frontiers in Forests and Global Change

GENERAL COMMENTARY
published: 14 April 2021
doi: 10.3389/ffgc.2021.639511

Response: Commentary: What We Know About Stemflow's Infiltration Area

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Keywords: stemflow, infiltration, funneling ratio, forest hydrology, precipitation partitioning, science philosophy

A Commentary on

Commentary: What We Know About Stemflow's Infiltration Area
by Carlyle-Moses, D. E., Iida, S., Germer, S., Llorens, P., Michalzik, B., Nanko, K., et al. (2020). Front. For. Glob. Change 3:577247. doi: 10.3389/ffgc.2020.577247

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Specialty section:
This article was submitted to
Forest Hydrology,
a section of the journal
Frontiers in Forests and Global
Change

Received: 09 December 2020
Accepted: 16 March 2021
Published: 14 April 2021

Citation:
Allen ST and Van Stan JT II (2021)
Response: Commentary: What We
Know About Stemflow's Infiltration
Area.
Front. For. Glob. Change 4:639511.
doi: 10.3389/ffgc.2021.639511

There are scientific gray areas where it is unclear whether process representations and assumptions in models have *adequate* empirical basis. Nonetheless, useful insights can come from interpreting data with the aid of models that use hypothetical parameterizations or process representations, even if they are uncertain and speculative. Indeed, virtual experimentation is often important in hydrology, and interpreting results from imperfect models can lead to improved hypotheses and interpretations of measurements from complex systems (Weiler and McDonnell, 2004). What we see to be less of a gray area is the need to (a) distinguish observations from hypotheses, and (b) use observations to test models to better understand their limitations. Models are hypotheses that are refined through iterative testing and falsifying, and this scientific procedure is crucial to their use in constraining theories and understanding of natural phenomena.

In Van Stan and Allen (2020), we reviewed the existing set of observations on stemflow infiltration areas and concluded that inadequate data exist to falsify any generalizable hypotheses on stemflow's infiltration area (I_T). Carlyle-Moses et al. (2020) have responded to our paper to argue that their prior proposed approach for quantifying stemflow infiltration area—dividing stemflow rate by saturated hydraulic conductivity, K_{sat}–"remains a theoretically sound approach." This rebuttal seems like it is a misplaced response to an unmade argument. In fact, we applaud Carlyle-Moses et al. (2020) proposal of a minimal-parameter model, which means it is more likely to be falsifiable and thus eventually support theory (Kirchner, 2006). However, Van Stan and Allen (2020) concluded that too few observations exist to rigorously test such a model, and those observations that do exist would mostly not pertain to natural systems.

The question of "how much evidence is enough" always depends on the potential questions or applications in consideration. First-order priorities could include characterizing I_T across multiple natural or managed forests with different climates and soils. Another focus should be aridlands, where high stemflow fluxes may play an important ecohydrological role by preferentially recharging root-zone soil moisture (e.g., Martinez-Meza and Whitford, 1996; it should be noted that Martinez-Meza and Whitford attributed preferential infiltration to rapid flow along root channels, and not to the hydraulic conductivity of the soil matrix). It is unclear that the existing I_T measurements are generally representative of any specific environment because (a) we suspect that those values could reflect a selection bias inherent to the primary literature (e.g., observations of especially small or large I_T might have prompted those studies to further investigate and quantify I_T), and (b) they mostly are from anthropogenic settings and thus soils could reflect unknown wide-ranging disturbances.

Regardless, while Carlyle-Moses et al. (2020) model is a hypothetically reasonable approach (at least for the initial infiltration absent of large influences by dispersive forces, and if the K_{sat} values used are measured at scales that appropriately match stemflow infiltration processes), we continue to advocate against blurring the lines between empirically observed I_T and estimated I_T (using K_{sat}). Several additional data points that Carlyle-Moses et al. (2020) included in their table are derived from observed stemflow rates and hypothesized infiltration processes, and thus they are fundamentally different from those previously reviewed in VS&A (that were observed or inferred from direct and indirect observations).

Echoing the arguments of the Van Stan and Allen (2020) review article, we emphasize that claims about whether something does or does not occur should be based on observations when feasible. Even if the model in question was fully supported by the small pool of extant data, we would still need evidence across more diverse environments to consider the model's key hypothesis to be rigorously tested. Otherwise, beyond disambiguating variable names (for the funneling ratio) and modifying or offering more assumed I_T values, the Carlyle-Moses et al. (2020) comment on our paper leaves us unsure of which conclusions are in dispute, because most of the discussion seems to affirm our conclusions. Specifically, our original paper found (and the commentary agrees) that observations of I_T are:

1) reported to range from 10^{-2} to 10^{1} m^2 $tree^{-1}$;
2) scarce, comprising <20 studies over the past 140 years;
3) dominated by agriculture, urban, and plantation settings, leaving natural forests underrepresented; and
4) collected using a wide range of methods, each with their own strengths/weaknesses.

Furthermore, Carlyle-Moses et al. (2020) conclude their commentary by re-stating two conclusions that were revealed by Van Stan and Allen (2020) Table 1 and discussed by Van Stan and Allen (2020), specifically:

(1) Dye experiments and indirect estimates modeled per K_{sat} "suggest that I_T associated with average rainfall and stemflow rates [may be] < 1 m^2 $tree^{-1}$ in ... mature, natural forests."
(2) All other observations of I_T (those ≥ 1 m^2 $tree^{-1}$) appear to occur under two conditions: (i) "during large/extreme rainfalls and stemflow rates in these [natural] forest environments" and (ii) "under relatively smaller rainfall and stemflow rates in [a greater diversity of] environments (e.g., agricultural plantations, orchards, agroforestry areas, and urban environments)."

We conclude this response by re-emphasizing our opening point: as contributors to a field where insights from models and calculations are increasingly prevalent, and observational studies are becoming rarer (Burt and McDonnell, 2015), we need to clearly distinguish between the types of insights they yield. Indeed hypotheses (models) may both precede and follow observations (Hempel, 1966); however, hypotheses should not be seen as a replacement for observations. The scientific process allows the cart to sometimes go before the horse—pushing and pulling are both progressive—but we should avoid having a runaway cart with a horse that is no longer in sight. Specifically, on the topic of stemflow I_T, this cart (K_{sat}-based modeling of I_T) may currently lead the horse (I_T observations and stemflow ecohydrological roles). As we believe that claims about our understanding should follow an iterative course of hypothesis and test to constrain that understanding (e.g., Platt, 1964), we defend the content of Van Stan and Allen (2020) and its pertinence for establishing such a course to empirically test stemflow I_T and its hypothesized ecohydrological roles.

AUTHOR CONTRIBUTIONS

Both authors contributed equally to the work, and approved it for publication.

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Conflict of Interest: The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.