Dear editor,

We would like to thank both referees for their constructive and helpful comments on our manuscript. We detail below point by point how we plan to revise the article for publication in Biogeosciences.

Anonymous Referee #1

Page 6335, Lines 18-20 reference Figure 1 which does not show these transects discussed here.

Since the KEOPS-2 transects have little meaning in our study because we sampled only the process stations we will remove the reference to transects in the text.

- Section 2.2 - adding 100% DSi of the 30Si tracer sounds like a recipe for horrible memory effects in the mass spec. I understand why this was necessary (i.e. RhoDISS measurements, especially given the high ratios of DSi:BSi and the thermal conditions restricting the physicochemical and bacterial-enhanced silica dissolution rates) but please comment on why memory effect wasn't an issue?

We will add the following information in the methods §2.3 "sample preparation and isotopic measurements": The sequence of analysis consists in: blank –natural standard – spiked sample 1 – natural standard– spiked sample 2 - natural standard – spiked sample 3 – natural standard - blank. The average of two blanks were subtracted to each standard and samples. To test if our dissolution measurements were bias by a ^{30}*Si contamination linked to an eventual memory effect in the HR-ICP-MS, we compared the average composition of first natural standard (without potential contamination from memory effect, n=55) with composition of natural standards analysed after a spiked sample (n=102) and see that there is no significant difference between natural standards passed before and after a 100% DSi spiked sample (T-test, p-value < 0.001).*

- Section 2.3 - please be more clear in the topic sentences of the first and second paragraphs that you are referring to the dissolution (i.e. DSi) and production (i.e. bSi) samples, respectively.

We will add reference to dissolution and production where necessary.

Results - Section 3.1 - Using the "æ" symbol is distracting, please stick to the conventions used in the previous literature you cite (for example, use Fripiat et al. 2009).

Unfortunately there is no universal convention on this aspect. While it is true that Fripiat et al. (2009) use "atom %", the "æ" symbol (meaning abundance in excess) is used by Elskens et al. (2007), Fripiat et al. (2011). Since "æ" is much shorter than Atom% we prefer keeping this writting.

Also, since you are using the 2-compartment model, why even discuss the Nelson and Goering model? It has been discussed elsewhere (e.g. Fripiat et al. 2011b, Elskens et al. 2007) so this one-compartment model discussion isn't necessary.

We agree, we will remove the equations (1) and (2) and associated lines 12 – 24 p. 6338.

- page 6340 - Since ICPMS is rarely used for silica dissolution (until Fripiat's method in 2009) please indicate your internal standard, this would be useful to the community.

We realise that the term "internal standard" we used in the ms might be confusing. In the revised version we will change it to "in house standard". We will mention that this in-house standard is a 10% spiked seawater from Southern Ocean analysed since several years with a æDSi at 11.83 ± 0.43

% (n = 40).

- Section 3.3 - if F station was limited by Fe, the trend in RhoProd with depth (i.e. strongly coupled to euphotic zone) fits very well with previous observation in the HNLC equatorial Pacific for RhoProd (perhaps a consistent feature in such regions).

This is an appealing comparison. However according to measurements of dFe at F station (Quéroue et al., this issue), this station is Fe-enriched likely via and/or across the Polar Front jet. We will add anyway a comparison with Krause et al. (2011) at the beginning of § 3.3 when we highlight the specific profile of F.

- Page 6342 line 25: "integrated rhoDISS did not vary over depth" this quantity is already integrated (no vertical variability) so I presume just "RhoDiss" was meant.

This is exact, we will remove the symbol "integrate" as suggested.

- Page 6343: Here you discuss figure 5; however, this figure is redundant, and therefore unnecessary, considering all this data is already reported in Table 2 and Table 3.

We agree we have the same information but displayed in different ways. We believe figure 5 provides a significant added value since we have an easy view of all stations for the main parameters (production rate, dissolution rate and dissolution to production ratio). In the discussion we also often rely on Tables 2 and 3 for specific values, so we would prefer keeping these tables and figure as it is currently.

- Page 6344, line 2: integrated VSi is a meaningless value (i.e. units of d-1 integrated over euphotic zone meters?). Do you mean that this integrated value was an average of all VSi or VDiss in the euphotic zone? Was the integrated Rho divided by the integrated BSi? Or were the VSi or VDiss values integrated to the depth of the euphotic zone and then divided by the depth of the euphotic zone to make the appropriate d-1 units? Please clarify.

We will mention that our integrated VSi are calculated by the average integrated ρ divided by integrated BSi and we will put the expression "integrated VSi" with quotes. We will change the column header and legend of Table 2 accordingly.

- Page 6344, line 4-5: if VSi is <0.1 d-1, this may be an artifact of siliceous detritus, which is important in other HNLC regions (e.g. Krause et al. 2010, L&O).

We will add the following at the end of sentence line 5 p. 6344 "(…) suggesting non-optimal conditions for the growth of diatoms and/or an artifact of siliceous detritus, which is important in other HNLC regions (e.g. Krause et al. 2010, Fripiat et al., 2011)".

- Page 6349 - line 2 - please correct spelling (Amazone to Amazon)

We will change "Amazone" to "Amazon"

- Page 6350-51 - 6.2 uM could potentially induce some degree of Si kinetic limitation (i.e. V at ambient DSi < Vmax), were the kinetic measurements done using this 30Si tracer instead of 32Si.

We already mentioned lines 26-28 p. 6350 that we do not have Si limitation from kinetic experiments performed during the cruise. In the revised version we will add that "V at ambient DSi were always similar to Vmax at all stations (data not shown)".

- Section 4.2.4 - while this is interesting, especially the implications for the Si leak hypothesis, ultimately, this is much weaker than the rest of the manuscript. For instance, a paleo- extrapolation is made by comparing one profile from this study in each of the plateau and HNLC regions, and even with the n = 3 for A3 reported by Mosseri et al., this seems like a considerable over-extrapolation given such limited data. Plus, the presence of the plateau (which is responsible for the natural Fe fertilization) also drives considerable differences in what types of physical mechanisms may alter local biogeochemistry (e.g. strong internal wave activity) relative to the open-water regions of the So. Ocean. Ultimately, all the discussion of decoupling between Si and C or N uptake seems to lead up to this section. Given the shaky ground, it may be worthwhile to tone down (also make more concise) most of section 4.2, especially since this line of discussion isn't one of the stated objectives in the introduction.

As suggested we will moderate this section and try to make it more concise in the revised version. We will also highlight the uncertainties associated to this discussion and the need of further measurements to confirm it.

- Page 6351, line 20-22 - the coupling of Si production with light has also be observed in the equatorial Pacific and north Pacific Subtropical gyre (from strong coupling of Si metabolism to Fe or N metabolism in each region, respectively).

We will refer to Krause et al. 2011 for comparison with Pacific Subtropical gyre.

Anonymous Referee #2

General comment

Closset *et al*. present a study investigating the seasonal evolution of net and regenerated silica production in the Indian sector of the Southern Ocean (KEOPS study). The manuscript is well presented, and the results are interesting. I only have a few comments, and I recommend publication of this manuscript after minor revisions.

Specific comments

Throughout the manuscript silica production and dissolution rates are defined as either P and D (abstract), or ρSi and ρDiss (Eq.1 and Eq.2, pages 10 for example). The authors should une only one of these acronyms to help the reader.

We will homogenise the spelling using ρSi and ρDiss.

Page 11-line 15-19 **:** The authors discuss the relevance of the 2 models and say that the accuracy and the sensitivity of each model was tested. the authors could mention what type of procedure was used to select the model (was it an Akaike information criterion or something similar ?).

In accordance with referee # 1 we have removed reference to model 1. For the two-compartmental model we will mention that it was tested according to the four criteria and check that residual of the cost function follow a chi2 distribution as detailed in Elskens et al. (2007).

Page 18 – and figure 6, page 51 **:** If I understand correctly, net production rates are calculated from the difference ʃρSi - ʃρDiss (x-axis). If so, I'm not sure that the authors can plot (1-ʃρSi:ʃρDiss) as a function of (ʃρSi - ʃρDiss), because the two variables are not independent.
For example, if we use a random set of values for ʃρSi and ʃρDiss (consistent with the range of values

listed in Table 2 : from 3.09 to 47.9 mmol m-2 d-1 for ρSi and from 4.50 to 9.99 mmol m-2 d-1 for ∫ρDiss), we can generate the same hyperbolic curve (Figure A below); the hyperbolic curve shown on Fig.6 might be an artefact. If the authors want to use Fig. 6, they should discuss this point (variable X and Y which are not independent).

It is true that the variables are not independent. We will state in the revised version that because of this, the data fall on a hyperbolic trend as expected. However the figure provides useful information to compare with other sites and previous studies. For instance the figure clearly shows that KEOPS-2 stations "encompass almost the full range of variability observed in very contrasting oceanic regions (e.g. HNLC, oligotrophic, coastal upwelling, river plume) ". More importantly it allows to differentiate stations at low net production rates (x-axis) that have very variable fraction of new Si production (y-axis). We will anyway discuss this dependence effect and we will moderate the validity of our quantitative estimates based on this plot (e.g. $K\rho_{net}$ *of 5.89).*

Page 25 – line 9-10 **:** although the data on phytoplankton taxonomy are not shown, it would be interesting to know how the ∫ρSi:∫ρN ratios change as a function of the proportion of diatoms in the phytoplankton community.

This is a good suggestion and we will add some discussion on the ∫ρSi:∫ρN ratios as a function of recent data of Lasbleiz et al., this volume, which provide pigment and phytoplankton size fractions.

Page 26 – section 4.2.4, line 17-24**:** The authors attribute the unexpected observation (higher ∫ρSi:∫ρN ratios under iron replete conditions, and lower ∫ρSi:∫ρN ratios at station E3) to the dominance of different diatoms populations. Here, it would be interesting to have some species names. Are the high ∫ρSi:∫ρN correlated to the dominance of some specific genus or species of diatoms ? Are these species commonly found and preserved in the sediment?

Because these data are not available yet, we cannot discuss this interesting point. However, the composition of diatom population at KEOPS-2 stations will be investigated in detail, and this discussion will be likely part of a future paper.