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Interactive Comment

Interactive comment on "Sediment yield model implementation based on check dam infill stratigraphy in a semiarid Mediterranean catchment" *by* G. Bussi et al.

G. Bussi et al.

gbussi@upvnet.upv.es

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We would like to thank the referee #4 for its useful comments. We will try to include most of his/her suggestions into the final version of the paper.

The reviewer states that "In the introduction, the authors refer to some other models but nowhere a critical discussion is provided making clear why these models are not suitable and why a completely new model approach needs to be followed. Why is it necessary to develop the TETIS model and include sediment transport? Why not using the sediment archive data behind the check dam to calibrate/validate existing models?

In the end, we are not waiting for as much models as there are studied catchments. The authors need to make this clear in the introduction."

In this paper, we did not intend to address the development of a new model. The aim of this paper was to propose a methodological approach for sediment sub-model calibration and validation using proxy information. One of the main reasons why we used the TETIS model is because of its split-structured effective parameters [Francés et al., 2007]. We think that this kind of structure helps simplifying the model calibration, which is a major advantage in poorly gauged catchment such as many semi-arid catchments. The TETIS model has been developed since 1995 and has been also extensively used in Spain and Latin America, both for research and consulting purposes (the most recent examples are Vélez et al., 2009; Andrés-Doménech et al., 2010; Francés et al., 2011; Bussi et al., 2012; Salazar et al., 2012). It has proved to be a reliable hydrological model for semi-arid catchments (and other climate conditions), and it is not a new model. But, as we stated in the paper, the aim was to present the calibration/validation procedure by means of a check dam deposit. We think that this procedure could be easily applied to other existing models. We will clarify this point in the final revised version of the paper.

The reviewer states that "One of the main problems associated with more process-oriented models (compared to eg RUSLE approach) is that it requires a lot of field data and many parameters need to be (locally) calibrated. Here, the hydrological TETIS models requires 9 calibration parameters and the sediment sub-routine another 2. With 11 calibration parameters, it is not surprising to see that the model predictions are quite good: the more knobs you can turn, the better the result will be. But this doesn't mean that model really captures well what happens. In fact, the model is only calibrated at the outlet of the catchment so all internal processes are lumped: despite the fact that the model is said to be distributed, it is validated/calibrated in a lumped way and there is no guarentee that the various processes operating in the catchment are simulated well. This also means that exatrapolation of the model to other catchments will be very difficult lowering the potential of the model for predictions strongly. None of these defficiencies is really discussed."

One of the main advantages of the TETIS model is that parameters do not need to be locally calibrated, but, thanks to the split structure of the model effective parameters [Francés et al., 2007], their spatial variability is preserved during the model calibration, only varying their absolute values. In this case, nine correction factors were calibrated, each of them affecting its corresponding parameter map.

Nevertheless, it is true that the reliability of its results is not proved if no spatial validation is carried out. Unfortunately, no spatial validation of the hydrological sub-model could be carried out due to the lack of stream gauge stations apart from the Rambla del Poyo station (catchment outlet).

In order to clarify this point, we carried out a spatial validation of the sediment sub-model, by using seven check dams, similar to the one described in the paper, distributed all over the catchment headwater, although we only know the present sediment volume at each of them. We applied the TETIS model in order to reproduce the sedimentation final state of each one. This validation was mentioned in the response to Reviewer #1 and was also detailed in Bussi et al. [2012b], but was initially not included in this paper. Nevertheless, we think that, seen the question raised by reviewer #1 and #4, the spatial validation should be incorporated to the revised version of this paper.

The results are shown in Fig. 1. Check dam 2 represents the check dam described in the paper. The agreement is substantially good for check dam from 1 to 7. The only relevant error can be seen in check dam 8, probably due to a poor characterisation of the check dam deposits.

In other words, the model (hydrology and sediment sub-models) can be considered spatially validated.

The reviewer states that "I also have doubts with the model calibration approach on

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page 3438. The soil moisture content at the end of an event is used as the initial state of SMC at the start of the next event. But, in semi-arid regions there can be a large time discrepancy between two events and thus the SMC can have changed quite a lot. This is apparently not considered but it can have a major impact on the model predictions (and on the calibration coefficients)."

As stated in the paper, we did not use the final state of one flood event as initial state of the following flood event. We used a daily scale model for simulating no-flood periods (i.e. the periods ranging between two floods, which can be very large, as stated by the reviewer) and obtaining soil moisture content maps for each flood event, to be used as initial state for flood period modelling with a 5 minutes time step (page 3438 lines 14-19). We will clarify better this procedure in the final version of the paper.

The reviewer states "The authors also use the Nash-Sutcliffe model efficience to evaluate the model. However, this method has a major drawback when the range in parameter values is much higher than the mean value: in that case it is more or less straightforward that ME is high. In this case, in a semi-arid environment with a few intense rain events, the range in Q is very high and a few high Q's are much higher than the average Q. Any model that more or less captures the runoff dynamics will predict much runoff after an intense rain event and thus it is logic that the predicted Q is higher than the average Q: the model will perform better than the mean and ME will be higher than 0. Figures like fig 6 should not be used to check the performance of models. It is better to plot the observed versus predicted value for either peak discharge or event runoff volume. The scatter on that graph will say much more on the behavior of the model than a temporal graph: off course when it rains Q goes up both for observed and simulated scenario's."

The first reason that led us to employ the NSE index is its massive use through the hydrological and sediment modelling literature. If the model performance is evaluated by means of the NSE index, it can be compared with many similar studies (see Moriasi et al. [2007] as an example of model evaluation and comparison using also the NSE

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index).

We totally agree with the reviewer about the limitations of the Nash-Sutcliffe efficiency index (NSE) in semi-arid areas. Nevertheless, we would like to point out that, as the reviewer states, in semi-arid catchments the NSE index tends to be higher if the model is capable to reproduce the highest values of the series (i.e. the water discharge peaks). In our opinion, this is an advantage: sediment transport is a time compressed phenomena which takes place only during very short periods of time corresponding with the highest values of water discharge. If the model is reproducing correctly water discharge peaks, it will describe better the sediment transport.

A NSE index simply higher than 0 does not indicate a satisfactory performance [Moriasi et al., 2007]. In our experience, we cannot agree with the reviewer when he/she states that NSE will be higher than 0 if the model is only capable to predict runoff generation. Something more would be needed. But in our case, the NSE index is not only higher than 0, it is 0.82 for daily time step model calibration, and 0.78 for 5 minutes time step model calibration.

The reviewer also states that "when it rains Q goes up both for observed and simulated scenarios". We think that this is a very rough simplification of Fig. 6 (of the original paper). In Fig. 6 it can be seen that not only "when it rains Q goes up", but that observed and simulated peak discharges are very similar.

The reviewer states that "The sediment processes are seperated for hillslope and river domains. For the hillslopes, the authors use equations from engineering handbooks but these are not common at all within the geomorphic community: most erosion equations not only varies the parameter alpha but also the slope and discharge exponent; yet these are fixed in this approach. Why ? Many studies have shown that the slope and discharge exponent can vary a lot (see eg Prosser and Rustmomjii in Progress in Physical Geography, 2000) and this has a major impact on the resulting sediment fluxes. Why do the authors choose for these equations ?"

The TETIS sediment submodel uses the modified Kilinc – Richardson equation [Julien, 1995]. This equation follows the typical formulation for sediment transport equations by overland flow: a coefficient (alpha) multiplied by slope and specific discharge, both raised to two different exponents. A review of these equations can be found in Julien and Simons [1985] and Prosser and Rustomji [2000]. Julien and Simons [1985] found that these exponents varies between 1.2 and 1.9 (slope exponent), and 1.4 and 2.4 (discharge exponent), being the mean values 1.66 and 2.035 respectively. These are the exponents used by the Kilinc – Richardson equations. Prosser and Rustomji [2000] recommended the ranges 1-1.8 and 0.9-1.8, suggesting the median values (1.4 for both exponents) in case of using a single value. These values are not far from the ones used in the Kilinc – Richardson equation.

Furthermore, the equation employed by TETIS is not a completely physically based equation, because it also takes into account empirical factors such as the C, K and P factor of the USLE. And there is always a spatial and temporal scale effect (discussed below). For these reasons, the exponents and coefficients partially lose their physical meaning and should be calibrated, if possible. We decide to calibrate only one parameter in order to avoid overparametrisation. After a simple sensitivity analysis, the alpha coefficient resulted to be the most influent between the three possible values to be calibrated (coefficient, slope exponent, slope discharge). This is a very interesting discussion, but model conceptualization is not the aim of this paper.

The reviewer states that “Absolutely no details are provided on the STEP model. How was this done ? In the original STEP paper, check dams are not modelled so how did the authors do this ? Part of the method is explained in the results section (page 3441, lines 15-20) and should be moved to the methodology section. Nonetheless, more info is needed on how STEP was used. Also lines 5-20 on page 3438 are in fact part of the methodology (of the hydrology model) and should not be places under the results section.”

We did not emphasise the STEP model description (which can be found at page 3435, between line 7 and 16) because the STEP model has already been fully described in Verstraeten and Poesen [2001]. In this study, we only coupled the TETIS model with the STEP model as described in its original paper, using the simulated water and sediment discharge as the input of the STEP model. We did not make any conceptual change in its structure. We also think that check dams are conceptually the same as the ponds modelled in the original paper. Anyway, we understand the key importance of the STEP model within this study and we will enhance its description in the revised version of the paper as demanded also by reviewer #3 (see reply to reviewer #3 for a more detailed description of the STEP model).

Concerning the position of both the hydrological and the STEP sub-model implementation, as we stated in the response to reviewer #2, we believe that this part should remain within the Results section; otherwise the reader will not have a comprehensive picture of the whole model implementation (which is the aim of the study). Therefore, we would be willing to move this part to the Methods section if the editor felt it necessary, and we would be grateful for his advice on the matter.

The reviewer states that "The authors provide no information on the spatial resolution on which the model is applied and thus also at which the input data were collected (figure 3). How accurate are eg soil hydraulic conductivities at higher spatial resolutions ? Is this realistic ? What is the impact of data input uncertainty on model outcome ? At present, no discussion on why the model predictions are not perfect is given but it would be interesting (and necessary) to see to what extent error in input data or rather an imperfect model approach are responsible for this. No discusson on the accuracy of the sediment yield data obtained from the check dam is provided either."

The model was implemented on a 100x100 m mesh. This mesh was chosen as a compromise between map accuracy and computational time.

Soil data were taken from the LUCDEME project database [Rubio et al., 1995]. 53 soil analyses were collected from points located into or close to the Rambla del Poyo catchment, within an area of around 1,500 km2 (one soil profile each 30 km2 approximately). Soil profile locations were chosen in order to cover all soil units. Soil texture data, organic matter content and soil salinity data were used to feed the Saxton and Rawls [2006] pedotransfer functions and obtain available water content and infiltration capacity. Percolation capacity was obtained by reclassifying the lithological map following permeability values from literature.

We will add these details to the revised version of the paper.

As stated by the reviewer, these parameter maps are certainly affected by errors and uncertainty. Nevertheless, as explained by Beven [1989], the calibrated parameters of a conceptual distributed hydrological model such as TETIS must be considered as effective parameters, and are scale-dependant. And this is true for any model which is not using the proper integral equations. This is what is called spatial and temporal scale effect (e.g. Wood et al., 1988, 1990; Blöschl and Sivapalan, 1995; Frances et al., 2007).

A discussion about the separate impact of parameter errors, input data errors or model errors would be certainly interesting, but we think that it would be out of the scope of this paper. In this case, both the TETIS hydrological and the sediment sub-model were proved to be reliable by showing their validation results.

The reviewer states that "Furthermore, the description of the sediment archive is not only too wordy but also dispersed. Paragraph 3.2 describes the sediment infill but not the volume as the title suggests. Part of section 3.3 discussed the event stratigraphy whereby comparison with the model is made but a proper discussion of the event stratigraphy as such should be made prior to this comparison. I suggest to have a single paragraph on the observed sediment stratigraphy and discuss the sediment volumes with it (both total as per event)."

A reference to Tab. 1 (of the original paper) is missing within the Section 3.2. Tab. 1 shows the estimated layer volumes. We apologise for the error. We will revise the entire section in order to be sure that it will not result too dispersed. We will also separate the sediment model validation from the stratigraphical analysis.

The reviewer states that "Based on figure 8 (the caption of fig 8 and 9 are switched) I wonder if the correlations made are all correct. Both trenches are located more than 20 apart and the spatial variability in sediment characteristics can very quite a lot. Eg: silt layer (unit 2) could be one layer as it is suggested here but it could also be a small pocket of silt deposited in a small pool but not as a continous layer."

The differentiation between layers was done by recognising all kind of discontinuity elements such as mud cracks, root marks, changes in the sedimentary structure, organic matter, non-natural materials, etc. These characteristics were also used for establishing a correlation between the two trenches. Texture and charcoal content were also used to corroborate this correlation (pag 3437, lines 18-22). Furthermore, the column depicted in Fig. 8 (of the original paper) is part of a vertical stratigraphical panel drawn with a 1x1 m mesh at centimetre resolution (pag. 3437, line13), which helped recognising the transversal structure of the layer. Specifically, in the case cited by the reviewer, the panel helped discarding the possibility of unit 2 being a small pocket of silt, given that the transversal shape of this layer did not show any evidence of it. We will include this explanation into the revised version of the paper. We will also correct the Fig. 8 and 9 captions.

"Eq 5 is certainly not proposed by Bellin et al but is a standard approach for calculating SY from dam sediments that is used much longer."

Yes, Eq. 5 is a simple standard approach used to compute SY from reservoir deposits. It was recently used by Bellin et al. [2011], among many others. We will correct this sentence in the revised version of the paper.

"It would be revealing to see a temporal graph with declining TE and varying SY"

We attach a graph (Fig. 2) in which the sediment yield and the trap efficiency are shown for each event. A decreasing trend in trap efficiency cannot be seen because many other variables are involved into TE computing a part of the reservoir capacity (influent water and sediment discharge, sediment texture and settling velocity, etc.). Three events show a TE of 100%, due to their low magnitude. We think it will not be profitable to add this figure and the corresponding comments in the final paper.

"It is stated that the two predictions of total SY are in close agreement but the opposite would be surprising as both calculations use the same assumptions and model outcomes. Since the model is calibrated on the total sediment volume and also TE is calibrated, it is thus logic that both values are more or less identical."

As stated by the reviewer, the model is calibrated on the total sediment volume, and the trap efficiency used for specific sediment yield in Eq. 5 is the average TE provided by the TETIS model, so the coincidence between the two values must not surprise. We will remove the sentence ", which is very similar to the value obtained by the model" (page 3444, line 14).

"The authors state the modelled texture of deposited sediment agrees with field measurement but nowhere data is shown to illustrate this and that could support this."

As a confirmation of this statement we attach a graph (Fig. 3) in which the comparison between the measured and the simulated sand content of each layer can be seen. This figure confirms the substantially good performance of the STEP model in reproducing the texture of the deposited material. We will add this graph to the revised version of the paper.

"Although the paper is relatively well-written, it is advised that a native speaker goes through the manuscript prior to resubmission. Especially from 2.2 on there are still many linguistic errors."

We will revise carefully the whole paper and will try to correct the many linguistic errors highlighted by the reviewer.

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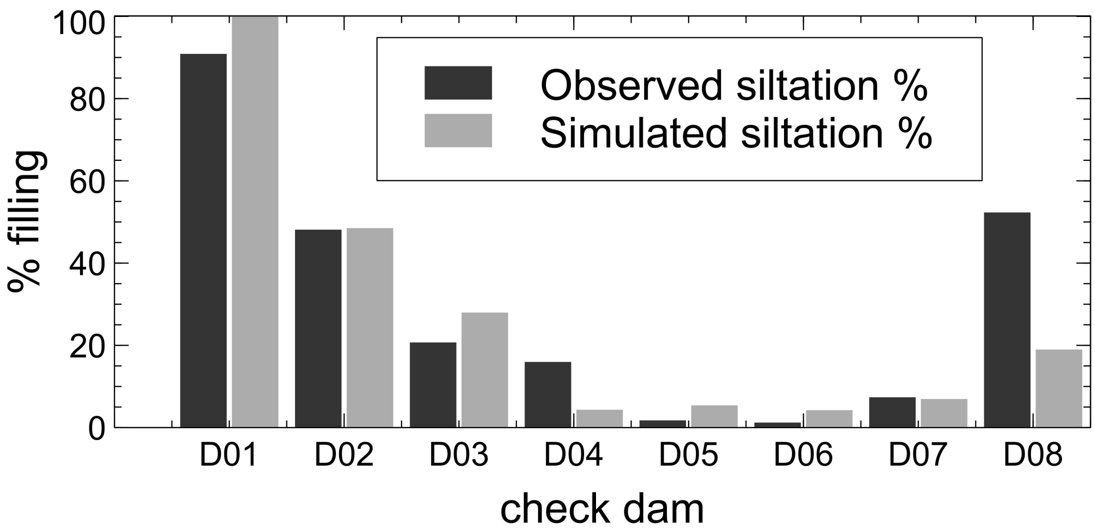

Fig. 1. Sediment sub-model spatial validation.

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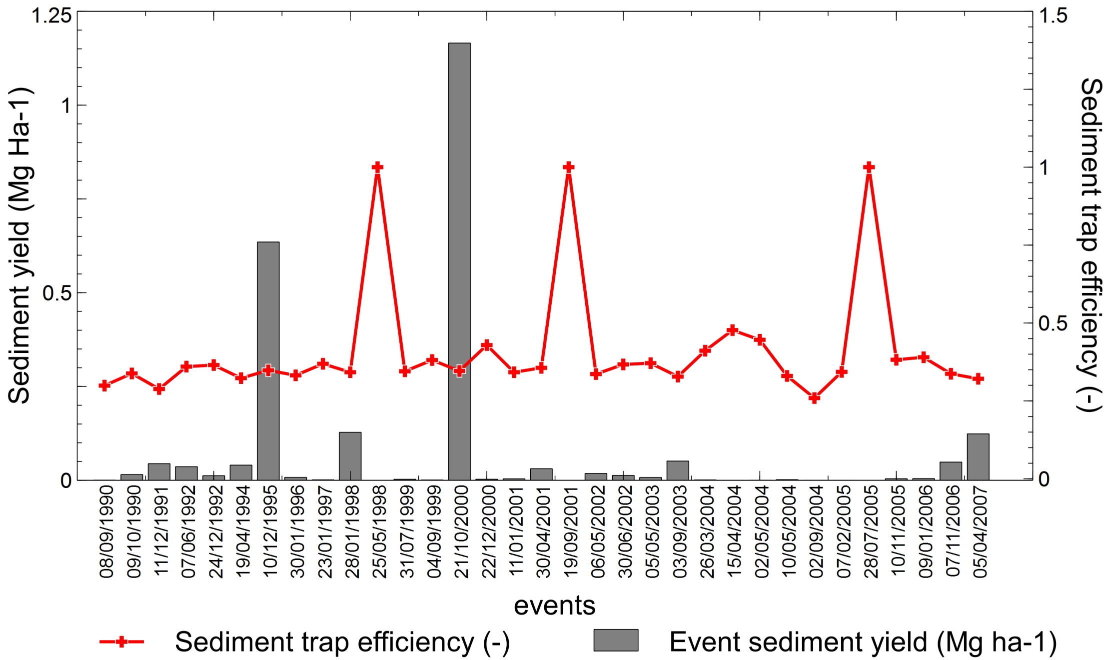

Fig. 2. Temporal evolution of sediment yield and sediment trap efficiency.

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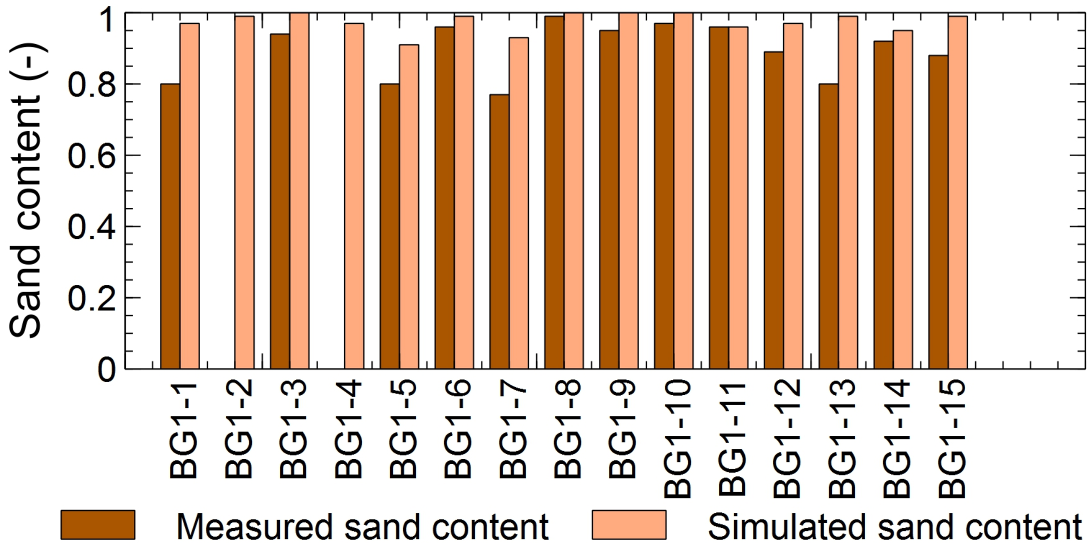

Fig. 3. Comparison between the measured and simulated sand content of each sediment layer (the texture of the layer BG-4 could not be measured).

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