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Interactive comment on "A survey of proximal methods for monitoring leaf phenology in temperate deciduous forests" *by* Kamel Soudani et al.

Anonymous Referee #2

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This study is very well written and the results, which compare phenological dates derived from multiple vegetation proxies using a double sigmoidal function to ground observations at a forest site in France, are clearly presented. The comparison of the dates derived from multiple proxies shows the strengths and weaknesses of each, at least when using a double sigmoid model.

My main concern is that the results are generally specific to the use of a double sigmoid function, and could change significantly if methods tailored to individual proxies were applied. In addition, each data source (camera imagery, photosynthesis estimates, radiometric) is taken 'as-is' and little quality control of the underlying time series is

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applied. The results are not presented as thus however, and instead are presented as a quantification of the underlying information content of the examined proxies. I suggest the authors either reconsider the framing of the manuscript as an assessment of the potential of using one approach applied to multiple and varied proxies with little quality control, rather than presenting the results as a direct assessment of the proxies examined.

Detailed comments: Line 10: the term 'flux measurement site' is ambiguous. I assume you mean eddy-covariance (EC) flux measurement sites. Note that tree phenology is recorded at many (if not a lot) EC sites.

Line 20: 'GPP provides the most biased estimates' should read 'our method to derive phenological dates from GPP provides the most biased estimates' or something along these lines (perhaps: "the assymetic double sigmoidal function we used to derive phenological dates provides the most biased estimates for GPP"). The point being that the results are both method and data dependent, and better estimates could in theory be derived from GPP using a method more suited to noisy time series.

Lines 87-107: Applying a '1 model fits all' approach to estimating the phenological thresholds is questionable. For instance, a lot of effort has gone into estimating robust transition dates for Gcc, including multiple signal processing steps to improve the signal to noise ratio and the fitting of multi-dimensional splines for date estimation (see e.g., Richardson et al. 2018 (https://www.nature.com/articles/s41598-018-23804-6) which performs a similar study as that presented here though with many more sites and fewer vegetation proxies). This is particularly important for GCC, as the seasonal cycle in deciduous forests does not follow a smooth double-logistic curve (which is why your model has trouble fitting it, e.g., in Fig. 2d). The same point applies to many of the other data sources considered. For example for GPP, detailed outlier detection and other quality control efforts are needed (see, e.g., https://essd.copernicus.org/preprints/essd-2020-58/essd-2020-58.pdf, or the PhAsT framework: https://www.nature.com/articles/nclimate2253). This is particularly impor-

tant for GPP given that it is subject to relatively large random error and shows high variability due to changes in the environment. It is hard therefore to determine whether the comparison results can be meaningfully interpreted. How much of the difference between proxies is due to the fact that the method you applied is less suitable for a particular proxy? Without detailed, proxy-specific, data processing based on the state of the art it is certain that your statistical characterization of the utility of each proxy considerably underestimates its true potential.

Line 246 Figure 2: It is very hard to distinguish the data from the ADS model here. Convention suggests using black for the data and red for the model. It is important for the reader to be able to clearly see the fitted model in order to assess the robustness of the derived dates.

Figure 3: This should be in color. It is very difficult to see which line corresponds to which data source.

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