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Interactive comment on "Impact of climate extremes on wildlife plant flowering over Germany" *by* J. F. Siegmund et al.

Anonymous Referee #2

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The manuscript deals with an important general topic, the impact of climatic extreme events on evosystems. This is a timely research arena, demonstrated e.g. through several large-scale projects in the last few years. Accordingly, the Introduction sets out with considerations which raise high expectations for the rest of the paper. The latter are, however, not really justified; one of the main motivations from the Introduction, that the impact of large precipitation events on plant phenology has virtually never been investigated, is not followed to sufficient depth. Also in general, the scope of the paper is diminished from page to page: only flowering dates are considered, rather than other key phenological events; only four (admittedly important) wildlife are considered; drought as combined effect of temperature and precipitation, heavily discussed in the literature, is omitted, and so on. Whenever really interesting stuff is about to start, the

authors claim that this would be outside the scope of the paper.

Unfortunately, the results actually presented are not really illuminating. There is no influence of precipitation detected; warm and cold spells (according to the 90th and the 10th percentiles - that would not be considered as "extreme" by many, but what can you do with just 40 observations per station?) in some cases coincide with "extreme" flowering dates more often than expected by pure chances, sometimes they don't. A geographical pattern is absent in these coincidences. If the situation on the result side is meager, than one would expect the authors to proceed in further directions: combined effects; explicitly considering delay effects (which are very plausible); setting up a model relating the flowering date to the temperature / precipitation history at the site (not necessarily only the extreme cases), and so on. Most of these ideas are "outside the scope of this paper".

Technically, the event coincidence analysis is a nice tool, but the paper fails to demonstrate its superiority to a conventional correlation analysis, as Fig. 7 demonstrates.

A few specific points: 1. p. 18396 l. 22: "weighted mean interpolation" - weighted with what? The (inverse) geographic distance to the phenological stations? 2. p. 18398 l. 25: "here, N=M by definition" exclude a universe of interesting combination effects (several extreme events in a row might lead to quite different effects than just one, even the latter is bigger) with a few words. Why were the authors forced to this simplification "by definition"? 3. p. 18403 l. 9/10: where and for what species are these future analyses planned? 4. p. 18405 l. 12-15 and Fig. 7: you can't seriously calculate the Pearson correlation between binary vectors. The 0's and 1's are indicators, not numbers (replace them with A's and B's to see the point). As association measure between two binary vectors, use the Phi coefficient. This will dramatically change the appearance of Fig. 7, right panels.

The paper is not finished yet; the really interesting aspects of the relationship between flowering dates and climatic extremes for wildlife plant species are yet to come. This

amounts to a "major revision", or rather a thorough extension and rewriting of the paper.

Interactive comment on Biogeosciences Discuss., 12, 18389, 2015.