Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-548-AC4, 2017
© Author(s) 2017. CC-BY 3.0 License.

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Water yield following forest–grass–forest transitions" *by* Katherine J. Elliott et al.

Katherine J. Elliott et al.

kelliott@fs.fed.us

Received and published: 6 January 2017

Interactive comment on "Water yield following forest–grass–forest transitions" by Katherine J. Elliott et al. Anonymous Referee #2 Received and published: 27 December 2016

Overall this is a good paper from a distinguished team using long-term data sets with a history of quality measurement. That's the good part. The bad part is that the paper does take a lot of following and reading, and after going through it a number of times, I'm still not entirely sure about the methodology. I think that this may reflect on the reviewer more than the writers, but the paper is a bit uncompromising in its terse presentation of information; to my mind that detracts a little from what is overall, a fine piece of work. Some of my comments relate to the need to perhaps "help" the readers

a little. I think that the paper would be improved a little by better graphics. Firstly a picture or two of WS6 at various stages would help. Similarly, a "time-line" of its treatments would also be useful. A small map showing the various watersheds would be good, too.

Response: We have included a Figure with the map of the watershed locations, weirs, and rain gages, now Figure 1. A time frame of WS6 treatments is provided in Table S1, supporting information. On line 153 "The disturbance regime in WS6, the treated watershed, was extensive (Table S1)."

I presume that the authors are trying to suppress a certain amount of detail – such as the development of Equation 1 (which presumably goes back a long way). For the non-hydrologists such an equation would be pretty enigmatic; I guess it is a judgement call for the authors, but it is asking readers a lot to swallow this at one gulp, so to speak. Ditto the frequency-pairing method.

Response: We have retained all of the Equations in the paper because they are important in understanding the hydrology methodology. The methods are also well referenced. We revised the section on methods based on comments from Reviewer #1 to provide addition detail: to clarify the model and provide previous use (references) we revised text on lines 222-261 as follows:

"We modeled WS6 annual Q and ET as a function of WS18, incorporating the effect of grass conversion and reforestation treatments over time. Annual Q was computed on a May–April water year to minimize the effects of year-to-year changes in storage, as soils are generally at their wettest by the beginning of May. The empirical chronological-pairing model was fit using PROC NLIN (SAS v9.4, SAS Institute, Cary, NC) and had the following form: Q ÌĆ_T=a+bQ_R+eM1t1+[M2c(h-1/(1+ãĂŰexpãĂŮˆ(-t2)))] (1) where, Q ÌĆ_T = predicted Q from treated watershed WS6 (mm yr-1), QR = measured Q from reference watershed WS18 (mm yr-1), M1 = management representing grass conversion; M1 = 1 for water years between and including 1960 and

1966, M1 = 0 otherwise, t1 = time since grass fertilization; t1 = water year – fertilization year for water years between and including 1960 and 1966 where fertilization years include water years 1959, 1961, and 1966, t1 = 0 otherwise, M2 = management representing reforestation after grass conversion; M2 = 1 for water years greater than or equal to 1967, M2 = 0 otherwise, t2 = time since reforestation after grass conversion; t2 = water year – 1967 for water years greater than or equal to 1967, t2 = 0 otherwise, P = annual precipitation (mm yr-1) a, b, c, e, h are fitted parameters. This overall modeling approach has been used in prior studies to assess the impact of forest management on Q (Ford et al., 2011; Kelly et al, 2016). The a+bQ_R term in EQ1 reflects the relationship between reference and treatment watersheds assuming no treatment. The increasing linear Q response (eM1t1 term in EQ1) accounts for the decline in annual grass production and water use after fertilization as noted by Hibbert (1969). The M2c(h-1/(1+ãĂŰexpãĂŮˆ(-t2))) term in EQ1 accounts for the exponential decline in Q as the forest regenerates that has been observed in numerous paired watershed experiments (Swank et al., 1988). As in Ford et al. (2011), we define the Q treatment response, DQ, as the difference between the observed Q in the treated watershed (QT) and that predicted by the model assuming no treatments had taken place (Q ÌĆ_T) : D_Q=Q_T-(Q ÌĆ_T; M1,M2=0). (2) The proportion of the variability explained by the model was quantified using the ratios of the error-to-total sum of squares and the total-to-error degrees of freedom as: R_adjustedˆ2=1-ãĂŰSSãĂŮ_E/ãĂŰSSãĂŮ_T × ãĂŰdfãĂŮ_T/ãĂŰdfãĂŮ_E . (3) Parameter estimates were interpreted as statistically significant at ïĄą = 0.05. Observed annual ET was computed as precipitation (P) – QT while expected ET with no treatment was computed as P - Q ÌĆ_T, both assuming the largely impermeable bedrock underlying the Basin that results in negligible deep groundwater losses (Douglass and Swank, 1972). Watershed P was estimated using a nearby eight inch (20.3 cm) National Weather Service standard rain gauge, SRG 96 (Laseter et al., 2012). The ET treatment response, DET, is then: D_ET=[ãĂŰP-QãĂŮ_T]-([ãĂŰP-Q ÌĆãĂŮ_T]; M1,M2=0) (4)."

The authors raise the very interesting point about non-stationary "controls" during these

long-term paired watershed studies. It is probably the weakest point of this approach once they get past four or five decades (but when these were put in, who envisaged them going that long?). The difficulty is that I am not sure what one might do on this matter. Perhaps the authors could talk about this a bit more?

Response: Long term studies such as this help researchers understand the changes in "reference" watersheds as well as treated watersheds, "non-stationary" aspects. Despite non-stationarity in reference watersheds over the long term, the paired watershed approach remains our best tool for evaluating the effects of forest management on Q and ET. However, our approach of scaling up daily water use from the tree to the watershed has the potential to provide additional information regarding how both treatment and reference watersheds change over time.

So overall, it is a fine paper but a bit hard-going in its methods. The "discussion" probably needs a bit of tightening since it is to some extent speculative. I think that it would be worthy of a longer paper in which the methods are teased out and there is more explanatory hydrograph detail. As an aside, I have always found it annoying that Dunford and Fletcher (1941) commented on the loss of the diurnal variation (if I recall correctly) but that no one ever seems to have looked at this again (i.e. how long did it take to reappear), and wonder if such a longer paper might also include indicators like this.

Response: we have revised the methods particularly with regard to the hydrology detail. See responses to Reviewer #1 above.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-548, 2016.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-548/hess-2016-548-AC4-supplement.pdf

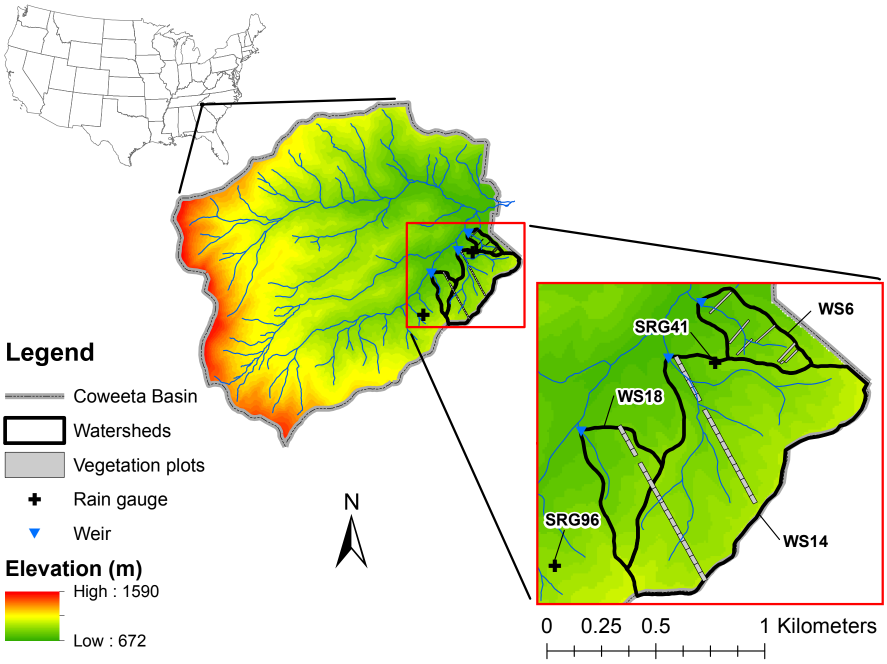

Fig. 1. Map of the Coweeta Basin

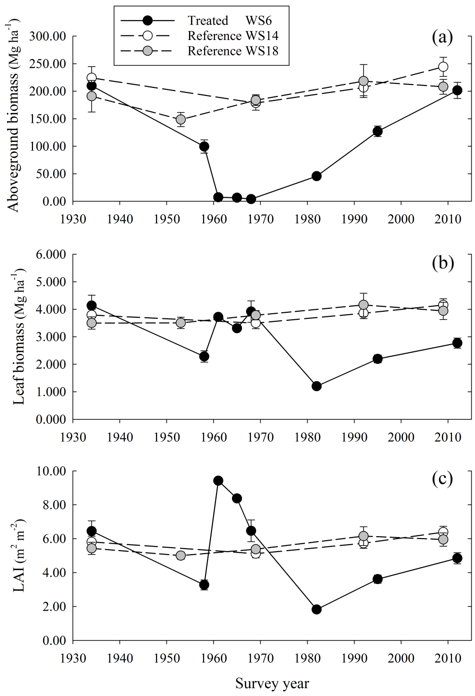

Fig. 2. Mean (a) aboveground biomass, (b) leaf biomass

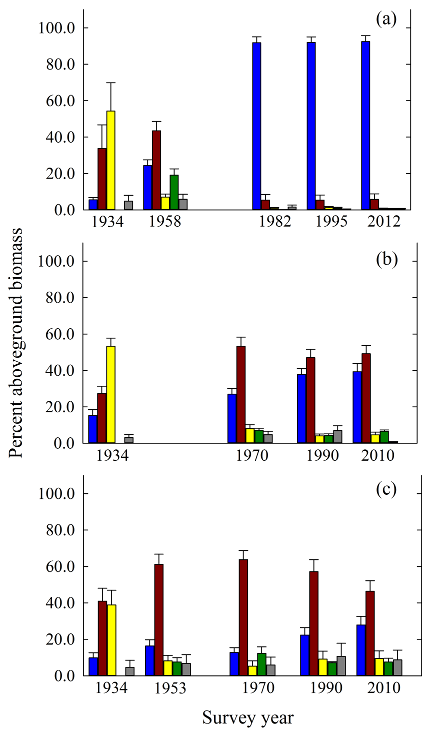

Fig. 3. Percetn aboveground biomass for the xylem

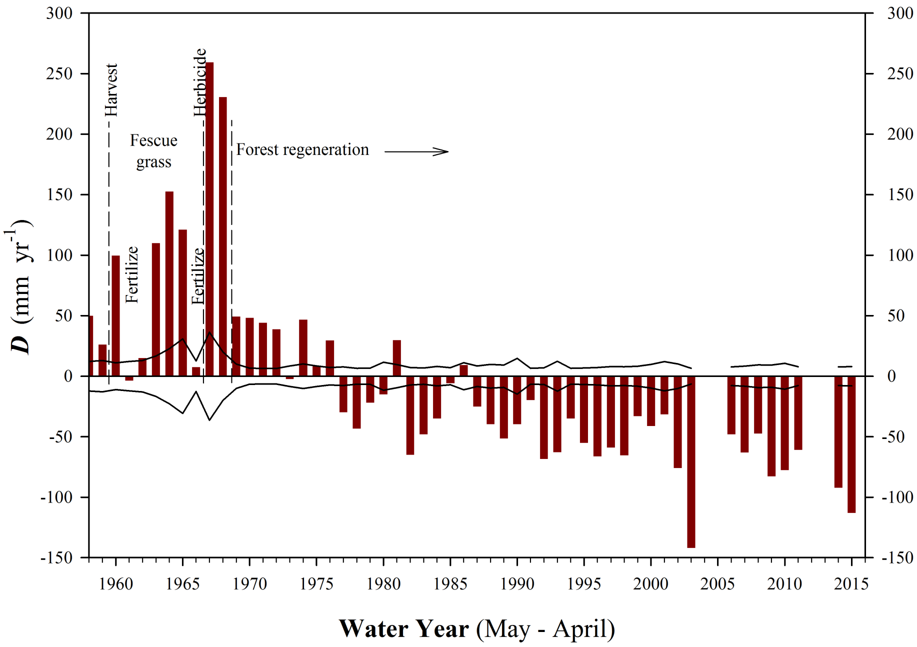

Fig. 4. Changes in water yield

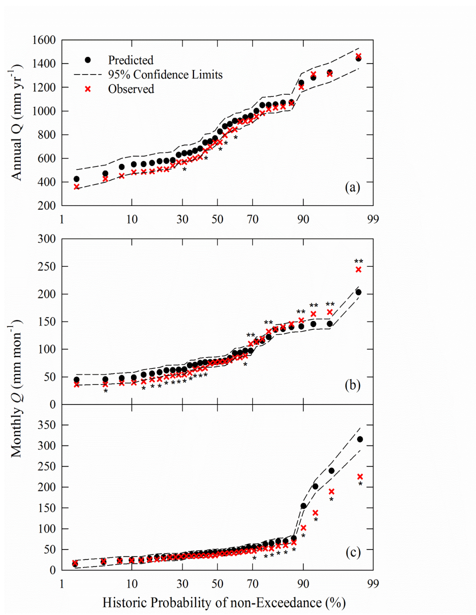

Fig. 5. Changes in the cumulative distribution function

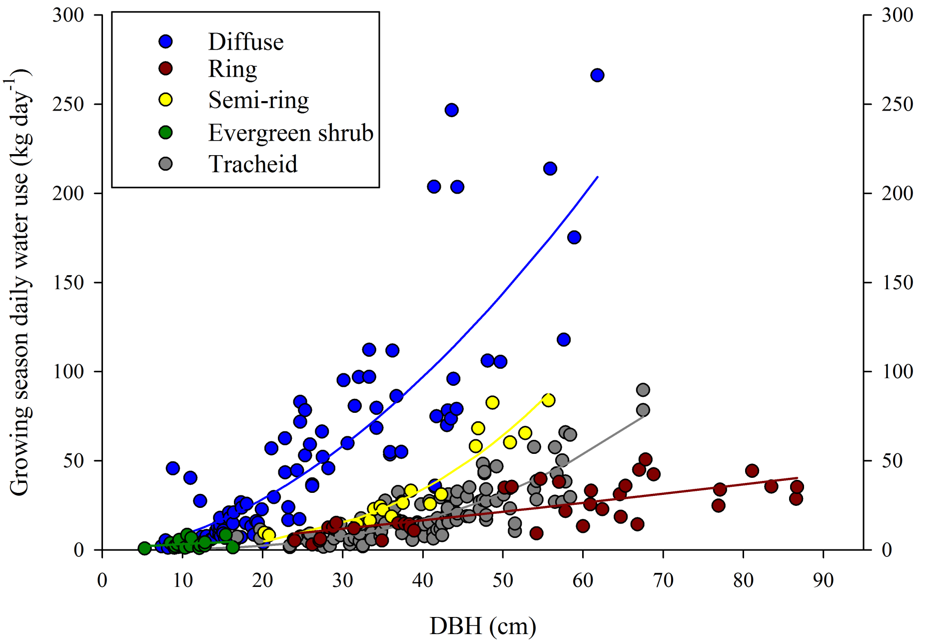

Fig. 6. Growing season dail water use of tree species

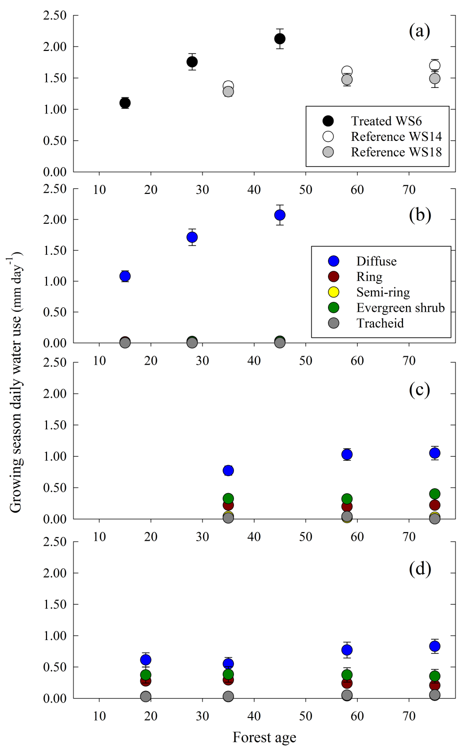

Fig. 7. (a) Mean growing season daily water use (DWU)