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Hydrology and Earth System Sciences Discussions

Interactive comment on "High resolution rainfall – runoff measurement setup for green roof experiments in a tropical environment" *by* T. Vergroesen et al.

Anonymous Referee #2

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Generals

The manuscript presents the experimental setup for green roof hydrological study in relation to rainfall-runoff production with particular emphasis in tropical environments. This reviewer initial excitement rapidly went down due to the lack of basic information on the topic, proper introduction, and unreliable calibration tests. These have led to problems with readability of the document as the authors failed to communicate and focus on the only single novel contribution aspects of this work.

To this reviewer surprise, the authors have put too much focus on the need for "high

resolution" measurements (less than 5 min) to differentiate this work from previous studies. By doing so, they have used existing technologies and pushed them beyond their intended capabilities to obtain what they claim "reliable values" for rainfall, runoff and evaporation. They also claim that they have found a way to deal with external disturbances that influence experimental results, as it was stated in the Abstract. Barely, a few of these goals have been achieved as it will be presented bellow. The study site has intrinsic novelty given by high rainfall intensities, warm weather and high relative humidity, but at the same time those factors impact in both the innovative system for runoff measurements and the evaporation estimates. As presented, radiation, temperature, and humidity have a large impact on the weighting technology used to estimate evaporation, particularly for the inexpensive bathroom scales.

The Introduction section does not properly present the problem as much of its content is a simple summary of the manuscript. For example, there is no information about the role of green roof in urban hydrology and the specific hydrological processes of interest, or a literature review on previous experimental settings to put into context the need for high frequency data sample and new instrumentation. Towards the end of this section, the authors mention the use of meteorological data to estimate evaporation (method?), which is not presented at all in the manuscript.

The method section is not well organized either, as the description of the instrumentation and results (for example calibration curve, runoff and evaporation calculations) are presented at the same time. As a result, there is unnecessary repetition of the calibration curve for the rain gauge and its levelling procedure. No vital information on the experimental roof such as material, slope, constituents (layer of soil, synthetic, etc.), vegetation cover, etc., is presented. This information is relevant to assess the runoff and evaporation estimates using the water balance (soil layer storage?).

It is also hard to find a clear Result section since results have been presented as 'Data treatment' (Section 3) and "Dealing with disturbances.." (Section 4). In the latter, around four pages were used to show the influence of radiation (and its effect on temperature and RH) on weighting scales but yet that effect was not used to correct or assess their impact the evaporation/evapotranspiration estimates. Most of the information on previous studies that used different sampling frequency and techniques are presented in the Discussion section.

Although the above comments are more related to manuscript organization and writing styles, the manuscript still contains some technical problems that could jeopardy the data measurements.

There are a few points in relation to the raingauge calibration procedure and results. The authors “assumed” that the factory calibration of 0.2 mm per bucket is correct, WHAT? A volumetric verification (with pipette) is ALWAYS the first step needed, in both lab and field installation. This calibration is done by using the two calibration stop screws (one for each bucket). There are other issues (which the authors are not aware of) associated with the setting up of the tip-counting scheme for a particular datalogger. This error is of particular interest when the rainfall intensity increases. Some dataloggers experienced recording problems when the time between two consecutive tips is less than 3 sec. The problem may results in either recording an extra count or not recording the tip at all, thus affecting the NRT. This is a different issue no associated to the “half tip time” problem as considered by the authors. There are two ways to verify this error by counting every tipping event (audible sound when it is tipping) or wire the raingauge to a “real intensity datalogger” or a PC with data adquistion software. Real time intensity raingauge and datalogger (cost US$ 90) have been available in the market since 1990 (extensively used in urban hydrology experimental catchments) and they effectively recorded the time for each individual tip. This reviewer had recorded rainfall intensity of up to 4 mm/min using both test simultaneously in a real event, and verified the need of removing some of the counts as the event approached extremely high intensities. This reviewer may be wrong but it is not stated in the manuscript whether the tip counting (NRT) during the calibration procedure was done by tipping counting (by a personnel) or by automatic counts from the datalogger. The extreme

event generated for calibration purposes in Figure 6 and its corresponding equation has an intensity of approximately 800 mm/h, resulting in an average tipping frequency of 67.5 tips/min or one tip every 0.8sec. This reviewer has two problems with this: what is the purpose of including an unrealistic intensity in a calibration equation for intensities values that exceed by eight times the operational limits by the raingauge maker? and what is the error associated to NRT under this condition given the fact the time between two consecutive tips is very close to the “filling+half tip” time together?? It is hard to believe that half tip time alone issue will account for the increase of 60% in the average tip volume as presented in Figure 6. The authors may consider a look at Grzegorz J. Ciach, 2002 (Local random errors in tipping-bucket rain gauge measurements, IIHR–Hydroscience and Engineering, The University of Iowa).

There also a few issues related to the runoff and evaporation measurements which will impact in obtaining high accuracy data as claimed by the authors. This reviewer agrees in that the electronic weighting scales will perform reasonable well under low intensity rainfall events and thus presenting an advantage over traditional methods. However, climatic forcing (radiation, temperature and humidity) has an impact on the weighting scales (up to 100 g) for the more expensive ones and it will certainly be even larger for the mechanical bathroom scales which spring and levers can wear with time as a result of both temperature changes and the continuous load from the tables. Bathroom scales are meant to be in the bathroom. Even though the density of the liquid is a key player in the system (after all we weight the liquid as runoff), a constant density for the water has been assumed here. Did the authors measure the water temperature of the rainfall or runoff? How valid is this assumption? What is the impact of the assumption in the system performance as a whole? Water temperature has shown to introduce another source of error in using weight systems (see Servruk and Chvita, 2005, Error sources of precipitation measurements using electronic weight systems). Something else is happening on the scale readings as a runoff event progressed, which may not be related to the tipping time and tipping action. Looking at Figure 3, it can be seen that as the rainfall event progresses the maximum weight of bucket with water (prior its tipping point) increases, it reaches a peak and then it reverses its trend. A similar behaviour can be seen in Figure 15. It also seems to be happening for the low weight as the bucket empties. Initially this reviewer thought in a possible density effect as the cooler rainfall water will initially increase its temperature as it encounters a warm air and plate's surface (during the earlier stages of the event), but it is expected that it will keep its cooler temperature signature afterwards (heavier water) resulting in higher weight readings. But in 1 litre of water a 2 g difference is expected due to a change in density due to temperature (if nothing else like particles get into the bucket), which is much smaller than the 200 g difference (sometimes 400 g) observed from the records. Any comment on that? The second source of error has been identified by the authors but not taking into account in the evaporation estimates. Error values ranging from 50 g to 100 g introduced by radiation are equal or larger than the weight differences presented on Figure 15 for evaporation estimates (i.e, 37 g, 20 g).

In summary, the manuscript will require major changes to bring up novelty of this work and to address main technical issues before its acceptance for publication in HESS. This reviewer suggestion is to work towards a better manuscript organization to improve readability and avoid unnecessary repetitions. Since source of errors for runoff and evaporation has been identified and quantified, these should be taken into consideration for runoff and evaporation estimates and properly discussed their effect on the final results. Evaporation estimates resulting from the different approaches need to be contrasted with evaporation calculation from the meteorological data available. Finally, and to the light of the above comments, the authors should exercise caution in the claim of achieving reliable values for the variables of interest and used rainfall at 1 min intervals as it has been a common practice since the late 80s in Urban Hydrology investigations. Instead, they should focus on the new way to explore alternative approaches to obtain sub-minute hydrological information for better rainfall-runoff processes description under high-intensity rainfall regime.

Specific comments:

Abstract

Page 9368, Line 4. Use "rainfall and runoff' or "rainfall-runoff" measurements.

Keywords Suggest not using "high resolution measuring equipment" as a keyword.

Introduction: It does not introduce information on alternatives techniques available to measure runoff in small experimental green-roof experiments. Please, move the relevant literature from the discussion section.

Page 9368. Line 14. It is not clear which hydrological processes the authors refer to.

Page 9369. Line 22. Add (NUS) after National University of Singapore. Line 25: Authors presented a "discussion" about differences between rainfall measurements.

Page 9370. Line 15. The raingauge needs an elementary volumetric calibration for each bucket prior to any other calibration procedure.

Page 9370. Line 24. An equation (result) is introduced. Move to the results section.

Page 9371. Lines 20-25. Suggest moving Figure 3 and text below to "Data treatment".

Page 9371. Line 20. The authors should assess the effect of water temperature.

Page 9372. Line 4. The authors have not mention vegetation type to account for transpiration due to lack of description on the green-roof experimental setup.

Page 9372. Line 20. Reference to a soil water content storage. At this point the reader does not have information about this storage due to lack of description in the experimental setup.

Page 9373. Line 5. The example on evaporation calculation should be part of a Result section. Also, the evaporation value of 6.6 mm in calculated based on weight. This reviewer will come back later to this point.

Page 9373. Line 20. Comment/suggestions: A real intensity datalogger combined with a 0.1 mm bucket will help in reduce issues for low intensity rainfall events.

Page 9374. Line 4. .."for 20 test the total number of tips and the time of the last tip are recorded..." Is this information obtained from the datalogger?

Page 9374. Line 12: Replace "overview the calibration results" by "overview of the calibration results". This reviewer will recommend rewriting this paragraph and delete reference to the extreme event calibration unless the authors provide evidences of no further potential issues with the NRT counts from the datalogger (see general comments.)

Page 9374. Line 16: Calibration equation has been presented in a previous section. Remove.

Page 9374. Line 18: "However despite proper levelling some additional tests directly on the single buckets showed that one of the two buckets tips at a structural higher volume than the other. The cause of this difference is not clear". Assumption of equal volume needs to be checked and volumetric calibration performed.

Page 9374. Line 25: Remove as it was mentioned at the beginning of this page.

Page 9375. Lines 5-9. The authors stated that the only purpose of the runoff tipping bucket gauge is to provide the mechanism for automatically emptying, and no datalogger was used to record that... WHY? A datalogger, in particular a real intensity one, will provide extra information needed to cross-check those values obtained from the electronic weight scales.

Page 9376. Line 3. Use "rainfall-runoff".

Page 9376. Lines 5-10. Repetition of the weight to runoff transformation procedure.

Page 9377. Line 18. Review assumption of constant density for water.

Page 9378. Lines 6-10. The authors refer to a storage volume in the soil. It needs to be clarified in the experimental setup description.

Page 9379. Line 25. The reviewer does not know how to treat this result. This reviewer

has serious concern with the intended used of the bathroom scale here.

Page 9381. Lines 1- 4. The authors indicated that meteorological data (NUS weather station) and soil characteristics were used to determine evaporation. Where are the methodology and the evaporation results presented? Are they the numbers next to the arrows in Figure 10? This is confusing as the value of 6.6 mm was previously presented and explained in Figure 5 as being obtained from weight differences. Please clarify.

Page 9382. Lines 1-4. "The accuracy of this approach depends mainly on the accuracy of the reading, which is in the order of few grams." This reviewer will argue that what follows in the manuscript on disturbances in runoff measurements will contradict this statement.

Pages 9382-9383. Around four pages (including Figures and Tables) were used to shown the influence of meteorological forcing on the implemented weighting system. This is an important finding as the authors documented its occurrence under field conditions. This reviewer will suggest to keep Figure 11 and 12, Tables 1 and 2, and to remove the others. Information contained in Tables 3 and 4 can be easily presented in a single paragraph. The manuscript needs this extra space to improve the Introduction and the Discussion sections.

Page 9383. Lines 14-16. "The error caused by these weather influences is normally very small, and that too only temporary...". This reviewer does not agree with that but this reviewer may be wrong. A 100 g difference due to radiation may result in 1.22 mm of evaporation which it can account for more than 25 % of the average evaporation for a given day (see Figure 10). The weighting system seems to be influenced by other disturbances during runoff events (changes in fluid density, climatic forcing?) and by meteorological forcing (radiation) during the dry period, thus neither is very small nor temporary. Please clarify this.

Page 9384. Lines 1-15. The title and the first sentence are confusing since both rainfall and runoff devices use tipping bucket. Please rewrite it. At this section now, this reviewer is completely lost. Is evaporation from the tipping bucket for the runoff? In section 2.2, the authors stated that "The setup of the experiment table is such that the runoff measurement equipment can be situated bellow the opposite table. A cover around the open sites below the table helps to minimize wind and other possible influences as much as possible". According to this, they are under a cover and consequently no influenced by radiation. What drives evaporation under these conditions? Looking at real time meteorological data at Marina Bay over January 2011, this reviewer found that overnight temperatures are constant between 25-27 o C, relative humidity between 75-80%, and wind speed of around 7 km/h. This reviewer is afraid there are no ideal conditions for evaporation to occur. Please clarify.

Page 9384. Lines 7-8. The authors refer to "the runoff of the concrete roof". What is it?

Page 9384. Line 23. Change " 7.62 mm" by "76.2 mm".

Page 9385. Lines 8-11. A 3% error for one tip per minute (or 12 mm/h intensity) is an unreliable figure. What causes that error? With this intensity, individual drops of water will be filling the bucket, reducing the half tip time error to a minimum. Could it be also the result of the release on water using the syringe for such a low intensity? That small difference may indicate the need for a proper adjustment of the calibration stop screws for each bucket.

Page 9385. Line 25. Check citation format for Devine and Van Woert et al.

Page 9386. Lines 1-9. It is not clear what the authors try to say here. A real intensity logger stamps the date and time corresponding to each tipping. In terms of memory, it may use more of it. For example for a 0.2 mm bucket, 250 records (or lines) will be used for a 50 mm rainfall event over 1 hour, but only 60 records if the logger is set for 1 min interval. All existing dataloggers have clock issues over long period of time, but the error will be negligible over consecutive tips over short period of time, thus the "real intensity" can be measured. Again, frequent resetting of the logger timer decreases

the clock issue. Since this is the same issue for the clock of the weight datalogging system used for the tipping runoff gauge, this reviewer does not see the point for those comments placed in the paragraph.

Page 9386. Lines 10-19. Discussion about wind and other effects on rainfall measurements which have no relevance to the lab calibration performed in this work. There are out there practical solutions for each of the listed problems in order to minimize their influence on the rainfall measurements. The authors may consider a look into the work by Grzegorz J. Ciach (2002).

Page 9386. Lines 20-25. First sentence of this paragraph is confusing. What is the "reference roof runoff measurement"? Since this is ongoing study, this reviewer will recommend the authors to wire all the tipping gauges (rainfall and runoff) to real intensity dataloggers.

Page 9387. The first paragraph is just a compilation of previous studies using different gauging setups and it should be moved to Introduction to put into context the present work. Unfortunately, the authors seem to confuse "capabilities of a device" with "scanning time and recording time" of a given method. Pressure sensor readings have the advantage of being compensated by temperature and atmospheric pressure (avoiding some of the issues experienced by the weighting system) and scanning and recording time can be set at 3 sec intervals which will provide enough data (20 points per minute) for high temporal resolution. The authors certainly have explored a novel methodology that may provide 1 sec time interval information but the method needs further testing of the assumption and external influences.

Page 9388. This reviewer does not have any observation regarding the water balance approach followed by this and previous works to account for evapotranspiration. However, the error introduced by radiation is of the same order of magnitude than the experienced weight differences used for evaporation estimates. Finally, previous studies should be used in the Introduction to provide the right context for the present work.

Page 9389. Conclusions are very poor and reflect the fact of lack of clarity on what this work is about. For example, the authors again concluded about the effect of wind on rainfall measurements subject for which they have not done anything in the present work (Lines 12-14).

Tables and Figures.

Table 1 and Table 2 captions: Misspelling " Februari 2010"

This reviewer suggests to remove Figures 13 and 14 as well as Tables 3 and 4.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 9367, 2010.