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# *Interactive comment on* "What determines the sign of the evapotranspiration response to afforestation in the European summer?" *by* Marcus Breil et al.

**Anonymous Referee #2**

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The authors have identified an important, and poorly understood, aspect of the effects of afforestation/deforestation in temperate latitudes on climate: do forests increase or decrease evapotranspiration (ET) compared with grasslands? Some observational studies suggest forests have greater rates of ET; some show the opposite. Many modeling studies show forests increase ET; others do not. The topic is fraught with confusion. I had hoped this manuscript would clarify the science and provide a strong, insightful understanding of forest ET and the factors controlling ET. However, by using a poorly documented model, and by not adequately describing the model, the rationale for parameterizations and parameter values, and the limitations of the model, the manuscript does not clarify the science and, instead, adds more confusion to the liter-



ature.

1. I have several concerns about the VEG3D model. From the few equations given in the manuscript, it appears to be a highly simplified land surface model. There is nothing wrong with that! But I suspect the findings of the study do not extend to more complex land surface models. The authors need to provide a thorough description of the model, justify the parameterizations used in the model, and justify parameter values. They also need to discuss how the simplifications of VEG3D might limit the generality of the results.

This is not the first time this issue has arisen. VEG3D was used in a previous study by the lead author:

Breil, M., and Coauthors, 2020: The Opposing Effects of Reforestation and Afforestation on the Diurnal Temperature Cycle at the Surface and in the Lowest Atmospheric Model Level in the European Summer. J. Climate, 33, 9159–9179

In full disclosure, I was a reviewer of that manuscript and noted in my review that VEG3D is a poorly documented model, is not widely known by the scientific community, and has not been tested in temperate forest/grassland simulations in comparison with flux tower measurements. That does not mean that the model is deficient or inappropriate for this study, but the description of ET provided in the current manuscript reveals some non-standard formulations in the model that likely limit the generality of the results.

1a. The authors describe the aerodynamic resistance ra used in the transpiration equation (eq 3). This is not a standard formulation of aerodynamic resistance (I have never seen it before). The resistance depends on wind speed at the top of the canopy, leaf area index, and some undescribed parameters. Classic textbooks on micrometeorology and boundary layer meteorology formulate the resistance using integrated flux-profile relationships between the apparent source/sink in the canopy (at a height equal to the roughness length [z0] plus displacement height [d]) and the lowest model level in the atmosphere [z]:

ra = [ln(z-d)/z0]ˆ2 / (kˆ2 * u)

u is wind speed at z. Depending on the specific model, z0 can be either that for momentum or for scalars, and ra is adjusted for atmospheric stability. What is the justification for eq 3, which seems to go back to two very old papers (Deardorff, 1978; Taconet et al., 1986)? Why is this equation used rather than classic boundary layer theory? It seems from eq 3 that roughness length only enters the model through wind speed at the top of the canopy (uaf), but there is no equation for uaf. It appears to go back to Goudriann's old work. This is very important, because the key outcome of the study is that surface roughness is the primary difference between forests and grasslands. Readers must understand precisely how surface roughness is used in the model and why particular formulations are used in the model.

1b. The formulation of canopy resistance to transpiration (eq 4) is also somewhat odd. It goes back to an equation in Deardoff (1978), in which canopy resistance depends on a specified minimum resistance that is modified for solar radiation and soil moisture. Most current-generation land surface models use an approach that couples photosynthesis and stomatal conductance through the Farquhar et al. (1980) photosynthesis model and semi-empirical stomatal conductance models such as proposed by Ball-Berry or Medlyn. In addition to light and soil moisture effects on stomatal conductance, those models also include temperature and vapor pressure deficit (VPD) effects on stomatal conductance. The VEG3D model ignore those latter two effects. That exclusion greatly limits the generality of the main finding of the study (that VPD, as modified by surface roughness, is a key determinant of differences in ET between forests and grasslands). The response of stomata to VPD is not considered (i.e. stomata close as VPD increases). Nor are the indirect effects of VPD on stomata through leaf temperature considered. Again, readers need to know why eq 4 is used in contrast with more common stomatal conductance models and what the implications of eq 4 are for the main findings of the study.



1c. The term (1+0.5*LAI)/LAI is common to both ra and rc. What does this term represent? It seems to be a scaling term for canopy LAI (i.e, from a leaf resistance to a canopy resistance). Aerodynamic resistance is commonly expressed per unit ground area. Why does ra need to be scaled by LAI?

2. The authors emphasize that differences between forest and grassland arise in terms of five model parameters: surface roughness, albedo, root depth, leaf area index, and minimum stomatal conductance.

2a. The justification for several parameter choices goes back to papers by Garratt (1993) and Henderson-Sellers (1993). There has been a lot of model development since then. How do these parameter choices compare with values used in the current generation of land surface models?

2b. Table 2 shows only a small difference in rmin between forest and grassland, and no difference between coniferous and deciduous forest. What is the justification for the parameter values? Are there physiological measurements that support them? The values for rmin are very important to the results of the study. The relative contributions of aerodynamic resistance and canopy resistance to total resistance determine the model sensitivity to roughness length. The fact that rmin is similar for all vegetation precludes physiological differences in stomatal conductance from determining differences in ET.

2c. No details are given on how root depth affects transpiration, or how the root depth parameter is used in the model. The root depth of deciduous forest is twice that of coniferous forest. Is this the reason for the differences between deciduous and coniferous forests when they are converted to grassland?

2d. No details are given for albedo. What is the radiative transfer parameterization in the model? Land surface models typically simulate radiative transfer for visible and near-infrared wavebands and for direct and diffuse radiation. Albedo is a complex result of leaf and stem reflectances, leaf and stem area index, solar zenith angle, and soil moisture. Because only a single albedo is listed as a parameter in Table 2, this makes me think there is no such complex radiative transfer parameterization in VEG3D and instead the model uses a bulk surface albedo that is prescribed as a parameter. Readers need further information.

3. A striking aspect of Figure 2 is the difference between coniferous and deciduous forests when replaced with grassland. Summer latent heat fluxes are larger in coniferous forest compared with grassland but are smaller than grassland in deciduous forest. This pattern is universally consistent throughout the domain, except for southern Spain and Turkey (smaller latent heat fluxes compared with grassland in a mostly coniferous forest region). The authors acknowledge the influence of forest type (lines 165-166), but for the most part discuss their results in terms of Northern Europe versus Southern/Central Europe. For example, the authors frame their conclusions as: "In Northern Europe evapotranspiration is increased with afforestation, in Southern and Central Europe evapotranspiration is decreased" (lines 261-262). The differing results of coniferous and deciduous forests are not even mentioned in the abstract. I would like to see more of a discussion of coniferous versus deciduous forests.

3a. What, specifically, are the differences between these forests that cause the results? One generally thinks of coniferous forests as having a more conservative water-use strategy than deciduous forests (seen, for example, in higher stomatal resistance). But both forests have the same minimum resistance. Is the different response related to rooting depth? In their analysis of soil water (Figure 3), the authors suggest it is not but the analysis is not definitive. It would be better to look at the soil moisture stress term in canopy resistance.

3b. Are the results consistent with observations? What do flux towers show? What does MODIS ET show (but remember that MODIS ET is a modeled product).

4. The crux of the study is Figure 4, which shows the difference in saturation deficit between the forest and grassland simulations. Saturation deficit decreases for forests throughout Europe, with a particularly large decrease in latitudes south of about 40N.



The authors discuss the results in light of VPD, resistances, and other parameters that affect transpiration (lines 219-234). No data or figures are provided to justify the interpretation. Skeptical readers need to see more evidence that supports the argument if they are to believe the study.

5. Figure 5d: Why does net shortwave radiation change when roughness length is changed to that of grassland?

6. Lines 288-290: The authors state that "the dependency of the evapotranspiration rates of forests and grasslands on the latitude is also documented in satellite observations (e.g. Li et al., 2015), showing for example higher evapotranspiration rates of grasslands in South-Eastern Europe, while in Central and Northern Europe evapotranspiration is lower than in forests (Duveiller et al., 2018)". Li et al used MODIS ET, which is a modeled product. What did Duveiller et al base their analysis on? And, remember, that the more striking aspect of Figure 2 is not the latitudinal dependence but the difference between coniferous and deciduous forests. What do observations say about that difference?

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