Reviewer comments in black, our response in blue.

Reviewer 1

I greatly appreciate the opportunity to review this paper. I also thank the authors for preparing this manuscript. This manuscript presents a method by which to validate the flow paths of paleo ice sheet models by seeding chemical tracers and comparing the evolution of the tracers with those measured in depositional environments. This technique is used to establish patterns of erosion and ice flow trajectories in Antarctica. In my assessment, this is a notable contribution to the field. Despite my favourable view of the manuscript's general intent and ambition, I believe that several matters must be addressed in the manuscript before publication. Particularly, I believe that substantial reorganization is needed. These matters are presented in detail below. In my opinion, this manuscript likely falls between major revisions or reject-resubmit. Below I outline my general comments about the manuscript, along with specific comments. My text is in normal font, while quotations from the manuscript are in italics. I wish the authors the best in revising the manuscript and moving forward with this work.

We are glad the reviewer recognises the potential usefulness of the goals of this paper. We have taken steps to substantially reorganise the paper in line with the reviewer's comments as outlined below and feel we are able to address all the reviewer's concerns.

General comments

I believe that the paper requires significant restructuring and more focused aims. For instance, the last paragraph of the introduction is about geographic area, rather than a traditional description of a knowledge gap and the paper's objectives. Furthermore, substantial space in the paper is occupied with the neodymium isotope map, whereas the objective set forth in the abstract and title of the paper is about the TASP model. Despite taking up a substantial amount of the manuscript, this dataset is not mentioned in the discussion. While the editor is in a better place to comment on this, the inclusion of this apparently novel dataset and some of the conclusions from the paper might make the manuscript more than a model description.

We have amended the introduction, moving the paragraph on the geographical extent to section 2.6 (new lines 428-431) and adding a short introduction summary paragraph describing the basic premise of TASP (new lines 61-69). We also focus the introduction by moving the epsilon Nd definition and discussion of the ε_{Nd} map to the end of the model description section (2.6).

We agree that the lengthy regional description of the ε_{Nd} map (Section 2.2 in the original submission) does not relate to the TASP model itself. However, this data compilation underlies the application of TASP, so we feel it is important to be transparent about the data, estimates and assumptions that produced this map. We therefore opted for moving it to an appendix (A1), so it does not interrupt the main text but is still available to interested readers.

I found few citations of other work that applies "tracers" be it airplanes, human bodies, erratic boulders or debris to examine or validate glacier flow. The best of my knowledge much of this work is focused on the alpine environments. However, I encourage the authors to examine the work of Guillaume Jouvet and David Egholm, amongst others, that may provide context to the methods and findings presented here.

We acknowledge that references to this literature were sparse, although as the reviewer notes the spatial scales and climatic/glaciological setting here are quite different to alpine-style glaciers on which most of this literature focusses. To address this, we add references to literature modelling erosion beneath alpine glaciers (new lines 80-82) and references to particle tracing studies (new lines 121-125).

The subject of the paper is well intentioned. However, the complexity and assumption of the processes at work is substantial, between erosion location, iceberg rafting, bottom currents... is there enough input data for these modules to be robust? Also, it was somewhat unclear to me the timestep and timescale of the model. There seems to be a mismatch between timescales between erosion of sediment and its introduction into the ocean and deposition. Can this be improved upon or discussed?

Input data for the modern ice sheet is relatively robust, as described in lines 40-48. As we know the ice sheet form and flow, the basal erosion rates and debris trajectories can be reasonably accurately estimated. Ocean surface velocity data in the reanalysis product are constrained by observations, so are accurate. Ocean bottom currents are perhaps the most poorly constrained variable due to a lack of observations, but we argue that the ocean reanalysis data is the best estimate available (lines 298-300). There clearly are substantial uncertainties in the approach used, but we aim to be transparent about these throughout the manuscript. The close match between the predictions from TASP for each transport mechanism and observations (which are broadly independent of one another) verifies this. We added lines 569-572 to clarify this in the manuscript.

TASP does not have a timestep. The need for time dependence is negated by assuming a steady-state ice flow and ocean transport (now explicitly mentioned in new lines 127, 147-148 and 367-369). We overcome the need for monthly/interannual variability in the ocean by using multiple 'snap-shots' (lines 174-178). This 'snap-shot' approach is a necessary simplification to reduce computational cost, although we note this effect could be somewhat assessed in time-evolving palaeo simulations by using multiple time slices. However, for the modern ice sheet an equilibrium state is considered reasonable; provenance signatures will not have changed significantly at a continental scale during the late Holocene. We are also primarily concerned with capturing the broad spatial pattern observed in seafloor surface sediment data, which is evaluated both by using idealised Nd isotope composition maps (Section 3.3) and our match with seafloor surface sediments (Section 4.2).

Is there a table of parameters/variables/model functions somewhere?

We have added a table with parameters (new Table 1) to the manuscript.

The description of model components (i.e. Section 4.2) include a mix of model description, model implementation, results and discussion in one section. I recommend the authors find a way to separate the presentation of these themes.

We significantly restructured the manuscript as follows:
Section 1 – Introduction
Section 2 – Main model description section and brief summary of Nd isotope input data.
Section 3 – Discussion of parameter choice (tuning/sensitivity experiments), verification using idealised basins.
Section 4 – Comparison of results to observations to evaluate the accuracy of TASP
Section 5 – Conclusions

We hope this restructuring helps better explain the model.

It seems like sensitivity tests of some type were conducted (i.e. Figure 5), however, I did not find how this done. Also, I am curious about factors such as how grid size was determined and if this impacts model outputs.

The sensitivity tests in former Fig. 5 were based on varying the number of random seed locations generated in the alternative method for ice flow line trajectory start points (original lines 416-446). However, this subglacial seeding method is no longer described in the manuscript as it is not the preferred method (as suggested by the reviewer), so the figure was removed.

Various other sensitivity tests were conducted during the development of TASP, including:

- Debris-rich basal ice layer distributions (linear vs exponential vs constant)
- Debris-rich basal ice layer thickness
- Suspended sediment layer thickness in bottom current module.

We acknowledge these were poorly described in the initial submission. We therefore now include a new Section (3) describing these sensitivity tests.

As stated in lines 90-91 of the original manuscript, the 10 km grid size of the ε_{Nd} map was picked to give some degree of detail, whilst avoiding using an unrealistically fine resolution given the substantial uncertainty beneath ice-covered areas. The ice sheet model grid size of 10 km was selected as this is typical for continental-scale models of the ice sheet. We note that the code is written to automatically adjust the input datasets to any coarser resolution palaeo ice sheet model, as such simulations often use 20, 40 or 80 km (line 195). Experiments using a finer resolution (e.g. 5 km) were not performed given that (particularly palaeo) ice sheet models do not often use such a fine resolution. However, we would expect a fine ice sheet model resolution to have a minimal impact on results given that the uncertainty in the underlying ε_{Nd} map is by far the largest source of uncertainty (and mismatch with observations).

To investigate the role of using a coarser grid for the ε_{Nd} map input, we ran a simulation smoothing the ε_{Nd} map to 20 km resolution. This marginally reduced RMSE compared to seafloor surface sediments, giving 2.825 (vs. 2.861 at 10 km), suggesting increasing the resolution (e.g. to 5 km) would certainly not produce meaningful results given the large uncertainty in subglacial geology.

Specific comments

Given my comments above, the specific comments here are not comprehensive and are generally representative of large issues with the manuscript.

Ln 391-403 How is sediment from erosion accumulated in the ice and transported? all of it is entrained and advected with the basal velocity?

This is addressed in Section 2.1. Yes, it is assumed all is entrained in basal debris and evenly mixed at the grounding line, as summarized in new lines 117-119.

Ln 415-445 There are two methods presented and one is recommended because it is better? Can the methods be compared? or possibly better, one method removed?

We agree with the reviewer and therefore removed the first method as the second method is better. New Figure 1 is updated accordingly. Old Figure 5, which referred to this alternative method, was removed.

Ln 442-445 *This approach does not account explicitly for detritus transport in subglacial hydrological networks, but these are unlikely to deviate significantly from ice flow vectors at the spatial scales of interest here*. In many glaciers, subglacial networks are how most of the sediment is transported from the glacier. Furthermore, subglacial drainage networks follow the hydraulic potential, which can deviate significantly from ice flow vectors. If this statement is included, then a citation supporting it is certainly needed.

Because of a lack of surface melt, Antarctic subglacial meltwater is produced entirely through basal melting. This is a gradual process, meaning flow rates are much lower than for mountain glaciers and the margins of the Greenland Ice Sheet, resulting in less sediment transport potential. Even lake drainage events are thought to be unimportant for erosion (Hodson et al., 2016). This is discussed extensively in Alley et al. (2019), which has been referenced here (new lines 130-138). We do now include a line mentioning that for palaeo Antarctic ice sheets, significant surface melt - and therefore larger subglacial hydrological transport - might have occurred (lines 135-137). However, accounting for this is beyond the scope of this paper.

Regarding the hydraulic potential broadly following ice flow vectors: we added a reference to Willis et al. (2016), who calculate hydraulic potential for all of Antarctica and use this to route basal meltwater to the ice sheet margin (their Fig. 5; new line 134). They state: "*Networks of drainage pathways within all basins trend towards the coast in the general direction of the ice surface gradient*", which is very similar to ice flow trajectories.

Overall, we argue that sediment transport in subglacial hydrological networks is very slow in Antarctica compared to other glacial settings, and that the small amount that does occur is likely to broadly follow ice flow directions. We are therefore confident that neglecting this component is a reasonable assumption for our continental-scale simulations (lines 134-135).

Figure 5 What is the misfit against? If I understand the model correctly, then shouldn't this figure be presented at the end after the sediment has passed through other modules, including the ice rafting and bottom current?

The misfit is against observed seafloor surface sediment measurements, although we note this figure has been removed in response to other comments. However, we note that the replacement figure (new Fig. 4) also uses RMSE for tuning parameters. We therefore restructured the manuscript to place this section describing parameter tuning (Sec. 3) after the model description section (Sec. 2).

Ln 489-495 These are contemporary trajectories, it seemed. How well do they represent the distribution of tracers? Also the model was run 354 times. however, by varying what? Parameters? is this a sensitivity test? Why was the model not run continuously from 1993 to 2019?

Our response to the earlier comment on time steps is relevant here. TASP assumes the system is in a steady state, so does not evolve through time. The use of 324 months ocean velocity data is therefore effectively using multiple snap-shots in time to capture seasonal and interannual variations. To trace surface currents (proxy for IBRD), we calculate the trajectory at every seed location on the ice sheet groundling line for each month's velocity field and assume the debris is distributed along this streamline as the 'iceberg' melts. We re-wrote section 2.3 to clarify this – see new lines 171-181 in particular.

The 324 times refers to the surface current flow lines being calculated once for each month in the 27 years (i.e., 12*27). We clarify this now in new line 175. We also noticed the 1993-2019 time period was incorrectly noted as 25 years instead of 27 years, so amended this.

Ln 510 *It is not realistic to assume that the volume of debris dropped by an iceberg remains constant over time... Transport of iceberg rafted debris (IBRD) over many hundreds of kilometres is possible (Dowdeswell et al., 1995), but typically transport is more local.* This paragraph is a bit strange in the sense that it starts by discussing temporal variability and ends by discussing spatial variability.

We apologise for the confusion and amended the first sentence to read: "*It is not realistic to assume that the volume of debris dropped by an iceberg remains constant over its entire transport pathway*" (new line 195-196).

Section 4 I did not find out how detritus is transferred to the "ocean transport" module from the ice flow model.

The process was described in lines 481-489 of the original submission. The paragraph has now been rewritten to clarify this (new lines 166-168).

Ln 575 Superglacial debris... this seems like a poorly constrained process in this location. I am also curious how this debris is linked to the ice flow model. Englacial debris will move faster than the subglacial debris, will this create issues as englacial debris from the model is transferred to the ocean.

Supraglacial debris is absent from most of Antarctica as rock exposure is rare. In TASP, we assume such supraglacial and englacial debris is very low compared to the basal debris content (10^{-5} times the maximum debris load at the base of the ice column). In some regions, such as the Antarctic Peninsula, supraglacial/englacial debris may be more common, but as estimating debris distribution in the ice column is very poorly constrained (lines 199-209), this was a necessary simplification. We point to the paragraphs discussing supraglacial and englacial debris (lines 195-211, 220-227).

As discussed, supra/englacial debris content is assumed to be very minor compared to basal debris implying that changes in its parameterisation will have little impact on results (see insensitivity to the concentration of this debris in new lines 497-502). Furthermore, we argue that debris production rate is much more important than transport rate to the sedimentary provenance record as this will control the relative amount of debris eroded from given locations. We therefore do not attempt to constrain (uncertain) transport rates at all in our approach.

Ln 649-650 *As suspended particles will not be deposited uniformly over a given flow pathway, deposition over a streamline must be approximated*. What is the difference between a flow pathway and a streamline?

There is no difference between a flow pathway and a streamline, as both are describing the tangent to the vector field. We changed 'flow pathway' to 'streamline' to avoid confusion (line 325), and do so throughout the manuscript to ensure consistency. See also our reply to reviewer 2 regarding the 'streamline' function.

Ln 717-720 I am having a very hard time establishing what was done here. *This method makes no attempt to account for travel distance along a gravitational transport pathway* isn't this somehow the aim of the model? *always be approximately perpendicular to the coast (on the shelf).* Doesn't it need to follow the steepest descent, regardless of the coasts?

We amended the corresponding sentence to "*TASP makes no attempt to account for variations in debris load over travel distance along a gravitational transport pathway.*" (line 484). Apologies for the confusion. Yes, sediment transport does follow the steepest decent – the term 'always' has been amended to 'usually' (line 380). This paragraph tries to describe why there was no need to include any decay in sediment transport along gravity flow trajectories (i.e., they rarely cross, so relative amounts of debris are irrelevant).

Ln 785 *There may be some relationship with the pathways of Ross Sea Bottom Water export, although the spatial match is not perfect (Orsi and Wiederwohl, 2009).* Spatial match between what and what? Also, I would not expect any match to be perfect, please clarify.

In regions with known strong Ross Sea Bottom Water flow off the shelf, we observe more cells in which the bottom current ε_{Nd} value estimate has the closest match with seafloor surface ε_{Nd} values (implying that bottom water sediment transport dominates). The sentence has been rewritten accordingly (lines 584-587).

Figure 10 How is "best match" determined? I did not find comments about model parameterization or inversion that would have resulted in this.

The ‘best match’ approach is the focus of Section 4.1. We added the term ‘best match’ to this paragraph to clarify the relevance (line 564) and place it after the main model description to make it clear that this is not necessary to demonstrate the functioning of TASP as intended.

Figure 12 There is a lot going on in this figure. Can it be improved upon?

In hindsight, we agree that the figure was too busy and redrew it, removing unnecessary labels.

Ln 955 *will permit application to simulations of palaeo ice sheets*. Simulations of what? something is missing in this sentence.

We rephrased to “*will permit application to palaeo ice sheet model simulations*” (line 767).