Reviewer 1, comment 1: As mentioned in the text, 49 Ma corresponds with the possible onset of Northern Component Water. The 49-47 Ma gap in BN silica deposition is intriguing. Witkowski et al. (2020, Fig 5) show a 47 Ma end of widespread chert deposition in the deep North Atlantic. Why would biosilica deposition shift from the shelf to the deep sea? This is discussed more thoroughly in Witkowski et al. (2020), but I believe a review of this discussion is warranted.

Response: The 49-47 Ma interval lacks any data because of a prominent hiatus, as explained in the manuscript (line 282 in the revised submission). The actual timing of peak chert/porcellanite occurrences in the Atlantic presented in Witkowski et al. (2020, Palaeo3 556: 109896) is slightly different. Peak chert/porcellanite frequency coincides with the Early Eocene Climatic Optimum, from 53 to 49 Ma. Also, in the Palaeo3 paper referred to by the Reviewer, we show that both the siliceous sediments that underwent diagenesis to chert/porcellanite and those sediments that retain their original biosiliceous composition occur along continental margins. Further, neither in the Palaeo3 paper, nor in the present manuscript do we suggest a shift in biogenic silica deposition from the shelf to the deep-sea, or vice versa. Based on the high proportion of neritic diatoms in the Blake Nose siliceous plankton assemblages, however, in the Palaeo3 paper we proposed that neritic siliceous plankton production must have been higher than that in the pelagic settings. We assume that by "biosilica deposition shift from the shelf to the deep sea" the Reviewer may be alluding to the Paleo3 paper, showing a peak in biosiliceous sediments that follows an initial peak in chert/porcellanite in the Atlantic Ocean, automatically associating the preserved biosiliceous sediments with neritic environments. We do refer to the middle Eocene peak in biosiliceous sediments in lines 319, 326, 585. By "biosilica deposition shift from the shelf to the deep sea" the Reviewer may also be alluding to the widespread occurrence of diatom-rich sediments in pelagic settings from the Late Eocene onwards. We believe at least part of the reason for this may have been associated with diatom evolutionary events that took place through the Paleogene, including the radiation of holoplanktonic species, as discussed by Sims et al. (2006, Phycologia 45: 361-402). We refer to this interpretation of the Reviewer's comment in paragraph starting in line 610 in the revised submission.

Reviewer 1, comment 2: Figure 4 –Biosilca and CaCO3 flux is pulsed in the equatorial Pacific, as would be expected due to changing climate driven productivity gradients. Narrow vs. broad band of equatorial flux.

Response: The lack of a productivity gradient context in the discussion is referred to below, in the response to the Reviewer 1, comment 4.

Reviewer 1, comment 3: Line 407 –high diatom to radiolarian ratios in BN sediments. What about diatom to sponge spicule ratios? Were these done? Could sponge spicules contributed to biosilica percent.

Response: As explained in the text (lines 423-424 in the original submission), no attempts were made to quantify the contribution of sponge spicules to the reconstructed biogenic opal flux. The relevant paragraph is now strongly expanded, with relevant references to show that a substantial contribution of sponge spicule silica to total biogenic opal at Blake Nose is unlikely (lines 482-500 in the revised submission).

Reviewer 1, comment 4: Line 449 –diminished BN silica fluxes at 42-38 Ma –imply diminished nutrient supply? How about increased productivity gradients? Explained beginning of line 470

Response: We have provided clarification on this issue in 588-590 and lines 605-609 in the revised manuscript. We refrain, however, from using the phrase "productivity gradient" in the sense of a difference between ocean basins. In the literature, this exact phrase is usually applied to smaller scale gradients within a single basin.

Reviewer 1, comment 5: Line 78 –flat continental relief –Does this imply reduced rates of terrestrial runoff?

Response: Yes, Misra & Froelich (2012, Science 353: 818-823) and Froelich & Misra (2015, Oceanography 27: 36-49) invoke reduced rates of terrestrial runoff (due to inferred flat continental relief) to explain the broad low in lithium and strontium isotope records spanning the late Paleocene and early Eocene subepochs. A comment to clarify this is included in lines 111-112 in the revised manuscript.

Reviewer 2, comment 1: In general, it would be helpful for the authors to discuss in more detail the conditions in which diatom productivity is limited by silicon rather than another nutrient(s). As the authors point out, productivity can be limited by silicon supply from weathering, supply from circulation/upwelling, OR the (lack of) other nutrients. It would also be helpful to see a discussion of whether/how we know that diatom productivity is limited in the long term/large scale by silicon supply from weathering, rather than by a combination of these other factors.

Response: A paragraph in the introduction, starting in line 73 in the revised manuscript, is expanded to include an answer to this comment of Reviewer #2 (specifically, new text is introduced in lines 80-87).

Reviewer 2, comment 2: Figure 2. Please label/identify the fit lines (assuming that's what those are) in the figure caption.

Response: The heavy red lines represent smoothed opal concentration (Figure 2A) and opal flux (Figure 2B) curves. These were derived from LOESS regression. These are now identified directly on the figure (Panels A and B).

Reviewer 2, comment 3: Section 3.2 How significant are these correlative relationships given that the bioSiO2 fluxes are based on 1) smoothed sedimentation rates and 2) interpolated DBD measurements? I'm skeptical that these mean much; the authors could certainly make a case for why they do, but they haven't done so thus far.

Response: Our presentation of the opal fluxes follows the convention adopted by Piela et al. (2012, Paleoceanography 27: PA2204). As explained in the original manuscript, the reason for smoothing sedimentation rates was to eliminate unrealistic, high-amplitude variations in sedimentation rates between consecutive age model tie-points. The focus of the study is on long-term rather than short-term trends. We argue smoothed datasets are of sufficient resolution for these purposes (i.e. in accurately reconstructing long-term trends in opal accumulation). The robustness of this approach, despite being based on smoothed and interpolated data, is best demonstrated by the multiple regression model, which reproduces the smoothed composite opal flux curve reasonably well. Unfortunately, by mistake, we have not included the multiple regression model (line 325 in the original manuscript) output in Figure 2B, despite a reference being made in the text in line 327 in the original submission. This omission is rectified in the revised version of the manuscript. A comment on this issue is also included in lines 375-376 in the revised manuscript.

Reviewer 2, comment 4: Figure 3. These axis labels won't make sense to many readers – what do the numbers represent?

Response: This issue has been resolved, and the labels now include units. We have also noticed a mistake in labelling of the axes in all three panels. This is now corrected.

Reviewer 2, comment 5: Figure 4. Some of these colors are quite hard to see, especially the light yellows and purples. There are a lot of different things to distinguish, but perhaps these can be a bit darker, or changed for different colors?

Response: The colors in Figure 4 are now changed, as requested by Reviewer #2.

Reviewer 2, comment 6: Lines 460-465 The previous paragraph attributes some flux changes to changes in preservation of bioSiO2. Can that be ruled out, or at least shown to be insignificant as it relates to the changes described in this paragraph and the next? If so, the authors should make that case, and if not, should discuss the implications for their interpretations.

Response: This comment is related to comment 3 by Reviewer #1. Preservational issues are more emphasized in the revised version of the manuscript, especially in an extended version of one paragraph in the discussion, specifically lines 482-500.

Reviewer 2, comment 7: Last paragraph of section 4.3 This last sentence highlights the difficulty of using bioSiO2 flux to test the effects of the weathering feedback – changes in flux are just as likely to reflect a change in ocean circulation as they are the weathering feedback. Could the authors try to disentangle these things a little more in this last section?

Response: Although in our final response to reviewers we declared a comparison to δ^{30}Si data from Fontorbe et al. (2016; Earth Planet. Sci. Lett., 453, 67-77) would be included in the revised manuscript, we eventually decided such comparison would merit a separate paper, with new isotopic data. Instead, we express that δ^{30}Si, paleocirculation proxies such as εNd, and more sophisticated tools for resolving sedimentation rates (orbitally tuned cyclostratigraphic age models) applied to opal flux data from other regions of the world's oceans have the potential for building a more comprehensive picture of opal accumulation through the Paleogene. With such comparison available, a more reasonable attempt could be made to deconvolve paleocirculation, weathering, preservation, and production signals from existing records, like the one presented here. All this is expressed in the final paragraph added to the conclusions, lines 682-691 in the revised manuscript.

List of changes

In the list below, we do not explain minor adjustments of style or correction of typos. Only the significant changes are indicated and discussed below.

Abstract

We have made some improvements to the flow of the abstract. Neither of these are significant changes.

Lines 80-88

This change is made in response to Reviewer #2 comment #1.

Lines 111-112

This change is made in response to Reviewer #1 comment #5.

Figure 2

The figure was replaced with a version that includes multiple regression model output (Panel B), and identifies LOESS roegressions in both Panels A and B. Both changes are made specifically to answer comments 2 and 3 from Reviewer #2.

Lines 375-376

This change is made in response to Reviewer #2 comment #3.

Figure 3
The figure was replaced with a version that has clearly labelled axes, including units. This issue was raised by Reviewer #2 in her comment 4. During the process of revising the manuscript, we realized that the version of Figure 3 included in the original submission included several other mistakes concerning axis labelling. All these issues are rectified in the present version.

Figure 4
The figure was replaced with a version that has higher contrast between the various colors used. This change was made in response to the request of Reviewer #2 (comment 5).

Lines 482-500:
This new paragraph is inserted as an answer to Reviewer #1 comment 3 and Reviewer #2 comment #6.

Lines 588-590:
These changes are made in response to Reviewer #1 comments #2 and #4.

Line 597:
This change is made in response to Reviewer #1 comments #2 and #4.

Lines 605-609:
These changes are made in response to Reviewer #1 comments #2 and #4.

Lines 611-614:
This change is made in response to Reviewer #1 comment #1.

Lines 678-680:
This change is made to better emphasize our interpretation regarding re-invigorated Northern Component Flow export.

Lines 682-691:
This paragraph is inserted in response to Reviewer #2 comment #7.

References:
Eight new references are added to the list.