Hydrol. Earth Syst. Sci. Discuss., 11, C3036–C3048, 2014
www.hydrol-earth-syst-sci-discuss.net/11/C3036/2014/

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "A new method to compute the groundwater recharge for the study of rainfall-triggered deep-seated landslides. Application to the Séchilienne unstable slope (western Alps)" *by* A. Vallet et al.

Anonymous Referee #2

Received and published: 11 August 2014

Numerical Rating:

Scientific Significant = 4; poor Scientific Quality = 3; fair Presentation Quality = 2; good. The tables and figures are excellent. The writing needs significant improvement.

General Comments:

In the manuscript titled, "A new method to compute the groundwater recharge for the study of rainfall-triggered deep-seated landslides. Application to the Sechilienne un-

stable slope (western Alps)", the authors present a workflow by which groundwater recharge may be estimated at their site on a daily basis. Their workflow includes 1) procedures for extrapolating point measurements of solar radiation across the entire study region, 2) application of previously developed methods for computing reference and actual evapotranspiration, 3) optimization techniques for finding ideal parameters that are utilized in equations involved in 1 and 2, 4) application of previously developed methods for computing runoff coefficients, and 5) a scheme for calculating the soil water balanceâĂŤone term being the groundwater recharge flux. As a rationale for the study, the authors indicate that groundwater recharge is more relevant to the pore-water-pressure dynamics than direct measures of precipitation, and therefore should be considered in efforts to predict the motion of deep-seated landslides. They suggest that, to date, this has not been the case, because an easily adaptable method for computing recharge is not available. The authors utilize correlation analysis to show that their estimated time series of recharge are better correlated with actual measurements of land-mass displacement than are direct measurements of precipitation.

At present, I do not think this manuscript warrants publication in Hydrology and Earth Systems Science. It is my recommendation that this manuscript receive major revisions before being resubmitted. I further recommend that the authors consider resubmitting this work as a technical brief, rather than an original scientific article. Below I outline the most important reasons for this recommendation, and allude to specific comments that I hope will be helpful for the authors in their effort to revise the paper.

Foremost, the rationale for the study is not strongly communicated. In the introduction to the paper the authors do not provide a convincing case that the current practice of predicting land-mass movement is inadequate specifically due to the failure to accurately represent groundwater recharge. The reader is left wondering if this work is really needed in the specific case study discussed in this paper. Specific comments 2-5 further address this point.

The authors also present this work as a method that can be readily adapted and used by practitioners and non-hydrologists. In the abstract they specifically state that the method requires measurements of only precipitation and temperature. This is entirely misleading. Within the manuscript, it is clear that the method also requires analysis of aerial photographs, field observations of plant-species distributions, literature review for retrieval of parameter values (e.g. vegetation coefficient and runoff coefficients), digital elevation models, geological maps, field investigations of auger holes (page 6353), a pedo-transfer function to estimate plant-available-soil water, and the results of previous isotope-based studies that delineated the relevant recharge area (page 6367, section 4.2). In light of these requirements, it is doubtful that this method can be easily adapted and used by practitioners or other researchers.

The soil-water-balance model is used in this paper to estimate groundwater recharge. There is no evidence provided to indicate if the model is even remotely accurate (e.g. measurements of water table fluctuations). The ultimate test of the model was to determine if the recharge estimates more closely correlate with measured land-mass displacements than do measurements of precipitation alone. In principle, we know that recharge is more relevant to pore-water pressures and water-table dynamics than precipitation alone. This is ultimately a very weak test, and still gives no indication about the accuracy of the model. Some references are provided that document other simple methods for calculating recharge-weighting functions. These methods can be implemented with data already utilized by these authors (in a much simpler workflow), and comparison with the recharge estimates generated here would provide a slightly more rigorous test of the utility of their more complicated scheme. These points are further discussed in specific comment 22.

In my opinion, a workflow, which presents no new quantitative representation of any process, does not constitute new scientific knowledge. It could be a potentially useful tool for practitioners. As such, I recommend that when this article is resubmitted, it is resubmitted as a technical brief rather than an original research article. Further, I would strongly encourage the authors to develop a simple software tool (in Microsoft Excel,

or other readily available platform like R), that executes their workflow and could be readily used by others. The authors suggest this was one of their primary motivations. Providing a readily usable tool might prompt people to use this workflow, otherwise it is doubtful that many people will wade through this 18-page methods section and appendices and develop their own software to execute the workflow.

Last, the paper is exceptionally long and in many places the methods are not adequately described. An independent researcher could not replicate this work based solely on the methods section that is presented here. Throughout the specific comments below, and technical corrections included in the .pdf file, I offer suggestions about how the paper can be restructured to improve clarity and reduce length.

Specific Comments/Questions:

1) Page 6344; lines 11-15: It is stated that the method requires only temperature and precipitation as inputs, though in the next sentence it is stated that soil available water capacity and runoff are determined from field observations and spatial datasets. Would it then be more accurate to say, “The workflow requires records of precipitation and air temperature, field observations, and spatial datasets”? It would also help to clarify exactly what kind of spatial datasets you are referring to.

2) Page 6345; lines 10-12: I suggest the authors omit this sentence. Measurement of precipitation (especially distributed over space) is far more uncertain than measurement of solar radiation or relative humidity. I don’t think it is justifiable to partially rationalize the need for a new method that uses air temperature and precipitation data based on the claim that solar radiation and relative humidity are not accurate enough to rely on.

3) Page 6346; lines 1-10: This paragraph seemingly provides the rationale for this study. In my opinion it needs to be strengthened. The first sentence implies that reduced-set methods of calculating ETo, elaborate methods, and indirect methods all lead to significant errors. I believe the authors must improve this assertion in several

ways. One, explain what constitutes an elaborate method versus an indirect method. Second, provide more details about exactly why these methods lead to significant errors (citing appropriate examples from the literature), and provide some quantitative statement about how great or small are the errors associated with each method. For example, estimating ETo without considering the effect of soil-water deficits and plant type will almost certainly lead to overestimates of actual ET in any environment subjected to prolonged dry periods when transpiration by plants becomes water limited. In a very humid environment, there error would be smaller.

The next sentence states that some studies use precipitation data as an infiltration input signal – then again implies this will lead to errors. Again, more detail is needed here about what exactly is wrong with the assumption that the infiltration rate at the soil surface is equivalent to precipitation. For example, the surface infiltration is likely less than precipitation if interception loss from the plant canopy is significant. If infiltration-excess overland flow is a prevalent runoff mechanism, then infiltration at the soil surface would be less than incoming precipitation. The authors need to state with more precision exactly how the methods employed in these previous studies (those cited earlier in the paragraph) lead to "significant" errors, and they should offer some quantitative statements about the magnitude of these errors (citing appropriate examples from the literature).

Last, I would suggest finishing this paragraph with an authoritative statement about what methodological improvements are needed to reduce the magnitude of error associated with recharge estimates. At present, this paragraph does not offer a clear representation of what is wrong with the status quo, and how this paper helps address shortcomings with the status quo.

4) Page 6346; lines 11-14: I suggest that the authors rephrase some of the statements in this paragraph, to better convey the purpose of the study. First, I would combine the first and second sentences, as it is redundant to talk about the objective of the study, then the purpose of the study in sequential sentences. Here is one alternative: "The

objective of this study was to develop a parsimonious, yet robust, method to calculate time series of groundwater recharge that can be used as a deterministic variable in studies of landslides. To maximize accessibility to diverse user groups, we strive to develop a method that is more computationally simple, but at least as accurate as methods proposed in previous studies."

5) Page 6346; lines 22-24: I would suggest that the authors revise this sentence to provide a more convincing rationale for the study. They state, "The aim of the demonstration is to prove that recharge is a more relevant parameter than precipitation for accounting for the motion of deep-seated landslides, and is performed with no intention to model or to quantify the displacement." In principle, we already know that recharge is a more relevant flux than simply precipitation for influencing the dynamics of pore-water pressure and water table fluctuations. This has to be true; there is no question about it. Perhaps the authors can rephrase this sentence to acknowledge this fact, but without negating the analysis they have already done. Here is one suggestion: "In principle, the actual groundwater recharge flux controls the dynamics of pore-water pressures and water table fluctuations, rather than the precipitation flux at the land surface. To test the utility of our method, we use correlation analysis to evaluate if the calculated groundwater recharge flux is more strongly correlated with measured land mass displacements than is precipitation measured at the land surface."

6) Page 6347; section 2.1: In an effort to make this paper more concise, I think this section can be significantly reduced in length. The key points are 1) you want to estimate recharge using a method that is accessible to a non-expert (i.e. easily executable algebraic equations; no complex differential equations), and 2) data are used at a daily resolution because they are generally available, and because aquifers that exist at great depth fluctuate in response to large cumulative rainfall events, rather that transient storms that last less than one day. Try to make those points in a single paragraph, rather than three. At a minimum, I suggest deleting lines 10-19 to reduce length.

7) Page 6348; lines 18-23: There are several remarks here about how Rs and ETo

reduced set methods need to be calibrated. It would be greatly helpful to add a statement here clarifying exactly why they need to be calibrated. What features of a particular geographic location require the underlying equations to be modified? What are the underlying equations (they have not even been presented yet)? Perhaps cut and paste the statement from page 6345; lines 18-20 to this location, and elaborate to address the questions above.

Also, the authors indicate that the calibration coefficients for each of the reference weather stations are averaged to provide a regional-scale parameter set. Presumably these are small sample sizes, but it would be appropriate for the authors to comment about how the calibration coefficients from the reference weather stations are distributed. Is an average a representative metric, versus a median, for example?

8) Page 6349; lines 3-4: In this sentence, you state, "The performance assessment and ranking of each of the regionally calibrated methods is based on the comparison between observed measurements and calibrated estimates." I am unclear about the phrase "each of the regionally calibrated methods". I understood that you will have a finite set of reference weather stations that will allow you to calculate a single set of regional-scale calibration coefficients (though again, we have not yet seen the actual equations). As such, I am not sure what is meant when the authors refer to "each of the regionally calibrated methods", which is plural.

9) Page 6349; line 11: I suggest you not use the phrase "reduced-set method". This phrase is first used on page 6345, with a few related citations. The calculation of reference ET (via the FAO endorsed method of Allen) can be performed in a whole variety of ways depending on what data is available to a worker (e.g. incoming shortwave, incoming and outgoing longwave radiation, relative humidity, temperature, soil heat flux, etc.) If the definition of a reduced-set method is that method which requires less meteorological data types than the standard method, then the definition is only relative. Any particular application of the method could be considered a "reduced-set method" if it happened to use one, or two, or three less data sources than the "standard" method

(note that the standard method has not been explicitly defined in the paper). For the reader, continually seeing this phrase does not aid with understanding, so I suggest eliminating it throughout the text to reduce the word count.

10) Page 6349; lines 11-12: Here you make first reference to Appendix 1. The equations in appendix 1 appear identical to equations 1 and 2 in the manuscript, except for the modifiers "DBC" and "CHS". As such, appendix 1 is redundant and unnecessary. Delete it to reduce the length of the manuscript.

11) Page 6350; line 9: You state that the cloudiness factor (alpha) becomes irrelevant after two days of rainfall because "the temperature and Rs get equilibrated." This statement is illogical. Shortwave radiation is a measure of flux of radiant energy associated with wavelengths spanning about 300-1500 nm; temperature is a measure of heat concentration within a substance. The two do not share the same dimensions (units of measurements). This is just one example of the very imprecise language that pervades this manuscript. I strongly encourage the authors to provide more precise descriptions of the concepts and methods – without them the validity of the work is hard to judge.

12) Page 6353; lines 10-18: The description of methods here is wholly inadequate. You say, "For one given parameter, the recharge area was divided into sub-areas, each being characterized by a constant value estimated according to field measurements, literature values or calculation." A methods section should be written with sufficient detail that another scientist could replicate your work based solely on its description within the manuscript. That would be impossible given only this description of how the average parameter values were determined based on landscape characteristics. The subsections that follow (within section 2.4) are similarly vague. For example, in section 2.4.2 the authors state that "SAWC is deduced from soil properties (type of horizon, texture and bulk density) and depth extent from auger hole cores, using a pedotransfer function." Did you actually measure the soil texture and bulk density using a laboratory method, or did you assume a value based on some soil survey data? Did you assume

that the maximum depth of your auger hole was the maximum depth of the soil? Or do you have other information that indicates the depth of the soil? What is the depth to bedrock, and is the bedrock impermeable, fractured, other? Do you think one core is sufficient to extrapolate to the entire sub-area for which you are estimating the SAWC parameter? Soil texture and hydraulic properties can vary by orders of magnitude over small distances. Last, you state that the dependency of SAWC on vegetation species is taken into account through the Kc coefficient. More detail is needed here. The description of Kc in the preceding section indicates that it is a function of vegetation height, albedo, canopy resistance and soil evaporation. It is not immediately apparent how any of those factors are related to the SAWC, which is a theoretical (and questionable) value indicating the fraction of the total soil-pore volume that can be utilized by plants for solution uptake. Also, you already stated that the SAWC was estimated from a pedotransfer function (all of which are rough approximations for any individual soil), so how is that estimate of SAWC from the pedotransfer function modified based on the Kc coefficient?

13) Page 6355; lines 1-8: Runoff can be generated from overland flow and subsurface flow. Since the authors are attempting to calculate a soil-water balance, it would seem to be very important for the authors to specify how this method distinguishes between the two. Subsurface flow is reciprocally related to water-table fluctuations, and would therefore influence the distribution of water pressure throughout the soil pore space. Overland flow would not affect soil-water pressure or water table fluctuations.

14) Page 6355; lines 19-26 and Page 6356; lines 1-8: I think you can delete most of this text. Instead, simply present the water balance equation and use the following text to briefly define each of the terms in the equation. All the readers of this journal are familiar with the water balance, and you have it illustrated nicely in Figure 2.

15) Page 6356; lines 9-13: Here you give your first conceptual description of SAWC, although you have already talked about it, and how you calculate it, in previous pages. This conceptual definition should precede all other commentary.

16) Page 6356; lines 14-19: Canopy interception, storage, and evaporative loss of water would reduce the fraction of precipitation that contributes to runoff or soil-water storage. Here you say that “water evaporated by the interception process is taken off the SAWC reservoir (Fig. 2b).” It is not clear what you mean by “taken off”. The most accurate representation of interception loss would be to modify the fraction of P that contributes to Rf or I in the schematic diagram representing your workflow (Fig 2). However, I do not see such a modification in that diagram. Can you elaborate on this point, and allude to the specific part of the schematic diagram where interception is accounted for. The reader has not yet been given a site description, but if you are working on a forested site the interception loss could easily be 10-20% of incoming precipitation.

17) Page 6358; lines 17-19: This statement is incorrect, and should be deleted. This workflow includes many free parameters representing many processes. The representation of canopy interception and evaporation is patently wrong – it will lead to SAWC always being biased upward. The sensitivity analysis may help you find the optimal set of recharge-area parameters, and their average, but it does not mean that you have correctly represented the underlying physical processes.

18) Page 6358; line 24: Can you please define what you mean by “aquifer saturation state”, indicate the dimensions of its measurement, and include notation for this variable on the left-hand side of equation 5, rather than the words “Decreasing sum”, which are physically meaningless.

19) Page 6359; section 2.6.2: Here you begin to talk about correlations between this “aquifer saturation state” variable and displacement. The problem is that the reader is not yet aware of what displacement data you are referring to, as you have not introduced it. The reader does not know exactly what was measured, in what units, with what accuracy, or at what temporal frequency. Is equation 5 calculated on a daily, weekly, monthly basis? Presumably it would be calculated at the same temporal frequency as the measured displacement data? Is the purpose of the grid search algo-

rithm to find the optimal set of n, alpha, and beta parameters that match the measure of displacement at a single point in time, or over all times that displacement was measured?

20) Page 6362; Section 3.2: It would be very helpful to have a drawing to illustrate the points made here about exactly how water-pressure dynamics contribute to the displacement. The first paragraph makes it sound as though the land displacement is driven more by the underlying fractured geology (with motion driven by gravity?), than by hydrology.

21) Pages 6347-6365: The methods section is exceptionally long and difficult to interpret. I suggest an entire restructuring of the section. The objective is to provide greater clarity and to reduce the overall length (perhaps to half of the current length). First, I suggest you open with a section that describes your proposed workflow (could have a section heading of "General Workflow"). This could be a short summary of your schematic diagram (Figure 1) and the ensuing soil-water balance model (Figure 2). Try to keep that at less than 1 page; the details will follow in subsequent sections. Conclude by telling the reader that you will now provide an example of how this workflow can be applied. Second, provide your site description and the climate stations and data sets that were available for use (currently sections 3.1 – 3.4). This could have a section heading of "Site Description and Available Data". Look for opportunities to decrease the length of that section, and provide better background information about how/why you think groundwater dynamics are an important factor causing displacement. At present, this section does a reasonable job of describing the underlying geology, but it leaves the reader doubting that water-table dynamics are really that important. You have cited some studies suggesting that it does – elaborate on the details of those studies. Third, develop a third section titled "Implementation of the Soil-Water Balance Model". Within this section you will have a few subsections: one describing how you estimate Rs, another for SAWC, another for Kc, and finally, ETo and ETa. Those would be followed by additional subsections that describe your runoff model and how you es-

timate this "aquifer saturation state". Most importantly, for each of these subsections very briefly describe how this calculation is a part of the overall workflow (in one or two brief sentences, possible alluding back to the "General Workflow" section and Figures 1 and 2), then describe specifically how you performed the calculation specific to your site. In this way, any reader who might actually want to use your workflow has a clear example. By taking this approach, you could also vastly reduce the length of the methods by combining much of sections 2.2-2.6 with sections 3.3 and 3.4 in a much more concise way.

22) Page 6371; lines 14-17: Comparing estimated recharge versus precipitation is a fairly weak test. We know, in principle, that recharge is more relevant than simply precipitation for influencing pore-water pressure. Are you familiar with the "recharge-weighting functions" used in tracer-based studies of soil-water residence times? These functions are basically accounting schemes that attempt to estimate which rainfall contributes to groundwater recharge and which does not. You could compute one of these functions (e.g. see Stumpp et al., 2009; Vitvar and Balderer, 1997)) using only your measured precipitation and estimated ETo (without the runoff model or all of the optimization procedures). This would be much simpler than the workflow you have executed here. As such, you could compare your more intensive method with this simpler method of estimating recharge, rather than comparing with precipitation alone.

Technical Corrections:

Technical corrections are written as comments in the .pdf file.

References: Stumpp, C., Stichler, W., and Maloszewski, P., 2009, Application of the environmental isotope delta O-18 to study water flow in unsaturated soils planted with different crops: Case study of a weighable lysimeter from the research field in Neuherberg, Germany: Journal of Hydrology, v. 368, p. 68-78. Vitvar, T., and Balderer, W., 1997, Estimation of mean water residence times and runoff generation by 18O measurements in a Pre-Alpine catchment (Rietholzbach, Eastern Switzerland):

Applied Geochemistry, v. 12, p. 787-796.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/11/C3036/2014/hessd-11-C3036-2014-supplement.pdf

---

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 6343, 2014.