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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.

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RE: "Organic carbon sequestration and discharge from a deciduous forest catchment in Korea", by S. J. Kim et al. (MS#: bg-2009-247)

Authors' Reply to Referee #2:

We thank you for the review of the above manuscript submitted to Biogeosciences. Referee's comments were carefully considered and incorporated in the revised manuscript as below.

General Comments: 1. "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."

→ In this study, we have focused on the estimation of organic carbon (OC) efflux from a deciduous forest catchment to estimate how much OC is escaping from the system through stream discharge. Hence, we have measured the amount of DOC that was infiltrated/transported with water, but have never used this as an estimate of carbon stored in the soil. In the discussion, we introduced an overall carbon budget of the forest ecosystem to provide a picture of the relative magnitude of OC efflux.

2. "Second, one of the primary objectives was to calculate riverine C fluxes, and the authors found that these yields are quite small compared 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."

→ Indeed, the contribution of DOC was relatively small in terms of whole-ecosystem carbon budget components such as net primary productivity (NPP) and ecosystem respiration (RE). However, one of the key variables of interest is the net ecosystem exchange (NEE) of carbon, which is a small difference between the two large quantities whose signs are opposite (i.e., NPP and RE). The annual efflux of organic carbon was estimated to be 4 to 14% of the annual NEE at this site. Again, here we emphasize the comparison of OC efflux against NEE (= NPP-RE). To incorporate the review's suggestion, we have added the relationship between DOC & POC effluxes and stream discharge in the revision (section 4.1).

Specific comments

Title

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

→ We have removed the term, "sequestration" throughout the manuscript. Accordingly, the title and the first sentence in the abstract were changed as follow: "Organic carbon efflux from a deciduous forest catchment in Korea" ". . .. . . critical processes that affect the efflux of dissolved organic carbon (DOC) and . . .. . ."

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.

→ To incorporate the reviewer's suggestion, we have changed the word "discharge" to "export" or "efflux" throughout the revised manuscript as:

Abstract ". . .. . .in most forest ecosystems and their efflux may not be negligible . . .. . ."

". . .. . .(2) how much DOC and POC are exported from the catchment . . .. . ."

". . .. . .The annual effluxes of DOC and POC from. . .. . ."

". . .. . .Overall, the annual efflux of organic carbon"

". . .. . .ignoring the organic carbon efflux from forest ecosystems. . .. . ."

Introduction

". . .. . .strongly affected organic carbon export from. . .. . ."

". . .. . .Understanding the flow paths of DOC export from. . .. . ."

". . .. . .DOC and POC effluxes from Korean forested. . .. . ."

". . .. . .(2) how much DOC and POC are exported from the catchment. . .. . ."

Discussion

"4.1 Effects of storm events on DOC and POC effluxes"

". . .. . .DOC and POC effluxes were used. . .. . ."

". . .. . .annual DOC and POC effluxes from the. . .. . ."

"4.3 Effects of antecedent precipitation on DOC efflux"

". . .. . .The organic carbon exported from the ecosystems. . .. . ."

Also, changed the word to "POC efflux" and "DOC efflux" in Fig. 7.

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."

→ We have rewritten the sentence without the DOC production part that we did not discussed in this study. Hence, the revised sentence has been changed as follow: "Soil infiltration and surface water discharge of precipitation are critical processes that affect the efflux of dissolved organic carbon (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."

→ Changed as suggested.

Pg 10090, Lines 24-25: Change to "inaccurate estimation"

→ Changed as suggested.

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.

→ As mentioned in the manuscript, the transport of terrestrial carbon into streams, rivers and oceans is an important link in the global carbon cycle. We deleted sentence about C transport from terrestrial ecosystems, and changed the first paragraph as: "A significant portion of carbon stored annually in terrestrial ecosystems is exported with water movement in both organic and inorganic forms, which are defined as particulate organic carbon (POC), dissolved organic carbon (DOC), and dissolved inorganic carbon (DIC) (Meybeck, 1982; Hope et al., 1994; Prentice et al., 2001; Canadell et al., 2007; Battin et al., 2009). The transport of terrestrial carbon into streams, rivers and eventually the oceans is an important link between carbon and water cycles in various spatial and temporal scales (Ludwig et al., 1996; Warnken and Santschi, 2004; Battin et al., 2009). Hydrological processes strongly affected organic carbon discharge from terrestrial ecosystems especially in the monsoon climate regions. In East Asia for example, 60 to 80% of annual organic carbon is exported to the ocean during the summer monsoon (e.g., Tao, 1998; Liu et al., 2003; Kawasaki et al., 2005; Zhang et al., 2009)."

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.

→ Here, we meant carbon stored in ecosystem on an annual basis, i.e., the total amount of carbon sequestered by the ecosystem for each year. We have revised the text accordingly.

Pg 10091, Line 18: Forests along with wetlands 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).

→ Yes, forests are the major terrestrial biome. Forests cover approximately 9.4% of the Earth's surface (or 30% of total land area). Indeed, wetlands also cover about 6% of the Earth's surface and are one of the largest sources of OC. Also, the Han River basin (26,018 km2) in Korea for example, forests occupy about 67% (17,337km2) and wetlands occupy <39 km2 (http://www.wamis.go.kr, 2000). We have revised the sentence to add wetlands as, "Forests along with wetlands are the major terrestrial biome. . .."

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?

→ To incorporate the reviewer's point, we have revised the text as, "The identification of flow paths in forested catchments has been well-documented. However, their field measurements are very difficult especially when the sub-surface flows occur through macropores. Forest catchments are spatially complex and . . ."

Pg 10092, Line 29: Change "observed" to "measured"

→ Changed as suggested.

Pg 10093, Line 5: Change "water cycle" to "hydrologic fluxes"

→ Changed as suggested.

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?

→ We apologize for this inconvenience. The main emphasis in the naming was the date of occurrence (i.e., yy-mm-dd). Regrettably, these names have been registered already in the HydroKorea Database (http://www.hydrokorea.org) and we want to be consistent, if possible.

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.

→ We had attached the screen at the outlet of lysimeters so that the particulates were not allowed to enter the collector. Also, we had collected the samples immediately after the storm events to prevent such overflow and transformation. However, during the periods of baseflow, DOC transformation might have occurred in the collector. This point has been addressed already in the original manuscript in section 2.3.

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.

→ We calculated the infiltrated DOC by multiplying the water infiltration and DOC concentration in the soil water. However, this is not the DOC produced in the soil but the DOC that was exported from the soil system during precipitation. We calculated the downward movement of water and DOC from the groundwater recharge. In Fig. 5, we have shown the concentrations of DOC from throughfall, soil water, shallow groundwater (0.5 m), deep groundwater (0.8–1.0 m), spring water, baseflow, and stormflow. DOC concentration ranged from 3 to 8 mg L-1 in the throughfall. The DOC concentrations in the deciduous forest site were not as high as those in the coniferous forest site. Because coniferous trees intercepted more precipitation, it may have resulted from longer precipitation contact time in the coniferous trees. We assumed that soil water contains DOC from throughfall, litter layer, and soil layer. We did not consider sorption mechanisms (i.e., physical leaching).

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?

→ To incorporate the reviewer's suggestion, we have merged Results section and Discussion section into Result & Discussion, where section 3.4 has been assigned to the results regarding OC effluxes. Baseflow was determined based on the hydrograph separation (in Fig. 2), which was further verified by the interrupt of surface discharge observed about 4 days after storm events during the field observation.

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.

→ We collected soil water sample by employing tension free lysimeters after precipitation. If we used the tension lysimeter for soil water collection during non-precipitation periods, it might have resulted in overestimation of DOC due to collecting pore water. However, we collected gravity soil water by tension free lysimeters. During this study, we were unable to collect samples before the summer monsoon because of dry spell. Indeed, a single measurement during the dry periods may be insufficient to represent. However, 60 to 80% of the annual precipitation is concentrated during the summer monsoon season in Korea. We assumed that the concentration remained relatively constant prior to summer monsoon due to dry period in this region. Such a maximum concentration resulted from the increase in DOC concentrations following the rewetting due to small amount of precipitation (9.4 mm on 15 June) after dry periods. During the first storm event, much of the accumulated DOC discharged and infiltrated (Fig. 3 & Table 5). The increasing DOC concentration in the shallow groundwater prior to rainfall event is shown in Fig. 4. (Such a high DOC concentration in the surface soil and its seasonal variation were also reported by Kawasaki et al. (2005) and Boyer et al. (1996)).

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.

→ The explanation on API and T has been moved to the Method section as "2.7 Analysis of antecedent precipitation index". As indicated above to incorporate the reviewer's suggestion, we now have merged the results and discussion sections and the text has been revised accordingly.

Section 4.1: (a) Again, how can one compare one dry season soil solution point to the many collected during the wet season? (b) I think a baseflow vs. stormflow comparison

would be more appropriate. (c) 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. (d) 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.

→ (a) We already have answered as above. (b) Because there were differences in sources and concentrations of the baseflows in different seasons, the comparison between baseflow and stormflow is not appropriate. We have added and verified the DOC & POC concentrations during the baseflow period in the calculation of annual OC efflux. (c) We separated early and late events for DOC efflux in the revised Fig. 6 (as also pointed out by the other reviewer). (d) Hinton et al. (1997) also use a linear regression of DOC and POC discharge against the stream discharge.

Section 4.2: - (a) 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. (b) 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? (c) Do the authors mean OC that is relocated 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.

→ (a) As the reviewer pointed out, we did collect the sample immediately during events observation periods to prevent overflow and decomposition. (b) Areal DOC inputs from throughfall was not estimated in this study. (c) Yes. We mean OC that is re-located from upper soil horizons to lower soil horizons. We agree with reviewer that it is inaccurate to say that 0.44 t C ha-1 is stored in the soil. Therefore, 0.5% is meaningless here. Based on the above comments of the reviewer, we have deleted this section from the revised manuscript. Thank you.

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