Reply to comments of reviewer #3

We thank this reviewer for her/his very constructive comments. Our responses to the comments are provided in bold (below each comment).

General comments of reviewer #3

The authors conducted a large scale throughfall measurement campaign in 20 secondary forest plots in Panama which were 3 to about 130 years old. Throughfall changed rapidly with ongoing forest succession and surprisingly approached values of mature forest only after about a decade. A simple regression model explained throughfall better than a more complex modeling framework. The third conclusion drawn by the authors is not clear to me (see the following). In my view, another highlight of the work is the link which the authors draw to data on changes in soil permeability with ongoing forest succession (see Bruijnzeel (2004) on the importance of the link to the soil). Unfortunately that section is not explored in depth. It would also be good to have the authors make some guesses on how transpiration changes with ongoing succession (just a few words on that!) if they are discussing succession effects on runoff/streamflow. The paper is well written but requires some more precision and consistency in the terms used.

In the revised version of the manuscript we 1) improved the description of the landscape-scale throughfall estimates, 2) included a new figure (Fig. 5) that illustrates the changes in throughfall and soil permeability with increasing forest age, and 3) improved the consistency of terms used. In the original manuscript we briefly outlined the role of transpiration (cf. Sects. 1 and 4.1). Since we lack knowledge about transpiration rates in our study forests, we prefer not to speculate about their changes during secondary forest succession.

I find the paper both relevant and interesting and therefore would recommend it for publishing in HESS.

To me it is not fully clear how the authors arrive from their sections 2.3.5 and 3.5 at the conclusion that “the influence of young secondary forests on interception in real-world fragmented landscapes might be detectable only in regions with a substantial fraction of very young forests”(P8000 L20-26 and P8016 L16-22). How do “young” and “very young” differ and how do they compare to “old” and “mature”? Which direction has that “influence”, where can it be “detected” (in streamflow/runoff?) and what is the other fraction of the region you are talking about that serves as your reference to judge the “detectability” of your “influence” (pasture or mature forest)? Do you mean that a forest <5 years has hardly any interception and behaves pasture-like and thus needs to cover almost the whole catchment in order to have a detectable influence - which is a rise in interception compared to pasture? Or are you referring to the fast adaptation of the interception of forests <10 years to values reached by forests >10 years or “mature forests”? In opposite to the substantial fraction of “very young forests” needed to have an detectable influence on interception, an unsubstantially small fraction of young forest (>10 years but < 130 years) which already behaves like “mature forest” will yield the same big “influence” on interception in a region since the increase in interception

compared to pasture is dramatic as succession proceeds. Does all of that refer to interception only or does it also include the change in soil hydr. conductivity? I am lost! Please clarify!

The issues raised here appear later in the "specific comments" section where we will reply to them.

Further, the paper would benefit from a small section in the discussion where the authors set their measurements in perspective to other studies and discuss (only briefly!!!) how throughfall might change throughout the year when trees shed leafs and rainfall intensities change and how their measurements extrapolate to a full year. From temperate forests I learned that interception rates in winter can even be higher than in summer due to the change in rainfall intensity and distribution, although trees are leafless then. What do the authors think (again: only briefly!) how changing rainfall pattern and changing foliage influence interception/throughfall throughout the year in their study area?

We slightly extended the discussion and related our findings to previous research from other secondary forest sites.

We agree that the influence of phenological dynamics on interception deserves attention. Other than in temperate regions, however, phenological dynamics will have a rather small impact on rainfall interception at our research sites because 1) only a comparatively small fraction of the trees is deciduous and 2) the time during which trees have no leaves usually coincides with the driest months. As to the first issue, botanical surveys indicate that around 10 % of the canopy tree species in the old-growth forest on Barro Colorado Island are dry-season deciduous (Croat, 1978) and that around 13 % of the total stand basal area of Agua Salud's secondary forests is deciduous (van Breugel, unpublished). However, deciduousness at our sites is a complex phenomenon; some trees are leafless for weeks, some for months, and others drop their leaves only in particularly dry years (Foster and Brokaw, 1996). A previous study (Zimmermann et al., 2009) showed that relative throughfall at particular sampling locations changed little during times of leaf drop and leaf development. As noted by this reviewer, changes in rainfall characteristics are probably more important than phenological dynamics. At our research sites, rain events during the dry season (mid-December – April) tend to be smaller and less intense. However, even these changes of event characteristics will have a limited influence on the overall water balance because of comparatively low rainfall over the entire dry season (cf. January – April: 232 mm ± 146 mm vs. May – December: 2409 ± 425 mm, mean ± 1 standard deviation, n = 82, data from 1929 to 2010 monitoring station on Barro Colorado Island, data courtesy of the Environmental Sciences Program, Smithsonian Tropical Research Institute, Republic of Panama). Although we never intended to extrapolate our findings to a whole year (see objectives of this study), the distribution of rainfall between dry and wet season suggests that our findings are representative in terms of the overall water balance.

Because it is not within the focus of this study to assess the influence of deciduousness on throughfall and because previous studies already showed that leaf drop and leaf flush had only little influence on the amount of relative throughfall at our sites, we decided

not discuss the influence of deciduousness on interception within the manuscript. Nevertheless, we included information on deciduousness in a new supplement (Supplement 1)

Specific comments

Please be more consistent in your wording / in the terms or definitions you use: either use inventory data OR forest structure parameters OR forest monitoring data / throughfall site OR throughfall plot OR study plot / forest age OR recovery time OR succession time / old plots OR mature plots OR mature sites OR BCI plots etc..

As suggested, we replaced "forest monitoring data" with "forest inventory data" (1 occurrence). From our point of view, however, the term "forest structure parameter", cannot be replaced by "forest inventory data", because the former are derived from the latter and hence, have a distinct meaning.

As suggested, we replaced "throughfall sites" with "throughfall plots" (2 occurrences).

As suggested, we replaced "study plots" (1 occurrence) with "throughfall plots".

As suggested, we replaced "recovery time" with "forest age" (5 occurrences). Note: the replacement with "forest age" also follows a suggestion of reviewer #2.

In section 2.2.1 we now clarify what we mean with "very young forest". Moreover, following a suggestion of reviewer #2 we improved and simplified this paragraph. In addition we checked the use of "old" and "mature" but could not find any ambiguous phrases. For reasons of readability we still use the word "old".

Always make clear to which plots you are referring to when you say "all plots" and always provide the reader with the number of replicates used.

As outlined in section 2.2.1 we optimized site selection for a regression-type analysis. Therefore, we do not work with replicates. However, if "replicates" refers to the number of collectors, we already provide this information in the manuscript (see section 2.2.3 and Table 1).

Do the prediction sites always include the throughfall plots?

Our prediction sites include the plots where we measured throughfall except during validation (described in section 2.3.3). Predictions based on a "full dataset" are not a problem as long as the model is not used for validation purposes. Please note that we avoid the term "prediction site" in the revised manuscript. Instead, we now use the term "SFD plots" when we refer to all plots from the secondary forest dynamics study, which were used in this study, while the term 'throughfall plots' is now reserved for the plots of the throughfall monitoring campaigns (i.e. 16 of the SFD plots plus the four mature forest plots on BCI). This new nomenclature is described in section 2.2.1 of the revised manuscript.

Also introduce your concepts of “mature”, “old”, “young” and “very young forests” in earlier sections. Are you only referring to “secondary forests” if you are talking about “mature forests”?

Following this reviewer’s suggestions and comments of reviewer #2 we clarified and improved section 2.2.1. In the revised version of the manuscript we clearly state what we mean with “very young forest” and “young forests”. From the context of our manuscript it is clear that we always refer to secondary forests even when using the term “mature forests”.

Page 8000

L20-23: I am not sure if I can draw that conclusion from your work. Please see later comments and general comment!

We replied to the specific comment below.

L24: “undesirable effects” assumes that every reader knows what effects you are talking about and which ones of those are desired. Please indicate the effect found in case of forest re-growth and its direction. Do initial stages refer to “young” or “very young” forests?

We clarified the sentence. The text now reads as follows: “…Our results suggest that where entire catchments are undergoing forest regrowth, initial stages of succession may be associated with a substantial decrease of streamflow generation. …”. We believe that the strict differentiation between “young” and “very young” forests is not of relevance in the abstract section.

Page 8004

L1:15: In the “study area” section you indicated, that there is a mosaic of different land use patterns in ASP. Although I believe that there is little relationship between throughfall and distance from the forest edge (Klassen et al. 1996), I still find it important to mention how big the forest patches were in which you set up you plots. Specifically: How far are your samplers from your plot boundary and from the edge of the whole patch?

We now mention in the manuscript that all plots were surrounded by forest (see section 2.2.1). Since we placed the collectors within each plot using a simple random sampling routine (cf. section 2.2.3), their distance to the plot edges is random too.

Page 8005

L3-6: You write that you identified the species of all trees/shrubs/palms. In the introduction you are also referring to the importance of vegetation type for throughfall generation/interception (e.g. invasive plants versus natural succession). Thus I really would welcome a list of the most (!) dominant species of trees/shrubs/palms in your plots, maybe sorted by plot age classes – or even plot-wise (Appendix)! Which ones are deciduous? I suppose all species were fully foliated during your measurements? Is there any information on the species in the BCI plots too? That might also help to discuss if BCI is a good reference for ASP.

In the revised version of our article we provide a supplement which contains information on the five most dominant species, their share on the total basal area, and whether or not these species are deciduous (Supplement 1). The information on deciduousness is provided for descriptive purposes only as all species were fully foliated during our throughfall monitoring campaign.

The plots on BCI serve as a reference because the vegetation on the island represents the natural vegetation of the central Panama Canal Watershed (Foster and Brokaw, 1996).

L8-9: I would recommend a bit more detail here (e.g. on camera settings like exposure (see Beckschäfer et al, 2013) or height at which the pictures were taken, at what time were they taken etc.) to ensure comparability with other studies. Is HemiView only the software?

In the revised version of the article we provide more details on the acquisition of the hemispherical photographs. The revised paragraph reads as follows:

"During the throughfall measurement campaigns, we took hemispherical photographs above each throughfall collector at approx. 0.5 m height. All photos were taken during overcast sky conditions in the early morning or late afternoon using a Canon EOS50D camera equipped with a SIGMA Circular Fisheye 4.5 mm 1:2.8 lens. At each position we obtained a series of photos with varying exposure and selected the picture for further analysis which showed an optimal histogram of the brightness (cf. Beckschäfer et al., 2013). All selected pictures were analyzed using the software HemiView v8 (Delta-T Devices Ltd)."

L16: How far were your rainfall collectors from obstructing objects (houses, trees etc.)? Did you apply any correction to rainfall data (e.g. considering wind/radiation etc.)?

The distance of our rainfall collectors to obstructing objects varied among sites as the height of the surrounding vegetation varied. Following standard procedures (WMO, 2008) we ensured that no vegetation hampered the rainfall measurements in an angle of approx. 45°. We did not apply any correction to the rainfall data because the gauge positions (i.e. positions surrounded by vegetation) ensured minimum disturbance by wind.

L19-20: Where the gauges relocated randomly to "several randomly selected locations" during the measurements (like recommended for throughfall studies e.g. by Lloyd and Marques 1988) or where only the locations of the samplers chosen randomly and samplers were not moved during the two months? At what height did you measure? Please indicate!

We did not apply a roving sampling scheme (although we considered it at first) because the amount of daily fieldwork was already at its maximum. All collectors were placed at the forest floor which resulted in a height of the receiving area of approx. 0.5 m.

L22: What about the "very young plots" and "mature plots" you are referring to elsewhere?

To ensure consistency of the terms used we completely rewrote the criticized section.

L3: Please indicate the date of your sampling campaign.

Why is it of interest to give the exact dates of the sampling period (instead of writing "…We monitored throughfall […] continuously for two months in the middle and late rainy season of 2011…")? We would be happy to include the sampling dates if the reviewer provides us with a forceful argument to do so. What we want to avoid, however, is overloading the paper with unnecessary detail.

L9: You mention here that you sampled on event basis too. What happened to this data? I would find it helpful if you could at least give a little more detail on the rainfall characteristics (intensities/distribution) found during the year and during your study period.

Our study is based on accumulated throughfall data (cf. section 2.3.1). Event-based data was just obtained for two throughfall plots in the ASP area and the for the mature forest sites on BCI where we worked on additional aspects of rainfall interception. Given that we obtained our throughfall data in 18 of the 20 plots at the same time and given that we worked with accumulated data, more information on rainfall intensities is not required to answer the research questions posed in our study.

L12-16: You mentioned palms/shrubs in your plots in another place. Palms often have higher stemflow compared to co-occurring trees. (e.g. Jordan and Heuveldop 1981, Lloyd and , Marques 1988). The same is valid for e.g. Bananas or other herbs. Did you consider that in your 1% assumption? Did you find any hints on a stemflow : BA relationship in your unpublished stemflow data?

We measured stemflow on Barro Colorado Island on 60 stems, 7 of which were palms (*Oenocarpus mapora* H. Karst.). Our data show no clear relationship between stemflow volume and basal area. The revised version of our article contains supplementary material which provides detailed information on that stemflow data (see Supplement 2). We never observed stemflow in the young secondary forest plots in the Agua Salud area (plot #1 – #16, Table 1) and hence, did not consider measuring stemflow in these plots. Indeed, our throughfall data from three of the ten plots with forest regrowth younger than 10 years indicate that stemflow must have played a negligible role as interception in these plots (based on rainfall and throughfall data only) was around 100 % (cf. Table 1), which virtually precludes the occurrence of stemflow. Scrutinising the species list (see Supplement S1), we found two throughfall plots (#7 and #9) that contain a species (*Banara guianensis* Aubl.) which has been suspected to produce large stemflow volumes (Hölscher et al., 1998). However, even these plots show relative throughfall values which are well within the range of the other young plots. To sum up, our data, field observations, and data from previous studies in Panamanian secondary, mature, and plantation forests (Cavelier et al., 1997; Macinnis-Ng et al., 2012; Park and Cameron, 2008) encouraged us to consider stemflow as negligible at our study sites.

L20: What does "long-term" data mean? I was assuming you only measured for two months!

Following a suggestion of reviewer #2 we changed this sentence. In the revised version the ambiguous term "long-term data" is not used anymore.

Page 8007

L2: I think it would be better to refer to “throughfall” instead of interception, since 1) the calculation of interception requires knowledge about stemflow (which you only have for the BCI-Sites) and 2) you are, to a large extent, referring to throughfall measurements throughout the paper.

We prefer sticking to interception because this is the variable of interest to most researchers and decision makers. In our reply to the short comment of F. Holwerda, we justify calculating interception loss with the available throughfall and stemflow data. The new supplementary material (Supplement 2) of the revised version of our article provides sufficient evidence that stemflow in our research area is indeed negligible, which allows calculating interception loss based on throughfall data only.

Section 2.3.2 A reference to Table 2 would be helpful here.

We prefer a strict separation of methods, results and discussion section. We do not refer to table 2 in the methods section because this table presents results.

Pages 8008 – 8009

Section 2.3.4: I cannot comment on this section, since I am not familiar with the method. But it appears that my understanding could be greatly improved if it is simplified a bit.

Understanding this section requires basic knowledge of Bayesian statistics. In the revised version, we tried to give some more explanations on things like the prior on model size - we hope this helps a bit to understand the procedure.

Page 8009

L27: What does it mean when you say “We pooled forest inventory data ...within ... age classes”? Which age classes? What is n for each age class? From Fig.4 I learned that you have a lot of outliers in the age class from 10-20 years. Would that look different if you e.g. choose e.g. 5 classes?

We re-wrote section 2.3.5. Now it should be clear which data was pooled.

From our point of view it is not necessary to provide the information on the selected age classes in the method section (they are given in Figure 4).

Each age-class contains 1000 values. This information is provided in section 2.3.5.

The number of age-classes may influence the number of outliers in Fig. 4 but this is not relevant for our work.

L26 and Page 8010 L7: Please use “prediction sites and throughfall plots” if that is what you used. You defined the names of your different plots already in Fig. 1, so please stick to those!

We changed the naming of the plots in Fig. 1 because we predicted relative throughfall also for the throughfall plots.

L6-10: I assume that this section shall provide the answer to you research question #3 in the introduction. I don't understand how you estimate "landscape scale estimates". What is the landscape you are referring to? Whole ASP? How big is the fraction of young/very young forests or of forests for which you can predict interception in your area? See also my general comment and the comment on the conclusion section below.

First of all, all calculations described in section 2.3.5 are necessary to answer research question #3. As written in our manuscript, our calculations involve two steps.

In the first step we took all available BA_{ratio} data from the SFD plots (95 sites, 3 forest inventories) and grouped these data by pre-specified age classes (class 1 (< 5 years): n = 41, class 2 (≥ 5 years & < 10 years): n = 97, class 3 (≥ 10 years & < 20 years): n = 88, class 4 (≥ 20 years & < 30 years): n = 56). In a next step we fitted empirical distributions to the BA_{ratio} data. That is, we obtained four BA_{ratio} distributions, one distribution for each age class. Then we sampled these distributions and obtained 1000 BA_{ratio} data for each age class. We believe that this reviewer may have missed this important step of our analysis. Based on the 4 datasets we obtained 1000 predictions of relative throughfall for each age class. Because we used real-world data to fit the empirical distributions, our predictions are indeed related to a specific landscape, namely, the Agua Salud area. However, it is important to understand that the predictions of relative throughfall in this step of our analysis do not reflect the actual distribution of young and very young forest in the Agua Salud area. That is to say, the first step of our analysis was solely intended to assess differences of relative throughfall among forest age classes. Our predictions provide evidence that only forests < 10 years show relative throughfall values which clearly differ from values measured in old-growth forests.

In the second step of our analysis we predicted relative throughfall input to all secondary forest plots in the Agua Salud area. This analysis showed that "…The predicted values still differ from throughfall of mature forest on Barro Colorado Island (BCI), though more than half of the SFD plots already show relative throughfall values which are within the credible interval limits of relative throughfall at the mature forest sites…" (cf. Sect. 3.5). In other words, the fraction of forests < 10 yrs in the Agua Salud area is already too low to detect a pronounced difference of relative throughfall between the Agua Salud and BCI forests.

L20: Throughfall can also be higher as incoming precipitation if a sampler is located underneath a "dripping point" in the lower canopy to which intercepted water is diverted which originates from a larger "collection area" in the upper canopy.

We agree that single relative throughfall values > 100 % are nothing exceptional, particularly in tropical forests (cf. Lloyd and Marques, 1988). However, <u>mean</u> relative throughfall estimates > 100 % can only be explained by 1) uncertainty of mean throughfall estimates, 2) uncertainty of rainfall estimates, 3) occurrence of horizontal precipitation. Of course, mean relative throughfall values > 100 % can be the result of a combination of the latter three factors. In section 3.1 we refer to mean throughfall

estimates, which is why we relate negative values of interception to the (in our case) most likely reason: the uncertainty in estimating mean throughfall.

P8011 L1: What is recovery time?

Following a suggestion of reviewer #2 we replaced all occurrences of "recovery time" with "forest age", except at page 8002 line 1 where we replaced recovery time" with "regrowth stage".

L5: and elsewhere: again: Please make sure if you talk about "mature forests" or "mature secondary forests"!

In the revised version of our manuscript we only use the term "mature forest". From the context of our manuscript, however, it should be clear that we use the term "mature forest" also if we refer to old-growth secondary forest.

Page 8012

L25-26 and Page 8013 L6-7 and Fig 4: How did you estimate the "credible interval limits of relative throughfall in mature forests of our study area"? Were they taken from the sources provided in Fig.4 ? Did they originate from BCI data? Or were those "priors" you used in your model? I don't get it!

A (1 - α) x 100% Bayesian credible interval is an interval that has a posterior probability of 1 - α of containing the parameter of interest, in our case the mean (Bolstad, 2007). Please note that we give this definition in the figure caption of Fig. 4 in the revised manuscript version.

The credible interval is calculated as follows:

$$CI = m' \pm t_{1-\frac{\alpha}{2},\, n-1} \cdot s' \qquad \textbf{(1)}$$

where m' is the posterior mean and s' is the posterior standard deviation, which are calculated using Bayes' theorem (posterior = prior x likelihood; the likelihood is obtained from the sample data, in our case the data from the throughfall plots on BCI). The posterior mean constitutes a weighted average of the prior mean (the mean of the prior data) and the sample mean (the mean of the actual sample) where the weights result from the ratio of the precision of the prior to the precision of the sample data. That is, if the there is a lot of prior information but few sample data, the prior gets more weight than the sample, and vice versa. The posterior standard deviation is obtained from the prior standard deviation (see below) and the sample variance.

We used the *t* distribution instead of the standard normal distribution because the variance of the sample data is unknown a priori (using the *t* instead of the standard normal distribution widens the credible intervals as a function of sample size). We set α to 0.05.

In our case, it was reasonable to assume a normal distribution for our prior. We derived the prior mean and the prior standard deviation from information given in previous throughfall studies in tropical lowland rainforests (e.g. Asdak et al., 1998; Cuartas et al., 2007; Hutjes et al., 1990; Vernimmen et al., 2007). That is, our prior mean equaled the mean of the relative throughfall estimates given in those previous studies, and we derived our prior standard deviation from the difference between the maximum and the minimum of the previous relative throughfall estimates divided by 6 (cf. section 11.3, Bolstad, 2007).

Page 8013

L18: On Page 8007 you mentioned that you that you are going to use "openness hereafter" for canopy openness. Please continue to do so if you said elsewhere that you intent to do so!

Changed as suggested.

P8014 L4-6: I find it really good that you relate your throughfall data also to soil permeability! But how long does it take to recover permeability? This cannot be seen from Tab1 and Fig 4! (This is needed in order to hold on to your first conclusion!).

We included a new figure (Fig. 5) to corroborate our claim that the fast change in canopy interception during forest succession clearly predates the recovery of soil permeability.

Page 8015

L12: You cannot "improve" the relationship between forest structure and canopy interception. You can improve your models or your understanding of the relationships.

This is correct, we re-wrote the sentence.

Page 8016

L16-22: Again: The paragraph is hard to understand and confusing. We need to know what young secondary forests are (in opposition to "very young secondary forests"). Why do you think the influence of "young forests" might only be detectable where there are a lot of "very young forests"? Also the uncertainty with which you predict/measure throughfall does not have any impact on the throughfall of secondary forest which you somehow claimed in the first sentence but in contrast clarified in the last sentence of the paragraph. But I also wonder about that clarification: Why should one confuse the limited detectability of an interception signal with the relevance of the change in interception during succession? Why does a substantial fraction of "very young forests" overcome uncertainties in throughfall predictions/large variations in throughfall during succession that both influence the interception of young forests? Please clarify!

We rewrote the entire paragraph which now reads as follows:

"…Given the uncertainties associated with throughfall predictions and the inherently large variation of throughfall during early forest succession, the influence of young

secondary forests on interception in real-world fragmented landscapes might be detectable only in regions with a substantial fraction of forests < 10 years. This is because in tropical lowland regions, only forests in early successional stages show canopy interception values that are consistently lower than those of mature forests. …".

8022 and 8024 Please visualize the throughfall : age and throughfall : BAratio relationships. (Appendix?)

We consider the current presentation of our data; that is, presenting the values of throughfall and forest age in Table 1 and showing the strength of the throughfall – forest structure parameter relationships in a correlation matrix (cf. Table 2), as more informative than presenting these data in scatterplots. We therefore do not wish to further extent the supplementary material.

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