which we discuss our main findings in the context of past ENSO dynamics

determined off Peru and beyond. However, we were not able to follow the suggestion to discuss how the consistency with changes in ENSO compares with other datasets as there are no other datasets describing nutrient utilization or source signature changes. Instead we now point out that this is a new finding.

Line by line notes:

Line 47: remove "\"
"\" has been removed accordingly.

Line 48: confusing "material from the and…"
The sentence was changed to "..., investigations from the water column of the Southern Ocean did not find significant difference between the $\delta^{30}Si$ values of particles in the water column and in surface sediments..."

Line 62: Shouldn't this isotope effect or fractionation factor be negative if the other isotope effects (for uptake) are negative?
The reviewer is correct, we added a (-) accordingly.

Line 91: wrong tilde
The symbol was changed accordingly.

Line 118: this assumption of the depth of upwelled water is somewhat arbitrary, but I think it is ok. You could reference a study that has identified the depth of source waters.
It is known that upwelled waters at the Peruvian margin are mainly contributed by the PCUC which prevails at depth of 50-150 m. This is stated in the first sentence of the paragraph: "Along the Peruvian margin the main source for the high amounts of upwelled nutrients (30 µmol L^{-1} for both $Si(OH)_4$ and NO_3^-; Bruland et al., 2005) is the subsurface Peru-Chile Undercurrent (PCUC) flowing at depths of 50-150 m,..."

Line 121: It is here in the description of Fig. 2A that I realize that how this figure was made has not been described. Am I wrong in thinking that it uses the new data first shown in this manuscript? If so, it seems like the new data should not be included in the Introduction.
No, indeed all data, except the calculation of AR values for BSi and N, have been published before. This should be evident by the references in the text. A comment about the modification of previous figures has nevertheless been added to the figure caption.

Line 144: At this point I again realize that these figures (Fig. 2A, 2B, and 2C) seemingly are using new data that has yet to be introduced. Furthermore, while I think it is important for the reader to understand this spatial variability, the methods used to create these figures (even if they are from earlier work) should be described.
See comment to line 121. In addition, the calculations used to arrive at the values used in the figure are the same as for the downcore values described in the methods sections. We added a short comment corresponding to this fact: "The here calculated nutrient utilization for surface sediments is identical to the original publications (Fig. 2b; Mollier-Vogel et al., 2012; Ehlert et al., 2012)".

Line 147: The anaerobic oxidation of ammonia (Anammox) does not directly influence the concentration or isotopic value of nitrate.

"anammox" was removed from the sentence.

Section 2.5: I had a difficult time understanding this section and it was the second time I have reviewed this technique. I don't have a specific suggestion for editing this section, but I think the authors should take my difficulty into consideration. For example, could this be more easily explained using an illustration? Or an analogy? I'm simply suggesting that they should consider alternate approach for describing their methods here.
In response to these suggestions we added an illustration including the modern water column surface sediment data by modifying a scheme from Grasse et al. (2016). This illustration is showing modern values from 10°S which are used as an example within the nutrient utilization scheme. This scheme has now been altered to make it easier to understand. These are combined in the new Figure 3a-b. Furthermore, the text in section 2.5 was slightly altered corresponding to the adjustment to the figure.

Line 265: there is no Fig. 4E
The reference was changed to Fig 5b

Line 309: Doesn't this sentence need a callout to Fig. 5? The text confuses me because it is seemingly using the MAR as a proxy for upwelling strength (Fig. 3), but the text already quantifies nutrient supply in Fig. 5. These need to be considered together or the text should only use the Fig. 5 estimates.
We are not sure which paragraph the reviewer is referring to, as the text in line 309 discusses diatom species abundance in relation to upwelling strength. This paragraph has been added in response to a request of reviewer 1 to the previous version of the manuscript. As the mean diatom assemblages shifts are now shown in figure 6 as well, an additional reference to this figure has been added. However, there is still a reference to figure 4 (previous fig. 3), given that it is shows the detailed variation of the records and not only mean value calculations.

Line 317: needs a period instead of ;
";" has been changed to a period.

Line 317-318: This last part of the sentence is vague. "In phase" is another statistical term that should not be used to describe wiggles that look like they are going up and down at the same time. Statistics can prove me wrong.
The sentence has been removed in the new version of the manuscript.

Line 328: this sentence is confusing
The sentence has been rephrased to "The calculated NO_3^- utilization was higher during this time reaching values between 70 and 90%, while $Si(OH)_4$ utilization ranged between 6 and 60%. See line 351.

Line 330: remove lower
"lower" has been removed accordingly

Line 336: this is a great introductory paragraph for the Introduction! Gives the reader a good motivation for why the study is worthwhile. The beginning of the Discussion should be used to restate the question being addressed and point to the Results that improve our understanding. Also,

I am of the mind that time should move forward in the narrative of describing a time series. Beginning with the most recent events and moving back in time is awkward.
The paragraph was been slightly modified and moved to the introduction (now Lines 109-133). Furthermore, the narrative in the overhauled discussion has been restructured, starting with the LIA and moving forward in time.

Line 344: "latter" refers to the sentence above, but can also be misinterpreted / misread as "later" and should be removed.
"latter" has been removed from the sentence

Line 348: "This was inferred to result in" is awkward. Reword.
was reworded to "These changes resulted in "

Line 355: element
the "s" has been removed accordingly

Line 373: more reference to upwelling strength in Fig. 3. The estimate of nutrient supply rate is one of the cool, new things provided by this study. This should be the focus. Furthermore, it should be made clearer in the manuscript whether the new nutrient supply rate estimates were consistent with the sediment MAR. That is a new contribution to the field.
In the overhauled discussion more reference to upwelling strength based on diatom abundance as well as nutrient supply have now been provided. However, the nutrient supply is calculated based on the nutrient utilization and the MAR values. We therefore refrained from discussing the difference between supply and MAR values, as changes in the supply are ultimately dependent on changes in the MAR values.

Line 393: I though "correlation" was removed from the text?!!
Now it is and the word "correlation" has been changed to "comparison"

Line 401: There are a couple instances where the coretop and CWP values are used to estimate nitrate and silicate utilization at the surface. But how well do these compare with the observed modern values?
The core-top values are the modern values we refer to. Of course, there are estimates from the modern water column as well, but these reflect short snapshots of the prevailing conditions, which do not always represent the entire system. However, there is now additional reference to the water column evidence from Grasse et al. (2016) in the new figure 3.

Line 416: Almost all of this is a boring description of how the different proxies or metrics vary, which is not even altogether necessary in a Results section. It certainly does no good in a Discussion section.
Line 417: this reads to me like equating changes in source nitrate d15N with changes in nitrate utilization. It could happen, but need not be related.
Line 460 and 470: Another prime example (out of many) of text that belongs in a Results section.
This paragraphs/sentences have been completely rephrased in the new discussion.

Line 424-426: Despite there being no measurements, the reduction in denitrification predicts a lowering of nitrate d15N. Why can't we use this assumption here?

Of course, the reviewer is right and the $\delta^{15}N$ of nitrate would be lower during El Niño events, as it would be less elevated by denitrification and this is basically what we can show with our data. In the previous version we probably did not sufficiently emphasize this as the basic idea and we now included it in the overhauled discussion

Line 454: not “changes-in”
“-“ has been removed