Reviewer comments in black, our response in blue.

Reviewer 2 (Evan Gowan)

Marschalek et al. present a tracer program using Matlab, called TASP, that determines the trajectory of ice rafted debris in the ocean based on the results of ice sheet model simulations. They specifically are interested in neodymium isotopes, but such a module could likely be applied to any sediment proxy. They start by introducing a reconstruction of neodymium isotopes for Western Antarctica, based on geological and geophysical inferences. The next section describes how the program determines the transport of sediment by the ice sheets. The third section describes how sediment is redistributed in the ocean by icebergs, gravitational flows and currents, and how the program can discriminate between the mechanisms to determine the relative amount that is due to ice rafted debris. The final section describes the application of the model with the neodymium map, and demonstrates some model-data discrepancies, especially with respect to unincorporated features like Quaternary volcanoes. This kind of tracer modelling is important as there are few direct ways to determine past ice sheet configuration, so using offshore proxies is the only way to make inferences.

We are glad that reviewer 2 recognises the value of this work for providing important constraints on palaeo ice sheets.

Overall, there are a lot of interesting things presented in this paper and am supportive of what the authors are trying to achieve. However, I agree with the other reviewer that this paper is lacking in focus. This paper has been submitted as a model description paper, yet nearly 1/3 of the paper is dedicated to the description of the construction of the neodymium map. The algorithm used to determine the trajectory of the ice rafted debris is inadequate to allow someone to reproduce the model (which is likely desirable for people like myself who do not have Matlab access). There are a lot of assumptions made in the parameters used in the model (for instance, lines 567–572, 627–628, 636–637, 667, 680, 689 and 737), with no tests to show what the impact of these assumptions are on the results. The reported effectiveness of the model (section 5) is hampered by the usage of the estimated real-world neodymium distribution.

As mentioned in our response to reviewer 1, we moved the section describing the regional description of the ε_{Nd} map to an appendix.

The reviewer mentions assumptions regarding the following parameters:

a) Thickness of the basal debris-rich ice layer (old lines 567–572).
b) The bottom current threshold required to resuspend sediment (old lines 627–628).
c) The relationship between mean and peak bottom current velocities (old line 667).
d) Critical depositional stress (old line 680).
e) Roughness length (old line 689).
f) Depth threshold for the start of a gravity flow (old line 737).

We now include an explicit Section 3 describing sensitivity tests conducted and parameter optimisation. Here, we show the influence of varying parameter a), which has little impact on results (Fig. 5). Furthermore, parameters d) and e) both relate to the bottom current method, and these constants will therefore be accounted for in the tuning of the thickness of the layer with suspension load through Equation 4, as detailed in new Section 3.2 (lines 511-512).

Experiments were also performed varying parameters b) and c). If a lower threshold was used to mobilise sediments by bottom currents, this led to unrealistically large amounts of the domain having sediment remobilised. As sedimentary signatures of remobilisation, such as winnowing, are only occasionally observed in sediment cores, this is known to be inaccurate. In contrast, if a higher remobilisation threshold is used, this leads to unrealistically few areas being selected. The values used for b) and c) are therefore viewed as reasonable best estimates constrained by observations and experiments. We added a sentence to the text explaining this (lines 323-324).

Parameter f) was not tested with different values, as it had been carefully set to be as shallow as possible but to avoid interpolation on the over-deepened inner Antarctic continental shelf (line 398). It is therefore not appropriate to vary parameter f), or TASP would produce gravity flows in areas different to those observed.

See also our response to reviewer 1, who raised concerns about parameter sensitivity experiments.

We do not agree that "*The reported effectiveness of the model (section 5) is hampered by the usage of the estimated real-world neodymium distribution*." Although we tune parameters to observation (Sections 3.1 and 3.2), we note that these subtle parameter choices actually have little effect on the overall result (see little variation in Fig. 4) compared to simply ignoring marine transport processes (Fig. 10). Our individual sediment transport estimates (Fig. 7 a-f; Fig. 9 a-c) were therefore produced virtually independently from the seafloor surface sediment constraints, yet we observe a reasonably close match using each of these alone. We then go on to select the closest match to these observations for our 'best match' estimate, but this is not strictly necessary to achieve a close match with observed seafloor surface ε_{Nd} values. Matching seafloor surface sediment data is instead designed to achieve a better approximation for future applications to palaeo ice sheet simulations. To clarify this in the manuscript, we restructured it by placing the 'verification using idealized basin' section (3.3) before the comparison to seafloor surface sediments section (4).

I think the way forward here is to split the paper into two – one paper where the TASP model is described in greater detail, making use of artificially constructed neodymium concentration maps to test the sensitivity of the model to different parameters, and a second paper to describe the case study of the realistic distribution of neodymium described in section 2. The model description paper should have information like the types of input needed for the model (including a table of the parameters found in the User Guide in the source code package would be ideal). With idealized maps of the neodymium concentrations, you could then truly test if the model is capable to achieve the stated goal of determining past ice sheet configuration. You could, for instance, assign a single neodymium value to a single ice sheet sector and another value for the rest of the model domain, and trace how that affects the concentrations in the offshore region (in essence, testing if the signature of that sector can be detected). There should also be a description of the computational overhead. The second paper would be a case study that could be expanded to use TASP to, for instance, test the difference between present day and LGM ice configurations. This paper could then demonstrate whether or not sediment tracers can be used to determine past ice sheet configuration, a result that would be very useful for paleo ice sheet modellers.

We carefully considered the reviewer's suggestion of splitting the paper and appreciate the merits of doing so. However, we felt this was not the best way forward and have taken steps to address the concerns of reviewer 2 in other ways (i.e., by moving the ε_{Nd} map section to an appendix and substantially restructuring the paper). We also feel that splitting the paper as suggested and using just idealised ε_{Nd} maps in the first publication would prevent us from evaluating the model, as we could not compare model results with observed seafloor surface sediment data.

A section showing the results of using idealised maps of ε_{Nd} values has been added to show TASP can trace debris from specific sectors in a realistic pattern (new Section 3.3). Selected IMBIE drainage basins were set in turn to a nominal value of 1 and everywhere else as 0, so the predicted value represents a proportion of the sediment likely to be derived from this sector (Fig. 6, lines 534-544).

The computational demand was mentioned in the User Guide, but we have copied this statement across to the main manuscript now (lines 111-113).

We have preliminary results from applications to palaeo ice sheets which we indeed aim to present in a second manuscript. As we feel the comparison of our independent prediction to seafloor surface sediments validates that TASP produces accurate results, it seems unnecessary to include case studies here as this would go beyond a model description paper (as the reviewer suggests).

Other comments

Streamline function

In terms of generating reproduce-ability, a lot of riding on the Matlab "streamline" function. The details of this function are not described. I assume that it creates a vector map of the trajectory of the parameter (e.g. ice and water velocity), and traces the material along those lines. However, this is going to have some time dependence, and it is not stated how this is applied. At the very least, there should be some reference to what this function is.

The 'streamline' function calculates streamlines, defined as lines whose tangent vectors comprise the velocity vector field (Granger 1995, *Fluid Mechanics*, pp. 422–425). In MATLAB, this is solved using the Euler method and requires velocities (u and v components) and seed locations to be input. We used a step size of 0.25, which produces a reasonable streamline resolution without adding significant computational demand (i.e, using a smaller value has a minimal impact on results). Given that the ice sheet flowlines look indistinguishable from modern ice trajectories (Fig. 1) and our 'iceberg' drift patterns look similar to those observed (Fig. 2), we are confident this function is producing suitable results. We agree this function is crucial to TASP, and thus now highlight in the text that this function uses standard methods for calculating streamlines through a vector field (new lines 105-107). We note that any time dependence is not important here, as we assume equilibrium ice sheet flow (line 127) and replicate oceanic variability using multiple ocean 'snap-shots' (171-176).

Wind blown dust

Another factor that could influence the geochemistry of the ocean bottom sediments is wind blown dust. There are large dust sources in Patagonia, southern Africa and Australia that may be important, especially as you get further from the edge of the ice sheet. A cursory glance at Southern Ocean neodymium isotope research indicates that these dust sources influence the concentrations in the water (though I do not know the follow-on in terms of sediment concentration). The melting ice could also include dust from these sources, which may become important as the bottom of the iceberg melts away and it loses the sediment directly scraped from the ice-bed interface. I realize that separating these two factors would likely be impossible, but it probably should be commented on in the manuscript about the possibility that dust could influence the results.

Windblown dust from other continents is indeed unaccounted for here and reviewer 2 is correct that dust may become an important factor at some sites further away from the Antarctic continent. However, this study focuses on the Antarctic continental margin, where most sediment provenance core records are located; here, the effects of aeolian dust are negligible compared to the glaciogenic component (except, perhaps, where volcanoes are close by, as described in the text). This is apparent in numerous datasets from Antarctica looking at sediment grain textures, provenance etc., both in modern and glacial-time sediments (e.g., Diekmann et al. 2000, *Palaeogeogr. Palaeoclimatol. Palaeoecol.* **162**, 357-387; Walter et al. 2000, *Geochim. Cosmochim. Acta* **64**, 3813-3827; Wengler et al. 2019, *Geochim. Cosmochim. Acta* **264**, 205-223). The dominance of glaciogenic detritus is also demonstrated by the ability of TASP to reproduce modern measured seafloor surface data (Section 5.2). However, we added a sentence mentioning that these sources are not accounted for (new lines 722-724).

Details on the ice sheet modelling

The description of the ice sheet model used to test TASP is limited. Is it a modern day simulation? How was it run? Does it replicate modern day ice flow observations? There should be some more details so that we know whether the results will be reasonable given the geological data.

We addressed this point by referencing DeConto et al. (2021), which describes the ice sheet model physics/simulation used in detail (line 77). PSUICE3D is a well-tested and widely applied ice sheet model. We also now include the ice sheet model output in the data files with the code.

Colours on the plots

I found the colours used in figures 8 and 12 made it difficult to make out the values, especially with the yellows. I suggest improving the contrast.

Figure 12 (now Figure 11) has been adjusted based on the feedback from both reviewers. The colours in Figure 8 have also been adjusted.