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Interactive comment

Interactive comment on "Infiltration-Friendly Land Uses for Climate Resilience on Volcanic Slopes in the Rejoso Watershed, East Java, Indonesia" *by* Didik Suprayogo et al.

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Comment on 'Infiltration-friendly land uses for climate resilience on volcanic slopes in the Rejoso watershed, East Java, Indonesia' by D. Suprayogo et al. (HESSD; https://doi.org/10.5194/hess-2020-2)

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General comment: the paper by Suprayogo et al. describes the results of a short-term (ca. two months) field experiment aiming to identify which of the dominant land uses

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in the middle and upper parts of the 633 km2 upland Rejoso catchment in the volcanic uplands of East Java may be considered ‘infiltration-friendly’ under the prevailing rainfall intensities. In addition, the study seeks to identify which readily measured vegetation and surface characteristics may be used to define the critical threshold values associated with such ‘infiltration-friendly’ land uses. The rationale for this work partly lay in an observed decline in the quantity and quality of water resources in the study area believed to reflect changes in land use and land cover in the Rejoso basin as well as increased extraction of groundwater for irrigated rice cultivation in the lower parts of the catchment. The authors rightly point out that much of the debate on land cover and catchment water yield and/or streamflow regimes focuses on forested versus agricultural uses of the land, whereas preciously little is known on the comparative ability of such ‘intermediate’ land-use types as agroforestry systems in terms of maintaining soil infiltration capacity, groundwater recharge and dry-season flows under seasonal tropical conditions (Toohey et al., 2018; Nespoulos et al., 2019). Likewise, most global reviews of the literature on ‘forests and water’ have concentrated on the changes in total water yield associated with forest removal or addition, interpreting the observed changes in flow primarily in terms of increases or decreases in vegetation water use (evapotranspiration, ET) as a function of forest type or climate, but leaving the effects on flow regime by potential changes in surface conditions essentially non-analyzed (e.g. Zhang et al., 2017; Filoso et al., 2017; Bentley & Coomes, 2019). Only a few studies have paid explicit attention to changes in seasonal streamflow regime after removing or adding trees (e.g. Lane et al., 2005; Brown et al., 2013; cf. Van Noordwijk et al., 2017ab). In view of the fact that such changes in streamflow regime will reflect both changes in ET and in infiltration after the change in land use under given climatic conditions (Bruijnzeel, 2004; Peña-Arancíbia et al., 2019) the paper by Suprayogo et al. is to be welcomed in principle in that it documents infiltration (at the runoff plot scale) and erosion rates for a series of agroforestry systems (both terraced and non-terraced), rain-fed crops (mostly maize) and forest plantations (pine and mahogany) that may be considered typical for Java’s densely populated uplands. Moreover, the paper marks

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a valiant attempt at identifying some of the key vegetation and surface characteristics controlling infiltration rates, runoff production (i.e. overland flow) and surface erosion rates using low-cost / low-tech approaches for measuring rainfall, runoff and site characteristics.

The paper's key findings include strongly negative relationships between canopy cover fraction and either plot-scale runoff coefficient (Fig. 6) or surface erosion (Fig. 7), and to a lesser extent between the latter and surface litter amounts while understory biomass seems less important. However, individual land-use types deviated from this general pattern. For example, the 'young' and 'old' production forest plots (UT1 and UT2) had the same runoff coefficients (13-14%) but the young forest exhibited a far smaller soil loss than the older forest, despite having a more open canopy (by 12%, difference not significant), a much lower litter mass (2.0 vs. 9.2 t ha-1) and a much steeper slope (50-60% vs. 30-40%) (Tables 3 and 1). Likewise, agroforestry plots MT2 and MT3 exhibited the same (high) runoff coefficient (37-38%) but differed by a factor of 2.3 in soil loss with the amount of litter on the ground being the same (Tables 3 and 1). Such findings suggest that soil characteristics (as opposed to surface or vegetation variables) likely play a role as well and perhaps deserve to be included in any predictive equations (e.g. SOC?; applicable in the case of MT2 vs. MT3 but not for UT1 vs. UT2 (Table 2)). At any rate, it would be good to include such considerations explicitly in the Discussion section. On the downside, the paper gives the distinct impression of having been put together in some haste and the often highly concise text leaves much to be guessed (or derived) by the reader. This holds especially true for the sections describing the study area and methods, but also for the Results and Discussion. For example, the study area description effectively consists of a Table describing the land-cover types for the eight examined locations (Table 1) but fails to give basic location or climatic information, or a proper description of vegetation characteristics (e.g. tree height – important to assess the erosive power of crown drip) or the nature of the terraces of the mid-stream research plots (notably whether the plots included terrace risers or terrace beds only). As to the methods used, it is not clear – inter alia – in which plots ‘rainfall’ truly represents incident rainfall or rather crown drip (throughfall) (line 101); what size or type of funnel was used for the improvised rain gauges and what the catch efficiency of these gauges was compared to that of a standard rain gauge (cf. Ghimire et al., 2017; Zhang et al., 2019); how the collection drums with a stated capacity of 30 cm3 (!) (Fig. 2) could accommodate the runoff produced by 12 m2 plots with runoff coefficients of up to 41-64% (Table 4); how coarse sediment eroded from the plots was accounted for or what the effect of filtering runoff samples through ‘newsprint’ (lines 111-112) was on the magnitude of sediment concentrations obtained compared to more conventional filtration methods; how particle density was determined (line 124); or how many replications were used to determine undergrowth or litter mass (lines 140-141), etc. Ibidem for the Results section: e.g. the main results for soil properties are described in 1.5 lines only (lines 162-163); key Tables 2-4 give averages or period totals only but no standard errors or coefficients of variation as a measure of within-plot variability despite the fact that the large variation in rainfall (throughfall?) totals between adjacent (?) plots (e.g. 300 mm for plots MT4 and MT2 in Table 4) suggested major spatial variability; the captions to key diagrams 4 and 5 which count 8 panels each do not explicitly specify which panel refers to which plot in any descriptive way other than their relative position in the list of plots in Table 1. The Discussion section is rather basic and does not address such critical issues as the improvised nature of the rain gauges and the neglecting of stemflow as a possible input of water to the soil (which would lead to under-estimation of ‘rainfall’ and hence over-estimation of runoff coefficients), as well as the scale and ability of the plot-based measurements to represent the entire hilllslope’s hydrological functioning (see below for details and related issues)). Furthermore, the reference list – although remarkably up to date with more than 60% of cited references published between 2017 and 2019 – misses at least six references that are cited in the text and contains another six that are listed all right but do not show up in the text (see below under specific comments).

On a related note, rather than to refer largely to recent studies in such very different environments as the semi-arid loess plateau in China (Zhao et al., 2019; Liu et al.,

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2018 – erroneously referred to as Zhipeng et al., 2018) the paper would arguably have benefitted from the inclusion of related studies in the volcanic uplands of West and East Java for added perspective. Examples include the effect of trampling/footpaths on runoff production and erosion from upland fields (Bons, 1990; Rijsdijk et al., 2007; cf. Badu et al., 2019); the role of terrace risers versus terrace beds vis á vis runoff and sediment production (Purwanto and Bruijnzeel, 1998; Van Dijk & Bruijnzeel, 2004ab); the potential role of stemflow in the generation of localized infiltration or overland flow (high stemflow fractions reported for bamboo, bananas, shaded coffee, tall grasses and understory shrubs (Taniguchi et al., 1996; Cattan et al., 2007; Siles et al., 2010; Friesen et al., 2013; González-Martínez et al. 2016)); as well as a more holistic discussion of the interactions between canopy cover fraction, understory, leaf litter and hydrological processes (Wiersum, 1985; cf. Prijono et al., 2014; even Coster, 1938) – not only in terms of amounts of water involved but also the erosive power of the rain / crown drip as a function of falling height and leaf type (Wiersum, 1985; Hall and Calder, 1993). In contrast to what is stated in the paper (lines 209-211), rainfall interception does not reduce the erosive power of the rain. Rather, a tree canopy enhances it because of the typically larger drop size of crown drip compared to incident rainfall for terminal fall velocities (Wiersum, 1985), with broad-leaved species producing larger drops than do conifers (Hall and Calder, 1993). Likewise, the few measurements of throughfall intensities under humid tropical conditions suggest these to be very similar to those of incident rainfall (e.g. Hutjes et al., 1990). As such, the presumed effect on delaying the onset of overland flow (line 211) would seem rather negligible. Similarly, based on the lack of correlations with surface runoff or erosion (Figs. 6 and 7) the role of understory vegetation is considered to be minor. Yet Nespoulos et al. (2019) emphasized the importance of a well-developed understory for the development of macroporosity and preferential pathways in tropical plantations. In addition, the discussion could use a paragraph on the importance of including both infiltration and total ET (not just interception loss) for a proper assessment of the effect of land cover on groundwater recharge.

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Other points not considered in the Discussion include such aspects like (a) the need for an adequate number of throughfall gauges to quantify net precipitation beneath such spatially highly variable vegetation types as some of the studied agroforestry systems (cf. Holwerda et al., 2006; Zion Klos et al., 2014); (b) the role of auto-correlation in the presented relations between runoff coefficient (i.e. runoff/ rainfall) and rainfall, (c) the possible role of (high) short-term rainfall intensities as opposed to the presently used daily totals in determining measured amounts of surface runoff and eroded soil (cf. Wischmeier's EI30 index), (d) the effect of the small size of the runoff plots used (2 m x 6 m) on measured amounts of surface runoff vis á vis their representativeness for hydrological functioning at the hillslope scale (variations in slope steepness, re-infiltration on less steep foot-slopes in the case of the upper plots; cf. Stomph et al., 2002; Moreno-de las Heras et al., 2010), and (d) what might constituted a plausible value of 'tolerable soil loss' for the study area (Verheijen et al., 2009; cf. Bruijnzeel, 1983; see also specific comment *22 below for a possibly locally valid estimate).

On the basis of the above considerations my recommendation for this manuscript can only be rejection in its present form but allow resubmission if the main points raised in the previous paragraphs can be addressed satisfactorily. After all, the paper addresses an important topic and new information on infiltration behaviour of agroforestry systems is to be welcomed.

Specific comments: *1, Title: you may want to use inverted commas for 'infiltration-friendly' in the title as well. *2, line 20: based on the time line in Figure 3 (6 March–1 May 2017) a period of nearly two months would be more accurate than 'three months'. *3, line 21: strictly speaking, when using daily rainfall and surface runoff totals to obtain net infiltration amounts one cannot refer to the latter as infiltration rates. *4, line 24: 'preceding water use' is unnecessarily vague; suggest replacing by 'soil moisture status' because moisture values are governed by the interplay of rainfall/drip, evaporation and soil water uptake, not just vegetation water uptake. *5, line 24: relations with understory biomass or surface micro-topographic variation were not strong or absent

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(Figs. 6 and 7). *6, line 28: an average runoff coefficient of 62% (actually 64% in Table 4) is exceedingly high and more representative of compacted dirt roads and yards in the area than actively worked agricultural fields (see e.g. Rijsdijk et al., 2007). Such high values might suggest rainfall inputs may well have been under-estimated. Worth checking! *7, line 29: with porosities of 55-62% and bulk densities of 0.9-1.1 g cm-3 (Table 2) the soils of the mid-stream sites are not particularly dense. Rather, one would think of crusting or slaking as a potential cause for low apparent infiltration? *8, lines 41-42: this sentence seems to fall out of the blue; since the cited reference concerns a global review of the literature on surface erosion it might be possible that the authors meant Wiersum (1985) instead which documents the role of understory and litter layer with regards to surface runoff and erosion in an Acacia auriculiformis plantation in West Java, not pines. Incidently, drip from a pine canopy is less erosive than that from broad-leaved species like Eucalyptus and, especially Teak (Hall and Calder, 1993). *9, line 52: the sentence on infiltration recovery seems out of place here and had perhaps better be moved to the Discussion section. *10, line 54: whilst the influence of a very extensive and aerodynamically rough forest cover on rainfall may have an effect on downwind rainfall amounts (as opposed to 'events'), it would seem inappropriate to mention this in the present context of on-site water dynamics. Suggest to leave out this aspect. *11, lines 62-64: unclear why Climate Resilience is written with initial capitals?; also, relation between flow persistence metrics and peakflow transmission (routing? percolation?) is not instantly clear. *12, lines 77-78: soil fertility and agricultural productivity may be maintained sufficiently on these deep volcanic deposits even if surface erosion rates are high. Also, previous research on sediment production in similar terrain nearby in East Java (Rijsdijk and Bruijnzeel, 1990ab; Rijsdijk, 2005) has shown that contributions from rain-fed agricultural fields made up a comparatively minor proportion of overall annual sediment yield at the operational catchment scale with roads, paths, settlements contributing significant amounts each. *13, section 2.1, Study area: suggest to give a proper basic description of site locations (place names, latitude, longitude, elevations) along with information on the main environmental conditions, notably (a) annual rainfall totals and the agro-climatic classification of the two main sites in terms of rainfall seasonality (e.g. Oldeman climate types D3 and C2 for the middle and upper zones of the catchment?), (b) prevailing rainfall intensities (e.g. based on the authors their own measurements or the iso-erodent map of Java ?, and perhaps (c) FAO reference evapotranspiration (to help assess the importance of differences in infiltration relative to vegetation water use). Soil characteristics for the study plots are presented in Table 2 under Results but some general initial indication of soil types, their spatial extent in the catchment and their relative susceptibility to erosion (e.g. expressed as Wischmeier K-values?) would not go amiss here (instead of the scattered reference made to soil characteristics across different sections). More importantly, give information on the age of the tree plantations (plots UT1, UT2, MT1 and MT2) and the height of the trees (important for assessing the erosive power of the raindrops, see previous comments on Discussion section) as well as some indication of tree densities in the various agroforestry plots (MT2-4), the width of the Casuarina strips in UT3, etc., etc. Likewise, photos of the respective plots could be added as Supplementary Material to give the reader a better impression, also in terms of plot sizes relative to terrace dimensions (were terrace beds back-sloping? If so, were adjacent upslope risers in plots MT1-4 included? (cf. Purwanto and Bruijnzeel, 1998); what was the nature of the terrace risers (grassed, weeds, stones?). NB: Table 1 still contains a number of plant names in Bahasa Indonesia (e.g. mahoni instead of mahogany) plus a number of typos (Albizia, manggo, dapap). *14, section 2.2 Rainfall: 'ombrometer' is an obsolete (colonial) term. Give dimensions of the rain gauge funnels and indicate whether these improvised gauges were calibrated against standard gauges to assess degree of under- or over-estimation of rainfall (rain-splash out of funnels during times of high intensity or effect of a broad rim on drop partitioning, etc.). Make clear in what plots the measurements represented rainfall (e.g. UT4?) or throughfall? (NB: adjust subsequent language in main text accordingly whenever discussing 'rainfall' if needed, e.g. in section 3.1, etc.). Add photos of position of gauges in Supplementary Materials since using only five rain/throughfall gauges per plot would seem inadequate given the

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large variation in TF that is expected for such spatially variable vegetation? Also: two months duration, not three (lines 97-98) based on Figure 3. *15, line 104: awkward description of the runoff measurement system. Suggest to use the term 'divider system' instead and give maximum collection capacity for the two drums plus divider system in litres of water. NB: the volume given in Figure 2 for the drums (30 cm3) must be erroneous. Also, was the metal plate guiding the runoff to the drums sheltered against direct rainfall inputs? If not, runoff amounts will have been over-estimated somewhat. *16, line 109: strictly speaking, volume has the dimension of litres or cm3, not mm of water. You could simply give the volume in litres and divide by plot area in m2 and remain all right dimensionally. Suggest to remove the hyphens in d-I etc. in Equations 1 and 3 as they can be read as minus signs rather than hyphens. NB: second Dd-I in Equation 3 should read Dd-II. *17, lines 111-112: what was the efficiency of filtering your runoff samples using a newspaper compared to more conventional filters (e.g. Whatman or Millipore 0.45 μm)? Rijsdijk and Bruijnzeel (1990a) used simple coffee filters that were calibrated against conventional filtration. You may consider using a similar approach in future work. What about the sand fraction ending up in the first drum? Was the runoff water thoroughly stirred prior to taking the water sample? If so, inform the reader as such. *18, lines 122-125: did you take one block sample for bulk density measurement as suggested by the text or three? After all, you tested for differences in Table 2. How was particle density measured (by pycnometer?). *19, section 2.5.1. Canopy cover: it only becomes apparent in line 134 that the vegetation plots measured 20 m x 20 m; suggest to indicate this at the start of the plot descriptions. Lines 133-135: did you cover entire 5 m x 5 m areas with plastic/paper? References to Arumsari and Astutik are missing from reference list so cannot be checked (but might be in Bahasa Indonesia anyway and hence less accessible for most readers?). Line 136: suggest to replace CV in Equation 5 by another symbol to avoid confusion with coefficient of variation. NB1: one could also derive the canopy cover fraction from measurements of throughfall for small storms that do not saturate the canopy. The slope of the regression line between incident rainfall and free throughfall equals the gap fraction

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(p), hence canopy cover fraction c equals 1 – p (Jackson, 1975). NB2: one wonders why direct observation of the contact cover fraction was not preferred to the more cumbersome (weighing/drying) litter mass approach? Contact cover fraction has been shown to be closely related to surface erosion rates in numerous cases (see e.g. Yu, 2005). *20, section 2.5.2. Understorey and litter: reference to Hairiah et al. is missing from the reference list (presumably the CIFOR publication). Indicate the number of replications used please. Using 50 cm x 50 cm would seem inadequate for understory measurements in the case of Lantana or Chromolaena shrubs. Were these present in the forest plots like they are in many plantations across Java? *21, section 2.5.3. Surface roughness: awkward formulation ('elevation', 'vertically'). Suggest rephrasing. *22, line 187: daily rainfall totals do not represent rainfall 'intensity' although you might refer to 'event intensity' if event durations are known. Lines 188-190: this belongs to Discussion rather than Results and is rather speculative anyway given the non-linearity of the rainfall-erosion relationship. Add discussion on what might constitute 'tolerable soil loss' in the study area given the rate of chemical denudation of andesitic ashes (= approximate rate of soil formation; Verheijen et al., 2009). See e.g. Bruijnzeel (1983) who determined the rate of chemical weathering for a high rainfall area with Inceptisols in Central Java at ca. 85 t km-2 yr-1. Given the difference in rainfall between his site and the Rejoso catchment a value of ca. 40 t km-2 yr-1 might be defendable, suggesting the tolerable soil loss might be as low as 0.4 t ha-1 yr-1? *23, lines 207-218 and 219-229: see main comments above. *24, line 231: what was the original slope steepness in the mid-stream area before bench terracing? Line 232: awkward formulation. Line 233: remove reference to seawater intrusion since not pertinent to present case? Line 234: 'erodible', not 'erosive'. Line 240: Liu, not Zhipeng. *25, line 245: in the middle and upper Rejoso watershed; line 247: keep erosion at acceptable levels? Line 248: gentle slopes associated with bench terracing or inherently gentle? If so, one wonders about the need for bench terracing. *26, lines 251-253: remove 'was' (four times); Remove Didik Suprayogo in lines 252-253. *27, references: standardize journal abbreviations, use of capitals, etc. Remove references not mentioned in text

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(Alvarenga, Anache, Boongaling, Choto, Kellner, Teklay); add missing references given in main text including Astutik et al. 2015; Hairiah et al. 2005, Hoechstetter et al. 2008, Suprayogo et al. 2017, etc. *28, diagrams: Figure 1, add latitude/longitude indications; Figure 2, move to Supporting Materials as it does not add much or replace by a Photo?; correct the volume of the collector drums; Figure 3, use less awkward date indication; Figures 4, 5: indicate which panels refer to what land cover type; Figures 6-7: complete captions. Tables: add standard errors or coefficients of variation where appropriate.

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