

***Interactive comment on* “Suspended sediment fluxes in an Indonesian river draining a rainforested basin subject to land cover change” by F. A. Buschman et al.**

Anonymous Referee #1

Received and published: 21 September 2011

Review of “Suspended sediment fluxes in an Indonesian river draining a rainforested basin subject to land cover change”, Buschman et al.

1. General Comments

The paper submitted by Buschman et al. presents an original dataset of suspended sediment concentration in the tidal zone of an Indonesian river. This dataset is of particular significance because Indonesian mountains shed large amounts of sediments into receiving oceans that are hotspots of global diversity, in particular of coral diversity. Not only Indonesia naturally exhibit very high erosion rates because of its climate, relief

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and geology, but it has also undergone major human impacts in recent years (decade). Logging for timber and conversion to cultivated land has depleted vast swath of the natural rainforest, leaving bare soils exposed to erosion. The datasets presented in this paper were collected in a tidal region, giving hope that actual transport to the ocean may be calculated. Finally, both continuous data obtained with an OMS at a station over long periods (over 6 weeks) and vertical profiles obtained with an OBS at several locations across the river and each spanning about one tidal cycle are reported. Although the dataset is of significant value, the study is essentially descriptive and no real scientific question is addressed nor answered. The link between sediment transport and both biodiversity and land use is evoked as a motivation for the paper, and are of substantial scientific interest, particularly in that region of the world. However, no real insight into these issues is gained from the presentation of the dataset. The link with coral reef health is not addressed, and the land use changes are investigated by using a standard Universal Soil Loss Equation (USLE), so that the same conclusions could be reach anywhere. Only the structure of the equation drives the sensitivity analysis and no data is presented that could allow its calibration. The paper in general lacks focus and clarity. A better statement of the intended research in the introduction and of the new contribution in the conclusion would go a long way towards refocusing the paper. For example, the authors declare that “The present study serves as a benchmark reference for suspended sediment fluxes to the Berau coastal shelf for the situation in 2007”. Should we interpret that the study is ongoing and that there will be trends of sediment yield with land use change over time appearing in future studies? If this is so, maybe a description of the larger effort could help understand the significance of this piece or address its limitations. Also, a thorough editing of the language would help improve the paper. Although we understand that the authors are writing in a foreign language, form and content are intertwined and a clear exposition of the content is required. Finally, some of the tables and figures are also superfluous. For example, table 3 gives values of the C_m parameter of the USLE for various LU/LC, but a simple statement in the text would be enough. Reassessing which figures are necessary to

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present the research and support the conclusions would improve the paper.

2. Specific Comments

Abstract:

Rewrite the abstract after taking into account the general and specific comments.

Introduction:

The objectives of the study are not clearly stated. Although the background information on the impacts of sediment flux on coral reefs as well as the consequences of LU/LC changes are compelling, no link is made between the current study and the reefs. The LU/LC work is only presented shortly in the penultimate paragraph. If the impact of sediment flux on coral reef is important, then simulations under varying LU conditions of the transport to the ocean and reef may be interesting. As I mentioned in the general comments above, it would also be interesting to know if this paper is part of a broader effort as suggested by the assertion that this study is a benchmark reference. The data collected could be presented in more details, ie how many datasets were collected, for how long and when. Also, this dataset is unique as it comes from a tidal zone, yet no real emphasis is made. Why collecting sediment data in the tidal zone? How is it important compared to upriver stations e.g.

Field Site:

In the climate section, cite figure 2 after the first sentence. The spatial patterns of rainfall are discussed, yet only a lowland station is mentioned. It would be good to obtain a rainfall timeseries the year of the data collection (either at a station or from derived products). A plot showing rainfall, discharge and sediment concentration could then be constructed to highlight the data available and the relationship between the drivers.

Methods:

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#3.1: Even though the authors used methods reported in the literature and adequately referenced, more details on what they entail would help understand the procedure. Maybe a single sentence explaining how HADCP data are converted to discharge could be enough. However, since the discharge data is already published and referenced here, the scope of this paragraph may be sufficient. The authors should decide what works best for the reader. #3.2 and 3.3: Although the methods used to collect the data are reliable and have been published, their presentation in the paper was not very clear. It was not readily obvious what data were collected when, and I had to pry the information from table 2 and relate the dates to the text to have a sense of what was done when. It would help to describe the dataset either in the introduction or at the beginning of the methods section, clearly stating their length and frequency and time of collection. Also, Figure 4 is unnecessary. Just give the relationship equation and fit in the text. #3.4 : Using the USLE to assess sediment yields in a region with no prior calibration seems far fetched. Also, a discussion of why the authors chose the USLE over the Revised USLE may help understand their choice. Finally, a better argument that the USLE was only used to highlight the potential land use change impacts on sediment yields, rather than actual predictions, could improve this section. More specifically, the parameter R_e was taken from a relationship obtained in temperate climates and assumed to be transferable without further discussion. Also, in this section, all the terms in the equation should be given. K and P are only introduced in the corresponding results section (#5.2), but should instead be given here.

Results:

#4.1 and 4.2: Maybe the authors could present a plot of C vs Q , or S vs Q for the tidally averaged data. This would allow a better sense of the relationship between the variables.

#5.1: This should go in the intro and/or methods.

#5.2: The first paragraph belongs to the methods. Table 4 is superfluous. In the second

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paragraph, it is never said what values of C_m were used for the sensitivity analysis. Again, this should be explicit in the methods. The model predictions are not compared to the data obtained, although they seem to be of the right order (ie 2t/ha/y for USLE vs 2 Mt/12000km²/yr for the data =1.6t/ha/yr).

Discussion:

Table 6 should be presented in the climate data earlier. No new data should be given in the discussion section. Having recession curves would help understand where in the forcing the particular period under study falls. The results from the modeling are not discussed.

Conclusion:

The conclusions should restate the general questions addressed and bring the paper to closure. Here, no real conclusion is given and an open question is given in the last sentence.

3. Technical Corrections

I'd be happy to review a revised version of the manuscript and provide a list of remaining technical corrections at that point. As it stands now, there are too many details to provide a complete list of technical corrections.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 7137, 2011.

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