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Reply to comment by Jack Lewis et al. on "Forests and floods: A new paradigm sheds light on age-old controversies"

Younes Alila,[1] Robert Hudson,[1,2] Piotr K. Kuraś,[3] Markus Schnorbus,[4] and Kabir Rasouli[5]

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> To the extent that two scientific schools disagree about what is a problem and what a solution, they will inevitably talk through each other when debating the relative merits of their respective paradigms.
> [*Kuhn*, 1970, p. 109].

1. Introduction

[1] *Alila et al.* [2009] did not intend to present the frequency paired (FP) method for analyzing altered peak flow frequencies after logging as stated by *Lewis et al.* [2010]. Such a technique is well established in the wider hydrology [e.g., *Howe et al.*, 1966] and climatology [e.g., *Wigley*, 1985] communities; and the concepts on which it is based are not new to the forest hydrology community [e.g., *Troendle*, 1970]. *Alila et al.* [2009] expose a set of flaws of the most fundamental construct in methods that dominated decades of research in forest hydrology and as a result, cast serious doubts on the current state of science on the relation between forest land use and floods. *Alila et al.* [2009] illustrate, using philosophical, conceptual, physical and empirical arguments, how our prevalent scientific perception of the forests and floods relation is shaped by an invalid experimental design and irrelevant research hypotheses that focus on a change in magnitude between preharvesting and postharvesting floods when paired by equal meteorology or storm input. This type of chronological event pairing (CP) leads to incorrect changes in flood magnitude because it fails to account for the physical reality of changes in frequency of peak flows caused by harvesting, and further reaffirms decades of irrelevant research outcomes through the use of inappropriate statistical methods referred to as the analysis of variance and covariance (ANOVA and ANCOVA). Since many paired watershed studies published earlier did not have a sufficient record length to apply a frequency paired analysis, their outcomes may have been a manifestation of the "expediency" of the moment rather than a substantiation of "scientific facts" [*Yevjevich*, 1968, p. 1174].

[2] *Lewis et al.* [2010] choose to remain vague by neither fully denying nor admitting to the fundamental flaws in CP-based analyses but insist that CP can be modified to account for a change in frequency. *Lewis et al.* [2010] avoid the main question at hand by raising secondary questions of interpretive nature, the answers to which can only serve to further articulate (and not correct or invalidate) our FP method: How do we adjust observed peak flows for hydrologic recovery? How do we correct for the loss of variability caused by a calibration equation? How do we estimate uncertainty in a flood frequency relation? How do we increase statistical power to predict the effects on larger floods? These questions are important but must be considered as a part of a new era of research in forest hydrology guided by the new paradigm of pairing events by equal frequency. Since scientists can only be guided by one paradigm at a time, we contend that the main and real question at hand that we must confront head on remains; which of the two paradigms should guide the future science of forests and floods, CP or FP? The answer may lie in Francis Bacon's maxim: "Truth emerges more readily from error than from confusion."

[3] In this response, we explain why continuing the use of CP-based methods for evaluating the relation between forests and floods will reinforce the misconception, confusion and misinformation that are prevalent in the science literature, as opposed to increasing our understanding of land cover influences on hydrologic response. In our reply, we classified the major discussion points raised by *Lewis et al.* [2010] under the following six general headings.

2. Misconceptions About the Role of Frequency in Forests and Flood Research

[4] *Lewis et al.* [2010] state that "we agree that analyses of changes in flood frequency are useful for evaluating the effects of watershed disturbance" (paragraph 1) and "…attention to flood frequencies is merited and may shed light on the issue" (paragraph 29). Let there be no confusion that flood frequency distributions are not just a "useful" dimension that simply "merits attention"; they absolutely must be included in any evaluation of the relation between forests and floods. If the inextricably linked frequency and magnitude of a flood are not simultaneously invoked, as conducted in the convenient but irrelevant CP-based analysis

[1]Department of Forest Resources Management, University of British Columbia, Vancouver, British Columbia, Canada.
[2]Professional Geoscientists, Qualicum Beach, British Columbia, Canada.
[3]Northwest Hydraulic Consultants, Ltd., Vancouver, British Columbia, Canada.
[4]Pacific Climate Impacts Consortium, University of Victoria, Victoria, British Columbia, Canada.
[5]Department of Earth and Ocean Sciences, University of British Columbia, Vancouver, British Columbia, Canada.

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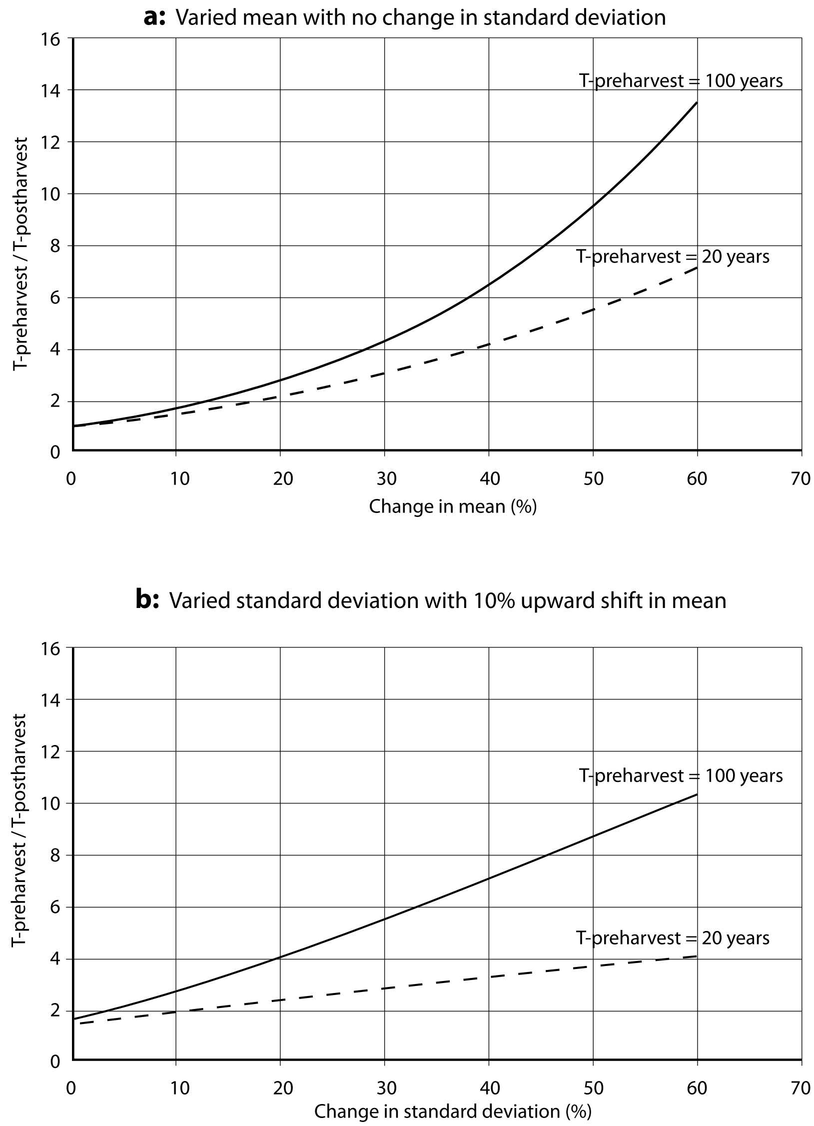

Figure 1. Hypothetical forest harvest–induced changes in return period of floods for a preharvest flood frequency curve generated by general extreme value distribution with coefficient of variation of 0.4 and shape parameter of −0.16 (T denotes return period in years).

of variance and covariance, not only we end up with the incorrect change in magnitude but equally important we obscure the most critical facets of the relation between forests and floods, namely, (1) small changes in the magnitude of floods can translate into larger changes in their return periods and (2) the larger the flood, the more dramatic the change in its return period. This is a direct consequence of the highly nonlinear and inverse relation between the magnitude and frequency of floods, which can only be represented by the flood frequency distribution and not a regression fit of any level of complexity.

[5] These arguments are easy to demonstrate. Under a stable climate, a flood event may be assumed to occur when, say, a peak flow magnitude, Q, falls above some critical threshold, Q_T. The probability of occurrence, P, of such a flood is given by the area under the tail of the frequency distribution when Q is larger than Q_T. This area also defines the return period or recurrence interval, T in years, which is the inverse of the probability of occurrence P. Shifting the mean of the distribution toward Q_T causes increases in the area under the tail in a highly nonlinear manner. Figure 1a shows how a 30% change in mean would change roughly a 20 year into a 7 year event, and a 100 year into a 20 year event. The frequencies of larger floods are even more sensitive to changes in the variability around the mean of the frequency distribution. Figure 1b shows how a 10% change in the mean combined with a 20% upward shift in the standard deviation will change a 100 year into a 25 year flood event, which amounts to quadrupling the flood risk. Although highly idealized, these rather "pedagogical" illustrations [*Wigley*, 2009, p. 67] serve to emphasize the importance of the overlooked frequency distribution conceptual framework in decades of forest hydrology literature.

[6] Climatologists have long recognized the significance of a frequency distribution framework, hence the critical aspects of extreme event theory such as return period and

risk, for understanding and quantifying the effects of climate change on weather extremes [e.g., *Wigley*, 1985]. In forest hydrology, however, over 40 years of ANCOVA and ANOVA studies stripped the “risk” out of what was meant to be an evaluation of the relation between forests and flood risk. This created a perception based on the conclusions of irrelevant research which claimed that there is ‘no evidence’ that forests affect larger flood events, albeit that those events were ambiguously defined (i.e., ranked by storm input or control watershed peak flows). The time has come for the forest hydrology community to put an end to working in isolation on the topic of forest land use effects on floods.

3. CP-Based Analyses Lead to Irrelevant Outcomes, With or Without Recovery Adjustment

[7] *Lewis et al.* [2010] argue that a comparison of Figures 3a with 3b is “inequitable.” They claimed that comparison should have been made between Figures 3a and 7b. The following quote shows how a “straw man” type of argument, which takes our words out of context, diverts the reader’s attention away from the real issue. *Lewis et al.* [2010, paragraph 7] state that

> The conclusion that only the frequency paired approach revealed that “all peak flows save the largest event were shifted upward” (AKSH, paragraph 29) reflects the fact that the AKSH procedure itself shifted the peaks used in the FP analysis upward. Figure 7b, showing the unadjusted analysis, is the appropriate figure for comparison to Figure 3a; both reveal a more modest upward shift converging at the two largest events.

[8] The quote “all peak flows save the largest event were shifted upward” was truncated and should have been reported as: “all peak flows save the largest event were shifted upward and the largest peak flows on the observed record became more frequent (Figure 3b).” An interpretation of Figure 3a, constructed with or without recovery adjusted peak flows, not only leads to incorrect estimate of a change in magnitude but equally important cannot be used to make inference about changes in frequency of any events, let alone the larger floods, because the CP-based analysis is not designed to reveal changes in event frequency. Figure 3b, however, reveals what *Lewis et al.* [2010] appear unwilling to admit; that forest harvesting may have increased the frequency of larger events. “Novelty emerges only with difficulty, manifested by resistance, against a background provided by expectation” [*Kuhn*, 1970, p. 64].

[9] *Alila et al.* [2009] state that interpretation of the FP analysis displayed in Figure 7b cannot be scientifically defensible because it was constructed using a nonstationary time series. Also, any analysis based on Figure 7 would be invalid because it does not distinguish between the effects of forest harvesting and recovery. These issues cannot be overemphasized and our Figure 7 was included to avoid such highly anticipated misinterpretations. While Figure 7b is admittedly wrong, Figure 3a is “not even wrong” (i.e., its interpretation is irrelevant to whether forest harvesting is affecting floods). We decided to use raw (unadjusted for recovery) data for constructing one of our plots in Figure 3 (i.e., Figure 3a) because it is this convergence of two regression lines that has shaped our prevailing perception: namely forests affect small and medium but not necessarily larger floods.

[10] The use of paired watershed data to illustrate the difference between chronological and frequency pairing is not possible without employing a calibration equation to estimate the expected peak flows. The empirical cumulative distribution function (CDF) of these peak flows may have been affected by a loss of variability associated with the use of such equation. As explained in our methods, we corrected for this loss of variability and the outcomes were discussed by *Alila et al.* [2009, section 4.2]. The calibration equations that we employed at WS1 and WS3 used log-transformed peak flows. Prediction using these regression equations produces a small downward bias in the estimate of the expected discharge. We have not made any adjustment for such downward bias and *Lewis et al.* [2010, paragraph 12] are correct when they state that “The required bias correction is typically small, but given the sensitivity of upper quantiles to a shift in both mean and variance, the differences reported cannot necessarily be attributed to logging.”

[11] Using the proposed bias correction technique, we indeed found the effect to be quite small (in the order of 1–2%) and therefore not substantial enough to change any of our results and conclusions. We find it remarkable that *Lewis et al.* [2010], on one hand, recognized how changes in mean and variance can have substantial effects on the upper quantiles of a frequency distribution, and are concerned about this small downward bias in the expected discharges, but are still defending the chronological pairing which, as we illustrated, leads to an equivocally incorrect and irrelevant change in magnitude.

[12] Our adjustment for recovery of peak flows is also based on chronological pairing; this may introduce uncertainty in our estimated changes in the magnitude and frequency of floods. We have explicitly acknowledged this in section 3.5 of our original article. While it is possible that our recovery adjustment may have affected our results, we think such effects are minimal, in part because of the naturally slow recovery of the cold snow environment at Fool Creek and the even slower recovery of road effects at WS3. Nonetheless, we would like to see the results of an FP analysis on the same data sets, adjusted for recovery using a model that is accepted by the forest hydrology community.

4. Like Lampposts, Statistical Tests of Significance Should Be Used for Illumination and Not Support

[13] *Lewis et al.* [2010] suggested that a valid analysis of uncertainty would require that potentially overlapping confidence limits be estimated for frequency distributions of both the expected and observed peak flows. The outcomes of statistical hypothesis tests cannot be used to justify a CP-based invalid research hypothesis, which we illustrate to be irrelevant to the forests and floods relations. Our conclusions that the prevalent perception of forests and floods relation is scientifically indefensible will not be invalidated by attempting to impose more stringent statistical tests of significance. Nevertheless, we have used in our original article two nonparametric tests which specifically test whether the two (pretreatment and posttreatment) sample distribu-

tions are "far enough apart" that they can be considered to be derived from different populations [*Alila et al.*, 2009, Tables 1 and 2].

[14] Our approach to estimating uncertainty using Monte Carlo simulations and our position on the concept of null hypothesis statistical testing are well documented in our methods. Regardless, since chronological pairing does not lead to estimation of correct changes in magnitude and provides no information on changes in frequency, it is irrelevant whether the two pairing methods provide changes in magnitude that have similarly high type 1 error probabilities.

5. Physical Significance Should Not Be Ignored in the Name of Statistical Significance

[15] We agree with *Lewis et al.* [2010] that the lack of statistical power may continue to be a challenge in detecting changes in unusual events and we agree with their suggestion of conducting metastudies to investigate whether analogous changes have repeatedly been measured but declared insignificant in the absence of sufficient statistical power. However, this is outside the scope of our article and should be a recommendation for future research on this topic guided by the FP- and not CP-based paradigm.

[16] *Lewis et al.* [2010, paragraph 19] suggested that "[i]f the available data are uninformative, the reader should avoid conclusions of any kind." The amount and relevance of information contained in experimental and observational data depends on the appropriateness of the method used to analyze such data. Our FP event analyses revealed how profound the implications of overlooking changes in flood frequency could be in evaluating the relation between forest harvesting and floods. For the first time, *Alila et al.* [2009] revealed how forest harvesting not only causes a 3 year to become 2 year event, but *may* also change a 30 year (Fool Creek) and a 40 year (WS3) into a 15 year event. We have acknowledged the uncertainties in the upper tail of flood frequency distributions [*Alila et al.*, 2009, section 4.2] but simultaneously articulated plausible physical explanations for such changing patterns of magnitude and frequency [*Alila et al.*, 2009, paragraphs 28 and 38], which cannot simply be ignored.

6. Interpretive Enterprise Can Only Articulate a Paradigm, Not Correct It

[17] Blocking is used in chronological pairing and the paired before-after control-impact (BACI) design to create the sort of controlled experiment that will allow for the isolation of the system response of interest. However, the system response of interest in our case is a flood, which has two inextricably linked attributes: magnitude and frequency. Therefore, the frequency distribution is the only framework that allows the control of one of the two attributes in order to calculate the change in the second. This is the only correct method of answering the purely stochastic research hypothesis: What is the change in magnitude (frequency) for an event of a specific frequency (magnitude) of interest?

[18] We have decided in our original article to use observed peak flows from well-established paired watershed study sites instead of peak flows simulated by a hydrological model in an effort to reduce uncertainties. Again, the use of paired watershed data in our paradigmatic comparison is not possible without a calibration equation for predicting the expected peak flows, which in itself is based on chronological pairing. It is this very use of chronological pairing, defended by *Lewis et al.* [2010], that has introduced uncertainties in the outcomes of our FP evaluation methods. Ironically, these are some of the same uncertainties that *Lewis et al.* [2010, paragraph 1] used to cast doubt on our study conclusions when they state "However, the proposed method and accompanying discussion have several problems that undercut the strength of the paper's conclusions." In addition, these problems should continue to be the subject of future research investigations once the new paradigm of FP is adopted and has replaced the old CP paradigm. For instance, answers to questions of how to estimate confidence bands around a flood frequency curve or how to increase the statistical power to improve detection of effects on larger floods can only further articulate and not correct or invalidate our FP paradigm [*Kuhn*, 1970, p. 122]:

> Given a paradigm, interpretation of data is central to the enterprise that explores it…But that interpretive enterprise…can only articulate a paradigm, not correct it. Paradigms are not corrigible by normal science at all.

[19] *Lewis et al.* [2010, paragraph 22] suggested carrying out paradigmatic comparisons using "data sets reflecting the shorter record lengths more typical of those generally available, such as those from WS1 and WS3, and for the 27 year Fool Creek data set" and not just Fool Creek 48 year data. The intriguing differences between the outcomes of the two pairing methods are best illustrated using a long record in a hydroclimate regime with a naturally slow recovery rate. Our analysis of the Fool Creek results at 27 and 48 years indicates that we need longer and not shorter records. Besides, WS1 and WS3 data sets of varying length have already been analyzed by three research groups using CP analyses and their outcomes have been summarized and compared to the outcomes of our FP analysis [*Alila et al.*, 2009, paragraphs 49–51].

[20] *Lewis et al.* [2010, paragraph 23] state that the CDFs are smoother than the CP-based regression analyses because "the data are sorted to create a nondecreasing display." The "nondecreasing display" is the result of using order statistics as opposed to CP-based estimates. Order statistics, which comprise a direct estimate of the CDF, afford a more powerful measure of frequency-based changes. The variability around a postharvest regression fit of ANCOVA is an artifact of the inappropriate type of event pairing [e.g., *Alila et al.*, 2009, Figure 3a]. Such variability must affect the statistical power of the CP-based methods and impedes the ability to detect a change caused by forest harvesting [e.g., *Alila et al.*, 2009, Figure 3e and paragraph 59]. Our point is that artificiality in the variability is introduced when one forces the treatment and control CP peak discharges to have the same frequency of occurrence; they do not. Furthermore, there may be a case-specific threshold return period beyond which a forest cover does not affect floods, but that threshold flood can only be identified with a frequency-paired approach. In some rain regimes, for instance, the effects of antecedent soil moisture on large floods may

decrease with increasing return period [*Wood et al.*, 1990]. In such regimes, however, an open question is where "large" begins or how rare must floods be for antecedent soil moisture to have no effects on floods? [*Sturdevant-Rees et al.*, 2001, p. 2161]. In other regimes, snow accumulation and melt processes can be more important than evapotranspiration, and their effect on flood response can increase with increasing return period [*Schnorbus and Alila*, 2004, Figure 9; *Harr*, 1981, p. 297].

[21] *Lewis et al.* [2010, paragraph 23] state that "Hypothesis tests for CP and FP have entirely different null hypotheses, so direct comparisons of statistical power may not be possible." "CP and FP have entirely different null hypotheses" was the argument we use to build our case against the CP-based paradigm, and we found it remarkable that *Lewis et al.* [2010] are now using the same argument against our attempt to compare the statistical power of CP and FP methods.

[22] *Lewis et al.* [2010, paragraph 24] state that "FP cannot be easily used to evaluate recovery." Our point is that recovery will occur when the preharvest and postharvest frequency distributions are identical. Therefore, FP should be used to assess recovery in this context; that it cannot be done as easily is irrelevant.

7. Is There Any Linkage Between CP and FP Analysis Outcomes?

[23] *Lewis et al.* [2010, paragraph 26] argue that CP-based regression analysis "(Figure 3a) does reveal that the frequency of large peaks increased after logging." We categorically disagree because CP-based analysis of covariance was not designed for such purpose. *Lewis et al.* [2010, paragraph 26] suggested that "[a]dditional calculations could be used to quantify those changes [in frequency]. Frequencies and the conversion of medium peaks to large peaks may indeed deserve more attention, but there is no reason to abandon methods utilizing CP." What *Lewis et al.* [2010] are suggesting is an indirect and convoluted way of achieving what can be done directly and with elegance under our FP-based paradigm. Hewlett made the same suggestion three decades ago when, facing the challenge of an apparent harvest-induced increase in the "variability" of peak flows collected in Japan, he stated that "…a large increase in the variance of peaks and volumes…..would be worth reanalyzing by more advanced regression techniques…" [*Hewlett*, 1982, p. 533]. *Hewlett and Helvey* [1970, p. 779] were aware that the question of forests and floods cannot be settled without invoking the dimension of frequency. Note that Hewlett's last paper on this topic was his 1982 paper quoted above.

[24] We recognized that Lewis and coworkers were among the few who invoked the frequency dimension under the CP-based framework [e.g., *Lewis et al.* 2001, Figure 29]. However, we see no linkages, in terms of physics or statistical theory, between the outcomes of CP and FP analyses. This type of linkage between the outcomes of CP-based methods and the frequency of floods, which does not necessarily preserve the all-important nonlinear and inverse relation between the magnitude and frequency, is not only ambiguous but projects a state of confusion. We need to recognize that since CP-based methods yield the wrong change in magnitude, there is no guarantee that they yield the correct change in frequency, and even if they do, it would be for the wrong reason.

[25] *Alila et al.* [2009] maintained all along that inferences about forest harvesting effects using the analyses of variance and covariance are invalid for flood events smaller and larger than an average peak flow. *Lewis et al.* [2010] claimed that we have not given any statistical justification for such argument. Our argument against the old paradigm of CP and the analyses of variance and covariance is about "statistical physics" and not pure "statistics" [*Koutsoyiannis*, 2010, p. 598]. Decades of peer reviewed research on the topic of forest harvesting and floods that used the old paradigm of CP and associated analyses of variance and covariance didn't account for the physical reality of changing flood frequencies and as a consequence stripped the physics from the research question at hand.

[26] *Lewis et al.* [2010] in another "straw man" type of argument implied that we are drawing support for our case against the flawed CP-based methods from a single claim by *Harris* [1977]. On the contrary, our article drew support for the case against the CP-based methods from decades of literature in several disciplines (hydrology, ecology, climatology, and statistics). Our extensive after-the-fact forest hydrology literature review revealed that a few hinted at the flaws that we exposed in CP-based methods when used to evaluate the relation between forests and floods: *Hewlett and Helvey* [1970], *Harris* [1977]; and most importantly *Harr* [1986, p. 1096], who explicitly referred to the convergence of two regression fits as "irrelevant" to whether or not forest harvesting affects floods. To the best of our knowledge, Harris and Harr have also written little, if anything, on this topic since then. Although we have been "tied up in irrelevancies" [*Platt*, 1964, p. 347] for decades, it is never too late to act on past cues from the luminaries of Forest Hydrology [*Hewlett and Helvey*, 1970, p. 779; *Berris and Harr*, 1987, p. 141]. In light of these past cues, which continue to be ignored, the proliferation of literature review documents ruminating over the outcomes of decades of irrelevant CP-based paired watershed peak flow studies remains the only life line of our aging preconceived forests and floods perception. "A theory which cannot be mortally endangered cannot be alive" (W. A. H. Rushton, cited by *Platt* [1964, p. 349]).

8. Conclusion

[27] *Lewis et al.* [2010, paragraph 29] in one sentence admitted that CP-based ANOVA and ANCOVA "have their limitations," but went on in the same sentence to assert that "there is nothing inherently 'inappropriate' about these techniques." This is not the rigorous language that the late J. D. Hewlett may have hoped for when he stated that [*Hewlett*, 1982, p. 546]

> Hydrologists have understandably been confused by the difficulties inherent in describing the nature and frequency of floods to laymen… But among ourselves we must drop back to rigorous language in order to discuss and trade information about land-use causes and flood effects.

Lewis et al. [2010] mixed messages leave readers from outside the forest hydrology community and the average graduate student feeling as if "any approach or opinion goes" in hydrology. These are the same mixed messages that contribute to the situation eloquently articulated by *Dunne* [1998, p. 795]:

> Forest hydrologists could be recruited to defend almost any side of a debate, because our confusion about the various processes and their interactions in forests and streams…has left much room for opinion and its attendant influences.

[28] In fact, while appearing supportive in principle of our FP-based paradigm, *Lewis et al.* [2010] arguments against it are simply nit-picking. We want to reiterate unequivocally that CP-based methods are scientifically indefensible. Although science is in general cumulative and progressive, this is one of these rare exceptions where building on the past would not be the right course of action. CP-based interpretations of the forests and floods relation have contaminated our view of the issue, brought obfuscation rather than clarity, and are ultimately dysfunctional. We must open ourselves to the possibility that our current perception of forests and flood relations is illegitimate. We echo the call for "an objective re-evaluation of this issue [forests and floods] by the forest hydrology scientific community" [*DeWalle*, 2003, p. 1256]. It is only a matter of time until it is universally realized that the CP-based paradigm has been a convenient irrelevancy.

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Y. Alila and R. Hudson, Department of Forest Resources Management, University of British Columbia, 2045–2424 Main Mall, Vancouver, BC V6T 1Z4, Canada. (younes.alila@ubc.ca; rob.hudson@telus.net)

P. K. Kuraś, Northwest Hydraulic Consultants, Ltd., 30 Gostick Pl., Vancouver, BC V7M 3G3, Canada. (peterkuras@gmail.com)

K. Rasouli, Department of Earth and Ocean Sciences, University of British Columbia, 2045–2424 Main Mall, Vancouver, BC V6T 1Z4, Canada. (krasouli@eos.ubc.ca)

M. Schnorbus, Pacific Climate Impacts Consortium, University of Victoria, PO Box 1700 Stn. CSC, Victoria, BC V8W 2Y2, Canada. (mschnorb@uvic.ca)