Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2017-621-RC2, 2018

Hydrology and Earth System Sciences
Discussions
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Interactive comment on "Timing of land–ocean groundwater nutrient fluxes from a tropical karstic region (southern Java, Indonesia)" *by* Till Oehler et al.

Anonymous Referee #2

Received and published: 16 January 2018

This paper focuses on the nutrient fluxes to the coast associated with groundwater discharge in the karstic Gunung Kidul region of Indonesia. The stated aims are twofold; 1) to identify groundwater recharge and flow to the coastal zone, and 2) to elucidate temporal variation in nutrient fluxes to the coast due to groundwater discharge. This subject matter is suitable for publication in HESS, but unfortunately neither of these aims is well addressed in the current manuscript. Robust conclusions are limited by a) assumptions that are not justified, and b)reliance on relatively sparse data on groundwater flow rates (high temporal resolution but only at one point in the groundwater system, 10km from the coast) and nitrate concentrations (at multiple spatial locations but temporally sparse) without adequate consideration of the inherent uncertainty in their

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approach. In the introduction, the stated novelty of this study was to capture temporal behavior, but in the closing statement of the conclusion, the authors themselves seem to be acknowledging that the data set presented doesn't adequately capture temporal variability. The authors should reconsider the novel contribution that can be reasonably made using these data before resubmitting their manuscript.

Specific comments

This first aim is very similar to a 2016 paper published by one of the authors (Eiche et al 2016) and it seems that perhaps this aim has already been addressed in the previous paper. If additional novel insights are provided in this new manuscript they should be more clearly highlighted. While precipitation data are presented, recharge is not explicitly estimated, and subsurface flow to the coast was only measured at one location within the karst system. The authors seem to assume that discharge measured within the subsurface is approximately equal to, or at least correlated to recharge, which is probably a reasonable assumption, but this is not clearly stated. What are the implications of "piston flow", as reported in Eiche 2016, on the assumptions made in this paper, does this change the time lag/concentration relationships relative to rapid transport of "new" event water through conduits?

The major reported finding is that nitrate fluxes to the coast are highest during heavy rainfall after a dry period, when both groundwater discharge and nitrate concentrations are high. The temporal resolution of the data, and the data gap in discharge measurements when highest NO3 was measured, makes it difficult to justify strong conclusions. The temporal resolution during the recession period April-July is good, and supports the interpretation, but the rest of the record is arguably too patchy to make strong conclusions about NO3 concentrations during high flow events. The increase in nitrate concentrations during the Dec 2016 rainfall event, and decreases in nitrate during the dry period May-July 2016 event do seem to support the conclusion that nitrate fluxes to the coast are highest during heavy rainfall after a dry period, when both groundwater discharge and nitrate concentrations are high. However, the highest nitrate concen-

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trations were actually measured during a period of approx. average discharge in Sept 2016 (there is a gap in the measured discharge time series at immediately prior to the measurement of these peak concentrations). Correlation and trend statistics are not presented, but the authors report both positive and negative correlations between discharge and nutrient concentrations, which seems to suggest that across the data set, there is actually no correlation. A more robust statistical treatment should be presented to support the authors' interpretation of the data and justify their conclusions.

Dissolved silica and nitrate fluxes to the coast associated with groundwater discharge are estimated by multiplying snapshot measurements of Si and NO3 concentrations at the coastal springs with groundwater flow rates measured at one location inland on either 4 or 14 days prior to the measurement of nutrient concentrations. These time lags (<4 and 14 days) are assumed based on tracer studies reported in a report published in the 1980's, and a tracer test conducted in 2012. It is not clear the extent to which groundwater flow conditions during these previous studies relate to the current study. Do the results of Eiche et al. 2016 not provide more recent insights? Regardless of the time lags used, the assumption that discharge measured approx. 10 km inland of the coast on one specific day and at one location is adequate to quantify the groundwater discharge rate at the coast 4 or 14 days later seems an oversimplification. At a minimum some attempt should be made to quantify the uncertainty in the calculated solute fluxes.

It is also not clear exactly what was sampled at the coastal springs. The authors report that these samples were brackish, suggesting these samples were a mix of groundwater discharge and seawater. In which case concentrations measured in these samples would reflect a mixture of these to end memebers. This seems to be what the authors are referring to on Pg 5 when they say brackish values were "normalized" to freshwater according to Hunt and Rosa 2009. Looking at the cited report, it seems likely that the authors did a mixing calculation to work out the concentration in the groundwater component of their brackish samples. If this is the case then the calculation and val-

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ues used need to be explicitly stated and uncertainty in this calculation quantified and propagated through the analysis.

Some of the data that is presented does not seem to link to the stated aims of the paper. In addition to precipitation, discharge and concentration data, the authors also present data on stable isotopes of water and sea surface temperatures. These data sets do not seem to be well linked to the stated aims and for the stable isotopes in particular, their inclusion in the manuscript could be reconsidered. The sea surface temperature data do show areas of low variability along the coast that the authors interpret as points of groundwater discharge. However, these zones of discharge seem to have been previously identified and named, so this qualitative confirmation of their location seems to be of limited value in addressing the stated aims. The interpretation of stable isotopes data is relatively superficial, and does not meaningfully link to either recharge processes or discharge fluxes. A link between the amount of rainfall associated with recharge events and stable isotopic composition could be expected (i.e. more depleted values in larger rainfall events), but this is not discussed. Increased temporal resolution of stable isotope data may have provided confirmation of time lags between groundwater flow at the subsurface discharge measurement point and groundwater discharge at the coast. However, given the resolution of the stable isotope data presented ($\sim$monthly at best), the value of these data in addressing the stated aims seems minimal. The same could arguably be said for the dissolved silica data (fluxes are calculated by their significance to the aims of the paper is not clear), Concentrations of nutrients other than NO3 are also presented in Table 2, but not discussed in any depth and fluxes are not calculated. Similarly, pH and DO data are reported in Table 2, but don't seem to be used in the analysis.

Technical corrections

Pg1

Abstract: L19 The timescale of recharge and transport to the coast is not explicitly

measured in this study, it is assumed from previous work.

L23 Dsi is not defined. L24 Consider rephrasing to avoid the word “counterintuitive”, this seems to demonstrate that dilution is not a dominant control on nitrate fluxes to the coast.

Q. How do these estimated nutrient fluxes to the coast compare to river outflow and runoff, or submarine groundwater discharge?

Pg2

L4-6 References required, and there seems to be more than one idea in here: one is about groundwater travel times, the other is about nutrient retention rates. And do you actually mean in the aquifer, or do you mean in the soil/unsat zone? It’s not clear by what mechanism nutrients would be retained in the aquifer under high discharge rates.

L12 “despite of the” needs changing, and L14 “at the example” needs changing

L16 I wouldn’t agree that 1 years worth of data constitutes a “long term data set”.

L33 The Gunung Sewu area is not clearly defined on Fig 1, the word is labelled, but where is the boundary?

Pg3

L6-7 What is an “underground full dam”? Is it simply a cavity in the limestone that is below the watertable? Is it filled with sand? What makes it a “dam”? And presumably 75000 people, not 75

L15 Nanonyo 2014 seems to be a PhD thesis (thought this is not written explicitly as such in the reference list). It would be better to cite the published journal articles that came out of the research work, rather than the thesis.

L19 For clarity I suggest the authors use something like “subsurface flow” to refer to water flowing in the subsurface, and restrict the use of “discharge” to the actual discharge

of groundwater at the coast. Water flow within an aquifer is not generally referred to as “discharge”, in a groundwater context “discharge” usually refers to water leaving the aquifer, as the opposite of “recharge”.

L29-32 Where are these branches on the site map? If the major subsurface flow paths have been mapped these should be shown on Fig 1.

L33-7 This seems to imply that groundwater flow within the matrix only happens during low-rainfall periods, which is not the case. Groundwater flow within the matrix would be continuous, but small relative to the amount of groundwater flowing along conduits following rainfall events. Also, be careful to be clear on the two separate processes of recharge and groundwater flow, the two seem to be used here almost as if they are the same thing. The relevance of the water quality comments to end this section to the study is not immediately clear, expansion of this paragraph may be helpful.

Pg4

L4 Multiple lines of evidence have been used in this study, but it doesn’t seem like multiple methods were actually “compared”. This implies that the same types of estimates (i.e. discharge volume) came out of each method and these values were compared, but this is not actually the case.

L15 This sentence needs rewording.

L17 It seems as though the location of groundwater discharge at the coast was identified prior to this study, the authors should clarify exactly what the new contribution of this SST data is relative to what was known previously.

L24 The relevance of the Siebert 2014 reference here is not clear. “groundwater uninfluenced” is a rather clunky way of putting it.

L28 Figure 3 is referred to prior to Figure 2 (not cited until pg 6)

Pg5

L1-4 What is the relevance of the pH, and DO data to the manuscript?

L5 Delete “In Germany” and correct “isotopy”

L8 Are the nutrients also dissolved? Or ar these total NO3 etc.? If you filtered then aren’t these dissolved? And why didn’t you estimate fluxes of all nutrients, why only Si and NO3?

Section 3.3 The assumptions made in this section do not seem overly simplistic, as mentioned above. Event-scale variation in stable isotopic values may be able to back up/test these assumptions – i.e. depleted signature during heavy rainfall.

Section 4.1 First paragraph is not results and is mostly repeated from earlier in the manuscript.

Pg6 L3-19 The value of this detailed description of precip data to the manuscript is not clear. A more concise treatment may be to calculate the time lags between precip and groundwater flow or discharge at the coast, rather than a full description of each event, which can be seen on Fig 2 anyway.

L22-23 “to which it is bounded” isn’t quite right. Avoid the use of the word “shows”, as it is not the correct word.

L28-32 The relevance of this stable isotope analysis to either groundwater recharge, flow or discharge at the coast is not clear.

L34-35 Why define DSi? Why not just use Si – NO3 is also dissolved isn’t it? Fig 5A+B – just call it Fig 5 (it only has the two parts).

L36-37 Avoid using terms relative terms like similar (without specifying what it is similar to) and high (what exactly does “high temporal variability” mean?)

Pg7 L5-15 Can you do some stats to back up your interpreted relationships? Are NO3 and Si correlated with subsurface flow? Or are there lag times between peak NO3 and peak flow rates? Also, what is the value of the Si data in this analysis? It doesn’t seem

link back to your stated aims.

Discussion: The discussion contains references to a number of figures, which suggests that these comments should have been made in the results section. The discussion should not present or highlight new information about the data that wasn't already presented in the Results section.

L19 Avoid general terms such as "a major amount" and "A further part".

L22 A lag time of 4 days between precip and groundwater flow is not self-evident from Fig 2, and why was this not highlighted in the results section? Previously you mentioned the lag times as having been assumed from the results of previous studies.

L25-27 (and Figure 4) Given rapid infiltration and recharge during rainfall events, why do the stable isotope values not plot on the local meteoric water line?

L30-37 This discussion seems tangential to the current study. You have said in your introduction that rainfall events increase turbidity in the subsurface, and referenced your earlier paper. It doesn't seem like this paper has contributed anything new to our understanding of E. Coli or tourism. The majority of 5.1 seems to have already been covered by Eiche et al 2016.

Pg8 L4-10 This paragraph discusses correlation between data sets, but no correlation statistics were reported in the results section. What does it mean to have both positive and negative correlation? Does this mean there actually isn't a correlation if you look at the full data set?

L11-19 The relevance of the Si data and interpretation to the stated aims of the paper are not clear. You say here that the Si concentrations are diluted during low flow events, does this then support your interpretation that NO3 stores must be released from the unsaturated zone during floods? On Fig 5, during the recession period where you actually have good temporal resolution of data, Si increases while NO3 decreases, what does this mean in terms of process? You write "a further DSi source" do you

mean further spatially, or an additional source? The relevance of the comments on colloidal transport to the current study is not clear.

L20-35 Delete “from these fertilizers” at L30. What is “temporal exhaust” on L 35?

Pg9 L1-3 This seems more like an introductory statement. Correlation is mentioned again, but stats not reported.

Section 5.3 The treatment of uncertainties is inadequate given the assumptions in the analysis (see comments above). This section identifies some sources of error, but does not actually report any quantified uncertainties. L9-10 What do you mean by “A general connection. . ..was deduced from temporal variability of hydrochemistry”?

L18-21 The conclusion begins by acknowledging that a vast area of hinterland may contribute to nutrient discharge at the coast, so it is not clear how does the spatially sparse data set (on groundwater sampling location) can provide a robust estimate of nutrient fluxes. L28 What is “highly variable”?

L32-35 (and L1-2 Pg 10) This seems to be suggesting a better sampling design for the current study, to capture temporal variability by measuring at a higher temporal resolution.

References: Eiche 2016 title is incomplete.

Fig 1. There is a light blue colour in the mid-left of the map that doesn’t seem to be explained in the legend.

Fig 2 Why are the discharge data so patchy?

Fig 4 X-type symbols are too similar.

Fig 6 Is the discharge shown on the day of the flux estimate, or the day used to calculate the flux estimate? Ie the discharge on the day NO3 concs were measured, or the discharge 4-14 days prior? Why use a bar chart instead of a time series?

Table 1 Use delta, not d for isotopes. NO2, NH4 and PO4 data are not discussed in any detail in the manuscript. Nutrients and NO3 are not the same thing. Is it a paper on all of these nutrients, or just on nitrate fluxes?

Table 2 Caption says average discharge rates, but table reports avg (presumably) as well as min and max, average and standard deviation would be more concise. Table should indicate which are dry season and wet season samples (=4 or 14 days prior to concentration measurement). Some uncertainty on the flux estimates should be provided. Why are fluxes of NH4 and PO4 not provided? Measured concentrations used to calculate fluxes should also be reported in this table.

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