Response to Reviewer 1 (Takahito Mitsui)

March 2026

Manuscript: "Quantifying Resilience in Non-Autonomous and Stochastic Earth System Dynamics with Application to Glacial–Interglacial Cycles"

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We thank the Reviewer for their thorough and constructive review. Below we respond to each comment in turn.

Major Issues

> ### 1. Inconsistency in the model equations.
>
> When $\frac{dv}{dt} \geq 0$, Eq. (3) is given as
>
> $$CO_2 = c_1 T - c_2 v + c_4.$$
>
> Eliminating T using Eq. (4),
>
> $$T = d_1 v + d_2 \log\left(\frac{CO_2}{278}\right)$$
>
> yields
>
> $$CO_2 = (c_1 d_1 - c_2)\, v + c_1 d_2 \log\left(\frac{CO_2}{278}\right) + c_4$$
>
> This expression implies
>
> $$v = \frac{CO_2 - c_1 d_2 \log\left(\frac{CO_2}{278}\right) - c_4}{c_1 d_1 - c_2}$$
>
> which contradicts the dynamical equation for v given in Eq. (1). A similar issue also arises for the case $\frac{dv}{dt} < 0$.

Eqs. (3)-(4) are diagnostic closure relations, not independent prognostic equations. Combining them (as the reviewer does) indeed yields an implicit algebraic constraint linking CO2 (appearing both linearly and through $\log(\mathrm{CO}_2)$) to the ice state (and, for $\dot{v} < 0$, also to $\dot{v}$). This does not contradict the dynamical equation for v (Eq. 1): it simply reflects that the model is a differential-algebraic system in which $v(t)$ is the only prognostic variable, while $\mathrm{CO}_2(t)$ and $T(t)$ are diagnosed from v (and $\dot{v}$ where applicable). We will revise Section 2.2 to state this explicitly and to describe the numerical update procedure used to diagnose CO_2 and T during time stepping.

We have, however, discovered a sign error in our manuscript Eq. (3). The correct expression is

$$\mathrm{CO}_2 = c_1 T + c_2 v + c_3 \min\left(\frac{dv}{dt}, 0\right) + c_4 + \mathrm{AnthCO}_2,$$

consistent with Eq. (7) of Talento & Ganopolski (2021) and our python implementation. We will correct this in the manuscript. With this correction, the reviewer's elimination still leads to an implicit constraint (as expected for a diagnostic closure), but there is no inconsistency with Eq. (1) because CO2 is not an independent state variable.

> ### 2. Relation to previous studies on stability and resilience.

> This reviewer views the work by Harteg et al. as valuable in that it explicitly places glacial–interglacial cycles within the modern framework of Earth system resilience theory. On the other hand, the resilience or (in)stability of glacial–interglacial trajectories has been discussed in several earlier studies.
>
> Finite-time Lyapunov exponents have been used as indicators of transient instability in De Saedeleer et al. (2013), Mitsui and Aihara (2014), and Mitsui and Crucifix (2016). In these studies, the finite-time Lyapunov exponent increases prior to the temporal separation of unperturbed and perturbed trajectories (e.g., Fig. 8 in Mitsui and Aihara, 2014; Fig. 6 in Mitsui and Crucifix, 2016). This behavior appears slightly different from that of the RAR. For instance, the RAR remains nearly constant during trajectory separation near MIS 3 in Fig. 3 of the present manuscript. One might ask whether log(RAR) could serve as a more sensitive indicator.
>
> In addition, so-called potential analysis has revealed temporal changes in attractors or basin structures across glacial–interglacial cycles (Livina et al. 2011). Dakos et al. (2008) discussed conventional early-warning signals prior to deglaciations, although these may not be applicable to strongly non-equilibrium systems. A clearer comparison with these existing approaches would strengthen the manuscript.

Thank you for drawing our attention these relevant previous works. We will strengthen the discussion of these contributions in the manuscript and will also include comparison to previously studied metrics where instructive.

- The RAR appears nearly constant during MIS 3 because, for the threshold used in Fig. 3, most perturbed trajectories are already outside the threshold distance from the reference trajectory, so the fraction is close to zero and varies only weakly. As shown in Fig. B1 (RAR across a range of thresholds), a clearer change in return behaviour is visible around MIS 3.
- Following the reviewer's suggestion, we examined $\log(\mathrm{RAR})$ (Figure R1 below). It increases sensitivity when $\mathrm{RAR} < 1$ but does not alter the qualitative interpretation; we would therefore keep RAR as the primary representation in the manuscript.
- We have computed the Finite-time Lyapunov exponent as given in Mitsui & Crucifix (2016):

  $$\lambda_\tau(k,t) = \frac{1}{\tau} \log \frac{|\tilde{v}_k(t+\tau) - v(t+\tau)|}{|\tilde{v}_k(t) - v(t)|}.$$

  where $v(t)$ is the reference trajectory and $\tilde{v}_k(t)$ the stochastic trajectory. This is plotted in Figure R2 and shows that the stochastic trajectories converge to the reference trajectory at interglacial onset and diverge again shortly after. Around MIS 3 there is a signal, the small magnitude of it is due to the much greater convergence that happens during the interglacial stages. We will add this figure (for $\tau = 10$kyr) to the paper as one such comparison to previously studied measures.
- Relation to potential analysis and early-warning signals (Livina et al., 2011; Dakos et al., 2008): We will add a brief discussion clarifying that these approaches infer basin/stability changes from time-series statistics, whereas our contribution is complementary: we examine *path stability* of a forced, non-autonomous trajectory via finite perturbations and quantify return behaviour.

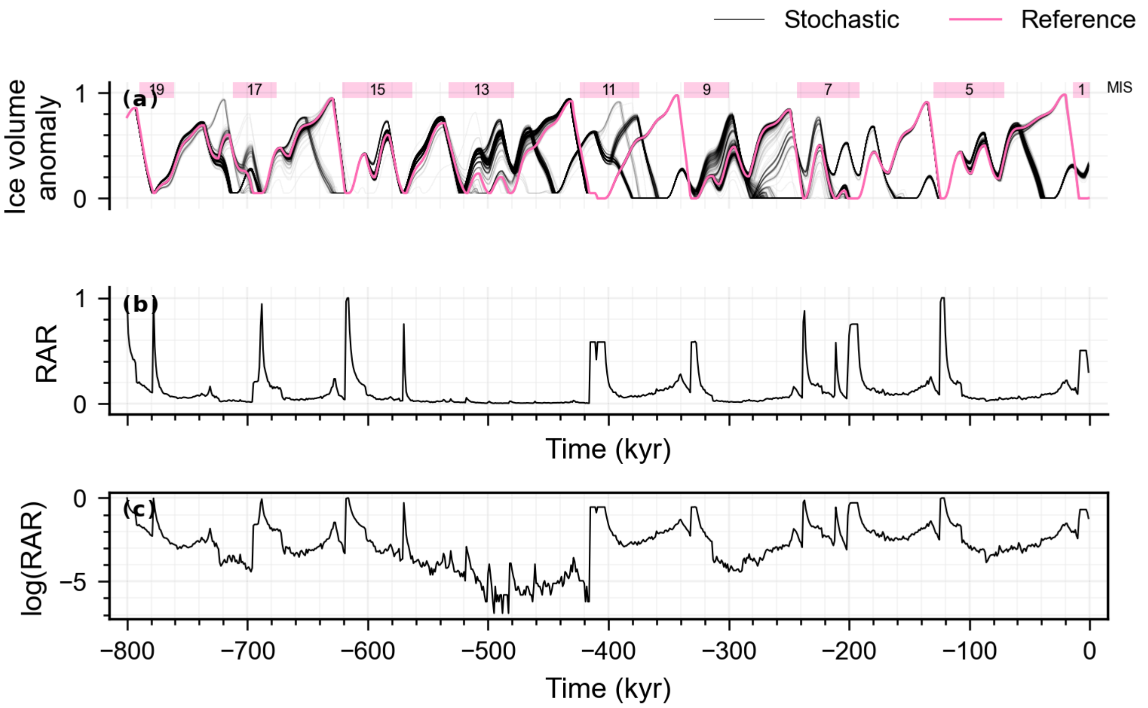

Figure R1: RAR compared to log(RAR).

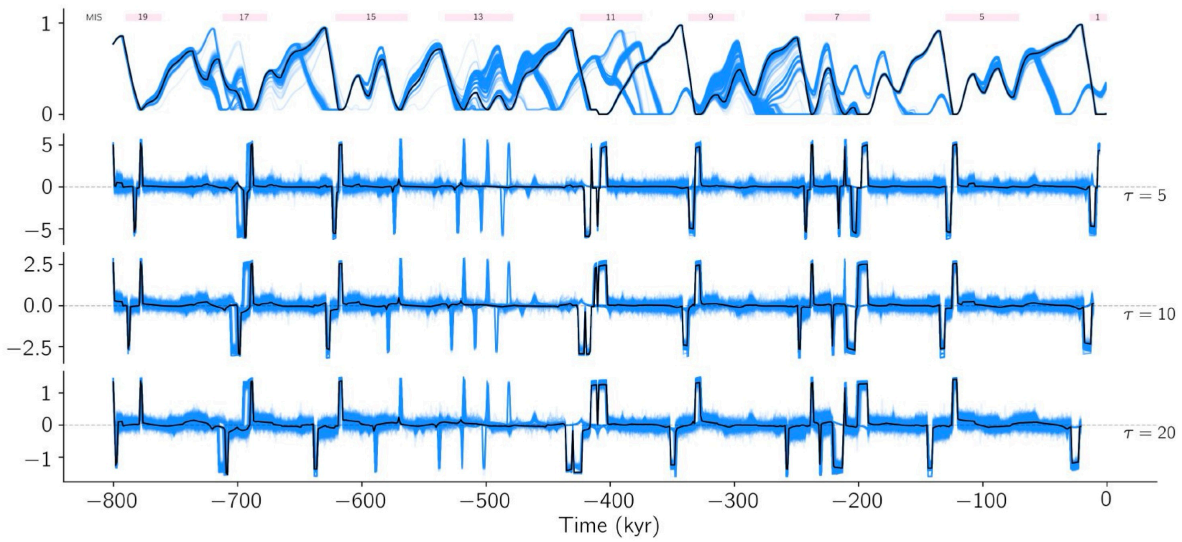

Figure R2: Ice volume trajectories are shown in the top plot (blue shows the stochastic trajectories and black the baseline). The remaining plots show the finite-time Lyapunov exponent for time window sizes $\tau = \{5, 10, 20\}$ kyr.

3. Importance of the proposed resilience measures.

In previous studies, finite-time Lyapunov exponents have been used to explain the existence of periods during which the simulated ice-age trajectory is vulnerable to perturbations. Although this article emphasizes the necessity of path-wise resilience concepts, it remains unclear how the proposed RAR and the return time can be used in practice. For example, the reviewer wonders how these measures can be useful for assessing the safety of long-term storage and disposal of nuclear waste, which was one of the original motivations of the work by Talento and Ganopolski (2021), whose model is adopted in Section 2. If this question is beyond the scope of the present study, it can be safely ignored.

We agree that the specific choice of indicator is not the central contribution. The aim is to introduce and demonstrate a path-resilience perspective for a non-autonomous glacial–interglacial trajectory, for which many classical (autonomous, attractor-based) resilience indicators are hard to interpret. RAR and return time are therefore used as simple illustrative diagnostics: in practice, they highlight phases of comparatively higher or lower robustness to finite perturbations (rapid re-convergence vs. sustained separation). We do not claim direct applicability to nuclear-waste safety assessment; this is beyond the scope of the present study and we will clarify this in the revised manuscript.

Minor Issues

> Section 3.1.3: This study assumes a unique deterministic trajectory in the absence of perturbations. However, previous work has demonstrated the possible coexistence of multiple trajectories (e.g., De Saedeleer et al. 2013). Moreover, in more complex climate models (including CLIMBER-2), deterministic trajectories can be chaotic and may depend on initial conditions, although they may remain largely synchronized with astronomical forcing. In such cases, defining a reference trajectory vref (t) becomes non-trivial. This issue does not necessarily undermine the usefulness of the RAR; however, it would be helpful if the authors discussed how the proposed framework could be applied in such situations.

We agree that a single "reference trajectory" is not always uniquely defined. Different parameter choices (as in Talento & Ganopolski (2021)) naturally give different deterministic trajectories, so in our analysis $v_{\mathrm{ref}}(t)$ is always meant for a fixed parameter set. A harder case is when, even with fixed parameters, the system can have multiple coexisting trajectories or be chaotic and depend on initial conditions. In that situation one could define $v_{\mathrm{ref}}(t)$ for a chosen initial condition, or define it from an unperturbed initial-condition ensemble (e.g. using the mean/median and the ensemble spread as a tolerance). We will add a short note about this in the discussion.

> Line 121: "In the model, orbital forcing f represents maximum summer insolation at 65◦N." Conventionally, maximum summer insolation at 65◦N refers to the mean daily insolation at the summer solstice, for which the relative contributions of obliquity and climatic precession are defined a priori. In Eq. (5), however, their relative weights are tuned. This distinction should be clarified.

We agree with the reviewer's distinction. In our manuscript $f(t)$ is not the conventional "mean daily insolation at the summer solstice at 65°N". It is the orbital forcing index used in Talento & Ganopolski (2021), defined as a weighted linear combination of climatic precession and obliquity (Eq. 5), with (γ) calibrated in that model. We will revise the text around line 121 to state this explicitly and to refer to $f(t)$ as a "65°N summer-insolation proxy / orbital forcing index" to avoid confusion.

> Line 124: It would be helpful to describe how pre(t) and obl(t) are preprocessed. The raw amplitudes of climatic precession and obliquity (expressed in radians or degrees) differ substantially.

We agree this wasn't clearly explained. In our implementation, $\mathrm{pre}(t)$ and $\mathrm{obl}(t)$ are the two insolation-component time series provided with the Talento & Ganopolski forcing files (`smx_p.mat` and `smx_o.mat`), i.e. values on an insolation-like scale (hundreds of Wm^{-2}). We use these series as-is and do not apply any additional scaling or normalisation. The only processing we apply is the mean subtraction $\mathrm{obl}(t) - \langle \mathrm{obl} \rangle$ already shown in Eq. (5). We will state this explicitly in the revised manuscript.

> In the caption of Fig. 1, f is the orbital forcing and f' is its anomaly. However, f appears to be anomaly in Eq. (5). Also $\mathrm{mean}(f)$ and $\bar{f}$ coexist.

$f(t)$ in Eq. (5) is not the anomaly, only $\mathrm{obl}(t)$ is demeaned. We do agree that the notation is inconsistent and will revise this in the manuscript.

> Line 148: The term "a steady state" is not appropriate for a non-autonomous system. A pullback attractor, or simply a trajectory after removal of transients, would be more accurate.

We agree with this criticism and will amend the manuscript.

> Line 173: $\mathrm{d}W_t$ is not the Wiener process; W_t denotes the Wiener process.

We will correct this distinction in the manuscript.

> Figure 2: "Mean period" → "Period"?

We agree the current x-axis label can be misleading. The x-axis is period (kyr); what is averaged is the spectrum across stochastic realisations. We will therefore relabel the axis to “Period (kyr)” and clarify in the caption that the black curve is the ensemble-mean spectrum across runs.

References:

Talento, Stefanie, and Andrey Ganopolski. "Reduced-complexity model for the impact of anthropogenic CO 2 emissions on future glacial cycles." *Earth System Dynamics* 12.4 (2021): 1275-1293.

Mitsui, T., & Crucifix, M. (2016). Effects of additive noise on the stability of glacial cycles. In Mathematical Paradigms of Climate Science (pp. 93–113). Springer.