**General Comments**

This manuscript presents new data from a sediment core located close to the Drake Passage. These data allow the authors to discuss lithogenic fluxes to their core site and their influence (as bioavailable Fe) in shaping the character and magnitude of export production in the Subantarctic region of the Southern Ocean, over multiple glacial-interglacial cycles. One innovative component is the fact that all their Mass Accumulation Rates are Th-corrected, which is a great practice as it takes into account focussing/winnowing effects, besides allowing the authors to provide insights on current dynamics, when coupled, as they do, with other methods (i.e., sortable silts). Another important aspect of this study is the comparison to a host of other records available from the different Sectors of the Southern Ocean, allowing the reader to get a circum-antarctic perspective of export production variability through time. Methods are valid and results presented in a very clear, easy to follow style, with appropriate referencing of related work. The paper is nicely written, logically structured and figures and tables are both of good quality and appropriate.

**Specific Comments**

Lines 44-46: Fronts as barriers: In principle, yes, but mesoscale dynamics/eddy fields allow a certain level of mixing across fronts.

Lines 159-160: “For tuning, we assumed that low Fe contents characterize interglacial periods, whereas high contents represent glacials”. Given how this paper is specifically addressing issues related to Fe-depleted/Fe-replete conditions, it would be good to provide a bit more information on how much (or how little) the tuning distorts the preliminary age model. Or, at least, a more in-depth mention of the supporting evidence for this choice (rationale behind it, and support from other measurements, like carbonate, Ca counts, etc).

Lines 176-177: “For terrigenous contents exceeding 75%, opal concentrations measured at UdeC are consistently 3–5% higher than those measured at AWI”. Even if the subsequent mention of similar patterns of variability is right on point, it would be useful to mention the potential reasons for such systematic difference.

Line 208: Does the relatively lower match (compared to Fe and Ca) between XRF and bulk sediment measurements for Ba relate entirely to the two main mentioned uncertainties (Ba as marine barite and assumed constant Ba/Ti ratio for terrigenous components), or are there other factors potentially affecting this deviation?

Line 246: Ba excess higher during interglacials: interestingly, mainly true for MIS 5, 7, 9, not so much for either MIS 1 or 11… So that peculiar pattern might have a reason behind it.

Line 250: Carbonate and Ba excess BMAR varying in parallel to their respective percentages. True in general, but different pattern (for both of them) during MIS5e compared to MIS 5a-d. Which, again, might be a peculiarity worth exploring.

Legend to Figure 3: The mention of export production is a bit misleading: these are burial rates in the sediment, as derived by MARs…. By their nature, compared to export production, they integrate the effect of dissolution and remobilization (focussing/winnowing) through the water column and at the water/sediment interface. In the same legend: how the error envelopes were derived should be mentioned somewhere in the text under Methods.

Lines 276 and 279: From MIS 7… From mid-MIS 9… To a certain extent, both of these expressions do not really indicate the timing of initiation of a clear pattern but have more to do with the availability of data dense enough as to recognize that pattern in the first place.

Line 368: "and strong similarities between the dust peaks in Antarctica and the advances of the PIS". Just as you did for the IRD record, mention explicitly why this observation is relevant to the glaciation status of PIS and its role in increased lithogenic material input to your core site during glacials. You mentioned this in general terms in lines 358-359. This is an important point to clarify also in light of your statements on the EDC dust record being mainly linked to eolian inputs… essentially re-iterating that enhanced glacial conditions in PIS lead to increased terrestrial input to the adjoining ocean. Or you may decide that it is not necessary to repeat this again here, as you describe exactly how this this interplay works when talking about bioavailable iron in lines 378-380.

Line 408: "all being lower during full interglacials (Holocene, MIS 5 and MIS 11". Two remarks: You do provide a PAGES statement for this statement, but usually MIS 7 and 9 are considered to be full interglacials as well… Maybe one way to avoid the diatribe of what is a full interglacial and what is not, one could rephrase this to "being lower during some interglacials…". Second remark: the patterns are difficult to argue for MIS 11 as based on just 2 datapoints… While the statement might still be ok for Holocene and MIS 5, it definitely isn't true for Baxs during MIS 11, where the measured value is actually the highest on your record.

Lines 412-413: “Since the preservation of organic carbon in sediments is globally scarce (about 1%”. Please specify what this 1% refers to: is it the percentage of TOC produced in the water column that gets preserved in sediments, or is it (a sort of globally-averaged) percent content of TOC in sediments? I guess the former, but not sure.

Line 419: “diatom growth”. Not 100% sure growth is the right word here… could “diatom frustule multiplication” or “diatom blooms” be better? Also, I think the sentence does not read right… suggestion “… that diatom frustule multiplication was more effective during the LGM compared to today (resulting in higher opal burial during the LGM), as diatoms reduce their Si/C…”

Lines 434-435: In connection to what discussed in my comment to line 408 (your/PAGES statement about “full interglacials”, excluding MIS 7 and 9), it would probably be interesting to mention how those two interglacials did not seem to manage to “offset” carbonate
dissolution as all other interglacials did in your record…

Lines 512-516: Besides the mechanisms you propose in this section, an additional one could be mentioned (besides frontal, and accompanying nutrient fields, movements and increased Fe supply) as playing a role in pushing/supporting the glacial system towards increased opal productivity during glacials: the better Si/N utilization by diatoms under Fe-replete conditions (that you mentioned elsewhere, with references to Brzezinski&al and Frank&al), which would also provide a larger available reservoir of dissolved silicon compared to interglacial conditions. Yes, the system eventually may run out of dissolved silicon (and thus get into Si-limited conditions, just as you hypothesize), but siliceous producers are at least set to succeed at the start of it all… Boom&Bust at its best… something diatoms are really good at.

Figure 6 and Subsection 5.3.3. in general: the description and interpretation of the data sounds fair and accurate enough, especially when it comes to relative increases/decreases of the various components during glacials and interglacials (i.e., the large-scale features and implications of the records). Nuances in these patterns, and variability between the different sectors, might be however a bit trickier to explain properly as, besides the large overestimation for some fluxes during glacials (thay you nicely presented and argued for, based on the Th corrections), the oceanographic significance of each station is a lot more heterogeneous than simply being representative of “subantarctic conditions” (see Fig. 6a, b). This might not be that drastic for dissolved silicon, as the vast majority of the stations have near zero values, with very few exceptions, but dissolved nitrate is highly variable across the sites, going from just above zero micromole/litre at the top of the SE

Atlantic transect, to almost 30 micromole/litre for the Pacific Sector and the easternmost station in the Indian Sector.

Lines 490-492. The discussion about the productivity response not scaling up to the magnitude of the lithogenic input shift…. I have two related comments to this: it does not necessarily need to scale in the same way… it takes very little additional bioavailable Iron to get out of Fe-limited conditions (two nanomoles or thereabouts?), so any additional lithogenic input, regardless of its source, above and beyond the one required to reach such concentrations in the surface waters, will not change the deplete/replete status of the water column and hence the main characteristics of the productivity system associated with those waters. My other comment is that maybe the main peculiarity of your site (main source of Fe coming from PIS as lithogenic input, not via eolian pathways as elsewhere… and in general the very close proximity to such source and land) plays a big role in this: this would presumably make it easier (and faster) to switch from an Fe-deplete to Fe-replete conditions, thus going very quickly into the "saturated" state I was mentioning above: regardless how much more Fe gets dumped in there, it won't make much of a difference anymore at some point.

**Technical Correction**

Line 32: as well as with a decrease

Lines 48-49: I would change this sentence around to "The Drake Passage (DP), located between the southern tip of South America and the Antarctic Peninsula, is a major constriction for the ACC flow and SO fronts". I would call it "a" constriction, as plateaux and gaps between ridges also exert quite strong disturbances to ACC flow.

Line 51: World Ocean. In the same line, before mentioning its low efficiency, briefly describe what the biologic pump is.

Line 67: Why "the last"? …explaining 30 to 50 ppm of atmospheric $CO_2$ drawdown…

Line 89: their link

Line 100: as a few lines below you are writing about CDW and carbonate undersaturation, it would be useful to mention the site water depth here (3781m, mentioned in line 149).

Line 112: … between silicate-poor waters to the north and silicate-rich waters to the south of it…

Line 119: phosphorus

Line 124-126: Probably perfectly fine as written, but slightly unclear whether the substantial modern dust contribution is also "excluded" or not. Please make it clearer by either switching the two sentences "… have demonstrated a substantial…. and excluded a westward…" or "… have excluded both a westward… and a substantial…".

Lines 131-132: During last glacial, the distance between core PS97/093-2 and the PIS (situated at ~56°S at that time, Glasser et al., 2008) … or: reaching as far south as ~56°S at that time

Legend to Figure 1, last line: remove this "Schlitzer, Reiner, Ocean Data View, odv.awi.de, 2021" as it is the full reference for Schlitzer, 2021.

Line 156: was developed by Toyos et al. (2020) using a two-step approach

Line 164: and homogenized sediment

Line 172: are expressed as biogenic opal percent by

Line 235: Is there something missing in this sentence: "…were calculated by dividing the average 232Th concentration"? Maybe something like:  "… by dividing the lithogenic material concentration by the average…"?

Lines 250-255: As you did elsewhere, please mention in the appropriate place the panel/letter, not just Figure 2, as it is sometimes difficult to understand which pattern/curve is being described, especially when you write about "other BMARs".

Legend to Figure 2: TOC and Ba excess have been swapped.

Legend to Figure 4: Would it be useful to specify what the MAR in panel d) refers to in this case? Sediment MAR, Bulk Sediment MAR, or a similar expression.

Lines 325-328: I found these lines difficult to follow… Yes, I do get that they are an explanation of why winnowing, due to cohesive effects, seems to (counter-intuitively) occur during slower current speed intervals, but the period doesn't read properly. Probably this is due to a very long sentence having been split in two, starting with "Since". How about rewording to something like "We suggest this is due to the fact that under strong…., while under slower…."?

Figure 5: Core site PS75/76, and even more so PS75/56, are not really located in Australia/Oceania… Yes, they might receive the far end of dust plumes from Australia but are really in the central South Pacific.

Line 354: "those reconstructions from"… replace with "those observed in", to avoid the more convoluted "those reconstructed for open Pacific… locations/core sites".

Line 356: Thus, in order to account for these very high lithogenic fluxes in the proximity of the DP, an additional non-eolian source of terrigenous material is required.

Line 451: Something is a bit strange in this passage "upwelled Fe south of the PF, because of in the hypothetical case"

Line 459: make the cause/effect relationship clearer/more explicit: "… may be the result of a reduced diatom Si:N…"

Line 466: suggests

Line 467: significantly the integrated

Line 549: leading to calcareous plankton (i.e., coccolithophores) becoming the

Line 551: was perceptible, as demonstrated by the occurrence of a prominent calcareous ooze

Table S1 legend. normalized MARs of export production proxies