I thank Prof. Martin Claussen for the careful and thoughtful evaluation of my manuscript. I appreciate the time devoted to this review and found many of the comments valuable in clarifying the conceptual objective of the study. The review made me realize that the original version did not sufficiently distinguish my proposed conceptual framework from existing theories of climate-system dynamics. In response, I have substantially revised the manuscript. The Introduction, methodological framework, discussion of temporal stratification, implications, and Conclusions have all been rewritten or extensively expanded to make explicit that the proposed framework is conceptual and interpretative rather than an alternative physical theory of the coupled Earth system. My central thesis remains unchanged. What has changed is the way in which it is presented: the revised manuscript now distinguishes much more explicitly between the coupled dynamics of the Earth-system and the conceptual definition of climate proposed from a geopaleontological perspective. I therefore believe that the revised version expresses the original objective with considerably greater precision and clarity.

I address Prof. Claussen's comments point by point below.

Reviewer Comments
Temporal Stratification in Climate Science: …
by
P.P.A. Mazza

The manuscript by Mazza aims at providing a structured narrative review to synthesize multi-disciplinary evidence in order to clarify the temporal and ontological foundations of the term ”climate”. To this end, three nested domains are distinguished: meteorological variability, sub-climatic oscillations, and long-term climate regimes. This hierarchical framework is supposed to provide a useful basis for interpreting recent climate trends within the broader temporal structure of the Earth system.

I am afraid that the narrative review does not achieve its goal, nor does it add any new insight compared to the concepts proposed in classical papers and textbooks on this topic by, for example, Hasselmann (1976) or Peixoto and Oort (1993) or Saltzman (2002). The narrative review also fails to acknowledge the meanwhile commonly accepted definition of “climate in a narrow sense”, i.e., the meteorological definition of climate in terms of weather statistics, and “climate in a wider sense”, i.e., climate in terms of state and statistics of a system which includes atmosphere, ocean, ice and land interacting via fluxes of momentum, energy, matter, and carbon, as put forward by the IPCC already in (2001), for example.

Author's Response: I sincerely thank Prof. Claussen for this important observation. I appreciate that the review highlighted an aspect of the original manuscript that was not sufficiently clear. The original version did not adequately distinguish the conceptual objective of the proposed framework from the existing physical theories of climate-system dynamics. Consequently, the relationship between the present conceptual proposal and the fundamental contributions of Hasselmann (1976), Peixoto and Oort (1993), Saltzman (2002), and the Earth-system perspective adopted by the IPCC (2001, 2021) could indeed be misunderstood.

In response, the Introduction has been extensively revised. The revised manuscript now explicitly discusses these foundational contributions and clarifies that the objective of the present study is not to propose an alternative physical theory of the climate system, nor to replace existing dynamical descriptions of Earth-system variability. Rather, it addresses a different question: the conceptual interpretation of the term climate when applied across disciplines whose observational windows span several orders of temporal magnitude.

Accordingly, the revised Introduction now explicitly acknowledges the coupled nature of the Earth system, the existence of interactions across multiple temporal scales, and the importance of the classical Earth-system framework developed by Hasselmann (1976), Peixoto and Oort

(1993), Saltzman (2002), and subsequent IPCC assessments. The methodological framework has likewise been rewritten to make clear that this study constitutes a structured narrative review leading to the development of a conceptual and epistemological framework rather than a new dynamical model.

My intention is therefore neither to replace nor to compete with the physical theories that describe the coupled dynamics of the Earth system. Rather, the manuscript addresses a different question: how the term *climate* should be interpreted when observations span temporal windows extending far beyond the instrumental record. The manuscript now makes explicit that the proposed framework is conceptual and interpretative rather than dynamical, and I hope that these revisions more clearly distinguish its objective from that of the classical works cited by the reviewer.

The narrative review tends to diffuse the physical concept of a (climate) system: the system includes all processes which a study focuses at, being separated from processes outside the boundaries of the system which affect the system but are not affected by the system - as clearly outlined in the textbook on the Physics of Climate by Peixoto and Oort (1993). According to Peixoto and Oort (1993), the definition of a system depends on the scope of the research which also implies a dependence on timescales. For the study of short-term climate variability on a decadal timescale, for example, some components of the climate system like ice sheets could be considered as outside of the system and hence could be taken as prescribed boundary condition. Most important, no inter-action between the system and the world outside of the system beyond boundaries is allowed. In contrast, the new concept outlined in this study does allow for an interaction between externally driven boundary conditions and internally mediated feedback processes (the present manuscript line 116/7). This lack of separation of processes hinders, rather than promotes, our physical understanding of climate variability.

Hasselmann (1976) separated the „coupled ocean-atmosphere-cryosphere-land system … divided into a rapidly varying "weather" system (essentially the atmosphere) and a slowly responding "climate" system (the ocean, cryosphere, land vegetation, etc.)". Thereby he was able to explain the slow changes of climate „as the integral response to continuous random excitation by short period "weather" disturbances." Saltzman (2002) also used the concept of gaps in the variance spectrum of climate change (see for example Figure 1-2 in his text book) separating the scales of "tectonic average" from "climatic average" from "synoptic average" – which interestingly resembles the three scales of temporal stratification proposed in the present study. The concept of spectral gaps was useful for constructing efficient paleoclimate models in which the fast components of the climate system could be parameterized in terms of some noise acting on the slower components of the climate system. See also the overview paper by Ganopolski (2024) – mentioned in the list of references, but missing in the text - who extensively uses climate models of intermediate complexity which are based on the concept of climate gaps.

But do these spectral gaps really exist and, thus, is a temporal stratification still useful? Climate variability exists on all time scales, from seasonal and interannual to multi-millennial, and climatic processes are intimately coupled, so that understanding variability at any one timescale requires some understanding of the whole spectrum. For example, vegetation interacts with the other

climate system components on timescales of minutes to multi-centuries via biophysical, biogeochemical and biogeographical processes. Instead of being separated by a spectral gap, low- and high-frequency surface temperature responses appear to be of nearly equal magnitude at centennial timescales (e.g., Huybers and Curry, 2006) which interestingly is the timescale at which the effects of global warming become prominent. It would thus be misleading to view the recent climate trend as just reflecting atmospheric and oceanic variability operating over decadal and centennial timescales (as stated in line 232-233). Of course, for predicting seasonal changes of the atmosphere and ocean the dynamics of glacial – interglacial cycles can safely be ignored. But the current state of the atmosphere-ocean system and its stability cannot be understood without understanding the development of Holocene climate variability, as the stability of the atmosphere-ocean system to external perturbation, like volcanic forcing or anthropogenic greenhouse gas emission, and the emergence of abrupt changes or so-called tipping points strongly depends on the climate state and its evolution. Consistently, climate modeling, specifically paleoclimate modeling, aims at bridging, not separating, timescales. Current comprehensive paleoclimate models describe the interaction of as many climate components as computationally feasible running from weather to glacial – interglacial cycles (e.g., Duque-Villegas et al., 2025; Mikolajewicz et al., 2025).
In summary, I very much regret to conclude that the new concept of temporal stratification does not add new value to existing classical definitions of climate. Moreover, the new concept fails to acknowledge the state-of-the-art of climate physics which aims at bridging, not separating, time domains.

Author's Response: I sincerely thank Prof. Claussen for this thoughtful discussion, which identifies the central conceptual issue addressed by the manuscript. I fully agree that the coupled nature of the Earth system, the existence of interactions across temporal scales, and the importance of understanding these interactions constitute fundamental achievements of modern climate physics. The revised manuscript has been substantially modified to make explicit that the proposed framework fully embraces this physical understanding.
Indeed, I recognize that the original version of the manuscript did not sufficiently distinguish dynamic coupling from conceptual stratification. This ambiguity may have suggested that the proposed hierarchy implied physical separation among climatic processes, whereas this was never the intended interpretation.
In response to these comments, the manuscript has undergone substantial revision. The revisions primarily concern the Introduction, the methodological framework, the new Section 3.5 ("Temporal stratification versus dynamical timescale separation"), the discussion of external boundary conditions in Chapter 4, and the Conclusions. Most importantly, a completely new Section 3.5 ("Temporal stratification versus dynamical timescale separation") has been added specifically to address this issue. This new section explicitly acknowledges the classical timescale-separation approaches of Hasselmann (1976) and Saltzman (1990, 2002), together with more recent work emphasizing spectral continuity, long-memory processes, state dependence, and cross-scale interactions (Huybers and Curry, 2006; Verbitsky et al., 2019; Verbitsky and Volobuev, 2025; Verbitsky and Omta, 2026). The revised manuscript now explicitly states that the proposed temporal domains should not be interpreted as dynamically isolated compartments, but as analytically useful levels of organization within a continuously interacting Earth system.
Similarly, the Introduction and Chapter 4 have been revised to clarify that externally paced boundary conditions do not operate independently of Earth-system feedbacks. Rather, they define the long-term framework within which internally mediated feedbacks, memory effects, and nonlinear interactions continuously operate. The previous wording, which could have suggested physical independence of external forcing, has therefore been replaced by language emphasizing dynamic coupling while preserving the distinction between externally imposed boundary conditions and internally generated responses.
The point on which my interpretation respectfully differs from that of Prof. Claussen concerns

not the physics of the coupled Earth system, but the conceptual status of the term *climate*. In other words, the question addressed by the manuscript is not how the Earth system functions—a question already addressed by modern climate physics—but what should be meant by the term climate when interpreting observations obtained over profoundly different temporal scales.

The revised manuscript explicitly recognizes that Earth-system processes interact continuously across the entire temporal spectrum and that understanding variability at any given timescale requires consideration of these cross-scale interactions. The proposed temporal stratification therefore should not be interpreted as implying physically separate domains. Rather, it distinguishes between Earth-system processes occurring across multiple temporal scales and the long-term climatic state that those processes collectively define. From the geopaleontological perspective developed in the manuscript, climate refers to this persistent state, whereas meteorological variability and sub-climatic oscillations represent processes operating within it. Accordingly, temporal stratification is not proposed as an alternative to coupled climate-system theory, nor as a framework for climate modelling. Rather, it is intended as a conceptual framework for interpreting observations obtained over different temporal windows while remaining fully compatible with the continuous interactions described by modern climate physics. I therefore hope that the revisions now make clear that the manuscript seeks to complement—not replace—the existing physical theories describing the coupled dynamics of the Earth system by addressing a different, conceptual question.

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