

Interactive comment on “Cascading climate effects and related ecological consequences during past centuries” by B. Naef-Daenzer et al.

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Dear Dr. Guiot, Dear reviewers,

We thank the editor and the two reviewers for the detailed comments on our manuscript. The reports mentioned several points that needed clarification and called for further explanations. We have now worked through these suggestions and revised the manuscript accordingly. We believe that the constructive criticism yielded substantial improvements in both conciseness and presentation. The re-submission is accompanied by a track-changes version of the revised manuscript. Please find following how we addressed the reviewers comments.

General comments by editor and both reviewers: 1) Validation issues The most impor-

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tant point raised by the editor and both reviewers was the difficulty to independently validate the analysis and retrospective model. We are aware that the options for ideally independent validation are precarious: On one hand, all population records of great tits cover the past decades, and our data set is amongst the longest records. Thus, there exist no data from a non-overlapping time period (2000 to 2011 would be by far too short). On the other hand, splitting the observation data into an analysis and a validation data set is impractical because the two subsamples would become too small to allow either solid modelling or valuable validation. Furthermore, resampling techniques would not help, either, because they still rely on subsamples drawn from the same data that were used for the analysis. Given the situation, we believe to have achieved the best-possible validation by using a data series from a distant tit population that is under similar influence of the NAO/NCP system, and by using alternative indices for spring phenology covering a 150-year period. We are aware of the drawbacks of the lack of non-overlapping validation data sets and have now included a brief discussion of potential collinearities that cannot be excluded with our approach. However, in consideration of the empirical and experimental evidence on the great tit's breeding ecology, these can be considered very unlikely.

2) Methods Both reviewers requested some supplemental details in the methods section, in particular regarding the retrospective model. These details have now been included into the section, making clear that the model is not a population model (making assumptions on survival and other factors) but a simple implementation of the structural equation system excluding any other potential causal factors. With regard to structural equation modelling, we believe that this is part of the standard toolbox of multivariate correlative techniques. Thus we are reluctant to expand the paper by adding general information. However, we improved the explanations related to the particular analysis, such as the meaning of particular models.

3) Retrospective model We now explain more concisely that the model given in the paper is just the implementation of the structural equation modelling results (equations as given in fig. 2) to evaluate the effect of annual variation in NAO/NCP indices on the dependent variables (breeding phenology and productivity). Thus, the simulation does

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not include any further assumptions on survival (either juveniles or adults), population density or further factors. It is now mentioned that making assumptions on such components of population dynamics is impossible due to lack of evidence (i.e. would be entirely speculative).

Particular points: Review 1: 1) All minor points have been addressed in the revised version. 2) P2048, L15 of ms: In multivariate analyses such as SEM with more than 20 d.f., a sample of 77 data points is rather small, and outliers may well distort the partial regressions. We detected the four points using pairwise correlation plots, however, did not run repeat analyses similar to the tests of whether years with small samples did affect SEM results. We believe that this procedure is appropriate and needs no further testing. 3) We now explain in the ms that the residuals are the average laying dates as corrected for the difference in latitude between the populations. Accordingly the validation tests the predicted fluctuation in dependent variables rather than long-term annual means which vary geographically. Review 2: 1) P 2049, L 15-16: We are grateful for the point and have included the sentence almost identically as suggested. The revised text now states clearly that the retrospective model is a partial estimate of the impact of the atmospheric circulation patterns on tit phenology and productivity. 2) Page 2049: The modifications of the text now clarify that no other variables than those of the SEM were included into the simulation. Indeed the study only partially addresses the forcing of global circulation patterns on tit populations. See also above. The revised text now clarifies that no further modelling methods (whether based on Leslie matrices or not) had been used because no evidence-based assumptions on these can be made for a 500-year period. Also, we included a sentence on our assumption that the correlations between the atmospheric teleconnections and surface climate remained constant over the 500-year period under consideration (The notion is made in the methods section). 3) Unfortunately there are no really independent data for validation available. Further details were given above. 4) Reviewer's point 5: We agree that one should be cautious about the productivity and population impact index for the recent global warming period, in particular in respect to extrapolations. For this reason we did not make any

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projections into the future. However, we did not find evidence for non-linear effect (e.g. in temperature-phenology relationships) over the ranges represented in our data. Consequently, the study does not allow estimating potential limitations as suggested by the reviewer. We also agree that the relationship between large-scale atmospheric patterns and surface climate may vary on the long term. Our reconstruction assumes that this was not the case for the past half millennium. We have included the point into the discussion and added the citation from the Möller et al. (2010) book. We also included a few more citations supporting the notion. 5) P 2043, L 5. We apologise for the misplaced citation of Grosbois et al., 2006, -the reference is now at the appropriate site.

Thank you for re-considering the revised manuscript for publication in CP.

On behalf of all authors, yours sincerely, Beat Naef-Daenzer

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