Our manuscript titled "The geometry of sea-level change across a mid-Pliocene glacial cycle" received two largely positive reviews that listed several suggestions for improvement. In the material below, we respond to the first comment. The reviewer comments appear in blue, and our responses are in black, with text quoted from the manuscript indented.

Reviewer #1 (Dr. Naish):

This paper is a nice model approach using state of the art GIA model to reconstruct GRD effects on global sea-level change during the time interval spanning the MIS M2 glacial to MIS KM3 interglacial as defined in the L&R05 d180 stack. Even though I have outlined below, why I believe the methodology is flawed, I would like to encourage the authors to consider trying a different range of ice sheet histories that might better reconcile with the far-field geological record of sea-level chnage. Its always difficult using a GIA model to evaluate a sea-level record when the ice sheet history is ambiguous.

I will declare up front that I am Tim Naish, and have been closely associated with the development of Whanganui Basin, NZ sea-level records. I also saw Meghan King present this paper at Fall AGU on 2022, where I discussed it briefly with her afterwards. I remian supportive of her work. I dont feel conflicted, but will leave that up to teh eds to decide.

It might help also if I mention the motivation behind the 2019 Grant et al study published in Nature. We were well-aware that the L&R05 d18O stack was of lower qulaity between 3.3-3 Ma due to low number of records and poor resolution of some of the records. The shallow marine glacial-interglacial sedimentary cycles in Whanganui are well dated in this interval as both Kaena and Mammoth paleomagnetic subchrons could be indentified as well as radiometrically-dated tephra. We argued for 2kyr sample resolution with an accuracy of +/- 4-5kyr at pmag transitions. On this basis we built an independently dated sea-level record and showed that it was largely in phase with Antarctic summer insolation. Given geological evidence precluding a large NH ice sheet until 2.7Ma (Haug et al., 2005; Jansen et al., 2000; Brigham-Grette et al., 2013; Berends et al., 2019, Eldrett et al., 2007; Thiede et al., 2011; Tripati & Darby, 2018). we also argued most of the meltwater was of Antarctic origin, and this informed the GIA modeling to we used to test how close Whanganui RSL was to ESL.

My comments follow.

We thank Dr. Naish for his review and constructive comments.

Line 100. Definition of global mean sea-level needs to be registered to the centre of the Earth, otherwise it is eustatic sea-level. Im OK with Pan et al 2022 method for estimating change in eustatic sea-level ESL (but shouldnt use GMSL unless you can register it to present day sea-level).

In studies of paleo sea level global mean sea level changes are not referenced to the center of the Earth. Any change in the elevation of a geological marker reflects *relative* sea level change – that is, the change in the sea surface height relative to any crustal elevation change. In modern sea level studies, tide gauge records also reflect the change in the distance between the sea surface and crust, while satellite records measure "*absolute* sea level" changes, i.e., changes in the sea surface height relative to some reference frame (e.g., the center of the Earth).

In the geological literature of long term sea level change there has been an argument that only the change in the sea surface height, or absolute sea level, is important (e.g., http://stratigrafia.org/sequence/accommodation.html) but this doesn't hold if there is any departure at all from hydrostatic (not just isostatic) equilibrium. In an ice age calculation, there is always such a departure because of the adoption of an elastic lithosphere and unrelaxed viscous stresses.

Perhaps the issue arises from the fact that our text does not always make clear that we are speaking of global mean sea level *changes*. To address this, we will revise the text to ensure that all mentions of global mean sea level are connected to the word "change".

Finally, we note that the terms "eustatic" and "eustasy" are being avoided because of the ambiguity associated with their definition (Gregory, et al. "Concepts and terminology for sea level: Mean, variability and change, both local and global." *Surveys in Geophysics, 40,* 1251-1289, 2019).

Line 145. The Brerends et al. ice sheet histories puts more ice on northern hemisphere continents during M2 than geological evidence implies (see above refs).

Yes, the Berends et al. (2019) ice sheet histories certainly suggest significantly more Northern Hemisphere (i.e., NAIS and EIS) ice than other studies. The reviewer's comment does, however, point to another way in which our manuscript can be improved. The normalization procedure we apply to the raw sea level calculation to yield Fig. 4 allows us in principle to scale these figures by any global mean sea level scenario since the sea level calculations are (quasi) linearly related to the total mass flux. As an illustration of this, the figure below compares the normalized sea level map of the NAIS scenario from Fig. 4 of the manuscript ($GMSL_P$ = 33.12 m) with the equivalent map for an alternate NAIS scenario characterized by $GMSL_P$ = 9.2 m. The two maps are nearly identical in areas away from the ancient ice cover. So, in this sense, even if the 33 m scenario is considered unrealistic, the normalized sea level change holds for any other scenario, at least for sites away from the ice cover.

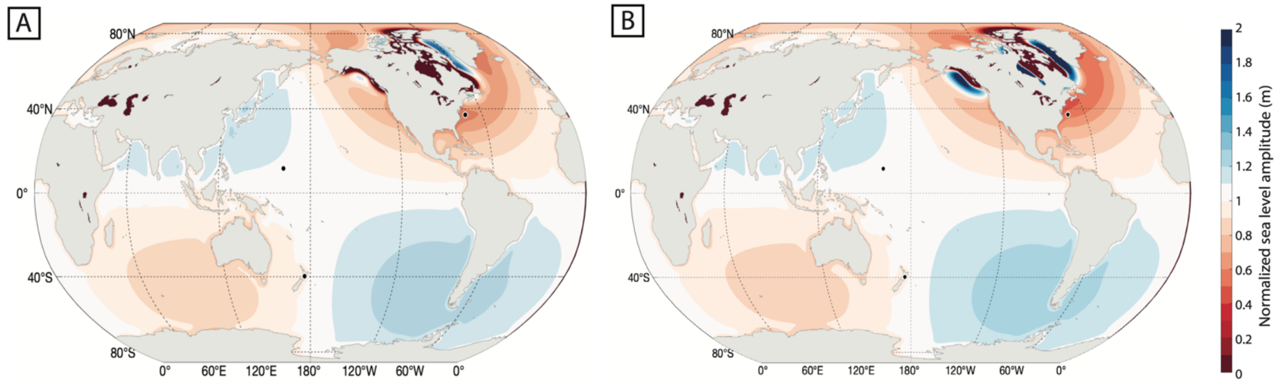

Figure S1. Comparison of normalized sea level maps for MIS M2-KM3 NAIS collapse where $GMSL_P$ is (a) 33.12 m and (b) 9.2 m.

We will include this new material in the main text of the revised manuscript to make the point that the normalized sea level map for the NAIS scenario is not very sensitive to a plausible range in the global mean sea level change assumed for the scenario.

Line 160. Uses the assumption that L&R05 is valid time variation in global ice volume for this time interval. Other studies have raised concerns about the frequency and amplitude of the stack between 3.3-3 Ma, where d18O records are low in number and resolution, leading to potential artefacts through the stacking process (Patterson et al., 2014, Nature Geo; Grant & Naish, 2021, Pages). Note that the highest resolution d18O record during this time interval is dominated be precession (ODP 846-849), as it should be due to a node in obliquity in the orbital solution. Note also the Grant et al 2019 sea-level record which is independent of the L&R05 stack correlates strongly with high southern latitude insolation dominated by precession.

Whereas the last comment dealt with the net magnitude of the global mean sea-level change between MIS M2 to KM3, this comment is focused on the details of the sea-level oscillations between these two times. These details may be subject to error, as pointed out by the reviewer. Calculations performed as we prepared our original manuscript indicate that the multiple small oscillations have only a minor impact on the normalized sea level maps of Fig. 4. The primary control on these maps is the time duration between MIS M2 to KM3, which we have modeled as 140 kyr. In the revised manuscript we will make this point by including a series of supplementary calculations in which the magnitude of the multiple smaller sea level oscillations between MIS M2 and KM3 - and the total time difference between these stages – are varied to explore the impact of the associated normalized sea level maps.

Note also that the proxy ice sheet histories for this time interval are not well constrained by d18O or the Berends modelling, or the ice berg rafted debris records from Antarctica and the Arctic (this should be brought into the discussion). Certainly high latitude northern hemisphere IBRD records (N Pacific, Norwegian Sea, E Greenland) show no continental scale NH ice sheet margins (as implied by Berends et al), with the exception of Greenland. This is why there should be caution taken rather just than accepting Berends et al., as a “series of Pliocene-realistic ice geometries” (line 144).

The new material (and figure) discussed above will emphasize that uncertainties associated with the size of Pliocene NH ice sheets will not map into large uncertainties in the the normalized sea level-change maps. As part of this revised text, we will include a discussion of proxy records that argue against the existence of a large NH ice sheet.

Nevertheless, we note that since we model each ice sheet individually to produce the normalized maps of sea level change in Fig. 4, the reader can consider scenarios that do not include NH ice sheets (NAIS and EIS) and only consider the traditional sources of Pliocene ice melt (EAIS, WAIS and GrIS; as we do, as case studies, in Fig. 6a and 7a).

Line 250. The age model for Enewetak Atoll has always been very uncertain. Table 3 of Wardlaw and Quinn paper is very hard to understand. The "mid-Pliocene" 3.6-3.5 Ma range of RSL change is -33m to +25m (extreme). This is not an equivalent interval to the M2-KM3.

Miller et al. (2012) cites the Enewetak Atoll backstripped record as having "peak sea level values among the three backstripped records are similar in the interval between 2.7 and 3.2 Ma (10-18 m in Virginia, 15-20 m in New Zealand, 20-25 m in Enewetak)." The age model is poorly constrained (3.0 + 0.5 Ma) and is averaged with 2.99 Ma peak observed in astronomically dated proxies.

But we emphasize that our study does not focus on peak sea level (and makes no assertion regarding the accuracy of estimates of this peak) but rather on the sea-level change across the MIS M2-KM3 stages of the mid-Pliocene and we merely use Enewetak Atoll as an illustrative site since there has been no published estimate of this sea-level change. Our maps indicate that if this sea-level change is constrained in the future it would overestimate $GMSL_P$ by ~10%. This is one illustrative example – the reader can make the same assessment of the difference between the local sea-level change and $GMSL_P$ for any site of interest given the results in the manuscript.

If I understand correctly this paper is modelling for the interval M2 to KM3 which is 3.3-3.15 Ma. In Grant et al P*lioSeaNZ* record this represents a change from +3 to +23m ESL (an overall increase of 20m over 8 eccentricity-modulated precision cycles). However, it should be noted that the PlioSeaNZ record floats, and while it constrains the amplitude and timing of ESL during G-I variability, it is only registered to present day sea-level through an assumption (see final paragraph). This paper would be greatly improved by the inclusion of a table showing the data used for amplitude and age of sea-level changes in the proxy records (e.g. Whanganui Basin, Enewetak Atoll).

We agree, but only to a limited extent. Our paper provides global maps of normalized sea-level changes across the MIS M2 to KM3 interval associated with various ice melt scenarios. Our aim is to provide any reader with the ability to assess the possible bias introduced by assuming that a local sea change across this interval faithfully records the global mean sea-level change, and we present some illustrative examples to emphasize the power of using these normalized maps for this purpose. We do not make assertions regarding the validity of any previous estimates of local sea-level changes across the interval and we will revise the manuscript to ensure that this point is emphasized. In this context, including information regarding data collected at Enewetak Atoll is not relevant to our study since there has been no published estimate of the sea-level change at this site across the MIS M2 to KM3 interval. In contrast, we do use the one site where such an estimate has been made – Whanganui Basin (by the reviewer and colleagues!) – and we will include more information regarding the observations that underpin this estimate.

However, we emphasize again, that any mapping between the local sea-level change at this site and $GMSL_P$ that one can assess using our normalized maps – i.e., the 20% difference noted in our abstract – will *not depend on the observed sea-level change*. Placing too much emphasis on the observations may undermine this important point. Our revised text will make this issue far clearer.

Line 290, The authors claim a 20% underestimation in GMSL (should be eustatic sea-level as stated above) from the Whanganui Basin, PlioSeaNZ record, which is unsurprisingly consistent with their ice sheet histories. However they should note, that Grant et al. 2019 used a series of quite different hypothetical ice sheet histories in their GIA modelling, which were based on best estimates from geological reconstructions, whereby +20 m of ESL was released under 4 scenarios **1**), 20 m ESL released from AIS only. **2**, AIS and GIS synchronously release 15 m and 5 m ESL, respectively **3**, AIS releases 25 m ESL while GIS accumulates 5 m ESL (that is, in antiphase). **4**, AIS and NHIS synchronously release 10 m ESL. In all cases the Whanganui Basin record lay geographically on the eustatic.

Any GIA model with an ice sheet history releasing 70% of the melt water from the northern hemisphere (such as in this paper) will overestimate the Whanganui record. Grant et al 2019 would have done the same had they used the ice sheet history used in Table 1 of this paper, which requires a total of 65m SLE ice is being melted between M2 and KM3. This ice sheet history is not supported by the published geological constraints. For example the total from Antarctic sector loss is greater than the total ice volume currently held in all the marine-based sectors and would require M2 glacial to be larger than present day ice volume, as well as having 45m SLE ice on the northern hemisphere continents.

We must emphasize that our manuscript does not argue for the accuracy of any specific melt scenario, and we will revise the text to ensure that this point is made clear. Table 1 provides the melt from each ice sheet in our various scenarios. These maximum melt scenarios were combined with the reference earth model and the calculate sea level was normalized using $GMSL_P$ to produce the maps in Fig. 4. As we demonstrated above, and will demonstrate in the revised text, these normalized maps can be used to determine what the local sea-level change will be for any net mass flux from each individual ice sheet. As example, per meter of $GMSL_P$ change, melt from North America across MIS stages M2 to KM3 will lead to 0.88 m of sea level change at Whanganui Basin (see Table 1 and Fig. 5). Providing the community with this information is important so that they can test *any scenario* for NAIS flux – or, indeed, flux from any other geographic region – against a future observation *at any site*. This utility is highlighted in our own discussion section where we considered five different melt scenarios that would produce 15 m of LSL change in the Whanganui Basin. These scenarios considered contributions to $GMSL_P$ from NAIS that ranged from 0 m to 9 m. We should also point out that melting from all 8 zones considered in Fig. 5 yielded a local sea level change across the MIS M2 to KM3 interval that was lower than the associated $GMSL_P$ of the melt – not just the NAIS melt – and this consistency is an important result. So, regardless of the mass flux scenario – including the 4 scenarios the reviewer mentions from the Grant et al. (2019) study – the local sea level change at Whanganui Basin will be lower than $GMSL_P$.