

Interactive comment on “Using globally available soil moisture indicators for flood modelling in Mediterranean catchments” by C. Massari et al.

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Replies to the comments of the Referee Thomas Graeff 1) The topic is relevant to the journal. Estimating plausible initial conditions are of great importance and interest for flood forecasting and many other related topics. To bring the text into a publishable article a lot of work has to be done. The text has to be restructured. In the present stage it is hard to follow the intention of the authors. There is no clear story line. The abstract needs more focus on the presented story and has to be rewritten. There is too much methods block included in the results, which should be moved. By focusing on runoff response based on a better estimation of initial conditions the two other major aspects of precipitation estimation as input and runoff observation as output are here

C5786

in a manner presented which is too weak. And the aggregation of the different products is not explained.

Reply: We thank the reviewer for his valuable comments. In the revised version of the paper, we will address all the reviewer's suggestions. In the sequel (please see the supplement PDF version of the replies), we provide the reply (in red) to each comment (in black); in blue, we report the parts of the manuscript that serve for clarifying our replies. We thank the reviewer for understanding the relevance of the topic, however, we respectfully disagree with him about its doubts of the story line that, to us, is reasonably presented. The goal of the paper was to introduce a new approach for flood modeling (not specifically for flash floods) in poorly gauged catchments. The proposed simplified continuous RR model attempts to overcome the limitations of event based models which require the estimation of initial soil moisture conditions and of “classical” continuous RR model, which need uninterrupted and continuous meteorological time series. In the paper, we present a detailed introduction with the relevant literature, followed by the description of the area and of the meteorological data. In the methodology, we describe the simplified continuous rainfall-runoff model and other relevant tools used to analyze the results. Eventually, we present the results comparing the performance obtained by the different soil moisture indicators both in calibration and in validation. Indeed, in the Abstract we highlighted the main goal of the paper: “This paper introduces a simplified continuous rainfall-runoff model, which uses globally available soil moisture retrievals to identify the initial wetness condition of the catchment, and, only event rainfall data to simulate discharge hydrographs”. We will change a part of the sentence: “and, only event rainfall data to simulate discharge hydrographs” in “coupled with a simple event based RR model to simulate discharge hydrographs” to improve the comprehension and the philosophy of the approach. Although we agree to improve the clearness and the readability of the manuscript – and we will certainly do in the revised version (e.g. method blocks in the results will be moved) – the referee comments are mainly focused on two main issues concerning precipitation estimation and the reliability of the runoff observations for flash flood prediction. The latter are

C5787

two important issues but not central in this case since the goal of the paper was not to reproduce as accurately as possible the discharge hydrograph at that site, but indeed to introduce a new methodology approach (not specifically for flash floods) and to test it using different soil moisture products *ceteris paribus*, i.e., same quality of precipitation and runoff data. To this end, it has also to be noted that data quality and availability was scarce in the catchment and this is – we would say unfortunately – often the reality for many catchments worldwide.

2) They are using Thiessen polygons for interpolation of precipitation. As far as I understood the authors they are looking for flood prone events and flash floods interpolation method is not the state of the art to estimate the specific patterns responsible for these types of event (Heistermann and Kneis, 2010; Sangati and Borga, 2009). A definition what is a flash flood has to be given and the relevant literature is missing (Sangati and Borga, 2009; Heistermann and Kneis, 2010; Marchi et al., 2010; Tarolli et al. 2013). Most of these flood prone events are convective events which appear in most cases during the summer period. The authors present only one such event. A description of the events is missing (advective, convective) to have an idea about the size and the speed of the event. The presented events have all low runoff coefficients, even the summer event, below 10 % except two winter events. From that perspective it is hard to follow the flash flood argumentation and how do the authors think that their model framework is able to estimate larger events if there is no data of that size to test it.

Reply: Although we stated that the area is particularly prone to flash floods in section 2.2, in the observation period most of the events (especially major events, 3 February 2011, 24 February 2011, 11 December 2009 and 25 October 2009) were triggered by frontal precipitations. Nevertheless, we are not only interested in flash flood events so we do not want to follow the flash flood argumentation because the main focus of the paper is not the modeling and the prediction of flash floods. Even though the proposed approach can be applied ALSO to flash floods, we will remove the only sentence in the paper which mentions flash floods to avoid misunderstandings. As concern the

C5788

interpolation methods different techniques have been used (Thiessen, IDW, kriging) and no substantial changes have been found. For that, in the revised manuscript, IDW approach will be detailed.

3) Relevant information about meteorological input is missing and the block is more referencing different networks than describing them. How many rain gauges were used for the estimation of the precipitation, 13 or more? What kind of gauge is used? In figure 1 only 6 stations are plotted. Where are the other 7? Are they not taken into account? Two rainfall radars (GR41 Imittos and GR45 Aigina) are available for the study area why are they not integrated? The rainfall observation is the major point by modelling the runoff response especially in areas with a flashy response. That factor is even more important than initial conditions.

Reply: We will improve the description of the study area and move unnecessary information (e.g. information about networks). Rain gauge stations used are mentioned in section 2.2:

“Rainfall data selected for this study were extracted from Penteli, Pikermi, R400, R600 stations of HOA network, and, Kantza and Spata stations of NOA network (Fig. 1). Temperatures were retrieved from Pikermi thermometer. Note that, except for Penteli station – 2km north outside of the catchment – all the stations are located within the catchment boundaries.”

and also they are shown in Figure 2. We agree that the use of radar datasets would improve the reliability of rainfall datasets. The Hellenic National Meteorological Service (HNMS) is responsible for the operational use of weather radar products in Greece and also responsible for the operation of the two radars mentioned by the reviewer (GR41 and GR45). Beside the fact that these datasets are not freely (if at all) available to the wider public, they are also limited, relevant time-series are not continuous and need to be adjusted to the area. For these reasons, and taking into consideration the fact that the area, which extends over only approx. 100 km², is adequately covered by a

C5789

dense, state-of-the-art raingauge network, the use of radar rainfall datasets was not considered in the study. Detailed information on the type of used gauges can be found at (<https://hoa.ntua.gr>). We will add more information, if available, in the revised version of the paper.

4) The second issue is the rating curve. What kind of velocity measurement device was conducted? Why used the authors such a short period to measure flow velocities. As far as I understood them correctly the sediment loads in the channel in the region are high. How could they guaranty that the geometry on the investigated channel profile of 2009 is representative for the complete time series? With such low knowledge about the second mass balance variable it is not possible to be sure that the authors are able to quantify the correct runoff response or even able to estimate runoff coefficients. Can the low runoff coefficients explained by an underestimation of runoff? In the description part four runoff gauges were mentioned but in figure 1 only one is shown and in the analysis as well. Are they taken into account and what was the quality of their simulation?

Reply: We agree with the reviewer but rating curve reliability is a very well-known issue in hydrological applications (Di Baldassarre et al. 2009). Although we struggled to obtain more information, we could not retrieve more than that we have reported in the paper. This is a quite common problem due to the recent decrease in the hydraulic monitoring network observed all over the world (Vorosmarty et al., 1999, Biancamaria et al. 2011). Moreover, it has to be noted that the monitoring of river flow velocity is not straightforward and is traditionally carried out for low-medium water level conditions, because sampling velocity pointing in the wetted flow area during severe flood events is not only difficult, but even dangerous (Moramarco et al. 2004). Therefore, the highest values of discharge are obtained by extending the rating curve through extrapolation which may determines significant uncertainties (Di Baldassarre et al. 2009). To sum up, we clearly know that the rating curve contains inherent uncertainties (including change in cross section area) but this is the best we could obtain. On the other hand,

C5790

the issue is beyond the main goal of the paper and should not significantly affect the results since the same errors are present for all the investigated soil moisture products.

5) Add a table with the different parameters of the specific soil moisture products (resolution, points taken into account, frequency of the remote sensing products, algorithm, penetration depth, etc.). It is not clear how they estimate the mean soil moisture of the initial conditions and how do they link soil moisture to model storage. It is just not presented. Have they taken variability into account? The block in the model part does only give information that the authors think there is a linear relationship between storage and soil moisture. That has to be presented.

Reply: A Table showing the characteristics of the soil moisture products will be added since it will help to highlighting the sampling points of soil moisture for the different products. Concerning “mean soil moisture of the initial condition”, if the referee refers to the spatial mean, due to the size of the catchment (100 Km²) and the coarse spatial resolution of the satellite and ERA LAND soil moisture products (ASCAT, 12.5 km, AMSR-E, 0.25°, ERA-LAND 80 Km) we selected the nearest point falling into the catchment area. We will clarify it in the revised manuscript.

The link between soil moisture and the model storage is presented in the Introduction:

“Some studies attempted to relate the RR model initial conditions with different external indicators of SM estimated by in situ, satellite and modelled data (Brocca et al. 2009a, 2009b, 2011a; Trambly et al. 2010, 2011, 2012; Beck et al. 2010 and Coustau et al. 2012). In situ data were employed in many studies investigating the relationship between SM and runoff (e.g. Penna et al. 2011; Matgen et al. 2012) thus indirectly determining their potential use for RR modelling. Brocca et al. (2009a, 2011b) and Beck et al. (2010) used the SM products from ASCAT and AMSR-E for RR modelling in Italy, Luxembourg and Australia.”

described in Section 3.1 eq.5:

C5791

“The RR model exploits the observed linear behaviour between the wetness state of the soil and the parameter S (Brocca et al. 2009a, 2009b) by the following linear relationship: (1) In Eq. (5) is the relative SM (or degree of saturation) and “ a ” is a parameter to be estimated”. and in Figure 2 of the paper (see below):

(Figure 2 of the paper: Structure of the simplified continuous RR model)

6) There is only ONE FDR – probe for ground truthing in the 100 km² catchment. FDR method is error prone to dry states, clay soils and organics. They do not present what kind of soil texture is dominant in the study site. It is hard to interpret if that kind of probe which is only one for 100 km² in 25 cm depth is able to observe accurate soil moisture data. Is it located on a representative position? They do not present what kind of probe they use.

Reply: We agree with the reviewer but only one soil moisture probe is available in the catchment. It is a water content reflectometer Campbell Scientific CS616-L installed in clay soil. We will add some details in the revised version of the paper. “Is it located on a representative position?” There is not a simple answer to this question. Somehow, the probe will reflect the temporal variability of the soil moisture in the catchment (Vachaud, 1985) but we do not know whether it will overestimate or underestimate the average soil moisture or if it is fully representative of the wetness state of the catchment. To this end, it must be said that several sites were thoroughly examined prior to the installation of the HOA meteorological stations, aiming to track the most appropriate locations in terms of representativeness. Pikerimi meteorological station location (and respectively the soil moisture sensor used in the study), can be considered representative in that, soil properties and land cover characteristics at this location reflects those of the entire catchment. Moreover, we tested different soil moisture products including satellite and modeled data, which, given their coarser spatial resolution, can be more representative of the soil moisture in the entire catchment. We did not observe huge differences between such products and in situ values, hence we may state that the measurements supplied by the probe can be considered consistent.

C5792

7) It is still unclear what the influence of vegetation is on the two different remote sensing products. For bare soil the algorithms will work well. With increasing vegetation density like for forest or crops at the end of vegetation state the presented algorithms will be not able to estimate soil moisture (Wagner et al. 2013; Jagdhuber et al., 2013). And even low vegetation coverage will have an effect on the algorithm (Jagdhuber et al., 2013). There are 30% forest and 20% of urban areas in the study site, how do they have taken that into account?

Reply: We agree with the reviewer that the effect of vegetation has to be investigated on satellite soil moisture products. This is an interesting issue that deserves more profound and separate discussion. The area has a low vegetation cover as expected in Greece and it has been affected by several fires during the past decades. In any case, we present the products of one active and one passive microwave sensors along with a global reanalysis product which is less affected of vegetation issues. It is one of the goals of the paper to answer which product behaves better and which are the problems encountered on using one or another product. Hence, by comparing the performance of the different indicators both on simulating soil moisture and in reproducing discharge may help to have an idea of advantages and disadvantages of one or another product. We will add some more information of the effect of vegetation in the revised version of the paper.

8) How are landuse especially urban areas and soil types integrated into EMCWF data?

Reply: ECMWF data do not consider (yet) a urban tile. Each grid box is divided into eight fractions: two vegetated fractions (high and low vegetation without snow), one bare soil fraction, three snow/ice fractions (snow on bare ground/low vegetation, high vegetation with snow beneath, and sea-ice, respectively), and two water fractions (interception reservoir, ocean/lakes). The tile for “high vegetation with snow beneath” is a combined tile with a separate energy balance and evaporation model for the high vegetation and the underlying snow. A mixture of land and water ocean/inland water) tiles is not allowed, i.e. a grid box is either 100% land or 100% sea. HTESEL uses the

C5793

dominant soil texture class for each gridpoint. This information is taken from the FAO (FAO, 2003) dataset. We will add further details in the revised version of the paper.

9) In the modeling study again a lot of unclear statements were presented. How is the soil moisture data introduced to the continuous model? Graeff et al. (2012) had a quite bad non-linear relationship between storage and soil moisture.

Reply: The model is not a continuous model in the sense that soil moisture data are not modeled neither assimilated but directly used (as observed) in the proposed model to estimate the initial condition prior to the rainfall-runoff event. We described the philosophy of the model at the end of the Introduction:

“Specifically, an event-based RR model able to use external SM information as input data is developed. This modelling approach shows the advantages of event-based RR models (reduced data and computational requirements, limited number of parameters to be estimated, simplicity in application) but at the same time overcomes the issues related to the selection of the SM initial condition”

and in the model description (Section 3.1):

In synthesis, the simplified continuous RR model proposed in this paper uses SM and event rainfall data (i.e., continuous rainfall time series are not needed) as input data to simulate hourly flood hydrograph. Since the SM is provided by an external indicator, the relationship becomes a relation embedded in the model structure and it is used to estimate the value of S for the analysed events. The calibration of the model involves only three parameters: the coefficient of initial abstractions, the parameter a of the relationship and the parameter of the lag time-area relationship. Please, see also reply to point 5 for further details.

A number of studies worldwide obtained quite good relationship between soil moisture and model storage: Brocca et al., 2009a, b, 2011a; Trambly et al., 2010, 2011, 2012; Beck et al., 2010; Coustau et al., 2012. We will add also the reference of Graeff et al.

C5794

(2012) to the paper.

10) What is a simplified continuous model? In the model description it sounds like they are only presenting event based modelling. Are the soil moisture data used to update the state variables?? If so have they tested antecedent runoff to update the model state? For a more profound review of the results and conclusion the authors first have to present a better structure of their manuscript.

Reply: it looks like the referee failed to get the simplicity of the approach so we will improve the description of the model in the revised version. We did not use any assimilation technique so we did not update state variables, or parameters of the model. We simply used observed soil moisture as input to the model along with rainfall data. The initial conditions are estimated by Eq. 5 and used in the framework of an event based model. The parameter a of Eq. 5 is a model parameter. Please see reply to point 5 and point 9 for further details.

This methodology aims to keep the advantages of the continuous simulation approach because it allows to take into account the temporal evolution of the soil wetness conditions of the catchment (from the observed soil moisture) and the complex interaction between rainfall and soil moisture conditions. At the same time, it offers the benefits of the event based simulation i.e., model parsimony and simplicity of use. For this reason we call the model simplified continuous RR model as we stated in the abstract:

Event-based rainfall runoff models are frequently employed for operational flood forecasting purposes because of their simplicity and the reduced number of parameters involved with respect to continuous models. However, the advantages that are related with the reduced parameterization face against the need for a correct initialization of the model, especially in areas affected by strong climate seasonality. On the other hand, the use of continuous models may be very problematic in poorly gauged areas. This paper introduces a simplified continuous rainfall-runoff model, which uses globally available soil moisture retrievals to identify the initial wetness condition of the

C5795

catchment, and, only event rainfall data to simulate discharge hydrographs in the Introduction and in the model description:

Continuous RR models simulate SM to take the variability of the wetness conditions prior to a rainfall event into account. If SM at the beginning of an event is provided by an external indicator, i.e., in situ or globally available SM observations such as satellite and model-based reanalysis SM products, the structure of a simplified but continuous RR model can be derived as schematized in Fig. 2.

11) Specific comments:

P 11001- 11002: Explain at the end of the intro what the reader can expect from that manuscript! Although we already clarified that we want to introduce a new methodology approach which keeps the advantages of continuous models but at the same time being simpler and parsimonious we will clarify better our goal and what we expect.

P 11002, L 20-26: Geology is irrelevant, soil physics are important. We will remove unneeded information about geology.

P11003-1104: What kinds of sensors are used? Shorten that paragraph to the important facts. Same as above. We will remove unneeded information.

P 1104, L 23: add the SWB model as the fifth method. The referee means fifth method? If so we want to stress that SWB is a model that simulates the soil moisture temporal evolution. Indeed, if simulated soil moisture data are used, a continuous RR model is built (e.g. Brocca et al. 2011, MISDC). We considered the application of the SWB model as the baseline configuration to assess how much degradation in the performance the simplicity of the proposed model would introduce with respect to a "classic" continuous simulation approach. Therefore, we prefer to leave the SWB model description in the block method.

P 11008-11009: Integrate 3.2 and 3.3 to 2.3. The SWB model is the fourth method to estimate initial conditions. Present the parameters of the model and how they have

C5796

been estimated. Is it lumped or distributed and how is aggregated.

We will try to improve the manuscript structure in these aspects. We will add information of the parameter of the SWB and add more details.

P11011, L 3: How is the normalization realised? We will add more details about the processing of the soil moisture indicators in the block method.

P11011 L9: Is the comparison done between the observed soil moisture at the probe location and the simulated value with the different products at that point or is the mean value of the catchment soil moisture presented? Please see reply to point 5.

P11011, L 10-17: Which method was used for the calibration? Which was the calibration period and which the validation period?

See line P11011, L 10: we minimized the RMSE. For model calibration, a standard gradient-based automatic optimisation method ('fmincon' function in MATLAB®) was used. We calibrated during the entire period where the soil moisture product was available. We will add these information in the revised version of the paper.

P11011, L 19: Explain the T parameter in the method block. The T parameter has been explained in section 3.2. P1109 L1-5:

To obtain the root-zone SM product (SWI, Soil Water Index) from the satellite-based surface observations, the semi-empirical approach developed by Wagner et al. (1999) is adopted. The approach is also known as Exponential filter and uses a single parameter T (characteristic time length) that represents the time scale of SM variation to obtain the SWI. The reader can find a more detailed description of this approach in Wagner et al. (1999) and Albergel et al. (2012). P11012 4.2.1 Move parts to the method block. Is it important to present all events in fig. 4? We will modify this part. We believe that showing all the events gives a clear picture to the readers of the results we obtained.

11013 4.2.2 and 4.3 Again methods and results are mixed. We will modify this part.

C5797

11013, 21-23: Unclear sentence. All tables and figures need a more profound description in the caption. They have to be stand alone. Table 1: Add the mean antecedent soil moisture or water level, to have a value of the catchment conditions. Table 3: What is NS the mean Nash of all results? Figure 3: The figure is difficult to read. Increase size and quality. Legend is too small and symbols are indistinguishable. What is AS-CAT (a) and (b)? T is not explained in the text and ASCAT (b) is not identifiable in the plot. Explain the different correlation coefficients and T values in c). Figure 5: a) and b) is not in the figure. Where is Sobs in a)? Figure 7: Simplify the labels and plot one legend. We will adjust these issues and improve the figures and tables along with the caption descriptions.

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C5798

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C5799

Please also note the supplement to this comment:
<http://www.hydrol-earth-syst-sci-discuss.net/10/C5786/2013/hessd-10-C5786-2013-supplement.pdf>

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 10997, 2013.

C5800