Review of "Rising atmospheric CO2 concentrations: the overlooked factor promoting SW Iberian Forest development across the LGM and the last deglaciation?"

The manuscript by Gomes et al. presents a new high-resolution pollen record for the last deglaciation from core U1385 on the Iberian Margin. Based on the palynological data, non-pollen proxies from the same core, and published marine and terrestrial pollen records, the authors investigate the role of temperature, moisture, and CO_2 for the development of forests on the Iberian Peninsula. The manuscript proposes that CO_2 plays a bigger role than previously thought, in particular promoting the existence of forests during the Younger Dryas while preventing forest development during the Last Glacial Maximum and Heinrich Stadial 1. The analysis is sound and the conclusions are supported by the presented data, although the wording and clarity of the manuscript could be improved. Therefore, I believe that the manuscript could be suitable for publication in Biogeosciences after addressing the comments below.

General comments

- While the topic of the manuscript is suitable for publication in BG, the paper is rather difficult to read without a strong paleoclimate background due to using a lot of not properly introduced paleoclimate jargon. Since BG is not a paleo-specific journal and the manuscript topic could also gather interest from outside the paleo-community, it would be valuable if you could make it easier to follow for non paleo-scientists.
- In Sect. 4.2, vegetation changes are directly associated with warmer/colder and wetter/drier conditions. Since the main conclusion of the manuscript is a potentially larger role for CO_2 than previously thought, I find this confusing and would recommend a more careful wording when inferring climatic conditions from the vegetation composition. In particular, it should be clarified that the interpretations are just describing changes "felt" by the plants but not necessarily actual climatic changes (unless they are supported by vegetation-independent proxies).
- The role of moisture availability variations remains largely unconstrained in the manuscript which complicates the attribution of the vegetation variability to temperature and/or CO_2 changes. The spatial pattern of SST anomalies in the North Atlantic was likely different between HS1 and YD (e.g., Pedro et al., 2022; Weitzel et al., 2024) which could lead to differing precipitation amounts during HS1 and YD. To what extend can you rule out that the moisture availability conditions during the YD were more favorable for forest development than during HS1/LGM? If you cannot constrain the moisture availability changes better at this point, this should be stated as a limitation.

Specific comments:

- l. 60: What is meant by "variable moisture condition"?
- l. 91: Do the 5 - 10°C refer to local, zonal mean, or global mean temperature changes? Please specify.
- l. 114 - 127: It would be worth mentioning that there are also previous studies inferring the importance of CO_2 changes for the vegetation evolution using multi-proxy approaches (e.g., Gosling et al., 2022; Koutsodendris et al., 2023) and model-data comparison (e.g., Adam et al., 2021). Of particular interest for this study, Koutsodendris et al. (2023) suggests a major role for CO_2 in modulating Mediterranean forest growth in Greece.

- l. 152: What simulations are meant here? As far as I know, most simulations of glacial vegetation include the effect of lower CO_2.
- Sect. 2: In addition to describing the current regional climate, it would be informative to briefly describe the present-day regional vegetation composition.
- l. 224: Is there a reason for not using IntCal20 instead of the older Marine13 calibration curve? Do you expect this to make a difference?
- Sect. 3.3: Why is the Villarquemado record not included in the regional compilation for which interesting work on the role of CO_2 exists (Wei et al., 2021)?
- l. 269: Using the original chronologies of the records will likely lead to some smoothing of the abrupt (centennial-to-millennial scale) variability when computing the regional averages. Could this affect your results?
- l. 278-280: What kind of response function do you use to fit the GAM? Given that pollen percentages are restricted to the interval 0 to 100, a beta or binomial response function is more suitable than a standard Gaussian response function (e.g., Adam et al., 2021; Wei et al., 2020).
- l. 301: Does 0.5°C correspond to a 1σ standard error?
- l. 336: Does "dry" here refer to the actual climate conditions or the perceived climate conditions of the vegetation (i.e., a combination of moisture availability and CO_2)? As stated above, a more careful wording would improve the clarity of this section.
- l. 378-380: Why would the moisture availability change less abruptly than the temperature? Could there also be a role for CO_2 or internal vegetation dynamics in explaining the delayed response of the vegetation?
- l. 441-443: Could the presence of large herbivores also play a role in promoting heathland rather than forests (Zhu et al., 2018)?
- l. 467: Why should the precession maximum specifically trigger heathland rather than forest development?
- l. 502: I don't understand why the bias could extend to vegetation reconstructions as these only associate pollen assemblages with vegetation composition. Are you mixing up reconstructions and simulations here? For simulations, it is of course important to account for CO_2 changes (which most of them do).
- l. 568-569: What is meant by "in a general assumption"?
- l. 593-597: My understanding of the methodology of Shao et al. (2018) is that they account for CO_2 changes by deriving transition matrices from the simulation output of Woillez et al. (2011). If this is correct, it is expected that the two studies agree on the influence of CO_2 on the LGM vegetation.
- l. 602: What do you mean by "with some seasonality"?
- l. 644-647: Under which baseline temperature was this experiment conducted? Is the effect dependent on the background temperature?
- Fig. 3: Equating the pollen zones and the geologic periods YD, BA, and HS1 can be misleading since the start and end dates of the pollen zones do not coincide with the official definitions of the geologic periods. For example, pollen zone 4 ending later than the actual YD is not in agreement with the SST reconstruction reaching Holocene level temperatures already during the later stages of pollen zone 4. Therefore, I would recommend to display the limits of the pollen zones separately from the limits of YD, BA, and HS1 in Fig. 3.

- Fig. 4: Based on what data are the schematics for moisture during LGM, HS1, BA, and YD assigned? Do the schematics represent absolute values, anomalies, or trends?
- Table S1: The Top Age for zone 4 should be 11050.
- Fig. S1 and S2: These two figures are rather blurry. Can you improve their resolution?

Technical comments:

- I kindly ask you to check the manuscript for typos and grammatical errors again. In particular, there are a number of missing spaces between words (e.g., "ofthe" in l. 178, "elements(Fig.", in l. 366, and "anda" in l. 366).
- The spelling and use of age units differs throughout the manuscript (e.g., "ka" in l. 9, "cal yr BP" in l. 200, "Ka" in l. 560). I kindly ask you to define an age unit in the introduction and use it throughout the manuscript.

References

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