

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

### **Anonymous Referee #2**

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General evaluation The paper does in my opinion present a relatively successful attempt at reconstructing climate-driven demographic and, to a lesser extent, population trends over a multi-centurial period. This reconstruction has been produced thoroughly using good quality demographic, phenology, and climatic data as well as statistical methods that allow to explicitly depict the mechanisms through which climate variation is believed to drive population dynamics. I feel that one particular aspect of the study, which is the reconstruction of population index over a multi-century period would deserve more detailed methodological explanations. Nonetheless I believe that this is a sound and useful study which presents a number of unavoidable limitations that I have tried to point below.

Major points 1) Page 2049, lines 15-16: I would write: “Accordingly, this relative index

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quantifies the component of the impact of NAOI and NCPI on a theoretical population related to their influence on reproductive parameters”. It is in my opinion important to acknowledge that NAOI and NCPI could also impact tit populations through their influence on juvenile and adult survival. So your study only partially addresses the forcing of global climatic circulation patterns on tit populations.

2) The model validation data is not perfectly suited for validation because it covers a period also represented in the data used to fit the model. The impairing consequence is that times series of reproductive parameters in the two data sets could be synchronized for other reasons than those depicted by the SEM. A more convincing validation would have been produced with a validation data set covering a period that is not represented in the data used to fit the model.

3) Page 2049: precisions are needed on what exactly are the expected population effects and how they are computed. Concerning the interpretation of the expected population effects index As far as I understand it, it expresses the size of the population on any given relatively to population size in year 2000. So an index of 40 on a given year means that the size of the population on that year would be expected to be 40% of the size of the population in 2000. So you should make it clear that it is a population size index, not a population growth rate (another important population dynamics parameter) index. Concerning the method used to derive the long term time series of this index. In order to do so, I imagine that you must have used a population dynamics model (probably a Leslie matrix model). More precisely you probably have parameterized successive Leslie transition matrices by setting juvenile and adult survival at constant values and using the time series of reproductive output estimations (in terms of number of fledglings per pair of breeding tits) derived from your SEM for the fecundity parameter. This is my understanding of what you might have done, but I am not sure at all since the method is not described in details in the MS. Furthermore, the constant values used for juvenile and adult survival are not given, and the initial state of the population (that should be defined with a vector including the number of adult and

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juvenile females on the first year of the reconstitution) is not defined. Could you please provide enough information in MS so that the reader can understand rather than guess the method used to produce the population effects index time series shown on figure 4E.

4) Page 2049, lines 15-17: “Accordingly, this relative index quantifies the impact of NAOI and NCPI on a theoretical population in which the long-term (500 yr) reproductive success is constant”. I don’t understand this point because in my understanding, the only demographic parameter which varies over the reconstructed period is precisely reproductive success. I probably completely misunderstood the way the population impact index has been computed. . . . .

5) One highlighted result in the manuscript is the high magnitude of the recent predicted population size changes due to global warming as compared to the relatively lower magnitude of the predicted changes experienced over the former multi-centurial period covered by the study. I would tend to be cautious about the reproductive output and population index predicted for the recent global warming period. The main reason of my relative skepticism is that the SEM used to produce reproductive outputs assumes that all the direct relationships are linear. My guess is that relationships such as those linking weather conditions to bud burst date or bud burst date to reproductive success components are not linear, but probably show a plateau or an optimum. If my guess is correct, reproductive output could be overestimated when NAOI and NCPI take values at the extreme of their ranges. I would indeed be curious to see the fit between observed reproductive output values in the data set used to fit the SEM and the reproductive output values predicted by the SEM for the recent global warming period. More generally, I guess that it is important to consider potential non linearities in the study of cascading climatic influences on ecosystem components.

6) One potential problem for reconstructing the dynamics of ecosystem components based on cascading relationships between these components and large scale teleconnection indices such as NAO is that “the strength of teleconnections and the way they

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influence surface climate vary over long time scales” (this sentence is taken from the chapter written by J. W. Hurrell K. E. Trenberth on climate change in the book “Effects of climate change on birds” edited by A.P. Moller, W. Fiedler and P. Berthold” at Oxford University Press. This means that the correlation patterns between NAOI or NCPI and surface weather components that prevailed centuries ago might differ from the present the current correlation patterns. Although I believe that such changes are not likely to occur over only a few centuries, I believe that it is worth mentioning in the discussion this potential limitation for studies covering much longer periods. Minor point 1) Page 2042, line 11: it would be more accurate to state that what you reconstruct is the circulation-driven component of fluctuations in great tit breeding phenology and reproductive success. Indeed, reconstructing circulation-driven population dynamics fluctuations would also require determining the influence cascade that link juvenile and adult survival variation to large-scale circulation factors.

2) Page 2043, line 5: in Grosbois et al. 2006, these are not the consequences of large-scale climate fluctuations on geographical distribution of a species which are addressed, but the impact of large-scale climate fluctuations on an important demographic parameter: adult survival, in several, relatively distant populations.

3) Table 1: the tit data used to fit the Structural Equation Model is not displayed.

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Interactive comment on Clim. Past Discuss., 8, 2041, 2012.

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