The manuscript by Pollack et al. presents the results of global ice volume simulations performed using a model based on the Parrenin and Paillard (2012) model. The reported results represent incremental progress compared to those presented in the recent Legrain et al. (2023) paper. The main novelties are the increased number of model parameters, a more advanced procedure for model calibration, and the simulations of future glacial cycles. Below I will not only review the manuscript by Pollack, but also present my more general appraisal of their modelling approach.

General comments

Conceptual model. The model used in this study originates from the Paillard conceptual model of glacial cycles (Paillard, 1998). This model was then considerably revised by Parrenin and Paillard (2012) (hereafter referred to as PP12). Firstly, the authors of this paper abandoned Milankovitch theory of glacial cycles and delegated the determination of the ‘orbital forcing’ to the optimization algorithm. Secondly, they introduced two completely different ‘orbital forcings’: one determines the linear response of the global volume while another one determines nonlinear response, namely transitions between glacial and deglaciation regimes. However, since they tune the model only to glacial cycles of the late Quaternary, both ‘orbital forcings’ at least qualitatively resembles each other and boreal summer insolation. Unfortunately, the authors did not explain why they need two orbital forcings instead of one. In Legrain et al. (2023) (hereafter referred to as L23), the same model has been applied already to the entire Quaternary, and the model parameters were fitted to Berends global ice volume simulations. Since Berends ice volume contains very little precession even for the late Quaternary, the result of such tuning is easy to understand. The ‘linear orbital frorcing’ (I_α) is dominated by obliquity while ‘nonlinear orbital forcing’ (I_k) by presession. (How such a situation could occur in the real world is not explained). Without strong precessional component in I_k, it would not be possible to simulate glacial cycles of the 100-kyr world, but too much precession disturbs the 41-kyr world. This is why, the optimization algorithm chooses model parameters for the early Quaternary in such a way that another central element of Pallard’s conceptual model - the existence of two distinct glaciation and deglaciation regimes - also vanishes. As can be seen in Fig. 2, the model switches several times between glaciation and deglaciation regimes during a single glacial cycle. As the results, what is described in the paper is not a conceptual model but rather a mathematical immolator of glacial cycles, where a good agreements with ‘reconstructions’ is achieved by using 16, 17 or even 19 model parameters, the physical meaning of which is hard to understand.

Model description. Although the model used in the study has been described in several papers, some aspects require clarifications and corrections. Firstly, it is unclear whether conditions for regime changes denoted by (i) and (ii) should be met simultaneously. According to PP12 it seems that both conditions must be met :

$$\begin{aligned}&\mathbf{g-to-d}: \kappa_{\mathrm{Esi}}\mathrm{Esi} + \kappa_{\mathrm{Eco}}\mathrm{Eco} + \kappa_{\mathrm{O}}\mathrm{O} + v > v_0,\\ &\quad(\text{and } \kappa_{\mathrm{Esi}}\mathrm{Esi} + \kappa_{\mathrm{Eco}}\mathrm{Eco} + \kappa_{\mathrm{O}}\mathrm{O} \geq v_1)\end{aligned}$$

but it is unclear to me why the second condition is in brackets.

Secondly, the model employs two different times denoted by the same letter ‘*t*’! One time, used in the differential equation for ice volume has ‘physical’ (i.e. normal) direction, while the second time, which determines the evolution of model parameters, goes in the ‘paleo’ (i.e. reverse) direction. This is very confusing. Using of the same notations (v_0 and τ_d) for two different characteristics (one constant and another time-dependent) is also not a good idea.

Improved model. The title of the paper begins from “*An improved conceptual model*”, but, surprisingly, it is not clear from the manuscript what they meant under “improved model”. The manuscript makes an impression that the ‘improved model’ is RAMP-l but in page 27 it is written ‘*we* *constructed an improved conceptual model of Quaternary global ice volume, including four different internal forcing scenarios, alongside another model configuration... We showed that the RAMP-l model...*' I am not sure how this should be interpreted.

The improvements compared to L23 are also not clearly described. Only after the comparison of the equations in Pollak and L23, one can understand that 'deprecated truncation function' means that the authors decided not to use truncation of insolation introduced in Paillard (1998) and then uses in PP12 and L23. Why the authors consider this an improvement is not explained. At the same time, the author introduced a new time-dependent parameter td which is the relaxation time scale during deglaciation and which is in Paillard (1998) model determined the duration of glacial termination. However, what is the meaning of this parameter in Pollak et al. is unclear: during the early Quaternary its value in GRAD, RAMP and RAMP-l models is about 40 kyr, i.e. close to the duration of the entire glacial cycles. Even worse, in ABR model version, the relaxation time scale is -113 kyr, but the 'relaxation time scale' cannot be negative by definition. The meaning of negative v_0 in the RAMP model is equally hard to interpret.

Insolation. According to the Milankovitch theory, changes in boreal summer insolation is the driver of glacial cycles, but since PP12 abandoned Milankovitch theory, it is rather surprising that the term 'insolation' appears in section 3.5. The term 'insolation' has a very clear meaning: 'insolation' is the abbreviation for 'incoming solar radiation' and is measured in W/m^2 (or equivalent units). The 'orbital forcings' $I\alpha$ and I_k used in the paper have nothing to do with the real insolation and therefore the terms 'insolation' and 'insolation maximum' in the context of the paper is misleading.

Ice volume reconstruction. The author used the Berends reconstruction of global ice volume. Like any other reconstruction, the Berends reconstruction contains significant uncertainties and deficiencies. For example, for the Last Glacial Maximum, for which there are numerous independent data, Berends underestimates global ice volume by more than 20% and also underestimates ice volume variability during MIS5. For MIS3 different reconstructions for global ice volume range between 30 and 90 meters (Farmer et al., 2023 PNAS), i.e. the uncertainties are about 50%. It is very likely that for the early times, the uncertainties are even larger. This makes reported improvements in RMSE order of several meters completely insignificant. Even more strange to see 6, 7 and even 8-digit numbers in Table A1. It is clear that so many digits originate from Monte Carlo, but in natural sciences, it is customary to report only significant digits. The paper also says nothing about the robustness of the modelling results with respect to the choice of model parameters.

Simulation of MPT. The main objective of the paper (similarly to L23) is to simulate MPT. Clearly, MPT cannot be explained by orbital forcing alone; therefore, the purpose of the ORB version is unclear. Secondly, even without modelling, it is clear from data analysis alone that the MPT (Mid-Pleistocene Transition) was a transition, not an event, and that it lasted for at least several hundred thousand years. This is why the purpose of the repeating of ABR scenarios is unclear. Regarding the mechanism(s) of the MPT (which are still debatable), it is unclear how the experiments presented in this manuscript can shed light on the cause of the MPT transition. The problem with strongly nonlinear systems, such as the Earth system, is that even gradual changes of the controlling parameters can cause a rather abrupt regime changes (e.g. Willeit et al., 2019). It is quite possible that the gradual decline in CO_2 and landscape evolution (including regolith removal) began not only well before the MPT but also before the Quaternary, and there is no way to derive this from the experiments described in the manuscript.

The role of precession. Firstly, the model used in the study of Pollak cannot be used to study the role of precession and obliquity. This is not a physically-based model and the model parameters are just chosen by the optimization algorithm in such a way that glacial termination of late Quaternary occur in the right times. And this 'right times' coincide with the periods of rising boreal summer insolation.

In turn, boreal summer insolation is dominated by precession. This is why the optimization algorithm picked up precession.

Secondly, the fact that glacial terminations of the late Quaternary are mainly determined by precession has been known well before Barker et al. (2025). Already Raymo (1997) noted that ‘the length between subsequent terminations is either four or five precessional cycles long’. This idea was further developed by Ridgewell et al. (1999) who wrote that ‘the spectral signature of δ^{18}O records are entirely consistent with Milankovitch mechanisms in which deglaciations are triggered every fourth or fifth precessional cycle’.

Future glacial cycle simulations. The authors also used their models to simulate future glacial cycles. According to these simulations, the next glacial cycle has either already begun or will begin soon in the absence of anthropogenic influence. The authors are, of course, aware that this contradicts to the results of the physically-based models and is therefore likely to be incorrect. This is why they attempted to defend their model by arguing that in Ganopolski et al. (2016) glacial inception occurs with the current orbital forcing if pre-industrial CO_2 were 240 ppm. This is absolutely correct, but among many uncertainties, there is one thing which we know for sure – the preindustrial concentration was 280 ppm, and this value is typical for post-MBT interglacials. Therefore, the unprecedently long Holocene is the robust and most striking feature of the next 100 kyr. Since this feature is not reproduced by Pollak et al., the value of such modelling exercises is called into serious doubt.

Model performance. The authors compared their version of the PP12 model with the L23 model, as well as the 19-parameters RAMP-l with the 16-parameters RAMP and using BIC, they claimed significant improvements. However, the MiM (minimal model which I reported in my 2024 paper), which is a simplification of the Leloup and Paillard (2022) model (which in turn is a simplification of original Paillard 1998 model) simulates the last 800 kyr (arguably the most interesting and difficult period of climate history) as good as RAMP ($R2=0.73$). But MiM has only 4 parameters (nondimensional version of MiM described in Ganopolski 2024 has three parameters). By contrast, RAMP uses 12 parameters to simulate the late Quaternary. The natural question is for what purpose we need models with so many parameters and what can we learn from them.

Specific comments

L10. The meaning of ‘*internal focing*’ is unclear. The authors prescribed temporal evolution of several model parameters, not forcing.

L.12 ‘*support the idea of a long-term climatic shift as a cause of the MPT*’. In fact, MPT is the climate shift, whereas the cause(s) of MPT may have nothing to do with climate, like the ‘regolith hypothesis’.

L.131. Unclear what the authors meant under ‘*versatile model*’ since even a slight change in the temporal scenario (GRAD to RAMP) causes significant changes of all other model parameters.

L. 147. ‘*a linear combination of three orbital parameters normalized to zero mean and unit variance that can reproduce the insolation at most latitudes and seasons*’. This is incorrect. An arbitrary combination of these three parameters does not reproduce insolation at any latitude and at any time.

L. 168. ‘*the model can also incorporate an internal forcing mechanism to account for non-linear feedback mechanisms within the climate system*’. Which forcing and feedbacks are meant here is unclear.

Table 3. Why number of model parameters in Table 3 does not coincide with Table A1.

L. 460. ‘*each or every second insolation peak resulted in a deglaciation*’. What is meant under insolation peak is unclear.

L. 502 ‘*integrated summer insolation (ISI) reported in the literature*’. Firstly, this is not an appropriate way to refer to the published concept. ISI was introduced by Huybers in his 2006 Science paper. Secondly, the authors should explain how ISI is defined. Thirdly, ISI was introduced by Huybers to support his (former) idea that glacial cycles are paced primary by obliquity; this is why ISI is defined such a way that it is completely dominated by obliquity. But since I_{α}, is also dominated by obliquity it is not surprising that two obliquity-dominated curves resemble each other. What can be learned from such a comparison?

L. 565. ‘*This suggests the presence of non-linearities within the climate system that modify Earth’s response to changes in the orbital forcing. This conclusion aligns with findings from other studies (Clark et al., 2006; Leloup and Paillard, 2022; Berends et al., 2021b)*’. I am just curious whether the authors are aware of the works of Weertman, MacAyeal, Oerlemans and others who discussed how nonlinearities modify the Earth’s response to orbital forcing already during 1970s and early 1980s? As for the Berends et al. (2021b) paper, which the authors cited ten times, it is merely a review paper. Although review papers are useful reference material, especially for early career scientists, citing them cannot substitute for reading and citing real scientific papers.