We would again like to thank Dr. Patricia Grasse for their thorough review of our manuscript and their helpful comments. We believe that we can address all of the major comments indicated by Dr. Grasse as indicated in the discussion below. Note that the italicized text represents the comments made by Dr. Grasse and non-italicized/bold text is our response.

P1 L12: I think the information that low DSI samples could not be measured does not necessarily need to be in the Abstract. I think it would be more important that this information is mentioned in the methods or results part. **We will make the changes recommended by the reviewer.**

P3 L20: Mesh size of the AG 1 X8 resin? **We will add the text "(100-200 mesh size)"**

P4 L20: Please check the equation. Shouldn't it be 26/24 Mg or 25/24Mg? **No, it does not matter if we use 25/26, 26/24, or 25/24 since this is used as a technique to normalize the data. The ratios should be constant and consistent and any fractionation observed is due to the effects of mass spectrometry. This allows us then to normalize the silicon isotope data.**

*P5 L15: It seems the manuscript Garcia-Inánez et al. is already accepted***. This was not the case at the time of submission and will be changed.**

P6 L5: Please include one sentence about the DSI concentrations in the upper 500 m and that the samples could not be analyzed. **We will make the changes recommended by the reviewer.**

P6 L8: I would add some information to describe the DSi and δ30dSi in more detail. E.g. St. 1 and St. 13 already increase at 1000 m depth. **We will make the changes recommended by the reviewer.**

P6 L18: Please give the exact values of the lowest δ30dSi (0.95 ‰ to 0.98 ‰. I think these very low δ30dSi values are actually quite interesting and need some more attention. See my comments below. **We will make the changes recommended by the reviewer.**

P7 L13: The constant offset between the data set from Brzezinski and Jones and de Souza et al., must be indeed a measurement artifact. However, the offset between your data set and de Souza is partly bigger than 0.2 ‰ and samples of the GEOVIDE section do not seem to plot on a straight line between DSi and δ30dSi. It looks that the data clusters as some samples show a wide range in δ 30dSi (1 ‰ to 1.7 C2 ‰ at nearly similar concentrations (approximately 40 µmol) and at low DSi (appr. 12 µmol) where δ30dSi ranges from 1.25 ‰ to 1.7 ‰. That could indicate that other sources or processes influence the waters of your study compared to the open ocean stations in de Souza at al., and Brzezinski and Jones. I think it would be helpful to modify figure 4. First of all, you should make your data more visible (e.g., bring your data to the front, use a light color for the already published data). You could try to group your data. e.g., only use open ocean stations vs. stations close to landmasses. Colorcode the stations or *samples that are characterized by specific water masses. It would also be helpful to add water mass end members, e.g., AABW, which brings a light source from the south 1.2 ‰ (0.01 DSi; Souza et al. 2012). That could show additional processes that influence your deep-water masses e.g. at St. 1 and St. 13. Generally, I think it is interesting, that you see such light δ30dSi values and it should be discussed in more detail. According to your intercalibration with de Souza et al. (Fig. 6) and your results from the intercalibration study Grasse et al. (2017) your δ 30dSi data agrees very well within error (0.1 ‰ 2sd). Therefore, a water sample of 1 ‰ together with slightly higher DSi compared to de Souza et al., might indicate that further remineralization influences the δ30dSi composition. Such low (or even lower, 0.6 ‰ values are typicallyassociated with much higher DSi of 130 to 150 micromol in the Pacific and (Reynolds et al.2006, de Souza et al., 2012, Grasse et al., 2013) at DSi concentrations (even though I know that some people doubt some of the δ 30dSi deep water values in the North Pacific). However, Grasse et al. 2016 observed δ30dSi values of 1.1 ‰ in bottom water of the Peruvian shelf (â Li j40 micromol), which were influenced by pore waters from the sediment and remineralization at the sediment-seawater interface (Ehlert et al., 2016). Not necessary an effect you observe, but if not dissolution at the seawater-sediment interface or in the water column influences your δ 30dSi, you could also have admixture with a distinct water mass that brings in a very light δ 30dSi signature (e.g., a water masses from Iceland? I am not so familiar with the water mass circulation in the Atlantic, but it seems that the NEADW can pick up its signature here?). Additionally ,the circulation is quite sluggish, or? Therefore, you can have a trapping effect? I do not C3 want, that you go too much into detail into the Pacific seawater δ 30dSi distribution and I also see that some of the values are identical within error, but I would like to have a better explanation why not all of your data does fall on the line for DSi versus δ 30dSi*.

Thank you for bringing up these important points, we will make the necessary changes to make this information clearer. I agree that we can provide more discussion here, and I like your suggestions. Due to the formation history of NEADW, we can pick up the light 30dSi signature from water masses from Iceland, since NEADW derives from ISOW. We briefly touched on this in the manuscript (p5 L31-p6 L2): "NEADW is formed as a result of entrainment events that occur along the journey of ISOW through the Iceland Basin (van Aken, 2000). NEADW recirculates in the West European Basin and mixes with the surrounding waters, including the Antarctic Bottom Water (AABW) (van Aken and Becker, 1996), resulting in the formation of LDW". We can expand in further detail again in the discussion. Also, the circulation within the West European Basin is slow, and NEADW is a relatively old water mass (see attached paper Fleischmann et al., 2001).

In addition, after I received the reviews, I noticed that one data point was missing from the supplementary table (although had been included in Fig.4) and that several data points were missing from Fig. 4 for the depth range between 1000-1500m. These data were available in the supplementary table, but I will now include them in the new Fig. 4.

P7 L8: Please give the values (low, high) for the study by de Souza et al. **We will make the changes recommended by the reviewer.**

P7 L25: please mention here (or at least above) the absolute δ30dSi values from the study of de Souza et al. for comparison with your values. The range can be similar, but that does not necessarily mean, that the δ30dSi are identical. **We will make the changes recommended by the reviewer.**

P10 L8: Please also explain, why the uppermost sample at station 26 has such high δ30dSi. **We will make the changes recommended by the reviewer.**

P11 L5: Please mention the stations you are talking about. High δ30dSi? Value? What values? P11 L10: What are the δ 30dSi values in the Labrador Sea? Please make clear that it is subducted surface water. **We will make the changes recommended by the reviewer.**

P11 L24: Please give me the station number and depth that makes it much easier to follow and understand your discussion. **We will make the changes recommended by the reviewer.**

P11 L25 Doesn't NEADW has high DSi? Here I am getting confused, isn't the NEADW influencing the eastern deep waters? At least according to Fig 4. in Garcia-Ibanez et al.? Please check the Garcia-Ibanez paper for water masses; it seems that there are some discrepancies, most likely as a result of the review process of the manuscript. **As mentioned in the response to reviewers (and in the text), NEADW recirculates in the West European Basin and mixes with the surrounding waters, including the Antarctic Bottom Water (AABW) (van Aken and Becker, 1996), resulting in the formation of LDW. The concentration of DSi in the NEADW is. NEADW is the dominant water mass for the deep Eastern area of OVIDE. As answered in the previous comment, NEADW is derived from ISOW so it is consistent to have similar d30Si in both ISOW and NEADW.**

Fig.2 I do not think that the Figures has to be in the Paper. In my opinion, it is enough to mention in the text, that all samples fall on the mass-dependent fractionation line. **We will make the changes recommended by the reviewer and remove Fig. 2.**

Fig4: Can you please adjust the y-scale from 0.5 ‰ to 2 ‰ Please add the studies indicated by different color directly to the legend. Would be good to modify the figure (see comments above) C4 **We will make the changes recommended by the reviewer.**

Fig. 5: It is quite tricky to distinguish the colors of different water mass types. You could only name the dominant water mass in the figure. Similar to Garcia-Ibanez et al. (Figure 4). Can you replace section distance with longitude? **We will make the changes recommended by the reviewer.**