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Biogeosciences Discussions

Interactive comment on "Organic carbon sequestration and discharge from a deciduous forest catchment in Korea" *by* S. J. Kim et al.

Anonymous Referee #2

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Review of "Organic carbon sequestration and discharge from a deciduous forest catchment in Korea" by S.J. Kim, J. Kim, and K. Kim. This study quantifies the production and export of dissolved and particulate organic carbon and evaluates how these carbon fluxes vary with stream discharge from a deciduous forest catchment in Korea. The subject is appropriate for Biogeosciences, but more importantly, there has been very little research attempting to compare streamwater organic carbon fluxes to net ecosystem carbon exchange. Overall, I found the paper to be generally well written with some interesting findings. However, the paper does have several deficiencies that should be corrected. First, the amount of DOC produced/infiltrated in the soil can not be used as an estimate of C that is stored in the soil. Thus, making comparisons with the overall C budget are inappropriate. Second, one of the primary objectives was to calculate riverine C fluxes, and the authors found that these yields are quite small com-

pared to whole-of-ecosystem carbon budgets. This is what I would expect in a forested catchment. This does not mean that DOC is not a tremendously important part of the ecosystem carbon balance. I would even argue that although DOC and POC fluxes are quite small compared to whole-of-ecosystem carbon budgets, they have a disproportionately large ecological role. There is still great potential with this manuscript, but I think the authors need to re-visit their flux calculations and re-structure the manuscript such that it is focused more on DOC/POC dynamics and how they change seasonally. What follows are more specific comments for the authors to consider.

Title I think sequestration is used inappropriately in this study and I would strongly consider removing it throughout the manuscript.

Abstract It might be just my opinion but generally speaking, "discharge" refers to streamflow and "export" or "fluxes" refer to C yields. Thus, I would consider changing the language throughout.

Pg 10090, Lines 2-4: Consider changing sentence to read as follows "Soil infiltration and surface water discharge of precipitation are critical processes that affect the production and export of dissolved (DOC) and particulate organic carbon (POC) in forested catchments." Pg 10090, Lines 4-6: Consider changing sentence to read as follows "Concentrations of DOC and POC can be very high in the etc." Pg 10090, Lines 24-25: Change to "inaccurate estimation"

Introduction This section needs some work. There are many sentences that are slightly inaccurate and need modification (see below for specific comments). I also think the first paragraph needs to be re-written because the focus of this study is not about global carbon fluxes and it certainly should not be about comparing riverine fluxes of organic carbon (OC) to the residual C sink. I think an opening paragraph focused on riverine fluxes of OC in relation to catchment carbon budgets would be more appropriate.

Pg 10091, Line 1: A significant portion of C stored in ecosystems is exported with water movement? If you look at the amount of C stored in a whole-of-ecosystem, the riverine

flux would likely be tiny I would think. Pg 10091, Line 18: Forests are the major terrestrial biome? I think it is pretty well documented that wetland soils are a tremendously large source of OC (see Aitkenhead and McDowell 2000). Pg 10092, Line 1: The identification of flow paths in forested catchments has been elusive" I think that sub-surface flowpaths are pretty well documented (see McGlynn and McDonnell 2003). It would be more appropriate to say in tropical forests? Pg 10092, Line 29: Change "observed" to "measured" Pg 10093, Line 5: Change "water cycle" to "hydrologic fluxes"

Materials and Methods The other reviewer commented quite extensively on this section so I only have a few comments. The naming of the six storm events is somewhat confusing and not intuitive? Can the authors think of a different way to name these six events?

Section 2.3: The water was allowed to sit in the lysimeters for as long as a week before collection. It seems like it to me there is the potential for substantial accumulation/removal/transformation (POC to DOC) of OC during this period. Because the OC production rates are based on this concentration, the potential for changes in OC during this period should be addressed.

Section 2.6: What is DOC infiltration referring too? I read it as the amount of DOC produced in the soil as water percolates through the soil? I think DOC production would be more appropriate, that is, unless the authors are referring to the downward movement of DOC in the soil profile. Also, what are the DOC concentrations in throughfall? I assume they were measured. How much of this DOC in the soil waters is attributed to throughfall rather than physical leaching with water movement through the soil? Throughfall concentrations of DOC can be extremely high (Michalzik et al. 2001), particularly if the lysimeters are sitting directly underneath a coniferous tree.

Results Are generally fine, but section 3.3 should be expanded considering one of the primary objectives is determining OC fluxes. Perhaps a table and some reporting of the OC flux rates, rather than reporting them for the first time in the discussion. Also,

how was baseflow determined for the comparison made in Fig 4?

Section 3.3: How can one collect water at 5 cm during the dry season? Considering there was only one measurement during this period, I would say it is a stretch to make comparisons between the wet and dry season. A concentration of 79 mg C L-1 C is extremely high and I find it difficult to believe there would be that much difference in DOC concentration between the wet and dry season considering the source of DOC would be the leaching of the soil surface horizons.

Discussion The discussion has a fair bit of results and methods that should be included earlier in the manuscript. For example, the API should be included in the methods section and Fig 8 should be described as part of the results. Also, how DOC and POC flux rates were determined should be included in the methods and their subsequent discussion should be as part of the results section. Removing these methods/calculations from the discussion will greatly improve the readability of the discussion section.

Section 4.1: Again, how can one compare one dry season soil solution point to the many collected during the wet season? I think a baseflow vs. stormflow comparison would be more appropriate. The calculation of DOC and POC flux rates using a linear regression is a common method, but looking at Fig. 6, this method is hardly appropriate. For DOC, I see two potential DOC vs Q regressions. Why weren't the data split like POC was in Fig 6b? For POC, I see absolutely no relationship between POC and Q for the late events and a linear regression is not appropriate for the early events. Yes, the regression coefficients are fairly high but they driven almost entirely by a few points. Considering the amount of data collected during the wet season, I think a sample-discharge interpolation method described by Hinton et al. (1997) would be the most appropriate. At a minimum, a better attempt needs to be made to relate discharge to concentration in order to calculate annual OC flux rates.

Section 4.2: - In order to accurately calculate DOC production with water movement in the soil, water needs to be collected from a lysimeter immediately following a rain event.

Second, areal DOC inputs from throughfall should be corrected for. It is likely that the amount of DOC produced is overestimated here. - How can DOC be stored in the soil through water movement? Do the authors mean OC that is re-located from upper soil horizons to lower soil horizons? - Where does 0.5% come from? - It is inaccurate to say that 0.44 t C ha-1 is stored in the soil. Based on the methods described here, there is no way to determined how much DOC is removed and potential stored through physical sorption. Soil sorption leaching experiments need to be conducted in order to estimate this number. Much of the 0.44 t C ha-1 could be re-mineralized to CO2, because freshly leached organic matter is highly labile. Based on the fact that the range in soil respiration is 2-6 t C ha-1 yr-1, I don't know how you could begin to compare these 2 numbers. I recognize that estimating soil carbon fluxes is very difficult, but the methods described are inadequate for the comparisons made.

Interactive comment on Biogeosciences Discuss., 6, 10089, 2009.