Response to review by Alan Aitken

Prof. Aitken's comments are in black, our response is in blue.

This manuscript provides a numerical approach explicitly to connect offshore sedimentary provenance records in glacial sediments with their source regions onshore, accounting for variation in sediment productivity and transport. The sediment transport problem is significant as it underpins our knowledge of cryosphere in past climate and therefore guides our ability to interpret past sea level and predict future sea level.

This problem has been approached with a range of techniques from educated guesswork to proximity studies and probabilistic assessments, and spatial modelling of individual parts of the transport system (e.g. subglacial erosion and sediment transport or ocean transport) but has not been comprehensively tackled from source to sink as is done here. As such the approach presents a unique addition to the ability to model such systems in their totality.

We thank Prof. Aitken for his detailed and useful review and are glad he recognises the uniqueness and value of TASP in trying to simulate the entirety of the system.

I have several key comments

1 - The paper is not written in the best way for the journal. I would advise a general rewrite with a stronger focus on the new approach, and less on the case-at-hand in which the authors at times get bogged down in details of the case-study and lose sight of the main goal for GMD (and also for uptake of the approach) which is to focus on the approach, its capacity and its veracity.

We extensively restructure the manuscript, removing all mention of the Antarctic Nd isotope case study from the Introduction (Section 1) and Model description (Section 2). We feel this helps prevent the manuscript getting too bogged down in case-specific detail by providing solely a description of how the model works before mentioning specifics.

2 - The degree of case-specific choices in the model is higher than I expected and I am concerned that this might limit the broader application that could make this a truly useful tool (see detailed comments). For example it is not clear if this model could, or could not be applied easily to Greenland or a model from the Pliocene. For GMD I think a more generic standpoint is needed. A simple synthetic model test case might add a lot if the authors can do so.

TASP was designed specifically to understand provenance signatures around Antarctica. Application to Greenland (or palaeo northern hemisphere ice sheets) would, in theory, be possible. However, some edits to the code would likely be required to account for the different geographic setting. Furthermore, Greenland Ice Sheets may have been mostly land-terminating during past interglacials. TASP focuses on marine processes with no representation of fluvial systems, so is not well-placed to predict the provenance signature of such an ice sheet. We now include a mention of the applicability of TASP to other ice sheets on lines 528-531.

TASP is readily applied to past Antarctic ice sheet simulations, as (alongside spatial coordinates) it only requires bed elevation, basal shear stress and basal ice velocities with horizontal directional components from the ice sheet model. There is an option ("palaeo") which, if selected, means TASP does not compare the results of the provenance tracing to

seafloor surface sediments and instead outputs just a predicted map of the provenance tracer. This is now explained in lines 499-501.

We appreciate the value that a synthetic test case would add, so include an example for the terrestrial component. We argue that constructing a synthetic model test for the marine component is less valuable given that it would be difficult to impose useful artificial ocean velocities, so opt to instead move former figure 8 (new figure 4) earlier in the manuscript, as this is useful for showing that offshore transport by the surface current method produces a sensible result with the amount of sediment from a particular sector diminishing with distance.

3 - I have some concerns about the deterministic nature of the approach and the large number of choices that are necessary for it to function. Several variables and assumptions are tested for impact and others are tuned to fit the data but the inter-relationships of parameters is not defined. In between all these moving parts there is overall a low chance that an optimal solution is found - indeed the Nd data is fitted somewhat better than a proximity-based approach but this does not indicate a minimum was found. For the paper I think a clear comment on the potential for unchecked errors to propagate through the model will suffice, but I would encourage the authors to pursue some potential ways to optimise fit to data in a more formal way.

We acknowledge that there are a large number of parameter choices necessary for TASP to function. We have presented an example application of TASP and shown sensitivity analysis to individual parameters. We add that the development of TASP over several years has necessarily involved tuning parameters multiple times. Furthermore, there has been experimentation with a range of values for other parameters not formally tuned here in previous versions of TASP, which gave a 'feel' for their sensitivity. We agree with Prof. Aitken that for an application of TASP an exploration of parameter inter-dependence should be undertaken and now make this recommendation to TASP users in the text (lines 619-620; for example through a Latin hyper-cube ensemble approach) – this is something we intend to undertake in our own application studies that will follow this publication.

4 - In my view, the true power of this model, which in each of its parts is relatively basic compared to contemporary approaches, is that it holds the whole system in one model. I would be very interested to know more in the paper about potential for modularity - for example if I wish to do detailed ocean transport but need a glacial input; or conversely if I am modelling the sediment transport in detail but need to model ocean transport to a site. TASP might be the ideal tool for this if I can "plug and play", but if it is a closed process I can't take advantage of it.

We agree that holding the whole system in one model is the true value here – as mentioned, other approaches may tackle each problem better individually, but coupling all these more accurate approaches would be impractical.

The code is structured to allow for modularity to some degree, with different functions holding the terrestrial component and each of the ocean transport methods separately. As such, it would be possible to use the output or input of another modelling approach to make use of a single aspect of TASP, provided the variables each function omitted were provided in the correct format. We feel it would be very difficult to implement any more flexibility regarding this in the code as it stands, as it would be case-specific depending on exactly which variables were wanted and an input/output.

Detailed Comments

Introduction

line 32 - in place of qualitative perhaps 'not constrained by a quantitative analysis'

Changed (line 30).

line 34-36 - I think it would be good to express the source-transport-deposition mixture problem formally. You could use Equation 1 of Aitken and Urosevic (2021) or some equivalent

We add this equation (line 38).

line 38 - I would note that there is *no clear basis* for changes in provenance to be interpreted to represent retreat and advance events unless other factors are able to be excluded (see introduction to Aitken and Urosevic (2021) and their eq 1 makes this clear). This emphasises the need for a model like TASP to define the system and reduce the potential for misinterpretation.

Although good sediment provenance studies will always consider all potential impacts on the measured signal, we add that "Unless other processes that influence provenance signatures can be eliminated, these (sediment provenance) records may be misinterpreted." (line 42-43). We agree reducing and quantifying the uncertainty of other processes is a key motivating factor for developing TASP.

line 52 - here and elsewhere 'erosion rate' should be replaced with 'erosion potential' as the true rate is never known in TASP

We thank Prof. Aitken for pointing this out and have corrected this here (line 51) and throughout the manuscript.

line 69 - I don't think the comparison to Aitken and Urosevic (2021) is particularly relevant - theirs is a probabilistic assessment of sediment production tendencies avoiding the need to model transport. There is no competition (in fact the outputs of their approach could be inputs to this approach)

We agree and remove this part of the sentence.

line 80 - An important simplification applied here is that there is no basal sediment layer. This is conceptually unappealing and also it is included in PSU ice sheet models since Pollard and DeConto, 2003 (https://doi.org/10.1016/S0031-0182(03)00394-8) and sediment transport is included in Pollard and DeConto, 2019. This layer is important as even a few metres of sediment protects the bed from erosion and spatially varying sediment cover would control strongly the provenance derived. It also can store sediments. If this truly cannot be included in TASP, then it must be made clear that the assumption is that sedimentary coverage is relatively uniform over the area.

Sedimentary armouring was carefully considered when constructing TASP. However, implementing this when using a 'snapshot' approach would be very difficult, as the thickness of the till layer will vary as a function of time, and is particularly uncertain in the past. Although the thickness of subglacial till now or in the past could be taken from the results of a time-evolving ice sheet model run (e.g. Pollard and DeConto, 2019), this would not account for the fact that till

may be incorporated into/transported by the basal ice even if active till generation is not occurring. Thus, even in an area with thick till, when no active erosion of bedrock was occurring due to armouring, till might be incorporated into basal ice and transported offshore. To truly model this, it would be necessary to step the model through time and record subglacial sediment transport so that the till provenance is known. However, this would require significant extra computational time and negate the strength of TASP as a post-processing tool that does not need to be incorporated into ice sheet model code. Knowing the composition of subglacial sediments at a resolution to make this useful would also require knowledge of subglacial geology at a far higher resolution than is currently possible.

To make this clearer, we add a paragraph discussing this problem (lines 117-125) and in some places change mentions of "generation" of debris to "incorporation" of debris, as this is essentially what is important for provenance at a given time.

line 83 - It is important to note also that subglacial fluvial transport is ignored, this too would strongly alter provenance as it can reach hundreds of kilometres into the ice sheet on short periods, and also is not necessarily aligned with ice flow.

Please see our response to former lines 213-221 below.

line 110 - Perhaps add a comment here on how it might be interfaced with complementary environment-specific transport modelling such as SUGSET or Parcels

Added that "such transport could be incorporated through interfacing with complementary modelling specifically targeting fluvial transport of subglacial sediment" at line 187-189.

Methods

I find the description of Nd data to be overly long for the paper, and too specific - it seems the model tracks a numerical quantity that can be safely mixed (i.e. it cannot track categorical data such as rock types, or numerical data that cannot be mixed (such as U-Pb zircon ages) … but it could probably be used to track bulk chemistry, for example.

We feel some small description of the provenance proxy used in our case study is required, but shorten this section considerably. As Section 2 now focusses on general description of TASP, this part is now moved to the case study (Section 3, lines 509-521).

We discuss the provenance data types that could be used and the modifications to the code/input datasets required in new lines 506-508. "In the case of the categorical data (e.g. clast types) or binned distributional data (e.g. specific detrital mineral age populations), TASP would require adapting to account for multiple input maps and saving of multiple output maps, with the associated extra memory demand."

line 148 - For the purpose of this work, the choice to use offshore data to constrain onshore distribution introduces a problematic circularity…what would be the result with onshore data alone?

Throughout the development of the subglacial ε_{Nd} map, circular reasoning was very carefully avoided. An ε_{Nd} map based entirely on exposed geology traced subglacially using geophysical data was the starting point for this map. However, such a map is biased towards rock exposure which, in some locations, obviously contrasts with unconsolidated sediment measurements.

For instance, the interior of West Antarctica has many Cenozoic volcanic exposures, but these are known to only comprise a very small amount of sediments offshore (Andrews and LeMasurier, 2021). Similarly, exposed rock in the southern Transantarctic Mountains is dominated by relatively radiogenic granites, yet the isotopic composition of tills beneath ice streams draining this region are much less radiogenic, more closely resembling sedimentary rocks which likely surround these granites beneath fast flowing ice (see compilation in Marschalek et al., 2021).

The discrepancy between exposed and subglacial geology means that using exposed geology and geophysical data alone will not produce a map consistent with knowledge from provenance studies. To incorporate knowledge from sediments whilst making as few assumptions about unknown subglacial geology as possible, an estimated uniform Nd isotope composition was applied only in areas where there is a known discrepancy (as described above), and applied in the simplest way (i.e., as a uniform value).

We note that we were incorrect in referring to these constraints as “offshore”, as the compositions were, in fact, predominantly informed by unconsolidated sediments recovered from subglacial settings along the Siple Coast (Farmer et al., 2006). These ice streams drain the southern Transantarctic Mountains and West Antarctic interior, where likely discrepancy in the isotopic composition of exposed rock and likely subglacial rock was highest. We therefore change references to “offshore” constraints to “unconsolidated sediment” constraints to reflect this important distinction. We also remove mention of “inverse” constraints in Fig. 5 (now “sedimentary” constraints), as we are actually using subglacial sediments to inform the composition, not offshore sediments.

line 152/153 - uncertainty here should probably be confidence

Changed (line 538).

line 167 - erosion potential as it is not realised as a rate

Changed throughout manuscript.

line 169 - eq 2 - sedimentary armoring of the bed is neglected. This limitation should be recognised as it is a common process to include in subglacial sediment models - Q could this be included?

Please see our response to the comment on former line 80.

line 177 - the choice of erosion scaling I think is not very important and neither is model resolution - I don't think this paragraph adds much to the paper

We agree and remove this paragraph.

Figure 3 - can we have a zoomed in view of the streamlines?

Added (inset in Fig. 6b).

line 190 - 195 A comment here (or perhaps in discussion) is needed for how TASP might scale up to a more dynamic model, or an ensemble. 8 hours is not too much to ask, but if you wanted

even to do 20 or 30 models it would become a problem. Perhaps a representative random sample of points would suffice?

To reduce the computational load, we include an option to reduce the ice sheet seed locations to only those over a certain basal ice velocity threshold, assuming that slow-flowing areas will have a smaller impact on the total debris load at the ice sheet margin (lines 129-131). However, the bulk of the computational demand arises from the surface and bottom current tracing. It is tempting to reduce the number of seed locations here, but we found doing so had a very clear impact on results. This is because the method used relies on multiple ocean streamlines crossing the same ocean cell to obtain a mean, and any reduction in ocean streamline seed locations increased the stochasticity of results in any given cell. Although this does indeed make large ensembles prohibitive, we find continual improvement using up to the maximum possible number of seed locations.

We add sentences (lines 135-137) mentioning that this option was investigated, but worsened results considerably.

line 196 to 197 To have unique streamlines for each cell-outlet pair seems excessive (perhaps I misunderstand). A more efficient approach might be to accumulate sedimentary material as it flows (e.g. using D8/Dinf algorithm and a flow accumulator)

The reviewer is correct to note that the streamline calculations are very computationally demanding and a good target for improving. However, we do not feel there is a good alternative to the current approach; the D8 and Dinf algorithms are not appropriate here as flow is not downslope. In other words, it is not possible to treat erosion potential as a DEM (with a single value in each cell), with debris passing to the 'lowest' neighbouring cell(s). If the velocity u and v components were used to route detritus to the next cell(s) based on their direction, this would also produce difficulties if a D8/Dinf type approach was used. If a D8 approach was used, this would require selecting only a single adjacent cell, neglecting important detail in flow velocities that are required to get realistic trajectories. If a Dinf-like approach was used, this would tend to produce an unrealistic dispersing pattern rather than a single path through the ice sheet/ocean. The (computationally expensive) streamline calculations therefore unfortunately represent the only feasible option.

line 198 -201 this description of mixing could be better expressed with an in line equation I think

Added an in-line equation as suggested (line 147).

line 210 - While I appreciate it is a steady state analysis - if I understand correctly you treat it as instantaneous delivery. I think there needs to be some expression here of the timeframe to transport…at 0.1 to 1 km a year you might be looking at several millennia to transport the sediment to the outlet; in somewhere like the Siple Coast, that is certainly enough time for the flow to reorganise substantially

Prof. Aitken is correct in that debris transport is effectively instantaneous in TASP. We appreciate that it might indeed take several millennia from the point of entrainment in ice for detritus to reach the grounding zone/sediment core site. However, the principal goal of TASP is to look at (broadly) equilibrium ice sheet states such as past interglacial configurations, when large-scale changes to ice sheet flow are not expected for thousands of years. Flow reorganisation in regions such as the Siple Coast will, therefore, not be represented, but such changes will likely

be mostly on scales of tens of kilometres in lateral movement along the calving front, and therefore introduce relatively small error into the results relative to the uncertainty in sub-ice geology. We briefly acknowledge that we do not account for the lag between entrainment and deposition in new lines 168-176.

line 213 to 221 - I don't think you can ignore subglacial fluvial transport even in Antarctica - high pressure channels exist and are at work evacuating sediments from far inland beneath the ice sheet. I think it is sufficient to say that TASP does not currently include this process - You could add a citation to the model codes that do tackle this such as SUGSET and GraphSSeT and if these could be integrated somehow with TASP.

As suggested to be sufficient, we state that "TASP does not account explicitly for detritus transport in subglacial hydrological networks" (line 177).

We discuss our reasoning for the omission of this transport mechanism in the paragraph at lines 177-184. An acknowledgment that "Subglacial hydrological networks will, however, evacuate some small amount of sediment beneath such an ice sheet" (i.e., without significant surface melt; line 181-182) is added. We argue that available data suggest relatively low sediment fluxes through subglacial hydrological networks at present (see review of Alley et al., 2019), and that hydrological potential tends to broadly follow ice flow trajectories at the continental scale of relevance here and given significant geological uncertainties (see Willis et al., 2016).

We acknowledge these assumptions may have a large impact for ice sheets with substantial surface melt and add a reference to papers discussing SUGSET and GraphSSeT as suggested (lines 185-189).

line 225 - Similarly here I think you needn't say it is infeasible, but it is not part of TASP and that is OK, so long as if I did want to do this in detail somewhere I can still use TASP for the rest!

As suggested, we amend this sentence to simply say that such complexities are not included in TASP.

Section 2.3 - I am less familiar with the oceans modelling sphere, but I do know there are a range of codes that can handle this in the specifics such as ROMS (Eulerian) and Parcels (Lagrangian). Similarly, to the above I think TASP has a simple approach relative to the dedicated codes and does not replace them, but gives a useful complement. Some degree of comparison is warranted.

Our description of particle tracking now includes some text mentioning that the method used for ocean particle tracking is simple compared to code designed specifically for the task (lines 273-277): "As we seek to approximate many debris transport mechanisms in a single framework, the method of ocean model particle tracking described here is relatively simple compared to code designed specifically for this task such as Parcels (Lange and Sebille, 2017) or ROMSPath (Hunter et al., 2022). Such tools are more sophisticated than required for the purposes of TASP, for instance operating in 4D and accounting for particle dispersion. TASP does not, therefore seek to replace them; they are instead a potentially useful complement."

Although not suggested by the reviewer, we also add references to iceberg models (lines 262 and 291) as we feel it was an oversight not to cite examples of these more sophisticated models.

Section 2.3.3

This section shows that with detailed observational data, we can get an acceptable representation of modern-day iceberg trajectories -- but how might this perform for, e.g. the Pliocene? Does the accuracy degrade to the point where we might as well say they travel west and not east?

This is very insightful comment as we have applied TASP using the output of unpublished Antarctic palaeo ocean modelling and found that the lower quality data do indeed impact results. The direction of the Antarctic Coastal Current (and therefore most debris transport) will remain westward under most climate scenarios as it is driven by the presence of ice on East Antarctica, so this feature is robust providing data with sufficient resolution are used.

We feel that the additional discussion here of application with palaeo ocean velocities is beyond the scope of the version of TASP presented here, as the problem is largely dependent on the resolution and quality of the ocean model data used. We are currently exploring ways to apply TASP in this way in future publications.

We now hint to the point raised in our conclusions (line 855): "… use of high-resolution modelled palaeo ocean currents would be highly beneficial."

line 327-329 - this ocean-ice harmonisation process was not very clear to me

We agree the description was hard to follow and have added some lines of pseudo-code to illustrate this (lines 580-589).

line 347 - eq 5 - the format of this equation is not very clear. It would be clearer I think to split the melt rate from the transit time d/v. Also the brackets are not necessary

The equation (now number 6) was reformatted as suggested and the brackets were removed.

line 416 to 427 - Are these processes Antarctic specific or might the processes be better represented by global data or data from data-rich margins rather than sparse local data?

Bottom current transport of sedimentary particles and their sorting along flow paths have indeed been studied in various regions of the global ocean by comparing detailed grain-size data of seafloor surface sediments with bottom-current velocities measured by moorings (e.g. McCave et al., 2017, Deep-Sea Res. I). Results showed that relationships vary slightly between different ocean regions (Fig. 3 in McCave et al., 2017) and therefore we felt it was best to focus solely on Antarctic literature as this will directly relate to the processes working in glacial (Antarctic) settings with physically-eroded sediment.

line 429 - are there not problems from the sharp cutoff? I think this could be better represented as a gradual transition.

The threshold sediment particle (re-)mobilisation by bottom currents is dependent on the composition of the sediment (grain size, mineral density, particle shape, cohesiveness etc.) and reliant on accurate modelling of bottom currents. As both of these are poorly constrained, we feel estimating some relationship between the probability of sediment mobilisation and bottom current velocities would add unnecessary complexity that would be unlikely to improve results.

line 479 - 483 - Is this the same as the D8 algorithm? and it stops when all adjacent sells are above the central cell?

Yes this is the D8 algorithm. It is now stated in the text and cited (line 425).

lines 592 to 560 - can global data or studies from data rich regions support this better?

The bottom current suspended sediment layer thickness is an extremely difficult parameter to constrain. As well as this parameter likely being significantly spatially – and in several regions, including polar margins, seasonally – variable at a fine scale, our parametrisation of this system is very simplified and the value used may not have much physical accuracy. Although literature from other regions and settings was investigated, this did not offer useful constraint, and we are wary of introducing bias towards non-polar settings by relying on studies from such data-rich regions.

Results

In the context of the GMD journal this section is overly focused on the case study -- which is in any case not a good basis for an accuracy test as the true result is not well known. The improved data fit is fairly equivocal due to the influence of a) parameter tuning to fit the data (which I assume was NOT done for the inverse distance) and b) I would say it is (probably) not a statistically significant outcome given the scatter in the data - although I do not have a good gauge as to expected errors in eNd data, there is a lot of horizontal scatter in Figure 9.

As described above, we now separate our model description into a separate section (2). This includes a synthetic case study to assess the accuracy of the terrestrial component (Fig. 3) and an examination of the fraction of sediment predicted to originate from each IMBIE drainage basin to assess whether the offshore component produces sensible results (Fig. 4).

We feel our data fit is notably improved compared to just interpolation offshore, as the reduction in the RMSE, from 3.70 to 3.05, is large compared to the sensitivity of the tuned parameters which never exceeds ~0.2 within the range of plausible values (see Fig. 8). The IDW parameters (distance weight e and number of neighbours ng) were tuned to an old version of TASP, but this has been updated (also Fig. 12). The results (RMSE) are shown below, with optimal parameters of 16 neighbours and a distance weight of 1. It is now stated on line 721 that the IDW parameters were tuned.

		e			
		1	2	3	4
ng	2	4.538	4.546	4.110	4.113
	4	4.105	4.107	4.193	4.224
	6	4.110	4.155	3.882	3.867
	8	3.895	3.893	3.749	3.734
	10	3.788	3.768	3.735	3.732
	12	3.760	3.745	3.701	
	14	3.715	3.702	3.718	
	16	**3.696**	3.701	3.714	3.716
	18	3.703	3.718	3.732	3.740
	20	3.697	3.741		

Although there is a lot of horizontal scatter in Fig. 9 (new Fig. 10), the majority of the error most likely comes from uncertainty in subglacial geology. We do not, however, claim statistical significance and make the edits suggested to avoid any chance of this (see response to reviewer comment on original line 648 below).

We also now add in a sentence on line 520-521 stating typical analytical errors for εNd values (~0.2 to 0.3) for other readers not familiar with this provenance proxy.

line 610 - realistic looking and reasonable results is a weak expression

We agree and re-write these sentences, now stating that results are encouraging because sedimentation decreases with distance from the sediment source, as expected (lines 462-464).

line 631 - close agreement is a bit of a stretch given the amount of scatter in the data and R-squared of just 0.58

Changed to “agrees well” (line 692)

line 648 - MSE of 3.77, while clearly worse, is fairly close to 3.05 given there was not any tuning applied. Unless you can prove statistical significance you should delete 'considerably' and also 'much' on line 649

We cannot prove statistical significance, so remove these words as suggested. However, we do feel the algorithm is worthwhile applying, as 41% of the sites have less than a 1 epsilon unit disagreement compared to only 18% if just using IDW. We also mention that the range in RMSE for different TASP parameter choices always remains well below 3.70 (indeed, below 3.19; lines 721-726).

line 649 - I don't think you can prove outright that the transport modelling was what caused the difference, therefore delete 'therefore'

If the seafloor surface sediment data and input map are viewed as one (as in Fig. 5a), it becomes apparent that the sediments offshore are not a good match for the rocks present immediately inland (particularly apparent areas such as in George V Land). Including westward transport of detritus in the ocean is the only feasible process that could explain this, so we feel it is extremely likely that our transport modelling will be the main factor improving the match with surface sediments and argue to retain the use of “therefore” (line 724).

Discussion

line 674 to 676 - The need for a high resolution observational record here works against the scope of the model for long-term examples…Add a comment here on if/how this process might be represented on long timescales to match the long-term assumptions? This is particularly true of the past

The surface current method is only being used in the ‘best estimate’ in very few deep-sea areas; the gravity flow method dominates beyond the shelf. The only regions where the surface current method was used in the deep ocean are far from the continent in areas not covered by sedimentary records and therefore not relevant for TASP. We therefore feel this paragraph was confusing and unnecessary, so deleted it.

line 705 to 745 - This is an overly detailed accounting for a detail of the specific application and not very relevant to the development of the model. Suggest to delete or shorten considerably

This section has been shortened as suggested (new lines 772-799). However, we feel that the outliers in Fig. 9 (new Fig. 10) near the Adare and Hallett peninsulas are useful to discuss, as they highlight that the model should not be applied to sites very near the coast where fine-scale geology is important. We also feel it is important to describe why some sites were excluded from statistics, and mention that seamounts/islands were not accounted for.

line 726 - why was 200 km chosen?

“This distance was chosen based on visual inspection of areas of obvious discrepancy with seafloor surface sediment measurements (Fig. 11).” (new line 783-784).

line 750 Figure 12 -- this figure is fairly poor and seems in part to have been clipped from a previous figure. The coastline and annotations are peculiarly chunky -- suggest to use digital coastlines from IMBIE or measures

The coastline is from the ice sheet model simulation. As the data shown are the TASP output using this model simulation, IMBIE or measures coastlines are unlikely to align and would leave gaps/overlaps, which would look messy. To improve the appearance, we remove the thick coastline outline. We also mark sediment core sites, as suggested by the other reviewer (new Fig. 14).

Conclusion

line 783 to 786 - the model seems to have confirmed the main features of sediment transport in the ocean…at least today

We already state here that the results are for “the modern sedimentary system”, so do not feel this requires further changes.

line 795 to 800 - I am less convinced by the paleo ice sheet application - it is not clear to me how the surface ocean transport can be modelled to a comparable standard without the observations and the approach has not been demonstrated with degraded data

Please see our response to the comment on Section 2.3.3. In the conclusions, we now state that “use of high-resolution modelled palaeo ocean currents would be highly beneficial” (line 855) for applications of TASP to palaeo ice sheets. We intend to discuss palaeo applications in a later manuscript.

line 802 - It might be worthwhile to point out a potential use for predictive targeting of core sites

We add “TASP also has the potential to better target potential sediment core sites for provenance studies, as the regions with the greatest sensitivity to a provenance proxy could be identified.” (lines 861-863).

line 806 - In terms of proxies, the model seems to be restricted to those that can be numerically mixed, which is probably fine for Nd, but problematic for more categorical proxies listed…

The provenance proxies listed could all be used – the only adaption needed would be to create multiple input maps and save multiple tracers. For example, one input/output could be created for each detrital mineral age bin. This would, however, increase the memory demand. We add text to our case study chapter explaining this (lines 505-508).

Appendix

I do not include detailed comments on the appendix for reasons of length in this commentary, and it is not very relevant to the development of the TASP model, only the application. My recommendation would be to publish the model here and the application (including this mapping) in another journal.

We prefer to publish both the model and these data together because TASP has been primarily designed for Antarctic applications. We feel it strengthens the model description to see a real-world application in the same manuscript as it allows comparison to measured data. Parameter sensitivity analysis would also be impossible without comparison to these data.

line 880 - no data regions should just be left as no data I think

If no data regions were incorporated, this would bias output to rock exposure, which is often not representative of subglacial geology (see response to comment on initial version line 148). It is therefore preferred to make some estimate in all locations beneath the ice.

line 900 - I think to include offshore data in the definition of onshore data that is then modelled to fit offshore data introduces a problem, however small its effect

Please see our response to the comment on line 148 in the initial version.