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Comment on egusphere-2022-208

Anonymous Referee #1

Referee comment on "A theory of abrupt climate changes: their genesis and anatomy" by Hsien-Wang Ou, EGUsphere, https://doi.org/10.5194/egusphere-2022-208-RC1, 2022

The manuscript by Ou presents a theory of abrupt glacial climate changes. The author postulates that Heinrich and Dansgaard-Oeschger events have the same nature and are related to abrupt changes in the ice calving into the ocean. In fact, the idea that abrupt climate changes of the glacial age were related to strongly variable freshwater flux into the ocean is by far not new, although one of many and not the most popular now. The only novelty of the proposed theory is that, according to the author, abrupt climate changes do "*not involve ocean mode change, as commonly assumed*" (L. 11). Although the author repeated this statement a dozen times, he did not explain what he understands under "mode change", and this is why it is difficult to assess how much the mechanisms proposed by the author differ from those that have been proposed in numerous previous studies.

The main problem of the manuscript under consideration is that it completely ignores a vast amount of recent studies on abrupt climate change. Excluding self-citations, only two (!) cited papers were published during the past ten years and the absolute majority of cited papers were published in the past century. This "statistic" is in odd with the drastic increase in the number of empirical and modelling studies of abrupt climate changes in recent times. This ignorance about the contemporary progress in understanding the mechanisms of past climate change results in the numerous erroneous statements and premises on which the theory is built, which made the manuscript under consideration totally inappropriate for publication in scientific journals.

General comments

The lack of up-to-date knowledge about the progress in the understanding of abrupt climate change gravely affects the entire manuscript. Normally, the introduction presents the current status of the progress in the related subject and the motivation for the presented study. Instead, the author used the introduction to introduce his own theory of abrupt climate changes. Already the first paragraph contains numerous inaccurate and

erroneous statements:

The author claims that both Heinrich events and Dansgaard-Oeschger cycles *"are all accompanied by ice-rafted debris (IRD) ... suggesting a common origin in the calving of ice sheet due to thermal switch at its bed*". The author referred here to a very old paper (Bond et al. 1997), based on a very coarse resolution record. In fact, the situation with IRD during Heinrich and non-Heinrich stadials is very complex. The late are found only in some locations and may imply better survival of icebergs in colder climates rather than an increase in their production. Moreover, there is a potential time lag of IRD compared to the onset of stadials (e.g. Barker et al., 2015), which contradicts the idea of freshwater forcing of stadial events. Thus, it is very likely that IRD during stadials represents the response rather than the forcing of abrupt climate changes.

The next sentence (L. 35) - *"since recurring time of calving is constrained by ice mass balance, the resulting freshwater flux is naturally availed the millennial timescale, a timescale not inherent to the ocean"* - is even more problematic. First, the author does not explain why recurring time of calving is constrained by ice mass balance and why it should be millennia. The last part of the same sentence, namely, that the millennial time scale is not inherent to the ocean, is just wrong. Numerous recent studies demonstrate that the ocean alone can produce millennial-scale self-sustained oscillations resembling Dansgaard-Oeschger events (Peltier and Vettoretti, 2014; Brown and Galbraith, 2016; Klockmann et al., 2018). In fact, this finding is not so new: the existence of millennial-scale self-sustained AMOC oscillations has been demonstrated already in Winton and Sarachik (1993).

The first paragraph ends with another erroneous statement: *"variation in the sea-surface temperature (SST) remains well short of mode change except during deglaciation"*. The authors did not explain which model is meant here, but I guess this is the "ocean mode." Then, the author explains what "short" means: *"SST variation is considerably smaller, ranging in low single digits"* (L. 228). In fact, SST variations in the Northern Atlantic during DO events were about 5°C (e.g. Martrat, 2007; Alonso-Garcia et al., 2011) which is fully consistent with what climate models simulate in response to the ocean mode change, namely a complete AMOC shutdown (e.g. Jakson et al., 2015)

In the next paragraph, the author postulates that H-cycle is related to *"to calving of the inland ice"*, whilst *"for DO-cycle it is the surface melt over the ablation zone that causes calving of the marginal ice"*. Unfortunately, the author does not explain why he believes thy HE events are associated with calving of "grounded ice" and what "marginal ice" means in the case of DO events.

As far as a very stimulating McAyeal's concept of binge-purge oscillations is concerned, the author should be aware that it has been advanced over the past decades significantly from 1-dimension to 3-dimensional case and evolved apart from geothermal and frictional heat, also strain heating, basal hydrology, activation/deactivation waves, etc., (Calov, 2002, 2010; Roberts et al., 2016; Feldmann and Levermann, 2017; Schannwell et al., 2022).

On pages 11 and 14, the author, at last, mentioned alternative mechanisms of abrupt climate changes. He wrote: *"H-cycle has been modelled as ocean mode change (Paillard 1995; Ganopolski and Rahmstorf 2001), which is unsupported by observation"*. Which observations do not support "mode change", the author did not explain. In turn, I am aware of numerous paleoclimate records which support qualitative AMOC changes during Heinrich events and non-Heinrich stadial (e.g. Lippold et al., 2009; Böhm et al., 2015). In the next sentence, based on Equation (16), the author claims that SST change should be *"proportional to the freshwater perturbation"* (L. 313). This, however, contradicts a vast amount of modelling studies which show a strongly nonlinear response of AMOC and SST to freshwater perturbation. They also demonstrate that abrupt climate changes can be caused by a very gradual forcing or even without any external forcing. Moreover, simulated self-sustainable oscillations resembling DO events have typically periodicity of one to several millennia. Since I have no reason to believe in Equation (16) more than in the results of realistic climate models, I cannot consider the proposed theory as a valid alternative to the modern concepts of abrupt climate changes.

Three pages later, the authors criticized results obtained with very simplistic models (Sakai and Peltier, 1999; Schulz and Paul, 2002), but similar results have been later obtained with much more realistic climate models (see above). Interestingly, here the author explicitly assumes that there were Dansgaard-Oeschger events during the Holocene, which is, of course, wrong - the last DO event (Bolling-Allerod) occurred well before the onset of the Holocene.

As far as the explanation of the deglaciation is concerned, one only can wonder why the author placed meltwater pulses 1A and 1B ca. 3000 years later than they happened in reality (Fig. 10).

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