

## ***Interactive comment on “Differences and influencing factors for underground water carbon uptake by karsts in Houzhai Basin, southwest China” by Junyi Zhang et al.***

**Anonymous Referee #1**

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The paper in its present form is meaningless. I am sorry to tell that, but the following points are not clear in the paper, making it unreadable.

1) Carbonate dissolution can be considered a carbon uptake if (and only if) the dissolved carbonate does not precipitate again within several thousands of years after it was dissolved. This point is not discussed at all in the paper and it is simply assumed that the whole carbonate dissolution fulfills these conditions.

2) Generally, the flux of dissolved carbonate out of a given catchment area is approached through the integration of the instantaneous carbonate flux, i.e. the carbonate concentration multiplied by the water flux. Because discharge rate and chemistry varies much and quickly at karst springs, the measurements of carbonate concentra-

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tion and discharge rate must be frequent enough to make a reasonable approximation of the real carbonate (carbon) flux. For catchment areas with a size between 50 and 100 km<sup>2</sup> a daily value is seen as a minimal frequency. In the paper, the frequency is about 5 days inducing a real uncertainty in this result. A further problem is that the formula given for the calculation (equation 4) suggests that the authors used the seasonal average carbonate concentration multiplied by the instantaneous flow rate, inducing a supplementary bias.

3) The discharge rates given for the three measurement stations are very unclear: a) the measurement frequency is not given, neither the measurement expected accuracy; b) Table 1 gives numbers ranging between 2.67 and 21.83, with no indication of the unit, and just below graphics give values for the same stations ranging between 70 and 450 m<sup>3</sup>/s, but the same numbers are given in m<sup>3</sup> in the text. It is very confusing. Even more confusing is the fact that the average runoff given for the whole year seems to be the sum of the wet and dry season average runoff values, instead of being an intermediate (average) value.

4) According to the rainfall quantity given in the introduction and to the size of the catchment area, discharge rates given (even in table 1) are much too high. The total rainfall (1300 mm) over a surface area of 85 km<sup>2</sup> gives a maximal annual average flow rate of 3.5 m<sup>3</sup>/s. Very confusing. What are the catchment areas of the three measurement stations? How were they determined?

5) For carbon flux, the transformation of the data measured in the field into the average data presented in the paper (§3.2) is quite uncertain. The measured data must be presented. The reader (or reviewer) could thus verify if transformations are correct.

6) The main conclusion is that most carbon is exported during the rainy season. The data presented in the paper (in its present form) enclose so much uncertainty that this result cannot be considered as supported by the data. By the way, if carbonate concentration decreases of about 10 to 20% during the wet season compared to the

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dry one, and that discharge rate increases by 50%, it is quite clear that the exported carbonate will be higher during the wet season. The paper by Gremaud et al. is a nice example of such a situation.

7) English is poor, the structure of the paper is not clear and logical, figures 5 to 7 are just unnecessary replicates of figures 1 to 3, figure captions are not sufficient.

8) Discussion and conclusions cannot be understood.

I can only expect that the authors wrote their paper in Chinese language and that the translation was done by a person, who doesn't understand the subject. Eventhough, the original paper could not be a good one because there are real mistakes in the method applied and a real lack of critical sense on the presented data.

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