

## ***Interactive comment on “Hydrological and ecological controls on dissolved carbon concentrations in groundwater and carbon export to surface waters in a temperate pine forest watershed” by Loris Deirmendjian et al.***

### **Anonymous Referee #2**

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#### General comment

Interesting, significant work, but presentation is poor and unacceptable. One of the reason for this is insufficient reviews of similar studies. Although the studies in which DOC/DIC exports are compared with NEE measured by eddy covariance method in the same site may be limited, there are many studies discussed about DOC/DIC exports and their mechanisms.

The result section is hard to catch up. It should be rearranged to show more clearly to guide the readers for your discussion. The discussion section is also need to rearrange.

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I suppose that the contents in this is version of manuscript may be true, but may be incorrect at this moment.

Refer the specific comments.

Specific comments L.59 ‘... generally’. Here, the authors must refer some studies. Moreover, clarify how different this study from these studies?? L.74. The Leyre watershed is very large, but the piezometers are only three. Consider and clarify whether or how the data shown here represent whole the watershed. In other words, do the results in Bilos site (0.6km<sup>2</sup>) represent the whole Leyre watershed? L. 80 & 90 XIXth. Change to Arabic. L. 91 ‘Consequently...’ References about hydrology in the Leyre watershed must be sited around this sentence. If the authors does not cite any studies here, the descriptions are suspicious. L.103. Although soil preparation, fertilization and seeding was done in 2005, the referred paper was published in 2003. Why? Is this correct? L.180. What is the ‘CO<sub>2</sub> SYS’? No information. L.258-. Result. Too complicated. I highly recommend the authors to reconsider what are your main points. L. 376. ‘groundwater uptake’. This term is suspicious. Generally, many plants including pine trees use the soil water within unsaturated zone, and cannot survive if the root immersed in groundwater, saturated zone. L. 378- and Table 2. The discussions are generally based on the Pearson’s correlation coefficient. However, the real hydrological and biogeochemical phenomena cannot be sufficiently described by this kind of simple value; for example, how do you explain the time lag among precipitation, response of groundwater table, and drainage? The relationship between the groundwater storage and drainage in your site (L.381) is unclear in your consideration, but I suppose that it means the nonlinearity between these parameters, and it cannot be shown by the Pearson’s correlation coefficient. The authors also mentioned about this at L.382, but why this occurs in the flat topography of the watershed? Any references? I think this occurs not only in flat watershed, but also in steep watershed. Again, many discussions are not supported by reliable previous studies, previous knowledge. L.388-395. The discussion about (annual?) soil water storage is not clear for me. The authors

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mentioned that it was larger in 2015 (126mm) than in 2014 (71mm). Is this correct? The annual rainfall was much smaller in 2015, and the soil water storage will be smaller as well as the groundwater storage. If the authors can show the data of soil water content and/or hydraulic head, we can get more reliable information. L. 402. Insert period after 'bicarbonates'. L. 463. Discussion of this paragraph is strange. (L. 470) 'Thus, when the forest ecosystem is a source of CO<sub>2</sub> for the atmosphere, it is also a source of CO<sub>2</sub> for the underlying groundwater.' ... Even under the drought condition or even when the ecosystem act as a sink of CO<sub>2</sub>, below ground part of the vegetation 'only' act as the CO<sub>2</sub> source; respiration by root always occur. Moreover, degradation of organic carbon, including DOC always occur. It's a respiration by microorganisms. L.482 and other parts. Mean residence time, define by S/flux in this manuscript, should be reword as 'apparent turnover time'. In addition, it was calculated between two sampling days. This method cannot consider the time lag between the storage and output flux, as same as mentioned above. The Pearson's correlation coefficient cannot explain this. L. 503. The comparison with peat systems in less meaningful. Should be compared to similar ecosystem with your site. Moreover, I agree with the contents of the four referred papers (L.503-507), but how related them with your results? As I mentioned above, your discussion does not consider about the time lag, or times for decomposition and transport. The referred studies essentially mentioned about this. After these processes, SOC will move as DOC or DIC. L. 508. What's the meaning of this sentence here? L. 511. 'reported' ... Need appropriate citation. 'elsewhere'... ??? Where? Too irresponsible!! L. 512. As I mentioned at L. 74, I wonder how the authors can show that the observed groundwater is representative of whole the watershed. I think the authors cannot discuss about the spatial variation of data. This is one of the weak point of this paper. L.513-. 'Also, ...'. I agree with the first part of this sentence (but need some references about this phenomena), but cannot agree with the second part. Why the absence of correlation support the phenomena? The correlation is just a correlation; it cannot attest the phenomena. Comparison of concentration is different from the correlation. L. 517. (Righi and Wilbert, 1984) This reference is too old.

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The studies about DOC quality is making steady progress. I think the authors can find more up-to-date studies to support your findings. L. 521. There are no data, analysis, and discussion about the relationship between the topography and DOC concentration. How and why the difference of DOC concentration between three piezometers was occurred by the effect of topography? L. 525. The authors have mentioned about the possibility of photodegradation (or photo oxidation) of groundwater DOC at L. 514. However, the authors also mentioned as 'DOC was not labile, and not degraded in the superficial river network' during base flow period. These two comments make me confuse. Generally, the base flow condition occurs in fine (or no rain) days. Why the photodegradation does not occur under base flow condition? Which comment is true for you (and for us)?

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