Streamflow response to forest management

James W. Kirchner[1,2*], Wouter R. Berghuijs[1], Scott T. Allen[1,3], Markus Hrachowitz[4], Rolf Hut[4], and Donna M. Rizzo[5]

[1]Dept. of Environmental Systems Science, ETH Zurich, Zurich, Switzerland

[2]Swiss Federal Research Institute WSL, Birmensdorf, Switzerland

[3]Dept. of Geology and Geophysics, University of Utah, Salt Lake City, UT, USA

[4]Dept. of Civil Engineering, Delft University of Technology, Delft, The Netherlands

[5]Dept. of Civil and Environmental Engineering, University of Vermont, Burlington, VT, USA

*e-mail: kirchner@ethz.ch

Forests play a key role in the water cycle, so both planting and removing forests can affect streamflow. In a recent Nature article[1], Evaristo and McDonnell used a gradient-boosted-tree model to conclude that streamflow response to forest removal is predominantly controlled by the potential water storage in the landscape, and that removing the world's forests would contribute an additional 34,098 $km^3\ yr^{-1}$ to streamflow worldwide, nearly doubling global river flow. Here we report several problems with Evaristo and McDonnell's[1] database, their model, and the extrapolation of their results to continental and global scale. The main results of the paper[1] remain unsubstantiated, because they rely on a database with multiple errors and a model that fails validation tests.

Database problems. We spot-checked the database underlying Evaristo and McDonnell's analysis[1] by comparing individual entries to the original cited references. Roughly half of these spot checks revealed substantial errors in the calculated changes in water yields, or errors in the classification of individual studies as forest planting vs. forest removal experiments. Here we describe four examples. 1) The Valtorto catchment in Portugal is classified as a forest clearing experiment[1] although the catchment was never forested, but rather covered by 50 cm tall heath[2]. The reported post-clearing streamflow increase of 363.6 percent[1] is also inconsistent with Table 3 of the original reference[2], which reports that average streamflow increased by 150 percent, from 1.0 to 2.5 $m^3\ day^{-1}$. 2) The database reports that forest clearing at the Lemon catchment in Australia increased streamflow by 631.8 percent[1], but from Table 1 of the original reference[3], we calculate that the average pre- and post-clearing streamflows were 18.0 and 27.9 mm yr^{-1} respectively, implying that streamflow increased by only 55 percent. 3) Brigalow catchments C2 and C3, which both appear twice in the database, are classified as forest planting experiments[1] although neither was planted with forest: C2 was planted with sorghum and wheat and C3 was planted with buffel grass for pasture[4,5]. 4) Several forest conversion experiments, in which forests were cleared and replanted with other vegetation (e.g., ref.'s 74, 114, 130, and 163 in ref. 1), are reported in the database as showing, counterintuitively, large streamflow *increases* due to forest planting[1]. However, the reported changes in streamflow were calculated relative to intact forest control plots, not cleared land, so they mostly reflect the effects of clearing the prior forest rather than the effects of planting. We suspect that this mis-attribution of forest clearing effects to forest planting may underlie the paper's surprising finding (see Fig. 2 of ref. 1 and associated discussion) that forest planting appears to *increase* streamflow by 100 percent or more at many sites, with the largest increases at sites with the highest evapotranspiration rates, a pattern that should normally arise from forest clearing instead.

Model overfitting and validation failure. Gradient-boosted trees are data-hungry, and although Evaristo and McDonnell[1] compiled every paired watershed study that they could find, the resulting databases of 161 forest clearing experiments and 90 forest planting experiments are much too small to reliably estimate their seven-variable model. We checked the model codes that Evaristo and McDonnell provided with their paper (see the code availability statement of ref. 1) and found that the boosted tree algorithm fits 200 free parameters (not counting the dozens of additional free parameters that define the tree's branch points), suggesting substantial overfitting. To test how this overfitting might affect the model's predictions, we split the forest removal and planting databases into training sets (80 percent of the data) and test sets (the remaining 20 percent of the data). To balance the distributions of the variables between the training and test sets, we used stratified random sampling; we also used un-stratified random sampling as a more stringent test. We then re-ran the boosted-tree analysis, using the same data, the same platform (JMP, SAS Institute, Cary, NC, USA), and the same algorithm options that Evaristo and McDonnell[1] used, for 300 of these random splits of the data, both with and without "early stopping" (in which the fitting algorithm stops whenever the next layer would reduce the R^2).

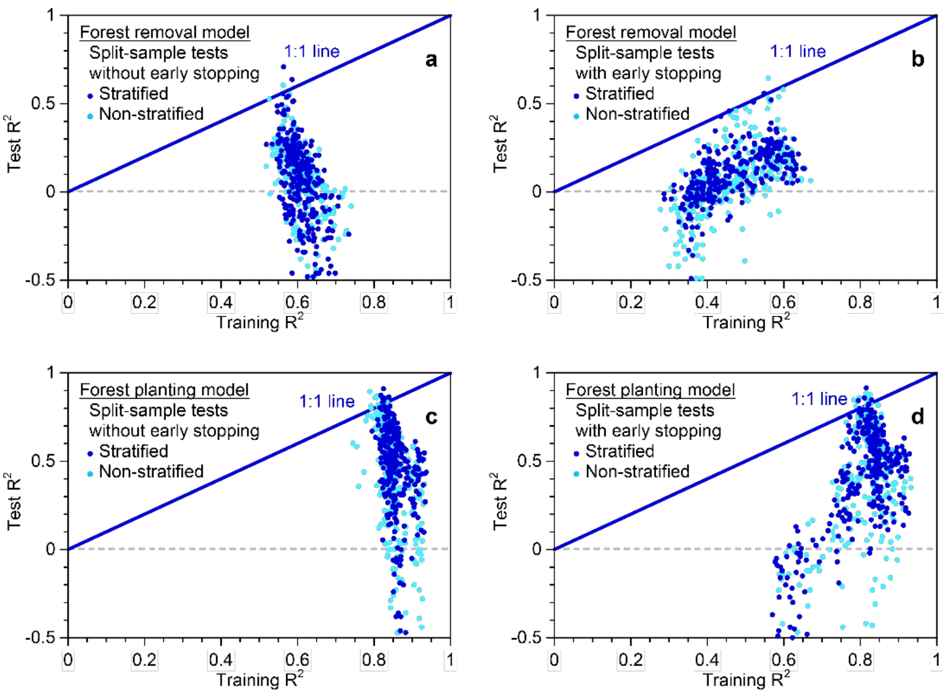

Fig. 1. Split-sample validation tests of gradient-boosted-tree model fitted to forest clearing (a, b) and forest planting (c, d) data. The source data were randomly split into 300 training and test sets in 80/20 ratios, as described in the text. If the model were not overfitted, the R^2 statistics obtained from the training and test sets would be similar to one another, and thus the dots would lie close to the 1:1 lines. Instead, the test R^2 statistics are generally much smaller than the training R^2 values. Points with test R^2 values less than -0.5, which indicate that model predictions were much worse than random guessing, are not shown.

The results in Fig. 1 show that the model fails these validation tests. If the model were not overfitted, the fits to the test data (as measured by the test R^2 on the vertical axis) would be similar to the fits to the training data (as measured by the training R^2 on the horizontal axis), and the dots would lie close to the 1:1 line. Instead, many of the dots lie far below the 1:1 line, and many test R^2 values even lie below zero, indicating model predictions that are worse than random guessing. Figure 1 thus shows that the model is overfitted and makes unreliable predictions (because it is too flexible, and thus has been "fitted to the noise" in the training data). This result holds whether one uses "early stopping" or not, and stratified and un-stratified validation tests yield broadly similar results.

Although individual randomizations can yield test R^2 values that are similar to the training R^2 (or even higher), one should not draw conclusions from such anomalies. Model performance is better reflected in the medians of the training and test R^2 values across many randomization trials (Table 1). Table 1 confirms quantitatively what Fig. 1 shows visually; in each case, the median test R^2 is much smaller than the median training R^2, and many test R^2 values are below zero.

Model and split-sample test performed (80/20 split in all cases)	Median training R^2	Median test R^2	Fraction of test R^2<0
Forest removal model			
Stratified, with early stopping	0.449	0.108	31%
Stratified, without early stopping	0.605	0.096	36%
Unstratified, with early stopping	0.458	0.053	34%
Unstratified, without early stopping	0.608	0.057	40%
Forest planting model			
Stratified, with early stopping	0.827	0.455	13%
Stratified, without early stopping	0.852	0.486	10%
Unstratified, with early stopping	0.826	0.475	16%
Unstratified, without early stopping	0.844	0.474	17%

Table 1. Summary of split-sample validation test results. Test results are shown for boosted-tree model fitted to forest removal and forest planting data. "Fraction of test R^2<0" indicates percentage of tests in which model predictions were worse than random guessing.

All of the paper's[1] main results are based on the boosted-tree model, so the validation failure documented here invalidates the paper's conclusions. The other machine learning methods in the paper have similar validation issues, but we will not explore them in detail because the paper's conclusions do not depend on them.

Exaggerated importance of potential storage. The finding[1] that streamflow response to forest removal was primarily controlled, not by climate, but by total potential water storage in the landscape, was puzzling to us for two reasons. First, it was hard to imagine how total storage, much of which may lie below the rooting zone of trees, could be the major control on the hydrological effects of tree removal. Second, given that forest planting and forest removal both alter the same variable (forest cover), but in opposite directions, it was hard to reconcile the paper's two main findings[1]: that potential storage is the dominant control on streamflow response to forest clearing (but not planting), and that actual evapotranspiration (AET) is the dominant control on streamflow response to forest planting (but not clearing).

Closer examination reveals that the apparent importance of potential storage relies on one extreme data point (Lemon catchment, Australia), which has a potential storage of 15 meters, more than twice the next-highest value in the data set. If we remove this one data point, potential storage disappears as the most important factor (Table 2), and is replaced by potential evapotranspiration (PET). This one data point is so influential because Evaristo and McDonnell's analysis[1] uses an "independent uniform" variable importance profiler. This profiler is intended for use where the likely values of each variable will be uniformly distributed over the range of the data[6], which is inconsistent with the strongly skewed distributions of potential storage in Evaristo and McDonnell's paired watershed data set (Fig. 2a) and in their global catchment database (Fig. 2b). Potential storages exceeding 7.5 m comprise only 0.6% of Evaristo and McDonnell's paired watershed data set (light blue bars, Fig. 2a) and 6% of their global catchment database (light blue bars, Fig. 2b), but 50% of the distribution used to calculate the influence of potential storage, exaggerating potential storage's importance.

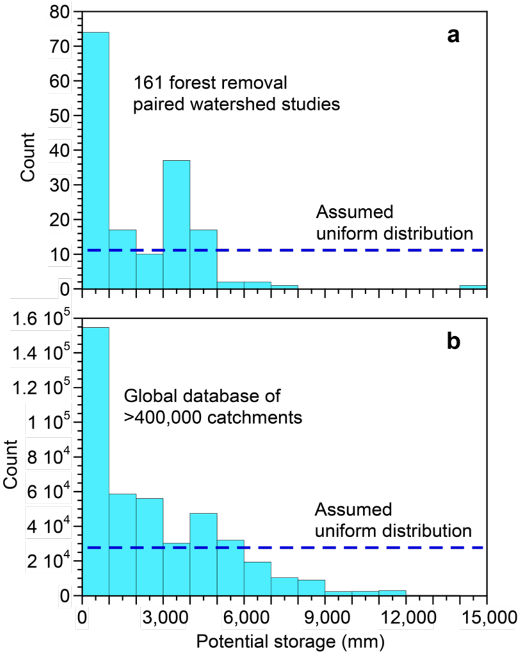

Fig. 2. Distributions of potential storage, compared to the uniform distribution used to estimate its influence in Evaristo and McDonnell's analysis[1].

Although Evaristo and McDonnell fully documented their choice of this "independent uniform" profiler[1], other choices, more consistent with the available data, lead to a different conclusion. For example, if we instead use a profiling method that takes account of the actual distributions of all of the variables ("independent resampled" profiling), PET becomes the most important variable, and potential storage drops to fourth place (Table 2). And if the profiling method also takes account of the correlations among the variables, in addition to their actual distributions ("dependent resampled" profiling), the most important variable is again PET, and potential storage drops to fifth place out of seven variables (regardless of whether we include or exclude the Lemon catchment; see Table 2).

Table 2. Relative variable importance using different profilers.

Profiling method and treatment of Lemon catchment	Potential evapotranspiration	Runoff coefficient	Drainage area	Potential storage	Actual evapotranspiration	Root zone storage	Permeability
Independent uniform							
Lemon included	0.317 (2)	0.098 (3)	0.036 (5)	**0.508 (1)**	0.041 (4)	0.007 (6)	0.000 (7)
Lemon omitted	**0.500 (1)**	0.056 (4)	0.031 (5)	0.299 (2)	0.179 (3)	0.001 (6)	0.001 (6)
Independent resampled							
Lemon included	**0.642 (1)**	0.114 (3)	0.165 (2)	0.094 (4)	0.030 (5)	0.005 (6)	0.000 (7)
Lemon omitted	**0.710 (1)**	0.077 (4)	0.134 (2)	0.091 (3)	0.050 (5)	0.001 (6)	0.003 (7)
Dependent resampled							
Lemon included	**0.440 (1)**	0.189 (2)	0.171 (3)	0.137 (5)	0.109 (6)	0.155 (4)	0.095 (7)
Lemon omitted	**0.433 (1)**	0.180 (2)	0.174 (3)	0.129 (5)	0.102 (6)	0.161 (4)	0.098 (7)

Relative importance scores for each of the seven variables in Evaristo and McDonnell's forest removal model[1] are shown for three different profiling methods, and including and excluding the Lemon catchment (see text). Ranks are shown in parentheses. The most important variable in each case is highlighted in bold.

Exaggerated global streamflow implications. To estimate the potential impact of forest clearing on global streamflow (Table 1 of ref. 1), Evaristo and McDonnell first applied their boosted tree model to a database of 442,319 catchments for which the required seven input variables are available (whether or not they are actually forested). Evaristo and McDonnell then multiplied the median of the modeled percentage change in streamflow for each continent's catchments, times the average continental river flow (see Table 3). Because less than 30% of Earth's land area is forested[7], however, the potential percentage increase in streamflow from forest clearing should not be applied to the entire continental runoff; one cannot clear forests from the 70% of Earth's land surface where no forests exist. Evaristo and McDonnell's calculation[1] implicitly assumes that Earth's entire landmass is forested, and leads to unrealistic results. For example, under Evaristo and McDonnell's median scenario[1], their Table 1 implies that total post-clearing runoff in Asia would be 95% of total Asian precipitation[8] (32,140 $km^3\ yr^{-1}$), a runoff ratio that is rarely observed even in urban areas (Table 3). For Australia and Oceania, the results in Evaristo and McDonnell's[1] Table 1 violate conservation of mass, with total post-clearing runoff (1,970+5,412=7,382 $km^3\ yr^{-1}$) exceeding total precipitation[8] (6,405 $km^3\ yr^{-1}$).

Distributed over the roughly 40 million km^2 of the Earth's surface that is actually forested[7], Evaristo and McDonnell's claimed global streamflow increase[1] of 34,098 $km^3\ yr^{-1}$ implies an average of 850 mm yr^{-1} more streamflow from cleared forest lands. This value exceeds the streamflow increases that were measured in every one of the 95 paired watershed studies reviewed by Stednick[9], and exceeds their average by a factor of five.

Table 3. Modeled effects of forest cover change on continental runoff.

Region	Total river runoff[1]	Change in runoff in response to forest-cover change[1] (km^3 yr^{-1})		Total river runoff after removal[2]	Total precipitation[3]		Change in runoff in response to forest-cover change (%)[4]			Median water yield in complete catchment data set (%)[5]	
	(km^3 yr^{-1})	Planting	Removal	(km^3 yr^{-1})	(km^3 yr^{-1})		Planting	Removal		Planting	Removal
Africa	4,320	-605(1,944)	8,986(5,616)	13,306	20,780		-14.0(45.0)	208.0(130.0)		-14(45)	208(130)
Asia	14,550	-1,979(5,835)	16,062(25,783)	30,612	32,140		-13.6(40.1)	110.4(177.2)		-14(40)	110(177)
Australia and Oceania	1,970	-412(725)	5,412(4,962)	7,382	6,405		-20.9(36.8)	274.7(251.9)		-21(36)	275(252)
Europe	3,240	-875(1,102)	813(1,426)	4,053	7,165		-27.0(34.0)	25.1(44.0)		-27(34)	25(44)
North and Central America	6,200	-806(2,034)	918(2,102)	7,118	13,910		-13.0(32.8)	14.8(33.9)		-13(33)	15(34)
South America	10,420	0(3,751)	1,908(17,559)	12,328	28,355		0.0(36.0)	18.3(168.5)		0(36)	18(168)
Totals	40,700	-4,676	34,098	74,799	109,755						

Values with parentheses are medians (and interquartile ranges). [1]From Table 1 of ref. 1. [2]Sum of total river runoff and median change due to removal. [3]Total precipitation from ref. 8, which is also the original source of the total river runoff values. [4]Median and IQR of runoff changes, as percentage of total river runoff. [5]Median and IQR of water yield predictions (each rounded to the nearest percent in the published database) for Evaristo and McDonnell's 442,319 "complete" catchments. These agree within roundoff error with the percentages calculated by dividing the change in runoff by the total runoff for each continent. This agreement demonstrates that the changes in runoff shown in Table 1 of ref. 1 were calculated by multiplying the median (and IQR) of the percentage water yield predictions by the total river runoff, rather than the runoff from forested areas.

Back-of-the-envelope calculations suggest different conclusions. Globally, evapotranspiration from forests is roughly 250 mm yr^{-1} greater than from croplands or grasslands[10], and multiplying this difference by the 40 million km^2 of global forests[7] yields a rough estimate of 10,000 km^3 yr^{-1}, less than one-third of Evaristo and McDonnell's[1] result. Even this may be an overestimate, because the lower evapotranspiration rates of grasslands partly reflect the fact that they often occur in drier climates; thus the difference between forest and grassland evapotranspiration may exaggerate the effects of converting forests to grasslands.

Concluding remarks. Evaristo and McDonnell are valued colleagues of ours, and we greatly appreciate their transparency in making their data and codes available, without which the issues described here would have been much harder to diagnose. We agree with them that streamflow response to forest management is an important issue that deserves a comprehensive analysis, including subsurface catchment characteristics as potential explanatory variables.

Readers should also keep in mind that this is not a purely academic exercise. How much, and under what conditions, forests should be cleared is an important policy question with wide-ranging consequences for economies, societies, and ecosystems. In that regard, we are concerned that the conclusion that "forest removal can lead to increases in streamflow that are around 3.4 times greater than the mean annual runoff of the Amazon River"[1] is overstated and could be misinterpreted. The Amazon flows continuously, but the streamflow benefits of forest clearing are transient, typically lasting only a few years, or at most decades, after felling[11]. One must also keep in mind that the water transpired by vegetation is an important source of precipitation farther downwind, estimated to account for roughly 40% of continental precipitation[10]. Thus, sustained large-scale clearing of forests would predictably lead to precipitation decreases and drying of continental interiors, although the precise magnitude of this effect remains difficult to constrain.

Data availability statement. All of the data analyzed here are available as described in the data availability and code availability statements of ref. 1, or from the cited references.

Author contributions statement. All authors discussed the issues raised here, and contributed to the writing. J.W.K. analyzed the data and led the writing effort.

References

1. Evaristo, J. & McDonnell, J. J. Global analysis of streamflow response to forest management. *Nature* **570**, 455-461 (2019).
2. Stoof, C. R. et al. Hydrological response of a small catchment burned by experimental fire. *Hydrology and Earth System Sciences* **16**, 267-285 (2012).
3. Ruprecht, J. K. & Schofield, N. J. Effects of partial deforestation on hydrology and salinity in high salt storage landscapes. I. Extensive block clearing. *Journal of Hydrology* **129**, 19-38 (1991).
4. Lawrence, P. A. & Thorburn, P. J. (eds.) *Changes in hydrology, soil fertility, and productivity of brigalow catchments following clearing* (Project Report RQR89002, Department of Primary Industries, Brisbane, 1989).
5. Thornton, C. M., Cowie, B. A., Freebairn, D. M. & Playford, C. L. The Brigalow catchment study: II. Clearing brigalow (Acacia harpophylla) for cropping or pasture increases runoff. *Australian Journal of Soil Research* **45**, 496-511 (2007).
6. SAS Institute Inc. *JMP 14 Profilers* (SAS Institute Inc., Cary, NC, 2018).
7. FAO. *Global Forest Resource Assessment 2015* (Food and Agriculture Organization of the United Nations, Rome, 2016).
8. L'vovich, M. I. & White, G. F. in *The Earth as Transformed by Human Action* (eds. Turner, B. L. et al.) 235-252 (Cambridge University Press, Cambridge, 1990).
9. Stednick, J. D. Monitoring the effects of timber harvest on annual water yield. *Journal of Hydrology* **176**, 79-95 (1996).
10. Ellison, D., Futter, M. N. & Bishop, K. On the forest cover-water yield debate: from demand- to supply-side thinking. *Global Change Biology* **18**, 806-820 (2012).
11. Andreassian, V. Waters and forests: from historical controversy to scientific debate. *Journal of Hydrology* **291**, 1-27 (2004).