In this manuscript, the authors seek to establish the existence and timing of ‘plateaus’ in the atmospheric radiocarbon record, and to demonstrate that these are also present in marine records from around the world. On this basis, the authors seek to argue that radiocarbon plateaus identified in marine records can be stratigraphically aligned to correlative plateaus identified in the atmospheric record, allowing calendar ages to be transferred to the marine records (and therefore allowing for ‘marine reservoir age’ offsets to be determined). This method of chronostratigraphic alignment has been termed ‘plateau tuning’ (PT).

This manuscript is quite unusual, as it does not appear to advance any new observations/data, arguments, models or insights. Some adjustments are made (again) to the proposed timing of plateaus identified in the atmospheric radiocarbon record, but this does not really make any difference to what has been proposed by the authors in several papers since 2007. A recent ‘review’ of the ‘PT method’ and its results was published by the authors just last year in this same journal. Primarily, it seems, the manuscript seeks to publish a rebuttal of a prior piece of work produced by Bard & Heaton (B&H) that was also reviewed and published in Climate of the Past last year. The latter was also accompanied by several pages of commentary by Sarnthein and Grootes, which was in turn responded to by Bard and Heaton over the course of the discussion phase of the manuscript.

Unfortunately, I find it impossible to recommend that this manuscript be accepted for publication. There are three main reasons for this: 1) it does not appear to present an
original piece of research, and insofar as it presents adjustments, they are not important enough for publication on their own merit; 2) its arguments against B&H are not coherent (regardless of whether or to B&H are correct); 3) the vast bulk of figures and tables referred to in the manuscript are included in a ‘supplement’ that has not actually been produced/included. On the latter point, the promise of a compilation of all the available PT data in useful tables would have been at least one welcome contribution: but it turns out that the intention of the authors was to include ~20 disparate data tables that are already available on PANGEA and that are not at all useful in reproducing the PT data that have been published to date by the authors (it took me days to do this, and the results are not the same as what the authors have published in many cases, which is both worrying and annoying). The fact that the PT data (and associated MRA etc.) that have been produced by the authors over several years, and presented in a series of ‘global synopsis’ papers, cannot be easily reproduced by others using the multitude of available data tables, is particularly worrisome. The same can be said for the fact that only one (?) PT study exists that does not include the authors of this study (the champions of the PT approach). Incidentally, this might already answer the question of whether or not it is a ‘trend setting’ tool.

With regard to the second point raised above, the authors state that they reject the arguments of B&H based on the basis of how plateaus are identified (i.e. as ‘sequences’, like a sort of Morse code), and on the basis that B&H use a 1998 box model to support their arguments. Regardless of the validity of B&H’s remarks, I don’t see how either of these points represent a coherent basis on which to reject a criticism of the PT method, where that criticism is founded in large part on the proposed difficulty of objectively identifying plateaus (let alone sequences of plateaus) in a noisy marine radiocarbon record whose offset from atmospheric radiocarbon varies over time, as well as the proposal that sedimentary processes (such as simple - and highly likely - sedimentation rate changes during periods such as Heinrich Stadial 1, or the Younger Dryas) can also produce ‘plateaus’ in the 14Cage-depth domain, without these being causally linked to atmospheric radiocarbon variability. In addition, the claim that a 1998 box-model is somehow incorrect because of its vintage seems to miss the point: the key purpose of deploying such a model is surely to illustrate in a very simple way how the phasing and amplitude-attenuation of an input signal will be altered (filtered) in the ocean, depending on the timescale on which the signal can be communicated to the ocean, and the frequency/duration of the signal variability. You can do this with a very complex biogeochemical coupled ocean-atmosphere numerical model if you like, but if it did not show a simple phase-attenuation relationship like the box model, it would mean that the complex model had a problem! In fact, by playing around with numerical model outputs it can be shown that they do show the same principles as a 2 box-model, and that should not be surprising, as it is an expression of a simple and fundamental physical principle: parts of the ocean that have small MRA offsets (such as the tropical ocean, MRA ~400 14Cyears) can respond quickly and can pick up shorter fluctuations from the atmosphere, whereas parts of the ocean that have large MRA offsets will take longer to pick up the atmospheric signal (since a larger MRA means that the isotopic exchange timescale for that water is longer) and will pick up a smoothed and lagged response. The limits of applicability of the PT method could readily be analysed and qualified in such a theoretical context, but the authors don’t do this unfortunately.
Ultimately, the manuscript sets out to answer the question posed in the title: “is the ‘plateau tuning’ (PT) approach a misleading approach or a trend-setting tool”? I would note that, at worst, PT could be both misleading and trend-setting, and my major concern is that the authors clearly wish for it to be the latter, but have not really (either in the present manuscript, or over the course of several publications that appear to present the same datasets repeatedly) demonstrated that the PT approach is indeed viable, either in theory or in practice. As suggested above, this is not to say that some sort of defence cannot be made, in theory at least. But the authors (still) have not managed to do this. My own view is that the chronostratigraphic principles that the authors wish to apply are not completely crazy: yes, the atmospheric radiocarbon record has ‘wiggles’ and these would be transferred to other reservoirs that exchange CO2 with the atmosphere rapidly enough to pick them up. However, the conditions under which these wiggles can be recorded in other reservoirs, such as the ocean, and the biases (in amplitude and phasing especially) that will inevitably and predictably arise (even prior to the complications of sedimentation changes, bioturbation, sampling/analytical noise etc.) need to be accepted and addressed by the authors at some point if this debate is to move in a useful direction. I can think of a variety of ways to test the PT method in theory (using models), and in practice using data, and I wonder why the authors have never done something similar. If a scientific study that achieved such goals was produced, it would be a welcome and useful addition to the literature (as B&H has proven to be, insofar as it stimulates critical thinking). Such a study would best come from the authors of the present study, who appear to be the main (if not the only?) champions of the PT method; however, this is not what the current manuscript provides.