Response to review by Stewart Jamieson

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

This paper introduces and then tests a computational framework for predicting the provenance of sediment delivered from the Antarctic continent to the Antarctic continental shelf (and slightly beyond) on the basis of interpreting ice sheet model output. This work fills a significant gap in capability in terms of helping understand the pathways that detrital particles take as they are eroded subglacially and then transported to and beyond the ice sheet margin and theoretically allows the tracing to be completed using any ice sheet model output. The model takes into account the movement of particles such that the neodymium isotopic composition of the material is computed – this is beneficial because it can be compared directly to sediment collected offshore in a number of locations. Thus the framework should enable erosion and sediment transport and thus sediment provenance to be computed under different ice sheet regimes which should therefore produce different maps of neodymium compositions – this will allow certain ice sheet models to be ruled in or out based on their fit to measured neodymium compositions. The framework incorporates a set of appropriate transport and concentration processes although it has to make some assumptions as it does so. Processes include glacial erosion, transport subglacially, movement and rain-out from icebergs, ocean bottom currents and downslope sediment transport based on slope and the overall result is a seafloor map of Nd isotopic composition.

The code and dataset for comparison is openly available and well documented - all very clear.

We thank Prof. Jamieson for his thoughtful review and are glad that recognises that TASP fills a significant gap in capability and necessarily must make some assumptions to do so. We are also pleased that the code and dataset is recognised as well documented.

General Comments:

The paper is largely well written and easy to follow and is largely well structured with useful figures. I have some relatively minor comments on the science and writing which would be good to see addressed.

First, in places the paper mixes description of the model with descriptions of the test area where it is being applied. For example at lines 129-135 there is discussion of the area that you examine in West Antarctica. However, for a paper that is introducing a framework/piece of software that should be applicable to anywhere, I think it would be beneficial to fully describe the model framework itself before then showing how it can be applied to a particular region. I think this means reviewing carefully the introductory and methodological sections to make sure that the framework is fully introduced and then you can roll into a description of the particular ice sheet model being tested and the particular region being tested. This would make the model easier to understand and potentially easier to apply to other areas of interest.

The issue of the focus on the case-study vs broader applicability was also raised in the review by Prof. Aitken. We agree that the intermixing of the model description and our application to Antarctica resulted in a lack of clarity in the previous submission. In response, we have carefully removed any text referring to Antarctica or Nd isotopes from the Introduction and Methods sections. This now separates the model description (Section 2) from application of the model to

Nd isotope compositions in Antarctica (Sections 3 and 4). We thank the reviewer for this idea as it improves the structure of the manuscript considerably.

Second, I would like to see a clearer discussion of uncertainty or a clearer capability to embrace uncertainty within the framework. This is because at the moment particular parameter choices are made to fit the particular model area but there is no real discussion of the extent to which the result would vary if particular parameters were adjusted. For example, which parameters perturb the result most strongly? Or in other words, are there particular processes that dominate the result? It would be great to see some more discussion of that. In addition, it would be useful to see a table of parameter choices made here as this would serve to show in quick summary what was done, but would also show clearly what parameters are generally changeable within the framework so that people applying it to other areas can then make their own adjustments. Much of that information is in the user's guide but a table in the paper would give that additional help to readers.

The issue of parameter interdependence and sensitivity is also raised in Prof. Aitken's review.

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 (Fig. 8). There were no parameters tested that impacted RMSE by more than ~10%, so we add a sentence stating that this means we are confident that the largest influence on results is the input ε_{Nd} map (new line 617-620).

Furthermore, the development of TASP over several years has necessarily involved tuning parameters multiple times. There has also 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 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.

The existing Table 1 includes a list of parameter choices which are input into the code. We add the bottom current velocity threshold parameter and interpolation distance around different marine transport estimates to this table, which now contains every parameter used in the model. We also add a "tuning range" column, to show the range used to tune relevant parameters.

Specific Comments:

Beyond these comments, most of my suggestions are minor – they are outlined below by line number:

Equation 2 and associated text – please explain/justify why that particular erosion rule was used. Other erosion rules are available (e.g. Herman et al, erosion under an alpine glacier) – would the results differ significantly? (link to discussion about uncertainty perhaps).

We now state that: "…various erosion laws have been employed to recreate glacially eroded landscapes, typically also using an erodibility constant and the ice velocity raised to an exponent, often ~2 (e.g. Herman et al., 2015)." (line 109-111) and that "Here, we opt to use the erosion law in Equation 2 as is has been proven to reproduce reasonably accurate modern

Antarctic topography starting from a pre-glacial landscape (Pollard and DeConto, 2019), and because using a different erosion law would be unlikely to result in a significantly different pattern of erosion potential, which broadly scales with basal ice velocity and increases towards the ice margin." (lines 113-116).

Although we do not examine results with different erosion laws, there is no clear reason why a different erosion law would be preferred. We feel that all erosion laws result in the same general distribution of erosion potential (i.e., increasing towards the faster-flowing ice sheet margin), so this is unlikely to be a major source of uncertainty in our estimate offshore.

175: here you discuss a negligible change to the match with seafloor sediment Nd values in terms of how you choose k – this feels like a result or a point for discussion as opposed to something which should appear in the methods.

We agree and move this sentence to the parameter tuning section, providing more details: "For the terrestrial component, experiments were performed with spatially variable k ('quarrying' coefficient) values for different ice drainage basins as in Pollard and DeConto (2019). However, this revealed a negligible impact on results. This is most likely because the map of k used only varied over very large-scale basins (Pollard and DeConto, 2019). A finer resolution estimate of 'erodibility' might lead to different lithologies being disproportionately represented offshore, influencing provenance signatures." (lines 621-625).

177-180 – you discuss the Aitken and Urosevic approach and mention your approach differs, but you don't really say why/if your approach is more appropriate or provide evidence for why it might be better etc. – you could link that to my point about equation 2.

As suggested in the review by Prof. Aitken, we remove this sentence as it was not well phrased and a comparison to the approach in Aitken & Urosevic (2021) is not necessary here – neither is inherently better, as they have different objectives.

188: 'Standard Euler method' could do with more explanation. Also, explain why you think this is an appropriate method and also in terms of velocity, are you using basal velocity or ice surface velocity?

Changed this to "an Euler integration method" for clarity. Streamlines ("curves tangential to the velocity vector field") are an established mathematical way of calculating flow paths through a vector field and produce plots that align with modern ice flow velocities for Antarctica (Fig. 6). We also already state that it is the basal ice velocity here (line 127).

193-195: The computational demand is rather specific to a particular machine (which we have no real info about in terms of its specification). Therefore you could perhaps mention the types of CPUs. I would lose the point about using fewer CPUs and less memory – it seems obvious although I guess one option is to rephrase in such a way as to indicate whether there might be minimum specs that would allow the model to run (or perhaps to compare how long it would take to run on a more standard machine (assuming it would run on such a machine).

We now state that the Imperial College HCP cluster uses AMD EPYC 7742 2.25 GHz processors (line 135). We agree the CPU/memory vs runtime trade-off is obvious, so remove this statement.

205-210: I wonder whether we might benefit from a schematic diagram to illustrate how particles might move through a grid framework (e.g. showing how particles will travel subglacially and then into the marine realm). It could have some grid cells schematically drawn out with a start and end point for the particle. to show how these values are calculated as it moves through different processes. This schematic could be done just for this component of sediment transport, or another option would be to schematize the entire sediment transport process from source to iceberg to deposition in the ocean - this might help us understand the overarching structure of the processes being accounted for in TASP.

We thank Prof. Jamieson for this idea and now include a schematic diagram illustrating how debris transport is approximated in TASP (Fig. 1). We felt that limiting this to the subglacial component and including something with grid cells would not add much, especially considering the new addition of the synthetic example suggested in Prof. Aitken's review (Fig. 3). We therefore opt for cross section cartoon.

210: steady-state ice flow is assumed. Please discuss this assumption and its validity either here or in the discussion section later.

We add the following to this paragraph (lines 171-176), which describes the validity of the assumption: "The time between debris entrainment and deposition offshore may in reality be hundreds to thousands of years, depending on ice velocities. However, the primary goal of TASP is to reproduce long-term, large scale provenance patterns, such as interglacial ice sheet configurations smaller than that at present; for these, ice flow will be broadly consistent for thousands of years. At the temporal and spatial scales of interest here, the approximations used are considered sufficient to capture the broad-scale trajectories of debris under the ice sheets."

222-227: This feels a little out of place/tacked on. It could be mentioned earlier where you mention the erosion rule that is implemented, or it could be saved for discussion in the discussion section later.

We agree and move this text to the paragraph where erosion laws are discussed (lines 108-116).

233: Its not immediately clear why debris distribution in the ice column is in a section on ocean surface currents and iceberg rafting - move as appropriate or flag at the outset why the debris in the ice column is related to the iceberg processes.

We now include a sentence near the beginning of this section: "If debris is incorporated further from the edge of an iceberg, more melt needs to occur for it to be released, potentially allowing for a longer transport distance." (line 193-195)

Figure 4: Fig. 4 is a result figure, but also it doesn't really tell us whether the fit is good or not. Thus is it a helpful figure right here? Perhaps Fig. 4b is relevant here, but fig 4a does seem to be a result figure which could be saved for later when results of the tests on the specific location are presented.

We move this to our case study results section as suggested (new Fig. 9).

272: A value of 4 m debris-rich ice layer thickness. Is this also based on observations of debris layer thicknesses at all?

We tune this parameter within the range of observed thicknesses (2-12 m), and here (line 231) refer to the later section where this is discussed in detail (Section 3.2).

610: It is not indicated why/how you know this is a ‘realistic-looking’ distribution. Please elaborate.

This point was also raised in the review by Prof. Aitken. We re-write this paragraph (lines 462-464), now stating that results are encouraging because sedimentation decreases with distance from the sediment source, as expected.

653: TASP is better in deep waters. Better in comparison to what?

We rephrase this sentence: “TASP also performs better in deep water locations than for continental shelf sites” (lines 710-711).

666: You could quantify some of the values (predicted vs. measured) so that we understand the fit. Overall, if quantification at particular core sites is being done, then a proper description of E_{nd} distribution would be good as part of the example science question you are addressing by applying TASP.

We now quote some core site predictions vs measurements for sites where down-core records exist to give an idea of the accuracy of TASP (lines 697-701): “We achieve a close match to surface sediments from specific core sites with down-core records, including International Ocean Discovery Program Site U1521 (Marschalek et al., 2021; measured = -7.7, TASP prediction = -8.0), Integrated Ocean Drilling Program Site U1361 (Cook et al., 2013; measured = -11.5, TASP prediction = -13.3) and Site PS58/254 (Simões Pereira, 2018; measured = -3.0, TASP prediction -3.7).”

As suggested, we also include a brief description of the ε_{Nd} value map predicted by TASP (lines 688-692): “This produces a map of ε_{Nd} values where the most radiogenic (least negative) values are found around the Antarctic Peninsula and along the Marie Byrd Land coast (Fig. 11g). Slightly less radiogenic values are found in the Bellingshausen and Ross seas. The East Antarctic coastline offshore of George V Land produces the least radiogenic values, although the influence of more radiogenic debris transported from the east is clear (Fig. 11g).”

Figure 12: Can core-top sites be shown? I don’t know how much of this map is actually constrained by measurements.

We thank Prof. Jamieson for this useful suggestion and add core sites to the figure (new Fig. 14).

Section 5 (Conclusions): The conclusion could be clearer about the key processes incorporated in TASP before it gets into particular science findings. Thus add a few points about the processes at the start of conclusions to properly say what the framework does. This is important because it’s a GMB paper which needs to therefore show the key points of any model before also showing any location specific results etc.

We add sentences summarising the general structure of TASP (lines 834-839). “Debris is incorporated and routed at the ice sheet bed based on ice sheet model results and an erosion law. Marine detrital particle transport mechanisms include representations of surface currents, which are used to approximate iceberg trajectories. Bottom currents redistribute sediment if a velocity threshold is reached, and gravity flows transport material downslope. These estimates are then used to make a provenance proxy map across the seafloor, thereby directly predicting sediment core data for a given modelled ice sheet extent.”