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Interactive comment on "Deglacial abrupt climate changes: not simply a freshwater problem" *by* Jorge Alvarez-Solas et al.

Anonymous Referee #1

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The authors present an intermediate complexity climate model study of the last deglaciation, particularly focussing on the effect of sea-level constrained meltwater scenarios on the Bolling Warming event ∼14.5 ka. The authors scrutinise the freshwater forcing scenario employed in the TRACE-21k simulation (Liu et al., 2009), and find it to be unrealistic. Using what they suggest are more realistic meltwater forcing scenarios to drive their climate simulations, the authors conclude that meltwater forcing cannot explain the Bolling Warming.

Mostly, the manuscript is well written and easy to follow, although some of the figures were difficult to understand due to having incomplete captions, or captions with errors. These need going through carefully to correct them and add in all of the relevant information so we know what the figures show, e.g. see specific comments below).

However, parts of the manuscript are too vague, disjointed or are set up to be contradictory. For example, the introduction is initially laid out to focus on D/O events, but this is never followed through and the manuscript really just becomes about TRACE21k simulation of MWP1a and the Bolling Warming, which is a much more limited scope than is implied at the start. Generally, the authors are overstating the significance of the work; e.g. in the abstract 'revealing an inconsistency between the generally accepted FWF mechanism and the resulting climatic impacts'. This is not true anymore. Even in 2006, Stanford et al. noted: 'clearly, deepwater formation and climate are not simply controlled by the magnitude or rate of meltwater addition.' Plenty of studies have discussed alternative mechanisms (including those referenced in the present manuscript) and my view of the current state of the science is a general acceptance that alternative forcings/mechanisms are likely required. That's not to say freshwater isn't important, and it is also true that the very famous TRACE21k simulation uses unrealistic meltwater forcing, as demonstrated here but many, many groups (admittedly not all) have moved beyond that way of thinking. In this sense, the present manuscript is also out of date in its framing of the problem and has over simplified the issue. The resulting narrative is, therefore, misleading for representing the current paradigms of climate forcings of the Bolling Warming, and its relationship to meltwater pulse 1a.

A further misleading aspect of the manuscript is that several premises are set up early, only to be subsequently knocked down in the 'Discussion' based on existing literature. For example, page 2 paragraph 2: 'FWF applied in these studies. . .in detail' and 'this issue has largely passed on unnoticed and deserves further attention': this is contradicted later (e.g. where the following are cited: Bethke et al., 2012; Ivanovic et al., 2018). I suggest removing it, or summarising those results (summarised later) here. On page 7, line 17-20: no, a clear comparison has been made that demonstrates this same point, e.g. for HS1 (Ivanovic et al., 2018). Another example of this is that it is implied that AAIS could be a helpful source of meltwater for triggering the Bolling Warming, but subsequent discussion in Section 4 demonstrates that this is already known to be unlikely: [paraphrasing] that Weaver et al. (2003) required AMOC to be shut down already for AMOC to strengthen in response to AAIS meltwater, but that this requires an hugely unrealistic freshwater flux (the manuscript states more than 5 × what is feasible) based on sea-level before the MWP1a, and that an AAIS meltwater pulse is not very efficient or long lasting and so would not have much impact. Similarly, the cited fingerprinting work (Clark and Mix, 2002) has been superseded (e.g. Gomez et al., 2013; Liu et al., 2016). That discussion needs to be laid out up front (i.e. page 3 where the concepts are introduced) so that the reader has all of the relevant information, otherwise the manuscript runs the risk of setting up a disingenuous premise. (In my opinion, the discussion at the bottom of page 5 is too brief and misses important relevant points for the AAIS contribution argument. It needs more info on the sources and constraints on sources of meltwater, and needs moving to the introduction.)

I really do not understand the philosophy behind the methods for creating the plausible meltwater scenarios based on sea level. My understanding of the literature is that Stanford et al. (2011) used a Monte Carlo method to fit curves through observations of sea level, producing 99, 95 and 67% probability envelopes. The authors are using the derivative of these statistical envelopes, which has no physical basis as far as I can tell. No glaciological evidence is used to back-up the scenarios, and more recent data (Lambeck et al., 2014) is not used. Furthermore, there has been some suggestion that some of the ages of the Stanford et al. (2011) paper need to be corrected (e.g. Barbados), which would have a major effect on the derived freshwater fluxes. Therefore it is highly questionable whether the scenarios presented here as being compatible with sea level data are even plausible. In this case, they are not really an improvement on the Liu et al. (2009) approximation in terms of realism, and I am struggling to understand what we actually learn from it (other than that a different freshwater scenario produces a different climate response, with more Northern Hemisphere freshwater reducing AMOC. . .which is already very well demonstrated). So I am concerned that the experiment design is quite fundamentally flawed.

A further concern is whether CLIMBER is capable of simulating the correct oceanic

and climate response to detailed freshwater forcing. The model dependency of these results is not discussed at all.

FURTHER SPECIFIC COMMENTS

A few sentences in the abstract are very subjective and in my opinion are just wrong, even from reading the later text: ‘an issue that has passed unnoticed’ -> no, this has been noted by others, e.g. the cited literature (Bethke et al., 2012; Ivanovic et al., 2018). ‘revealing’ -> no, as mentioned above and discussed in the text, this inconsistency has been noted before (Carlson and Clark, 2012; Stanford et al., 2006). ‘we propose that the trigger. . .should be explored more than ever’ -> not only is it being explored more than ever (e.g. Klockmann et al., 2018; Obase and AbeâĂŘOuchi, 2019; Zhang et al., 2014, 2017), but this fact is already known and based on these two aspects the sentence is virtually meaningless. I suggest removing all three of this segments.

Page 2, line 35: suggest not only relying on McManus et al., but the recent basin-wide Pa/Th compilation, which includes McManus et al. (2004) data, but on an updated age model and with much more data. Also, what about other ocean circulation proxies, e.g. Roberts et al. (2010).

There are a few notable omissions in the cited literature: The new manuscript by Obase and Abe-Ouchi (2019) should be incorporated into the narrative of the manuscript. The more recent compilation of sea level data presented by Lambeck et al. (2014) has not been used – why not? At the very least this needs to be justified because it would provide additional important constraints on the sea level scenarios used for freshwater forcing, and validation of the scenarios presented here.

I don’t think any of the figure panels are labelled (a, b, c. . .)

Page 4, line 7-8: none of the figures show both the sea-level and ice-sheet reconstructions alongside the derived freshwater scenarios. Furthermore, the way the scenarios have been derived is unphysical, I don’t think it makes sense – see comment above.

Page 4, line 18: The text suggests that a comparison between the presented results and those of Liu et al. (2009) is shown in Figure 1a – as far as I can tell it is not, but it would be a useful addition. Please add the data to the plot.

Page 5 line 4: 'slight recovery'. Looks like quite a good recovery to me, from the plot I'm estimating it's back to $\sim$75% strength from shut-down!

Page 5 line 4: 18.5 ka (rather than 17.5 ka)?

Page 5, line 6: 'to the data' -> what data? This is very vague and over-simplified. Same for 'These results remain robust' The early evolution is quite different, so what is robust.

Page 5, line 16: Lambeck et al. (2014) is more recent than Stanford (2011).

Figure 3: it is hard to see all the gray shades and the caption does not explain what they are or how they were derived. Most of the captions do not explain all parts of the figures so they cannot be completely understood.

Page 5, line 18: 'the most likely reconstruction for Stanford. . .' this seems like an over-simplification of what those data actually mean. See comment above on the technique for constructing these scenarios. I'm not even sure that they are temporally coherent in a physical sense, and so can't be used in this way – it doesn't make sense. In this same sense, 'none of these curves. . .' (line 28-29): I'm not sure these are actually [individual] sea level curves. Page 6 line 11: 'median value' -> but this has no physical meaning/basis, so it can't be considered physically realistic/plausible either.

Figure 4: caption is wrong, I think. I don't think the data presented by Liu et al. (2009) are shown. If it is, I don't understand this plot and how it corresponds to the caption.

Page 8, line 16: 'strong evidence that the AMOC did shut down during this time period'. This is not backed up. What is the evidence? E.g. the original McManus (2004) interpretation has been revised. It could demonstrate shoaling instead of weakening, and doesn't need to wholly shut down (Bradtmiller et al., 2014), so I don't think this is correct.

Page 8, line 24: 'FWF whose integral matches the available sea level records'. First, I am not convinced this is true and I think the Stanford data has been misused if this is what the aim was. Second, there is more available data that was not used and would provide further constraints. In summary, the case has not been well made that the presented scenarios are at all realistic based on sea level (and glaciological) data.

Page 8, line 26: 'MWP1-A helps' -> not according to section 4. Contradiction here.

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