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Interactive comment on "Impact of annual and seasonal precipitation and air temperature on gross primary production in Mediterranean ecosystems in Europe" *by* Svenja Bartsch et al.

Anonymous Referee #2

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General comment Overall, the topic presented by the authors would be fitting to the scope of BGS and, also, is of interest in the "response to climate change" of ecosystems, of relevance for process understanding.

However, the papers suffers of major drawbacks, the most important is the not completely correct consideration of the selected sites with additionally a lack of information/data on sites that would allow the readers (and referees) to evaluate the sites in the perspectives of what the authors want to affirm.

When assessing the impact of T/PPT on GPP or another flux parameters, a reasearcher should be sure that climate (T/PPT) is the driver that can be really considered for such an analysis.

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Discussion paper

For example, in the case of the selected sites: - site 6. It is subject to salvage logging, after a fire or similar (no reference is given). Here "management" (salvage logging) could be expected to seriously influence production (GPP, NPP) and respiration, with potential "decoupling" from climate influence. - site 11. This site is a short rotation forestry stand where underground irrigation and fertilisation was applied. Also in this case, GPP may be decoupled from PPT, particularly in summer - site 21 and 22: it is the same site, with two stands under a treatment with increased water supply (wet) and reduced water supply (dry) by using water interception gauges in the dry treatment and relocation of the water in the wet one. Also in this case, PPT is not the proper variable to be possibly considered, unless information on effective water supply has been used in the analysis. Furthermore, those two sites are evergreen broadleaf trees, as site 23 (which is the same site with natural water input).

As the authors grouped the sites according to vegetation, an incorrect assignment of a site to the current vegetation hamper the analysis, averages, box plots and the subsequent analysis.

Apart from this fundamental comment, table 1 with site description does not provide mean climate data, elevation (as a minimum), main species, to allow the reader/referee to have a clearer view of what are the mean conditions at the sites. Also, it would be interesting to know the number of site/years used for each site. A reference to published work from those sites is needed (for correctness but also to have a better picture of what the site is)

The figures are lacking of a "symbol legend", it is not clear which symbol represents which system/site.

Although the topic is of interest, the current quality of the paper and the drawbacks illustrated above suggests me to reject the paper.

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