Requirements for throughfall monitoring: The roles of temporal scale and canopy complexity

Alexander Zimmermann*, Beate Zimmermann[1]

Institute of Earth and Environmental Science, University of Potsdam, Karl-Liebknecht-Straße 24-25, 14476 Potsdam, Germany

ABSTRACT

A wide range of basic and applied problems in water resources research requires high-quality estimates of the spatial mean of throughfall. Many throughfall sampling schemes, however, are not optimally adapted to the system under study. The application of inappropriate sampling schemes may partly reflect the lack of generally applicable guidelines on throughfall sampling strategies. In this study we conducted virtual sampling experiments using simulated fields which are based on empirical throughfall data from three structurally distinct forests (a 12-year old teak plantation, a 5-year old young secondary forest, and a 130-year old secondary forest). In the virtual sampling experiments we assessed the relative error of mean throughfall estimates for 38 different throughfall sampling schemes comprising a variety of funnel- and trough-type collectors and a large range of sample sizes. Moreover, we tested the performance of each scheme for both event-based and accumulated throughfall data. The key findings of our study are threefold. First, as errors of mean throughfall estimates vary as a function of throughfall depth, the decision on which temporal scale (i.e. event-based versus accumulated data) to sample strongly influences the required sampling effort. Second, given a chosen temporal scale throughfall estimates can vary considerably as a function of canopy complexity. Accordingly, throughfall sampling in simply structured forests requires a comparatively modest effort, whereas heterogeneous forests can be extreme in terms of sampling requirements, particularly if the focus is on reliable data of small events. Third, the efficiency of trough-type collectors depends on the spatial structure of throughfall. Strong, long-ranging throughfall patterns decrease the efficiency of troughs substantially. Based on the results of our virtual sampling experiments, which we evaluated by applying two contrasting sampling approaches simultaneously, we derive readily applicable guidelines for throughfall monitoring.

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1. Introduction

Accurate and precise estimates of throughfall are required for a variety of applications in meteorology, hydrology, ecology, and biogeochemistry. One such application is interception modeling which enables the prediction of interception loss. These models require specification of parameters such as the canopy storage capacity and the free throughfall coefficient (Muzylo et al., 2009), which are usually derived from regressions of throughfall on rainfall (e.g. Gash and Morton, 1978; Herbst et al., 2008; Jackson, 1975; Leyton et al., 1967; Lloyd et al., 1988; Schellekens et al., 1999). Not surprisingly, the quality of throughfall data strongly influences the accuracy of these parameter estimates and hence, the performance of interception models (Hutjes et al., 1990; Lloyd et al.,

1988). Given the large within-stand variation of throughfall and the resulting difficulties with sampling, Vrugt et al. (2003) supposed that throughfall measurements contain insufficient information for parameter derivation. The same authors therefore suggested calculating the canopy water storage from the attenuation of microwave signals. Linking microwave attenuation to actual water storage, however, requires a careful calibration on throughfall data (Bouten and Bosveld, 1991; Bouten et al., 1991; Calder, 1991). In other words, there is no escape from the ugly truth: precise and accurate throughfall estimates are mandatory regardless of the method applied to derive the canopy storage parameters for interception modeling.

Other applications requiring throughfall data involve assessments of land cover changes on local water fluxes (Dietz et al., 2006; Holwerda et al., 2010; Macinnis-Ng et al., 2012; Ponette-González et al., 2010; Schrumpf et al., 2011), studies of the influence of forest structure on rainfall partitioning (Brauman et al., 2010; Krämer and Hölscher, 2009), or comparisons of solute fluxes in different forest ecosystems (Dezzeo and Chacón, 2006; Hofhansl et al., 2011; Macinnis-Ng et al., 2012). All these investigations

* Corresponding author. Tel.: +49 331 977 2047; fax: +49 331 977 2068.
E-mail address: alexander.zimmermann.ii@uni-potsdam.de (A. Zimmermann).
[1] Present address: Research Institute for Post-Mining Landscapes (FIB e.V.), Brauhausweg 2, 03238 Finsterwalde, Germany.

also critically depend on the quality of throughfall data because they often attempt to detect comparatively minor, yet important, differences in rainfall partitioning among forests.

Given these quality requirements, one could assume that throughfall sampling strategies are constantly optimized. A literature survey (Supplementary material S1), however, reveals that sampling efforts changed little throughout the past four decades, and applied sample sizes are constantly small (e.g. the median sample size of applied funnel-type collectors was 20 in the years 1970–1999 and did not change thereafter; Supplementary material S1). Hence, evidence seems to be ignored that many sampling schemes may not be well adapted to the heterogeneity of the studied forest (Holwerda et al., 2006; Kimmins, 1973; Zimmermann et al., 2010). Apart from logistical and financial constraints, the mismatch between theoretically required and practically applied sampling routines is due to the scarcity of general guidelines that would help adapting throughfall sampling schemes to the forest system under study. Although a variety of studies calculated sample sizes to reach pre-specified error limits (Kimmins, 1973; Rodrigo and Àvila, 2001; Price and Carlyle-Moses, 2003; Puckett, 1991; Seiler and Matzner, 1995), compared the performance of different collector types (Cuartas et al., 2007; Kostelnik et al., 1989), or assessed the results of roving versus fixed sampling schemes (Holwerda et al., 2006; Ritter and Regalado, 2010; Ziegler et al., 2009), it is difficult to derive general guidelines from these studies as most of their findings are only valid for the tested collector types and sampling designs (Holwerda et al., 2006). In practice, however, one has to decide on the entire sampling scheme including collector type (trough versus funnel), sampling design (simple random sampling versus other random sampling designs), and sample size. Interestingly, the German Association for Water, Wastewater and Waste (DWA, formerly DVWK) published guidelines for interception monitoring (DVWK, 1986), and similar manuals are available in other countries as well (e.g. Clarke et al., 2010; McJannet and Wallace, 2006; Žlindra et al., 2011). Yet, most of these technical reports focus on practical issues, such as optimizing the construction of throughfall collectors, but do not consider aspects of sampling theory.

A possible way forward is to conduct virtual sampling experiments based on stochastic simulations of realistic throughfall fields (Zimmermann et al., 2010). These experiments, which have been already used in other disciplines such as soil science (Papritz and Webster, 1995), permit to derive more generally applicable findings. In a previous study, Zimmermann et al. (2010) tested various throughfall sampling designs (e.g. simple random sampling versus cluster random sampling) and sample supports (e.g. trough versus funnel-type collectors) in respect of their suitability to estimate mean throughfall in an old-growth tropical forest and derived two main findings. First, sampling designs that avoid a strong clustering of sampling locations, such as simple random sampling or stratified simple random sampling are to be preferred, particularly in the presence of pronounced spatial dependence. Second, troughs are usually more efficient than funnel-type samplers. However, Zimmermann et al. (2010) suggested that the efficiency of trough systems may deteriorate in the presence of pronounced spatial structures in throughfall, but their data base (one forest type and 14 rainfall events) was too small for a complete picture. This work builds upon the findings of Zimmermann et al. (2010).

2. Objectives and outline of the article

The main objective of this study is to test the performance of sampling schemes for the estimation of mean throughfall. More precisely, we address the following research questions. (1) How do

event size and temporal aggregation of throughfall data influence the accuracy of mean throughfall estimates? (2) Given a certain aggregation level, is there an influence of canopy complexity on throughfall variability, which involves the need to adapt sampling strategies to the ecosystem under study? (3) How do temporal aggregation and canopy complexity influence the characteristics of throughfall spatial correlations and hence, the efficiency of trough-type sampling systems? The tested sampling schemes involve a variety of funnel-type and trough-type collectors and a wide range of sample sizes. Our calculations are based on the virtual sampling of simulated throughfall fields which we generated using real-world data from three forest stands of contrasting canopy complexity. To check the plausibility of the simulation results, we also applied two contrasting sampling approaches at one of the study sites simultaneously.

The article is arranged as follows. First, we briefly describe the study sites and the sampling of the real-world throughfall data. Second, we explain the generation of simulated throughfall fields and the virtual sampling experiments. Third, we characterize the throughfall data which is used for the virtual sampling. Fourth, we describe the performance of the tested sampling schemes. Fifth, we check the plausibility of the results obtained by the virtual sampling experiments. Finally, we discuss how the findings of this study can provide guidance for future throughfall and interception studies.

3. Methods

3.1. General description of the research area

We measured throughfall in three square 1-ha plots which span a gradient of forest diversity and complexity. Additionally, we measured rainfall in a distance of around 50 m to the throughfall plots. All sites are located in the central part of the Panama Canal Watershed (Fig. 1). The natural vegetation of the area is classified as semideciduous lowland forest (Foster and Brokaw, 1996). At present, the central part of the Panama Canal Watershed comprises several land use types including mature and secondary forest, cattle pastures and other farmland (Fig. 1). Protected old-growth forests cover large areas along the banks of the canal as well as the islands in the Lake Gatun. The climate of central Panama is tropical with distinct wet and dry seasons. The wet season lasts approximately from May to December. According to long-term records from Barro Colorado Island (Fig. 1), annual rainfall averages 2641 ± 485 mm (mean ± 1 standard deviation, $n = 82$, data from 1929 to 2010, courtesy of the Environmental Sciences Program, Smithsonian Tropical Research Institute, Republic of Panama). Mean daily temperature, measured at the latter site, varies little throughout the year and averages 27 °C (Dietrich et al., 1996).

3.2. Description of the study plots

Plot 1 (Fig. 1) is a 12-year old teak (*Tectona grandis*) plantation located in flat terrain near the village Las Pavas. The stand structure in this plot is uniform: trees are planted in regular rows of 3 m distance, there is only little understory, and stand height is even and approaches approximately 10 m. The sparse understory in plot 1 consists of a few palms (*Oenocarpus mapora*) and some pioneer trees (*Cecropia insignis*).

Plot 2 (Fig. 1) is a 5-year old secondary forest located on a 20° slope in the Agua Salud Project area (Hassler et al., 2011). Its stand structure is non-uniform with dense vegetation in some parts of the plot and very open spots in others. Tree height in this stand varies between 2 m and 6 m. According to a botanical survey accompanying the throughfall monitoring, plot 2 contains 72 tree species
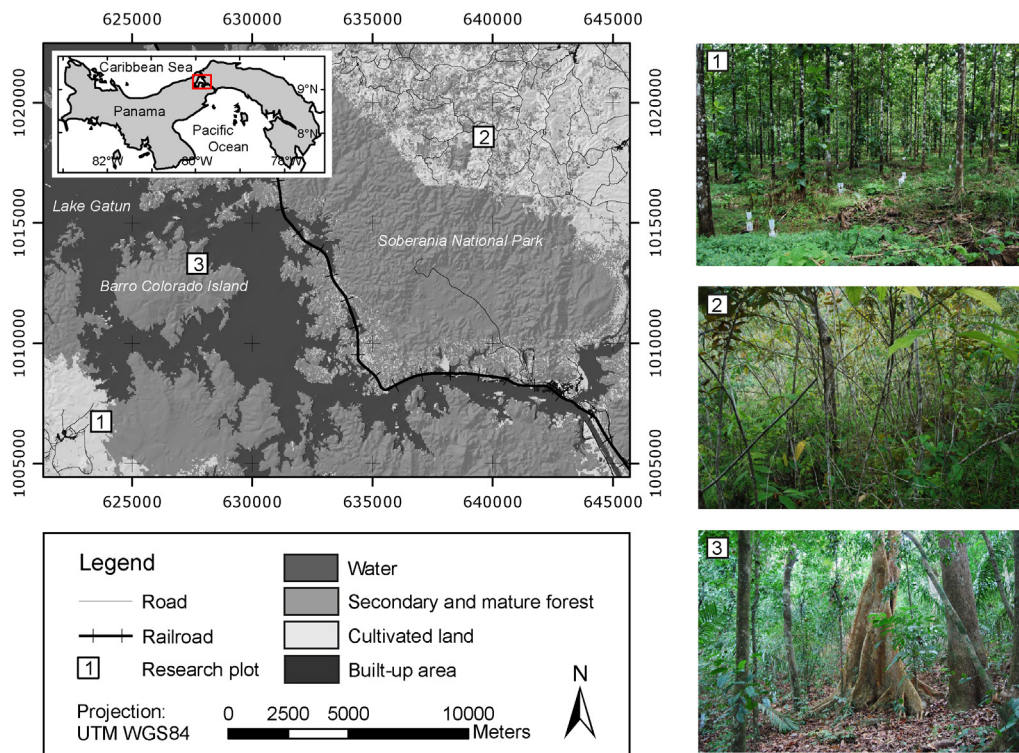
Fig. 1. Map of the study area (left) and impressions of the studied forests (right). The numbered squares refer to the throughfall plots and locate them on the map: plot 1 is the teak plantation, while plot 2 and 3 are covered by young and old secondary forest, respectively. The inlet shows the location of the study area in Panama.

with a diameter at breast height (DBH) >1 cm. Three species dominate the young secondary forest stand: *Conostegia xalapensis*, *Vismia baccifera*, and *Vismia macrophylla* comprise 46%, 19%, and 13% of all trees, respectively.

Plot 3 (Fig. 1) is a more than 130-year old secondary forest (Foster and Brokaw, 1996; Kenoyer, 1929) located on a 10° slope on Barro Colorado Island. Its stand structure is the most heterogeneous of all plots: patches with thick understory alternate with relatively open areas which are dominated by single large trees. Stand height is approximately 25–35 m with few emergents approaching 45 m (Foster and Brokaw, 1996). A botanical survey accompanying the throughfall monitoring revealed that plot 3 contains 89 tree species in the DBH class >5 cm. The most abundant tree species are *Faramea occidentalis*, *Alseis blackiana*, *Macrocnemum roseum*, and *Heisteria concinna* which comprise 12%, 10%, 9%, and 7% of all trees in the 1-ha plot, respectively. For further details on stand structure of the three plots we refer to Table 1.

Table 1
Stand characteristics of the three study sites.

	Units	Plot 1[a]	Plot 2[b]	Plot 3[c]
Age of trees	Year	12	5	~130
Number of species (>1 cm DBH)	Count	–[d]	72	–[e]
Number of species (>5 cm DBH)	Count	3	26	89
Basal area (1 cm < DBH ≤ 5 cm)	m^2/ha	–[d]	9.0	1.6
Basal area (>5 cm DBH)	m^2/ha	13.9	3.9	45.1
Stem density (1 cm < DBH ≤ 5 cm)	Number/ha	–[d]	18,440	3060
Stem density (> 5 cm DBH)	Number/ha	319	325	1013

[a] Teak plantation.
[b] Young secondary forest.
[c] Old secondary forest.
[d] No trees in this DBH size class.
[e] No data available for this DBH size class.

3.3. Sampling and instrumentation

3.3.1. Sample support and sampling frame

In each plot, we used funnel-type throughfall collectors ($n = 350$ per plot), which consisted of a 2 L polyethylene bottle and a funnel. The receiving area of each collector was 113 cm^2. A polyethylene net with a 0.5 mm mesh at the bottom of the funnel minimized measurement errors due to organic material and insects.

We applied a complex sampling frame that comprises a design-based and a model-based sampling component (Fig. 2). The design-based component consists of a stratified simple random sampling design with compact geographical stratification (de Gruijter et al., 2006). We implemented this component by dividing the 1-ha plots into 100 square subplots of edge length 10 m. In these subplots we randomly selected two throughfall sampling points (Fig. 2). Using a stratified instead of a simple random sampling design assures a more even spread of sampling locations, which is advantageous for spatial predictions. Because of the random sampling, design-based inference allows for an unbiased estimate of the spatial mean and an objective assessment of its uncertainty. In contrast, model-based approaches build on purposive sampling and are beneficial if e.g. the distribution of values over the entire plot is of interest, as with spatial prediction. Because our virtual sampling experiments rely on simulated fields and hence, variogram models, we complemented the design-based sampling locations with a model-based sampling component. Our main objective here was to improve the estimation of the variogram near the origin. The model-based component comprised 150 additional sampling points, which were selected as follows: First, we randomly chose 50 of the 200 design-based sampling locations. These locations received an additional sampling point each at a distance of 0.1 m in quasi-random direction (N, S, E, or W). Next, we randomly chose 25 of the 50 already selected points and positioned further four sampling points at distances of 0.5 m, 1 m, 2 m, and 3 m (Fig. 2).
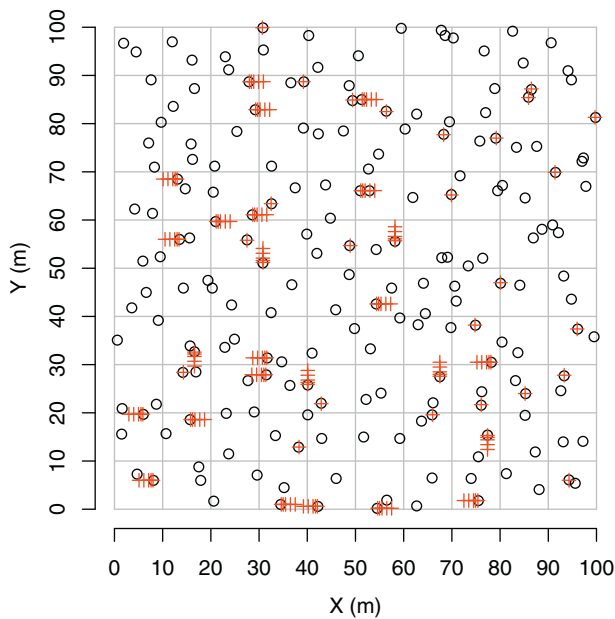
Fig. 2. Graph showing the throughfall sampling scheme. Gray lines delimit the 100 subplots, open circles and red crosses mark locations of throughfall collectors of the design-based and model-based sampling component, respectively. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

3.3.2. Sampling protocol

Throughfall samples were collected on an event basis during a 10 week period from August till October 2011. Thus, no attention could be given to the sub-event scale, which becomes important in case of continuous throughfall measurements with tipping buckets (e.g. Herbst et al., 2008). We applied the following sampling protocol: To qualify for an event, a period of rainfall had to be preceded and followed by at least 2 h without rainfall. Observations of previous studies (Zimmermann et al., 2010) indicated that throughfall completely ceased during these 2 h, which is why we usually started sampling 2 h after the end of rainfall. If rainfall ended after 5 PM, we sampled the next morning. During the study period we sampled all rainfall events. In case rainfall started during sampling, we stopped our measurements and proceeded with sampling 2 h after rainfall ceased.

3.3.3. Sampling approach for the plausibility check of simulation results

In the upper half of plot 2 (the young secondary forest), we continuously measured throughfall with two large trough systems (Fig. 3a and b) over a multi-year period that involves the funnel-sampling campaign. Each trough system drains into its own 3-L tipping bucket. The systems have receiving areas of 12.13 m² (system no. 1) and 12.65 m² (system no. 2) with resulting tip resolutions of 0.247 mm for system no. 1 and 0.237 mm for system no. 2, respectively. We used the data of these systems for a comparison with throughfall volumes obtained with the funnel-type collectors (Fig. 3c) at the design-based locations within that upper half of plot 2 (n = 100).

3.4. Calculations

3.4.1. Rationale for virtual sampling and background information for the modeling of simulated throughfall fields

Following Zimmermann et al. (2010) we assessed the influence of various sample supports and sample sizes on the estimation of mean throughfall by means of a virtual simple random sampling of

simulated throughfall fields. Since we had already shown the satisfactorily performance of simple random sampling (Zimmermann et al., 2010), we did not test other possible probability designs here. Extending our previous work, our evaluations now include three forest types (see Section 3.2) and three temporal aggregation levels of throughfall data (events, weekly, bi-monthly). In order to obtain datasets in weekly and bi-monthly temporal resolution we simply summed up our event-based throughfall measurements. The datasets comprising two months combine all measurements of this study.

The simulated fields are derived from sequential Gaussian simulations, which in turn are based on variogram models fitted to our throughfall data. The virtual sampling of these simulated throughfall fields has several advantages compared to pure empirical approaches (e.g. Holwerda et al., 2006; Ritter and Regalado, 2010; Shinohara et al., 2010; Ziegler et al., 2009). First, we can test various sample supports by aggregating values of the simulated field. Second, we can compare estimates of mean throughfall against the notional population mean because in a simulated field all values are known. Third, we can repeat each virtual sampling setup an unlimited number of times, which results in a distribution of sample means whose parameters can be compared against the notional population mean.

Testing various throughfall sampling setups is straightforward, whereas the construction of throughfall fields, which are the cornerstone of our study, is somewhat more laborious. The main problem is that throughfall data often contain large outliers which originate from drip points (Lloyd and Marques, 1988; Zimmermann et al., 2009). From a statistical point of view, throughfall data can be regarded as the superposition of two processes (Zimmermann et al., 2009): a dominating process that forms the underlying distribution and a contaminating process which is reflected in a few large outlying values. In order to obtain simulated throughfall fields we need to model the underlying distribution and the contaminating process separately because some outlying values cannot be forced to the center of the distribution even after transformation (Zimmermann et al., 2009). In the next sections we describe, step-by-step, the modeling of simulated throughfall fields. In case our calculations match exactly the procedures described previously, we refer to Zimmermann et al. (2010) for computational details.

3.4.2. Step 1: Exploratory data analysis

At first, we plotted the throughfall data against the coordinates to check for non-stationarity of the mean that may be caused by local trends. This check is necessary as the following geostatistical analysis requires second order stationarity. If we detected trends in the data (e.g. caused by non-uniform rainfall patterns) we omitted the dataset from further analysis. Next, we checked the skewness of the underlying distribution of our throughfall data because even robust variogram estimators rest on the assumption of an underlying normal distribution (Lark, 2000). For this purpose, we applied a robust measure of skewness, the octile skew (Brys et al., 2003), $skew_8$. The octile skew is a measure of the asymmetry of the first (O_1) and seventh octile (O_7) of the data about the median

$$skew_8 = \frac{(O_7 - \text{median}) - (\text{median} - O_1)}{(O_7 - O_1)}, \quad (1)$$

and hence, is insensitive to outliers. A rule of thumb (Rawlins et al., 2005) suggests that the data need to be transformed if the $skew_8$ is larger or smaller than 0.2 and −0.2, respectively. We adopted this approach and transformed our data, if necessary, to minimize the underlying skewness.
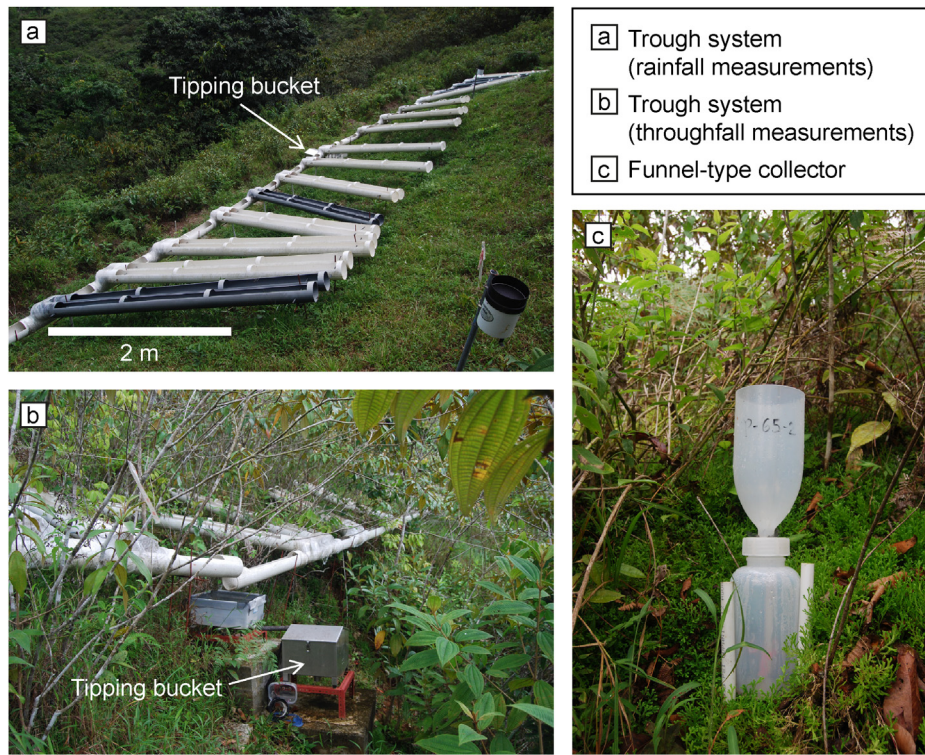
Fig. 3. The photographs show (a) the setup of a 12.07 m^2 trough system used for rainfall measurements, (b) an identical trough system used for throughfall measurements (support: $A = 12.13$ m^2) and (c) one of our funnel-type collectors (support: $A = 113$ cm^2).

3.4.3. Step 2: Deriving variogram parameters using method-of-moment estimates

This step is necessary for the later separation of outliers from the underlying distribution. First, we calculated experimental variograms using the estimator as defined by Matheron (1962):

$$2\hat{\gamma}_M(\mathbf{h}) = \frac{1}{N(\mathbf{h})} \sum_{i=1}^{N(\mathbf{h})} \left\{ z(\mathbf{x}_i) - z(\mathbf{x}_i + \mathbf{h}) \right\}^2, \tag{2}$$

where $z(\mathbf{x}_i)$ is the observed value at location $\mathbf{x}_i$, and $N(\mathbf{h})$ are the pairs of observations that are separated by lag $\mathbf{h}$. Next, we fitted three theoretical models (exponential, spherical, pure nugget) to the experimental variogram, and chose the one with the smallest sum of squared residuals from the fit. Matheron's estimator is the prime choice in the method-of-moment variogram estimation as it is asymptotically unbiased and efficient (Lark, 2000).

Unfortunately, Matheron's estimator is also very sensitive to outlying values. Therefore, we assessed the fitted model by leave-one-out cross-validation; that is, $z(\mathbf{x})$ is left out and estimated by ordinary kriging to yield a prediction $\hat{Z}(\mathbf{x})$ and the kriging variance $\sigma^2_{\mathbf{K},\mathbf{x}}$. To check whether the semivariance is overestimated due to an unduly influence of outlying values on Matheron's estimator we applied the statistic $\theta(\mathbf{x})$ (Lark, 2000):

$$\theta(\mathbf{x}) = \frac{\left\{ z(\mathbf{x}) - \hat{Z}(\mathbf{x}) \right\}^2}{\sigma^2_{\mathbf{K},\mathbf{x}}}. \tag{3}$$

In case of a correct variogram, the mean of $\theta(\mathbf{x})$ will be 1 and the median of $\theta(\mathbf{x})$ will be 0.455 (Lark, 2000). Because the mean of $\theta(\mathbf{x})$ is itself subject to the effects of outlying observations, we follow Lark (2000) and use the median of $\theta(\mathbf{x})$ as a robust diagnostic. A sample median significantly less than 0.455 suggests that kriging overestimates the variance, whereas one which is greater than 0.455 underestimates the variance. In order to decide when the median of $\theta(\mathbf{x})$ is significantly less or greater than 0.455 we

computed confidence limits of $\theta(\mathbf{x})$ as proposed by Lark (2000). In case $\theta(\mathbf{x})$ was outside of these confidence limits we calculated the experimental variogram applying the less efficient (Lark, 2000) but robust estimator due to Dowd (1984):

$$2\hat{\gamma}_D(\mathbf{h}) = 2.198 \left\{ \text{median} \left(\left| z(\mathbf{x}_i) - z(\mathbf{x}_i + \mathbf{h}) \right| \right) \right\}^2 \quad i = 1, 2, \ldots, N(\mathbf{h}), \tag{4}$$

where $z(\mathbf{x}_i)$ is the observed value at location $\mathbf{x}_i$, and $N(\mathbf{h})$ are the pairs of observations that are separated by lag $\mathbf{h}$. Next, we fitted the theoretical variogram as outlined above. Then we applied the statistic $\theta(\mathbf{x})$ again and checked whether $\theta(\mathbf{x})$ was within the confidence limits. The thus obtained covariance parameters (nugget, sill, range) provided the basis for the next steps of our analysis.

3.4.4. Step 3: Estimating variogram parameters by residual maximum likelihood (REML)

The estimation of variogram parameters by residual maximum likelihood (REML) is preferred to the methods-of-moments estimation of the variogram (cf. Section 3.4.3) because REML is more efficient and the fitted model is independent of arbitrary decisions such as the definition of lag bins (Lark et al., 2006; Zimmermann et al., 2008). Before we applied REML to model the underlying normal distribution, we had to remove the outliers that originate from the contaminating process. To identify outliers at a particular location $\mathbf{x}_o$ we calculated the standardized error of cross-validation (Bárdossy and Kundzewicz, 1990), $\varepsilon_s(\mathbf{x})$, with the selected variogram obtained in step 2. The standardized error of cross-validation is defined as follows:

$$\varepsilon_s(\mathbf{x}) = \frac{\hat{Z}(\mathbf{x}_o) - z(\mathbf{x}_o)}{\sigma_{\mathbf{K},o}}. \tag{5}$$

If we have a particular large value at location $\mathbf{x}_o$ (e.g. due to a drip point) then the variance of the deviation $\left\{ \hat{Z}(\mathbf{x}_o) - z(\mathbf{x}_o) \right\}$ is likely to be underestimated by the kriging variance $\sigma^2_{\mathbf{K},o}$, which increases the

absolute value of ε_s (**x**). Bárdossy and Kundzewicz (1990) proposed the standardized error to be used to classify a datum as an outlier if ε_s (**x**) is smaller than -1.96 for a 95% confidence level; in this study, we set the confidence level to 99%. Once identified, we removed the outlying values. Next, we checked the bivariate distribution of these adjusted datasets using **h**-scattergrams (Goovaerts, 1997; Webster and Oliver, 2007). These plots show all pairs of measurements that are separated by a pre-specified distance and should be produced for a number of distance classes. In case the **h**-scattergrams displayed no outlying values we estimated the variogram parameters using REML. All remaining datasets were discarded from further analysis.

3.4.5. Step 4: Analyzing spatial pattern of outliers and modeling the outlier distribution

The procedure described so far provides us with a variogram of the underlying multivariate Gaussian random process. Because large outlying values are an important characteristic of throughfall data we wish to include these data in our simulations. To achieve this aim we require a distribution of the outliers whose values we can implement in a simulation. In addition, we have to assure that implementation at random locations within the field is justified. For the latter, we followed the procedure described by Zimmermann et al. (2010) and checked the assumption that outliers are distributed independently and at random among our sampling points, i.e. we tested whether they represent a completely spatially random process (Cressie, 1993). If we detected clustering or overdispersion of outliers we omitted the dataset from further analysis. Next, we obtained the distribution of the outliers by pooling spatial outliers from all datasets of a study site in order to obtain a sufficient sample size. All outliers were standardized by first subtracting their mean value and then dividing by their standard deviation for any given event. In case the histogram of the pooled standardized outliers did not match a normal distribution we adopted the procedure described by Zimmermann et al. (2010) and estimated an empirical probability density function (pdf) by kernel density estimation (Silverman, 1978, 1986). In case the distribution of the standardized outliers appeared approximately normal, we sampled from a standard normal distribution.

3.4.6. Step 5: Simulation of throughfall fields

We obtained throughfall fields by unconditional simulations of a Gaussian random variable applying the function GaussRF, which is implemented in the R-package RandomFields (Schlather, 2001). For our simulations we used the mean value and covariance parameters which were estimated using residual maximum likelihood (see Section 3.4.4). The generated fields have the dimension of a square grid of interval 10 cm and edge-length of 100 m; that is, the simulated field consists of 10^6 nodes. We assume that the basic grid cell with an area of 100 cm^2 corresponds to the support of our original measurements, which is a reasonable approximation as our funnels have a receiving area of 113 cm^2.

In order to obtain realistic simulated throughfall fields we contaminated the simulations by randomly replacing a fraction of grid cells, which equaled the fraction of outliers in the original dataset, with values sampled from the distribution of outliers. Prior to installation, the standardized outlier value was rescaled to an outlying value for the dataset of interest by multiplying it by the outlier standard deviation for that dataset, and by adding the corresponding outlier average value.

For datasets where the original throughfall data had been transformed, the simulated values were back transformed to the original scale. The mean of a particular realization of the random process was computed from all 10^6 nodes. This is our notional population mean that we wish to estimate in the virtual sampling.
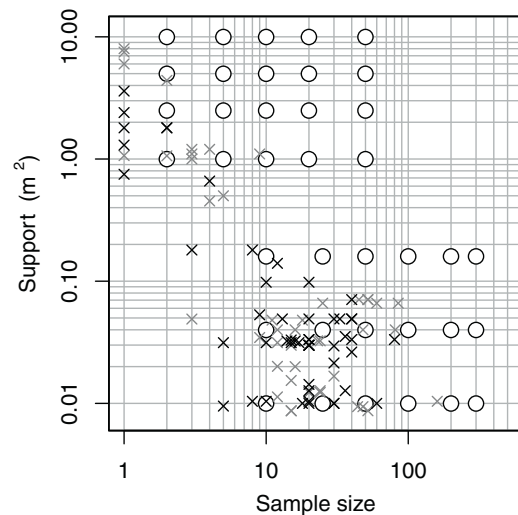

Fig. 4. Overview of sample supports and sample sizes tested in our simulations (open circles) and used in previous throughfall and interception studies during the period 1970–2012, respectively (crosses). Black crosses refer to studies in tropical areas, whereas gray crosses refer to studies conducted outside the tropical zone. The crosses depict sampling setups of selected empirical interception studies (Asdak et al., 1998; Blume et al., 2008; Brauman et al., 2010; Cao et al., 2008; Carlyle-Moses et al., 2004; Dietz et al., 2006; Elsenbeer et al., 1994; Häger and Dohrenbusch, 2011; Huber and Iroumé, 2001; Jackson, 1971; Johnson, 1990; Krämer and Hölscher, 2009; Macinnis-Ng et al., 2012; Manfroi et al., 2006; Marin et al., 2000; McJannet et al., 2007; Oyarzún et al., 2011; Ponette-González et al., 2010; Schrumpf et al., 2011; Veneklaas and Van Ek, 1990; Viville et al., 1993), interception modeling studies (Bruijnzeel and Wiersum, 1987; Carlyle-Moses et al., 2010; Cuartas et al., 2007; Deguchi et al., 2006; Dykes, 1997; Fleischbein et al., 2005; Gash, 1979; Gash and Stewart, 1977; Gash et al., 1980; Germer et al., 2006; Gerrits et al., 2010; Herbst et al., 2008; Hölscher et al., 2004; Holwerda et al., 2010, 2012; Jetten, 1996; Lankreijer et al., 1993, 1999; Limousin et al., 2008; Link et al., 2004; Lloyd et al., 1988; Loustau et al., 1992; Murakami, 2007; Niedzialek and Ogden, 2012; Price and Carlyle-Moses, 2003; Pryet et al., 2012; Pypker et al., 2005; Schellekens et al., 1999; Takahashi et al., 2011; Vernimmen et al., 2007; Vrugt et al., 2003; Wallace and McJannet, 2008; Zhang et al., 2006), and studies which investigated solute fluxes (Chuyong et al., 2004; Clark et al., 1998; Crockford et al., 1996; Currie et al., 1996; Dezzeo and Chacón, 2006; Filoso et al., 1999; Hofhansl et al., 2011; Liu et al., 2003; McDowell, 1998; Moreno et al., 2001; Olson et al., 1981; Tietema et al., 1993; Uyttendaele and Iroumé, 2002). For more information on the sampling schemes of these studies we refer to Supplementary material S1.

3.4.7. Step 6: Testing throughfall sampling schemes

To obtain simulated values which correspond to certain supports, we first back converted the simulated throughfall values from standard rainfall units (mm) to volumetric, support-dependent data (mL). Next, we summed up neighboring grid cells to achieve the desired increase of support. In total, we tested the performance of 7 different supports. Three of the tested supports are in the range of typical funnel-type sampling devices (Fig. 4, Supplementary material S1): they have a square shape with areas of 0.01 m^2, 0.04 m^2, and 0.16 m^2. The remaining 4 supports cover the range of usual trough-type collectors (Fig. 4, Supplementary material S1). The latter supports have a rectangular shape with a width of 0.5 m and lengths of 2 m, 5 m, 10 m, and 20 m (1.0 m^2, 2.5 m^2, 5 m^2, and 10 m^2, respectively).

For the three funnel-type supports we tested sample sizes of 10, 25, 50, 100, 200, and 300. A sample size of 10 to 50 funnel-type collectors seems to be typical for many studies (Fig. 4, Supplementary material S1); only a few throughfall and interception studies had sampling schemes which involved a larger number of collectors (Gerrits et al., 2010; Manfroi et al., 2006; Price and Carlyle-Moses, 2003; Zhang et al., 2006). For trough-type collectors we tested sample sizes of 2, 5, 10, 20, and 50. Previous interception studies typically used 1 to 12 troughs (Fig. 4, Supplementary material S1). We did not test the performance of one trough because a sample

size of one does not allow for the calculation of the uncertainty of the mean estimate and hence, cannot be recommended.

We assessed the outcome of each virtual sampling experiment using the relative error of the mean as a quality criterion, which is defined as follows:

$$\left| \frac{(\hat{\bar{z}} - \bar{z})}{\bar{z}} \right|, \tag{6}$$

where $\hat{\bar{z}}$ is an estimated sample mean and $\bar{z}$ is the true mean, i.e. the notional population mean of the tested throughfall field. For each combination of support and sample size, we repeated the sampling of the simulated throughfall fields 10,000 times. This procedure resulted in the desired distribution of sample means (cf. Section 3.4.1); the broader this distribution is the larger is the uncertainty in estimating mean throughfall for the given setup. In the range between the 2.5% and 97.5% percentile of the sample mean distribution we find 95% of all sample means. We calculated the relative errors of the mean associated with each of those percentiles and kept the larger of them for the comparison of sampling procedures. To obtain robust assessments, we eventually used average relative errors for a given event category and sampling setup.

3.4.8. Plausibility check of simulation results

Because our approach so far is purely statistical, it is useful to evaluate it by means of real-world experiments. The trough systems described above (cf. Section 3.3.3) serve for the purpose of a plausibility check. Here we proceeded as follows: For events that we recorded both with the 100 funnel-type collectors and with the two trough systems, we first evaluated the consistency of the trough and funnel data by calculating the difference between the sample means, which we normalized with the respective event's rain amount. Next, we computed the relative standard errors for each sample mean, that is, we divided each standard error by the sample mean. We then compared these relative standard errors with the outcome of the simulations to test the hypothesis that the difference in relative error limits is reflected in a respective difference in standard errors: If, for instance, the geostatistical simulations suggested a larger relative error attached to the employment of 100 small funnels than to the use of 2 troughs of >10 m² support (the largest simulated trough size), the standard errors resulting from the sampling with funnels should also be larger.

4. Results

4.1. Characteristics of the throughfall data

During the study period we sampled 39 events in the teak plantation (plot 1), 35 events in the young secondary forest (plot 2), and 36 events in the old secondary forest (plot 3). Our check for trends (Section 3.4.2), a bivariate normal distribution (Section 3.4.4), and the spatially random distribution of outliers (Section 3.4.5) reduced the original data to 16, 31, and 25 events of plot 1, 2 and 3, respectively. Particularly the analysis of datasets from plot 1 was complicated due to the presence of spatial trends and deviations from a bivariate normal distribution. Processing the weekly data resulted in 6, 8, and 9 datasets for our simulations of plot 1, 2 and 3, respectively. As for the event-based data, spatial trends and deviations from the bivariate normal distribution explain the lower number of datasets at site 1. The bulk datasets appeared to be less problematic to process which is why we could obtain one simulation for each site. For further information on the throughfall data (e.g. throughfall depths) we refer to Supplementary material S2.

In the following we describe the throughfall data used for simulations by examining four measures. First, we present the coefficient of variation (CV) of our data which we calculated using the design-based data only ($n = 200$, cf. Section 3.3.1). The CV is a common measure to characterize and compare throughfall spatial variation (e.g. Holwerda et al., 2006; Kimmins, 1973; Staelens et al., 2006; Zimmermann et al., 2007). Second, we present the octile skew, skew$_8$, to provide an impression of the underlying skewness of our throughfall data (cf. Section 3.4.2). Third, we show data on the nugget-to-sill-ratio, which describes the strength of the spatial correlation. Fourth, we provide the effective range which is the length scale over which measurements correlate. We calculated the latter three measures (skew$_8$, nugget-to-sill-ratio, effective range) based on the full datasets ($n = 350$, cf. Section 3.3.1).

The CVs of our throughfall data show two trends (Fig. 5a, Supplementary material S2). First, CVs at all sites decrease with increasing throughfall depth. For instance, throughfall data of small events (<5 mm) in the old secondary forest are associated with an average CV of 90% (Fig. 5a, upper panel), whereas throughfall data of the same site that comprise weekly or bi-monthly periods show much lower CVs (Fig. 5a, lower two panels). Second, CVs of throughfall data show an increase with increasing forest complexity (Fig. 5a). That is to say, the throughfall data from the rather simply structured forests (plot 1 and 2) contain less variation than the data from the old secondary forest (plot 3). Moreover, datasets from the teak plantation (plot 1), which is the least complex forest of our study sites, often feature the lowest CVs.

The octile skew, skew$_8$, shows a characteristic decrease with increasing throughfall depth (Fig. 5b, Supplementary material S2). Particularly, throughfall data from plot 3 show a pronounced drop of the skew$_8$ with increasing throughfall. For the other plots, this decrease is less pronounced. The decrease of the underlying skew with increasing throughfall depths reflects a shift of the underlying distribution's shape: small events are distinctly skewed to the right, whereas large events, or bulk data, show an underlying distribution that matches a Gaussian shape.

Considering the nugget-to-sill-ratio (Fig. 5c, Supplementary material S2) and the effective range (Fig. 5d, Supplementary material S2) we can state that the spatial structure of throughfall clearly differs among the three study sites, though differences diminish with increasing throughfall depth. Throughfall in the teak plantation (plot 1) consistently shows a strong but short-ranging spatial dependence. In contrast, throughfall patterns in the young secondary forest (plot 2) are weak except for the bulk data. In this plot, throughfall shows strongly varying ranges for small events and displays small ranges for large events and accumulated data. In the old secondary forest (plot 3), throughfall patterns are strong and ranges vary depending on event size. Small events in plot 3 show comparatively long-ranging structures, whereas large events and accumulated data at this site display smaller effective ranges. As a result of decreasing effective ranges with increasing throughfall depth in plot 2 and 3, and due to the decrease of the nugget-to-sill-ratio with an increase in throughfall in plot 2, bulk data at all three sites are characterized by a strong (i.e. low-nugget-to sill-ratio) but short-ranging (<2.5 m) spatial dependence.

4.2. Test of throughfall sampling procedures

Based on our tests of various throughfall sampling procedures we derive three main findings. First, regardless of the forest type, relative errors of mean throughfall estimates vary as a function of throughfall depth (Figs. 6 and 7). For small events, relative errors can increase to more than 40% (and might even be larger at the sub-event scale), whereas for large events and accumulated data relative errors are lower than 15% for most of the tested sample sizes and supports. The decrease of the relative error with increasing event size and aggregation level, respectively, results from a decrease in throughfall variability with increasing throughfall depth. Hence, in situations where throughfall variability declines
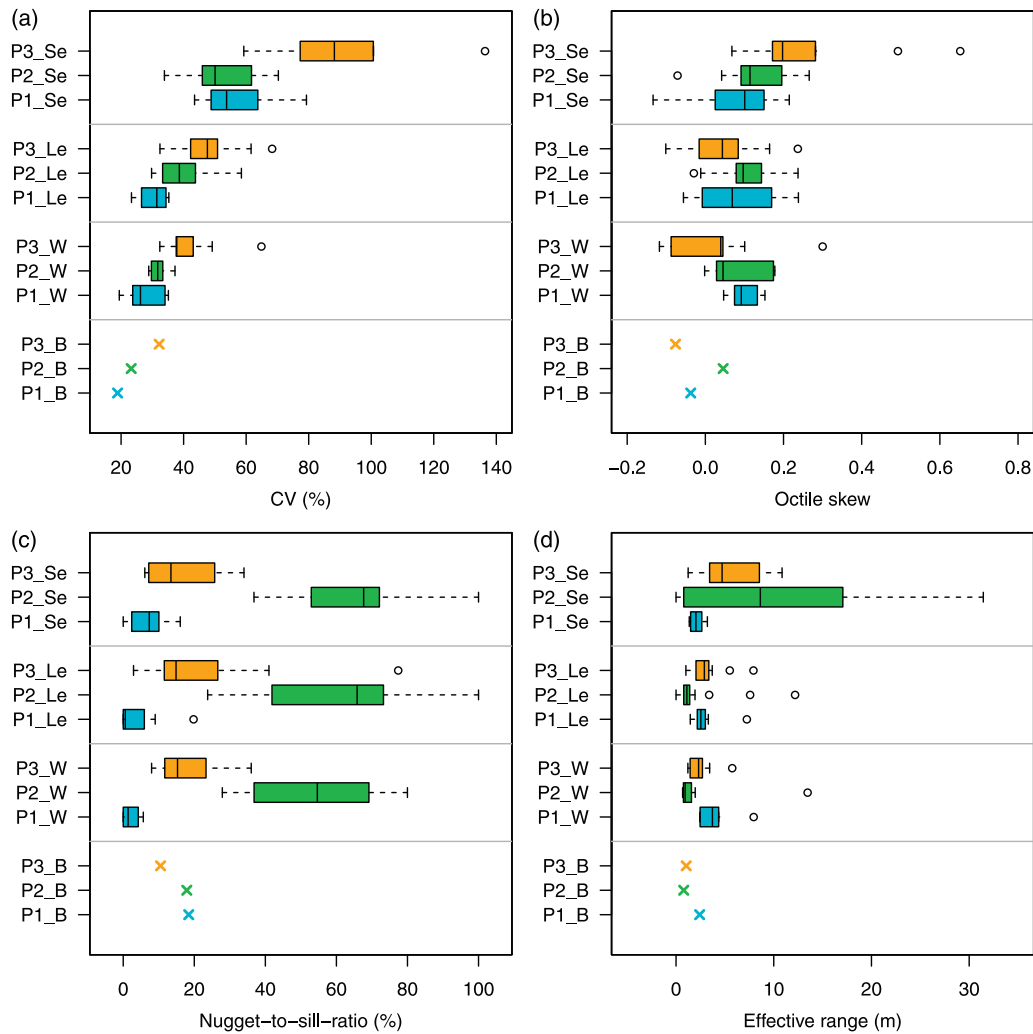
Fig. 5. Boxplot comparisons of the (a) coefficient of variation (CV), (b) octile skew, (c) nugget-to-sill-ratio, and (d) effective range of throughfall data from plot 1 (P1), plot 2 (P2), and plot 3 (P3) for small events (Se, <5 mm), large events (Le, ≥5 mm), weekly data (W), and bulk data (B, accumulated throughfall of the entire study period).

strongly (cf. CV's of small and large events of plot 3, Fig. 5a) relative errors show a pronounced drop too (cf. Fig. 6e and f).

Second, given a certain temporal scale, the uncertainty of throughfall estimates can vary considerably depending on canopy complexity. The varying uncertainty reflects the differences in spatial variability of throughfall among forests (cf. Section 4.1). Canopy complexity influences relative errors particularly for small events. For instance, sampling those events in weakly structured forests (plots 1 and 2) with 100 medium-sized funnels (support of 0.04 m²) ensures relative errors limits of mean throughfall estimates of 10% (Fig. 6a and c). In contrast, double the amount of this collector type is required for the same error limit in heterogeneous forests such as our old secondary forest plot (Fig. 6e). If focus is on monitoring of large throughfall depths only, the influence of forest type on relative errors of mean throughfall estimates decreases (6b, d and f and 7).

Third, the efficiency of trough- versus funnel-type samplers depends on the spatial pattern of throughfall. The presence of strong and relatively long-ranging spatial structures in throughfall, which we detected for small events in the heterogeneous forest (Fig. 5), eliminates the advantage of the large supports of trough-type collectors (Fig. 8). For instance, two large trough systems with a receiving area of 10 m² per trough can be associated with larger errors of mean throughfall estimates than 25 funnel-type collectors with a receiving area of 0.04 m² per collector (6e and 8a). In other

words, two extremely large (and costly) throughfall systems are less efficient than 25 relatively small (and cheap) funnel-type samplers in the presence of pronounced spatial patterns. In contrast, in situations where throughfall patterns are weak or short-ranging, the employment of a moderate number of troughs ensures comparatively low relative errors of mean throughfall estimates (Figs. 6–8). Yet, even in the absence of distinct throughfall patterns, the use of standard trough-type sampling setups (e.g. two troughs, 2.5 m² each) may still involve substantial errors, particularly for small events (Fig. 6a and c).

4.3. Results of the plausibility check

Sampling throughfall with funnel-type collectors during the 10 week main study period (cf. Section 3.3.2) resulted in 11 single events (e.g. daily sample volumes that were generated by a single rainfall event) with matching trough recordings. These 11 events comprised rainfall volumes between 0.6 mm and 52 mm. The estimated throughfall means were very similar for both sampling approaches during all but the very small events (Fig. 9a). At two events with <1 mm of rainfall, the difference between the means calculated from the funnel- and trough data was larger than 10% of the rainfall (Fig. 9a).

Our simulations for plot 2 imply larger relative errors on the event scale for the 100 small funnels than for two large troughs
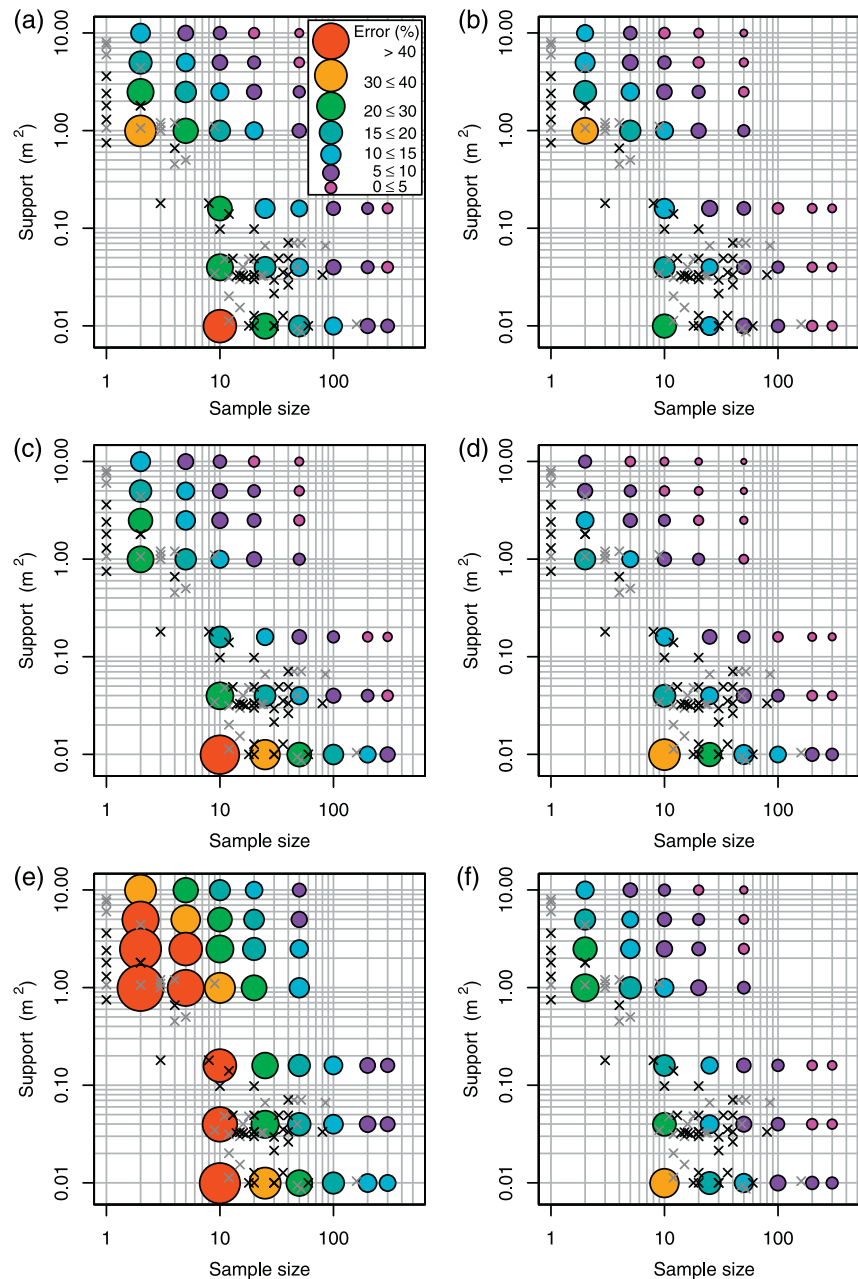
Fig. 6. Performance of throughfall sampling schemes; the graphs show average relative errors of mean throughfall estimates for small events (<5 mm) (a, c and e) and large events (≥5 mm) (b, d and f) from plot 1 (a and b), plot 2 (c and d), and plot 3 (e and f). *Note*: Size and color of the circles correspond to the average relative error for a given event category and sampling setup (see legend in (a)). Crosses refer to sampling setups of previous studies, for details see Fig. 4 and Supplementary material S1.

(Fig. 6c and d). That is, for events <5 mm, errors are predicted to fall between 10% and 15% for the troughs and between 15% and 20% for the funnels; for events of 5 and larger, errors should reduce to 5 ≤ 10% and 10 ≤ 15%, respectively. These trends are generally reflected in the real-world data: In most of the cases, the two troughs are associated with smaller relative standard errors than are the 100 funnels, and the errors are largest for the three small events (Fig. 9b). However, the two small events with the pronounced difference between trough and funnel means deviate from the hypothesized pattern as they involve unexpectedly large relative standard errors for the trough systems (Fig. 9b). This is most likely due to insufficient tip resolutions (0.237 mm and 0.247 mm, cf. Section 3.3.3); that is, during small events a one-tip difference between the two systems may result in large differences between recorded throughfall amounts.

5. Discussion

5.1. Implementing throughfall sampling schemes: General remarks

We wish to use the findings of this study to provide guidance for sampling schemes of future throughfall and interception studies. Given a chosen temporal scale of sampling, the application of our findings requires prior knowledge of the spatial variability and structure of throughfall in the study area of interest. Usually, such detailed information is unavailable which is why some studies propose to determine the required sample size based on a pilot study (Clarke et al., 2010; Kimmins, 1973; Thimonier, 1998). This approach, however, has two drawbacks. First, pilot studies usually work with a small sample size; hence, sample means and variances
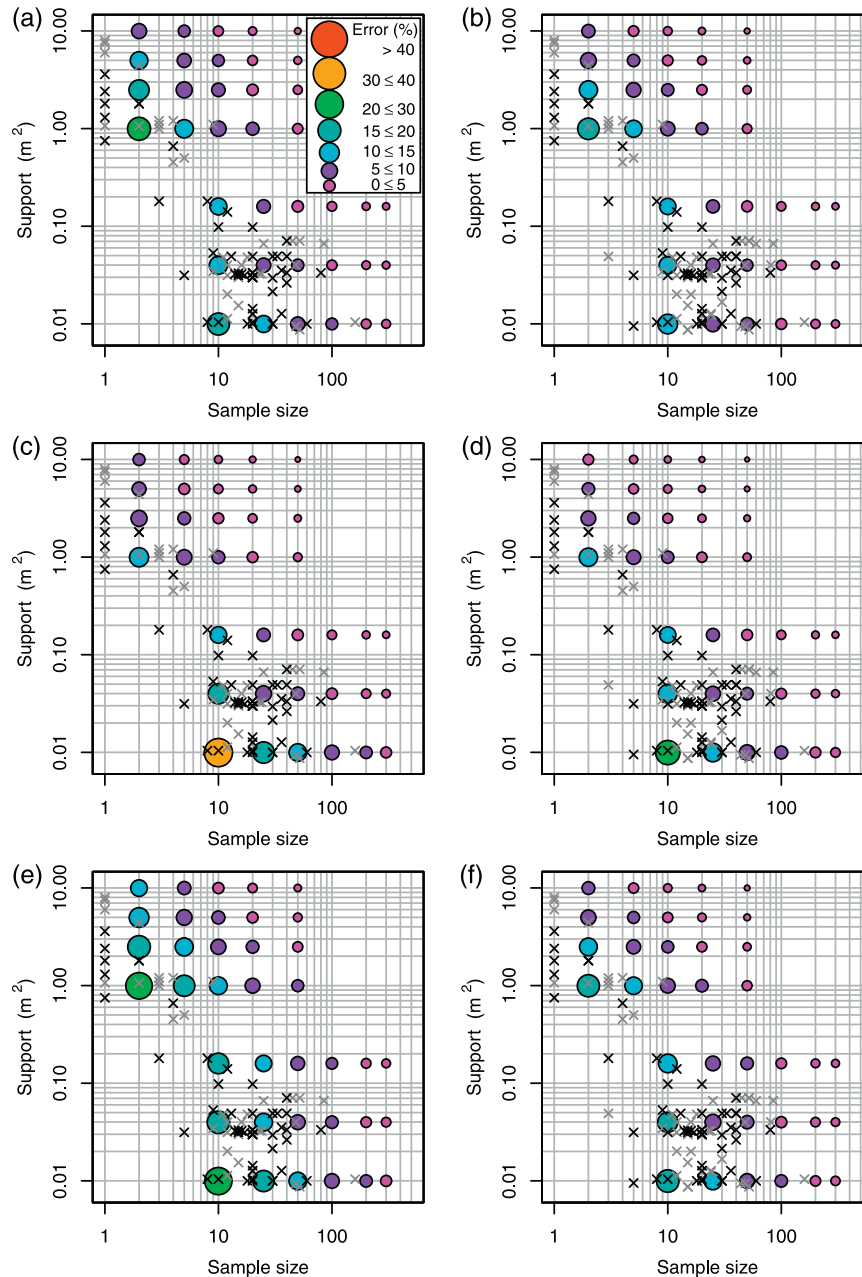
Fig. 7. Performance of throughfall sampling schemes; the graphs show average relative errors of mean throughfall estimates for weekly data ((a), (c) and (e)) and relative errors of bulk data ((b), (d) and (f)) from plot 1 ((a) and b), plot 2 ((c) and (d)), and plot 3 ((e) and (f)). *Note*: Size and color of the circles correspond to the average relative error (weekly data) or the relative error (bulk data) for a given temporal aggregation of throughfall data and sampling setup (see legend in (a)). Crosses refer to sampling setups of previous studies, for details see Fig. 4 and Supplementary material S1.

vary depending on the presence of outlying values in the pilot data (cf. Fig. 6 for the uncertainty of mean estimates applying small sample sizes). This uncertainty propagates into the estimate of the coefficient of variation, which is needed to calculate the sample size of the main study (e.g. p. 84 in de Gruijter et al., 2006; Eq. (1) in Kimmins, 1973 and Thimonier, 1998). Needless to say, the latter issue is particularly problematic in heterogeneous forests which are associated with a large variation in throughfall (Levia and Frost, 2006). Second, pilot studies usually employ one type of collector, i.e. one support. Consequently, all decisions regarding the sample size of the main study are specific to this support (cf. Fig. 6 for the distinct performance of individual supports). Therefore, instead of solely relying on a pilot study we envision using information on the principal mechanisms that determine throughfall spatial variation

(cf. Sections 5.2 and 5.3) for the implementation of an appropriate throughfall sampling scheme in the study area of interest.

In the next two sections, we will follow this approach considering the temporal scale of sampling and the ecosystem under study. Designing a complete throughfall sampling scheme, however, also includes the choice of acceptable error limits. These limits, in turn, strongly depend on the research objective. In some cases, their definition is relatively straightforward. In comparative studies, for instance, errors should be small enough to be able to detect relevant differences. The required sample sizes can then be calculated using the standard formula (e.g. p. 84 in de Gruijter et al., 2006; Eq. (1) in Kimmins, 1973 and Thimonier, 1998) if reliable prior knowledge on the support-specific spatial variability of throughfall exists (future studies in tropical regions may derive sample sizes
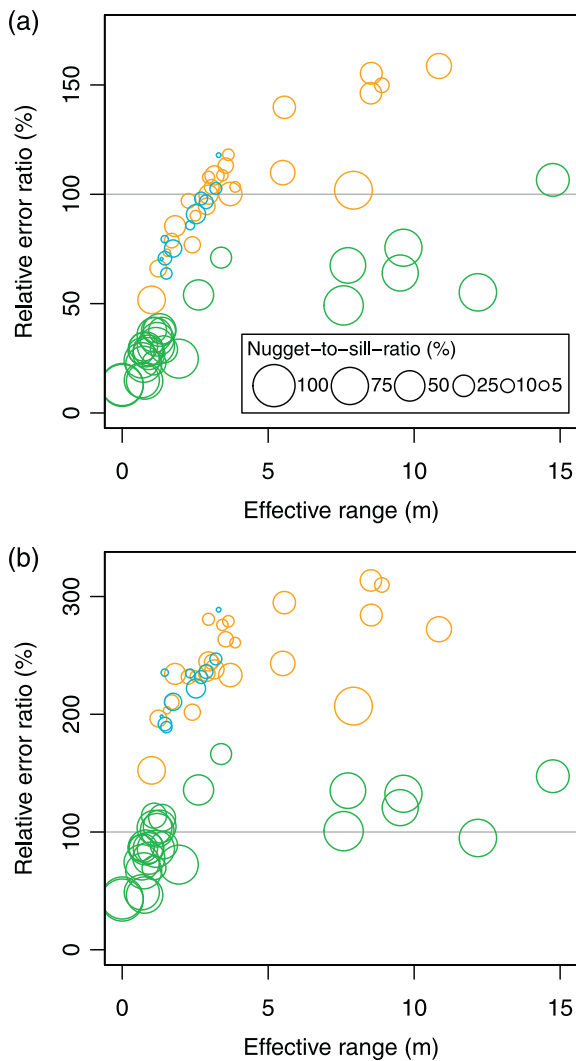
Fig. 8. Ratio of relative errors of mean throughfall estimates for (a) large trough-type collectors (sample size: $n = 2$, support: $A = 10\,m^2$) versus funnel-type collectors (sample size: $n = 25$, support: $A = 0.04\,m^2$) and (b) small trough-type collectors (sample size: $n = 2$, support: $A = 1\,m^2$) versus funnel-type collectors (sample size: $n = 25$, support: $A = 0.04\,m^2$). The plot shows event-based relative error ratios ($n = 25$ events) collected at site 1 (blue circles), site 2 (green circles), and site 3 (orange circles). The size of the plot symbols varies as a function of the nugget-to-sill-ratio (see legend in (a)). A gray line marks a relative error ratio of 100%. Above this limit, funnel-type collectors are more efficient, i.e. they show a lower relative error of mean throughfall estimates than the trough system used in the comparison. *Note*: for illustration purposes 3 events of site 2 with effective ranges >15 m are not shown. All of these events showed nugget-to-sill-ratios >50% and relative error ratios ≤100%. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

for various collector types and error limits from Figs. 6 and 7, see below). Regarding interception modeling, more research is needed to define error margins for throughfall data.

5.2. Implementing throughfall sampling schemes: Influence of the temporal scale of sampling

When planning a throughfall sampling scheme, the first decision should concern the temporal scale of sampling (Fig. 10). The derivation of rainfall interception model parameters from throughfall data, for instance, requires the monitoring of small events (e.g. Gash and Morton, 1978; Jackson, 1975; Lloyd et al., 1988; Rowe, 1983) or a continuous recording by automatic measurements (cf. examples for automatic systems in Supplementary material S1). For other
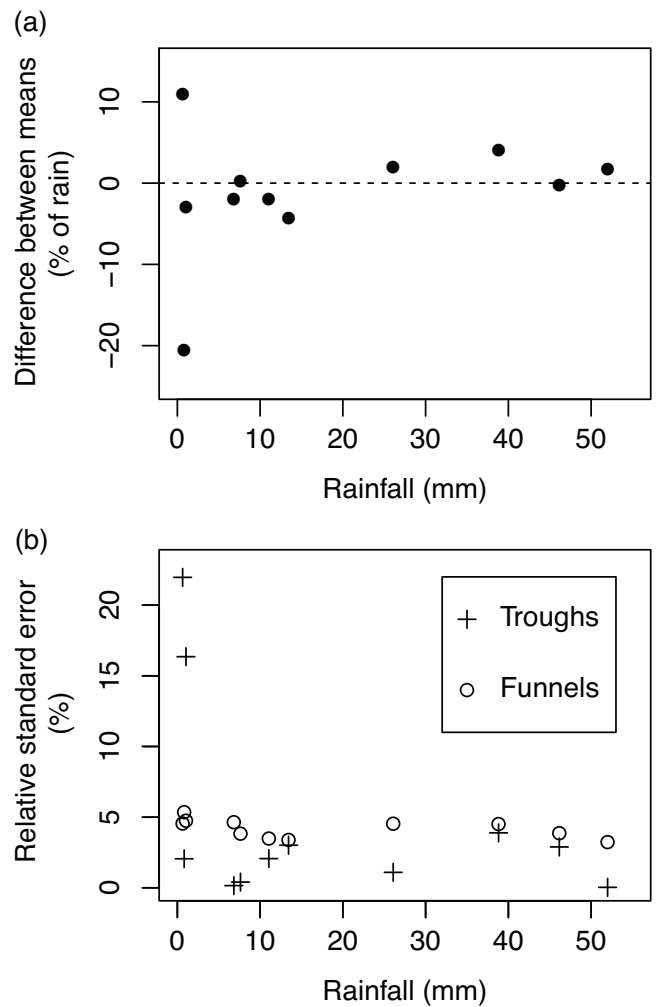

Fig. 9. Comparison between funnels and troughs in the field experiment: (a) difference between sample means of throughfall (mean funnels – mean troughs) scaled to the respective event's rainfall amount and (b) relative standard errors of the mean.

applications such as comparative estimates of interception, data on a weekly or even larger temporal scale often suffice (e.g. Dietz et al., 2006; Macinnis-Ng et al., 2012; Oyarzún et al., 2011; Ponette-González et al., 2010). In general, the temporal scale matters when choosing a sample support and sample size for two reasons. First, due to the decreasing coefficient of variation with increasing event size (Fig. 5a) (Brauman et al., 2010; Carlyle-Moses et al., 2004; Holwerda et al., 2006; Loustau et al., 1992; Price and Carlyle-Moses, 2003; Staelens et al., 2006; Vrugt et al., 2003; Zhang et al., 2006; Zimmermann et al., 2009), the sampling effort necessary to reach a pre-specified error limit of mean throughfall estimates decreases alike (Kimmins, 1973; Price and Carlyle-Moses, 2003; Rodrigo and Àvila, 2001; Zimmermann et al., 2010). The results of our study are in line with these findings (Figs. 6 and 7). Second, the characteristics of the spatial correlation functions of throughfall change systematically with event size and temporal aggregation, respectively (Fig. 5c and d). The detected decrease of correlation lengths with increasing event size confirms results of previous studies which indicated a particularly pronounced influence of the canopy structure on throughfall patterns for small events (Carlyle-Moses et al., 2004; Loustau et al., 1992; Zimmermann et al., 2009). The consequence in terms of sampling is the poor performance of troughs when low nugget-to-sill ratios coincide with effective ranges that exceed a couple of meters (Fig. 8).
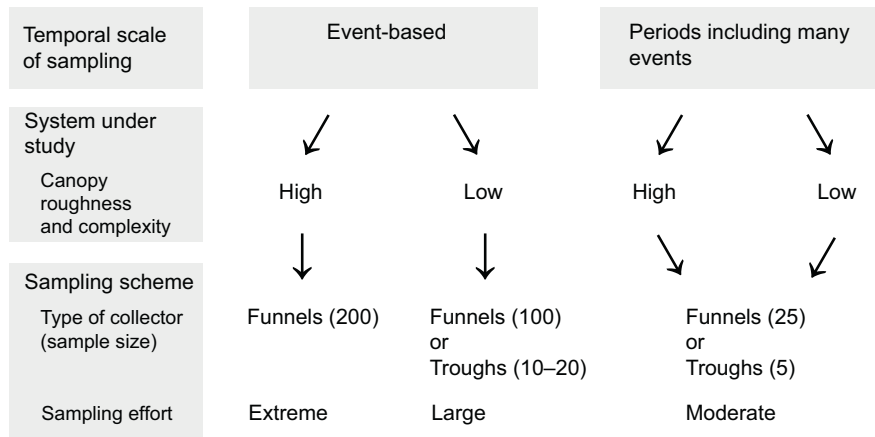
Fig. 10. Decision tree that shows the main steps involved in the design of an optimum sampling scheme for estimating mean throughfall. The type of collector refers to troughs with a receiving area of 2.5 m^2 and to funnels with a support of 0.04 m^2, respectively. The corresponding sample sizes ensure a maximum relative error of 10%. Note that while the preferred sample supports are not site-specific, the recommended sample sizes are specifically tailored to this study's forests and should therefore be transferred to similar settings only. Other possible combinations of support, sample size and error limit can be derived from Figs. 6 and 7, respectively.

These findings lead to the following general recommendations. First, in studies requiring aggregated throughfall data only, e.g. on the scale of months or seasons, none of the collector types shows a superior performance. That is to say, the choice between trough and funnel-type collectors can be made considering logistical aspects (Fig. 10). For instance, long-term throughfall monitoring studies may employ a small to moderate number of troughs. For rapid assessment surveys, in contrast, employment of a moderate number of funnels might be more convenient. Second, in investigations on an event basis, the choice of sample support and corresponding sample sizes depends on the system under study. This issue will be discussed in the next section.

5.3. Implementing throughfall sampling schemes: Adapting to the system under study

We have shown that throughfall in uniform forest stands (e.g. plot 1), or in forests without pronounced structures such as young secondary forests (e.g. plot 2), likely exhibits only short-ranging or weak spatial correlations (Fig. 5c and d). Results of other investigations tentatively confirm this observation (Loustau et al., 1992). Accordingly, event-based throughfall sampling with an appropriate number of trough- or funnel-type collectors will be efficient in weakly structured forests (Fig. 10). Previous manuals on interception monitoring (DVWK, 1986) also provide recommendations for the number of troughs to be employed in weakly structured forests. Unfortunately, it is not clear how these numbers were calculated and what error limits they help to keep.

Forests that show a larger structural variation, that is, a high canopy roughness and pronounced variations in canopy density (e.g. plot 3), require more attention for designing an event-based throughfall sampling scheme. Our data suggest that these forests are potentially associated with pronounced spatial dependencies among throughfall measurements (Fig. 5a). A comparison with other investigations of throughfall spatial structure from mature forest sites, however, reveals no consistent patterns. That is to say, some studies detected strong spatial dependence (Keim et al., 2005), whereas others did not (Zimmermann et al., 2009, 2010). At this point it is important to remember that a variety of processes such as the redistribution of rainfall in the understory influences the characteristics of throughfall spatial correlations (Zimmermann et al., 2009). In structurally diverse forests where pronounced throughfall patterns cannot be ruled out a priori, a save choice is to use an appropriate number of funnel-type collectors,

particularly when reliable throughfall estimates for small events are required (Fig. 10). This recommendation modifies the findings of Zimmermann et al. (2010) which indicated that troughs are often more efficient than funnel-type samplers. This is because the throughfall data of Zimmermann et al. (2010) showed only weak or short-ranging spatial dependence regardless of event size. Our recommendation also modifies previous reports (e.g. DVWK, 1986) that favor the use of troughs in event-based measurement campaigns for all forest types.

5.4. Implementing throughfall sampling schemes: Practical considerations

Our plausibility check confirmed the general validity of the simulation approach. The unexpectedly large relative standard errors associated with the estimated trough means for two small events (Fig. 9b), however, point to possible practical limitations. In our case, an insufficient tip resolution constituted the most likely reason for this failure of the otherwise satisfactorily performing trough systems in plot 2 (weakly structured forest). This problem could have been avoided by using tipping buckets of a lower capacity that optimize tip resolution along the entire spectrum of local storm sizes, including the very small events. Nevertheless, the presented case highlights the importance of all and not just the statistical aspects of sampling that one needs to consider when designing a complete scheme for survey or monitoring.

5.5. Errors of mean throughfall estimates

In the previous sections we illustrated how the findings of this study can provide guidance for designing future throughfall and interception studies. In addition, our calculations can also be used for a post-hoc quality check of published mean throughfall estimates. Yet, such an assessment is restricted to tropical forests because temperate zone forests usually show a markedly lower spatial variation in throughfall than their tropical counterparts (Fig. 11); hence, mean throughfall estimates in temperate zone forests involve a much lower error for a given sampling setup (Kostelnik et al., 1989; Loustau et al., 1992; Puckett, 1991; Rodrigo and Àvila, 2001).

A comparison of previous sampling setups with our results suggests that sampling schemes of many throughfall studies in the tropics are not commensurate with the typical large spatial variation of throughfall in this climate zone. For instance, published
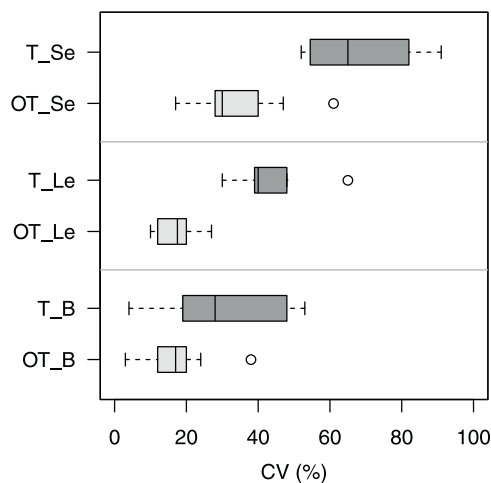
Fig. 11. Boxplot comparisons of published coefficients of variation (CV) for small events (Se, <10 mm), large events (Le, ≥10 mm), and bulk data (accumulated throughfall data) both for tropical forests (T, dark gray boxplots) (Dietz et al., 2006; Holwerda et al., 2006; Jackson, 1971; Manfroi et al., 2006; Ziegler et al., 2009; Zimmermann et al., 2007; and this study) and for study sites outside the tropics (OT, light gray boxplots) (Blume et al., 2008; Cao et al., 2008; Carlyle-Moses et al., 2004; Duijsings et al., 1986; Herbst et al., 2008; Krämer and Hölscher, 2009; Loustau et al., 1992; Oyarzún et al., 2011; Price and Carlyle-Moses, 2003; Puckett, 1991; Raat et al., 2002; Seiler and Matzner, 1995; Staelens et al., 2006; Vrugt et al., 2003; Whelan et al., 1998). For more information on throughfall data characteristics of the cited studies we refer to Supplementary material S3.

mean throughfall estimates for small events, which were derived from trough data in heterogeneous tropical forests, are likely associated with relative errors exceeding 30% (Fig. 6a, c and e). Even for long-term data, estimated means might deviate from population means by more than 20% (Fig. 7b, d and f). This substantial uncertainty of mean throughfall estimates from tropical forest sites is reflected in various problems. For instance, in an interesting study Macinnis-Ng et al. (2012) investigated the influence of land use change on rainfall partitioning in a tropical montane forest region where throughfall is even more variable than at our sites. Unfortunately, their sampling setup (sample size: $n = 30$, support: $A = 0.049\,\text{m}^2$) did not allow the detection of differences in interception even between very contrasting vegetation types. Likewise, several studies reported net rainfall estimates exceeding gross rainfall even though horizontal precipitation did not occur (Germer et al., 2006; Hutjes et al., 1990; Lloyd et al., 1988)—a problem which not only challenged the estimation of the canopy storage capacity (Germer et al., 2006; Hutjes et al., 1990; Lloyd et al., 1988) but also caused (at least partially) large deviations between modeled and observed interception loss (Hutjes et al., 1990; Lloyd et al., 1988). These examples demonstrate that it will be necessary to allocate more resources to throughfall sampling, particularly in tropical regions.

6. Conclusions

The research questions posed initially can be answered as follows. (1) The relative error of mean throughfall estimates varies as a function of throughfall depth. Errors associated with small events (<5 mm) are largest and drop substantially with increasing event size (≥5 mm), or increasing temporal aggregation of throughfall data. Because of the strong influence of event size, sampling schemes have to be adapted to the required temporal scale of sampling. (2) The relative error of mean throughfall estimates depends on the structural diversity of the studied forest; hence, increasing forest complexity calls for increased sampling efforts. (3) The efficiency of trough-type throughfall sampling devices diminishes in

the presence of a pronounced spatial structure of throughfall. Particularly in situations which require the sampling of small events under heterogeneous canopies, funnel-type samplers should be preferred over trough-type samplers. In all other situations, the choice between trough- and funnel-type collectors can depend on logistical considerations.

Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at http://dx.doi.org/10.1016/j.agrformet.2014.01.014.

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