This paper is a nice model approach using state of the art GIA model to reconstruct GRD effect son 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 beleive the methodology is flawed, I would like to encourage the authors to consider trying a diffrent 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.

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 presnet day sea-level

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

Line 160. Uses the assumptiion 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 artifacts 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 prcession (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.

Note also that the proxy ice sheet histories for this time inetrval are not well constrained by d18O or the Breneds modelling, or the ice berg rafted debris records from Antarctica and the Arctic (this should be brought into thediscussion). Certianly high latitude northern hemsiphere IBRD records (N Pacific, Norwegian Sea, E Greenland) show no contiental 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).

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.

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 *PlioSeaNZ* 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 in the proxy records (e.g. Whanganui Basin, Enewetak Atoll).

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 70m 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.

Notwithstanding all of the above, paper is a very nice piece of work, and should be published, if the authors can consider the following. That the…

1. ice sheet history used based on Berends et al is a modelled outcome and not supported by the weight geological data. Therefore, the result of this modelling exercise is a function of the ice sheet history used rather than a true test of the far far-field sea-level proxies.
2. The Enewetak sea-level reconstruction is for a different part of the Pliocene (not M2 to KM3), it is like comparing apples with oranges.