Note to ESD's Editorial Support Team:

We do not yet revise our Ms, as instructed by your email dated 21 September 2021. Nonetheless, in our response below to Reviewer #2 we indicate which revisions we envisage. These will be done in a track-change mode to facilitate making decisions about the further handling of our Ms.

Dear Reviewer #2,

Thank you very much for the review of our Ms (esd-2021-27). We highly appreciate your efforts in terms of time and input! And, to stress, it is a pleasure for us to respond to your valuablecomments because this will help to improve our Ms and also uncover the details which underlie our Ms but which we did not mention for the sake of keeping the Ms focused.

In responding, we follow your general comments.

General comments:

1st para, last sentence:

Particularly, the readability needs to be improved.

This we aim at as well → see our responses below.

1. General Comment 1:

One of my main concerns is about the stress-strain model. Why is this model useful in understanding the memory/persistence in the system? More detailed discussions and arguments are needed.

This question is best answered by decomposing it wrt two insights:

(1) Rheology is the study of the flow and deformation of matter reflecting the interrelation between force, deformation and time.[1] Once one begins thinking in a stress-strain (modeling) context, it becomes immediately obvious to explore the advantages of a rheological approach, an important, if not the most important, one being consistency. Above and beyond, "memory" is known in rheology, as is "hysteresis" (not so "persistence").[2,3]

(2) However, the usefulness of a stress-strain approach achieves its full potential only in combination with the insight that (i) mathematics offers a direct (and quantifiable) handle on delay time, thus on memory and persistence; and that (ii) the emergence of memory is contained in each ordinary differential equation describing processes which come with a lag-

[1] Cf. also https://cdn.technologynetworks.com/TN/Resources/PDF/WP160620BasicIntroRheology.pdf

[2] Cf. also https://www.itcp.kit.edu/wilhelm/download/Introduction%20to%20Rheology_2019.pdf

[3] Cf. also https://metalurji.mu.edu.tr/Icerik/metalurji.mu.edu.tr/Sayfa/PlasticityW9.pdf

time —as it is the case with a stress-strain approach in the context of a system exhibiting memory. That is, the definitions of memory and persistence are not arbitrary; they are a logical consequence of how processes with a lag time are described in physics traditionally.

That is, the combination of insights 1 and 2 offers the potential of exploring four assets in an Earth systems context: consistency, delay time, memory and persistence—what had not been done before.

Still, however, to do so requires, in addition, the workability of this endeavor / our global atmosphere–land/ocean carbon systems approach to be ensured. This is why we say, e.g., on p. 5 (12–16):

Under optimal conditions (referring to the long-term stability of the temporal offset), the temporal-offset view even suggests that we can refrain from disentangling the exchange of both thermal energy and carbon throughout the atmosphere–land/ocean system, as it is done in climate-carbon models ranging from simple to complex (Flato et al., 2013; Harman and Trudinger, 2014).

(There exist more favorable conditions → see also our response to Reviewer #1.)

In brief, this is what our Ms is all about—it demonstrates the workability of a stress-strain approach under favorable conditions and in an Earth systems context! We can only speculate that the afore-mentioned circumstances explain why a stress-strain approach had not been applied so far.

We suggest complying with your request of providing additional background knowledge along these lines (without adding more mathematics).

2. General Comment 2:

The "Abstract" should be improved. Now it is quite similar to the first three paragraphs of the "Motivation".

This can certainly be done.

3. General Comment 3:

A more detailed overview of the previous progresses on climate persistence may be helpful. In the current version, the authors only mentioned the previous studies very briefly, as shown on page 7 (lines 24-25), page 8 (lines 1-4). However, what are the limitations of the previous studies? I would suggest the authors to make a more detailed introduction.

We will extend our Motivations section accordingly.

Wrt an overview of "persistence": As a matter of fact, we have done a survey on how "memory", "persistence" and other / equivalent terms are understood across climate studies, economics and finance, and geophysics and physics. It should not come as a surprise that, therein, these terms are generally "defined" statistically, based on correlation principles. By way of contrast, definitions of memory and persistence are coming as quantifiable system parameters / for free under a stress-strain approach (→ see also General Responses 2 and 4). This was the main reason why we were overly brief on "memory" and "persistence" in our Ms.

4. General Comment 4:

The definitions of "delay time", "memory", and "persistence" should be clearly given in the main text. What is the differences between "memory" and "persistence"?

We defined delay time, memory, and persistence in equations (3) to (5) in the Methods section. However, we suggest referring to these equations when they are used in the Main Findings section, where we will also embed them also more clearly in a physical context.

To the difference between memory and persistence, in particular:

Memory is given by

$$M(q,n) = \frac{1-q_\beta^n}{1-q_\beta} = \sum_{i=0}^{n-1} q_\beta^i = \sum_{i=0}^{n-1} (q_\alpha q)^i = past;$$

that is, as the sum over an exponential (q -weighted) strain q_α which the system had experienced in the past; with $n = today$ and $q = exp\left(-\frac{K}{D}\Delta t\right)$.

By way of contrast, persistence is given by

$$P(q,n) := \left(\frac{\frac{1}{M}}{\frac{1}{q_\beta}} \frac{\partial M}{\partial q_\beta}\right)^{-1};$$

that is, as the inverse of the (normalized) derivative of M (i.e., over the entire past!) by q_β, the meaning of which we explained on p. 11 (7–11):

Assuming that q can be changed in retrospect at $n = 0$ [while a is held constant], *this equation tells us that, if …* $\frac{\Delta M}{M}$ *per* $\frac{\Delta q}{q}$ *… is small, P is great because the change in the system's characteristics (contained in q) hardly influences the MB's past; with the consequence that the past exhibits a great path dependency; and vice versa.*

We will try to carve out also this difference more clearly in physical terms.

5. General Comment 5:

Regarding the "Data and Conversion Factors" section, I would suggest the authors to add more information (about the data used in this study) here. It is not convenient for the readers to search for the data information in the supplementary information. At least, some basic information should be provided in the main text.

This can certainly be done.

6. General Comment 6:

When estimating the compression modulus K, why is the atmosphere assumed to be represented by a Hooke element in the MB? What is a Hooke element?

A Hooke element is a linear elastic body (Hooke body, Hooke model, Hooke element, elastic spring). It represents the behavior of a perfectly elastic (lossless) material. Stress is proportional to strain.[4] Hence, our careful note on p. 19 (7–10):

Compared to the slow uptake of carbon by land and oceans, we assume the atmosphere to be represented well by a Hooke element in the MB and this to serve as a (sufficiently stable) surrogate physical descriptor for the reaction of the atmosphere as a whole (Sakazaki and Hamilton, 2020).

7. General Comment 7:

The current manuscript is very technical. For me, I would like to see more explanations of the results from the perspective of climate sciences.

This can certainly be done (while keeping our Ms stringently focused). We can think of an additional section at the end of our Ms where we can hint at the value of a global stress-strain model as a support model for the support of complex, geographically explicit models (e.g., general circulation or global vegetation models) → see also our response to Reviewer #1).

Again, with great thanks to your very constructive comments,
along with our sincere greetings,

Matthias Jonas, Rostyslav Bun, Iryna Ryzha and Piotr Żebrowski

[4] Cf. http://www.earthphysics.sk/mainpage/stud_mat/Moczo_Kristek_Franek_Rheological_Models.pdf