Reviewer 1 (R1):

We thank R1 for the comments and firmly object the statement on 'misguidance' (below) which appears to be potentially misleading particularly when a large part of the community have outright enthusiasm in developing analytical approaches for estimating terrestrial evapotranspiration (E) (or latent heat flux, λE) and sensible heat fluxes (H) to overcome the ambiguities associated with parameterizations of aerodynamic (g_A) and canopy surface conductances (g_C) (**Kleidon et al., 2014; Matheny et al., 2014; Ershadi et al., 2015**). Besides, we would like to clarify that the abbreviation for our model framework is "STIC" and not "STICS" as referred to by R1.

Major comments:

(1) My main concern is that the manuscript does not present any new theory (and on top of that uses an approach (STICS) that in my opinion is misguided, despite the fact that it has been published).

Summary response: R1 claims that the manuscript does not present any new theory. To the opinion of the authors, this claim is flawed because STIC (Surface Temperature Initiated Closure) introduced a novel analytical method to integrate radiometric surface temperature (T_R) into the Penman-Monteith model to overcome the limitations associated with empirical parameterisations of the aerodynamic and canopy surface conductances (g_A and g_C) which are not directly measurable either at the canopy-scale or at the large spatial grid-scale. To our knowledge, this research objective is unquestionably novel and the behavior of the analytically retrieved canopy-scale conductances as well as transpiration are compliant with the theory earlier postulated in the literatures (Jarvis and McNaughton, 1986; Monteith, 1995, Raupach, 1998). In addition to its simplicity, STIC has the capabilities for generating spatially explicit surface energy fluxes and independent of submodels for boundary layer developments.

Detailed response: The most tangible accomplishment and uniqueness of STIC (STIC1.2) is the physical integration of land surface temperature (i.e., radiometric surface temperature, T_R) into a combined framework of the Penman-Monteith (PM) and Shuttleworth-Wallace (SW) model for simultaneously estimating E, H, g_A, g_C, surface moisture status, and E components (evaporation, E_E and transpiration, E_T). The intrinsic link between the PM-SW model and $\mathbf{T_R}$ emanates through the first-order dependence of the biophysical conductances (g_A and g_C) on the aerodynamic temperature ($\mathbf{T_0}$) (through $\mathbf{T_R}$) and soil moisture (through $\mathbf{T_R}$). However, until now the explicit use of $\mathbf{T_R}$ in the PM-SW model was hindered due to the unavailability of any direct method to integrate $\mathbf{T_R}$ into these models, and, furthermore, due to the lack of physical models expressing biophysical states of vegetation as a function of $\mathbf{T_R}$. Therefore, the majority of the E modeling approaches strongly rely on surface reflectance and meteorology; and thermal approaches require significant parameterization of land surface properties (e.g., g_A and g_C) which are very empirical in nature (Schulz and Beven, 2003; Prihodko et al., 2008; Bonan et al., 2014; Ershadi et al., 2015).

To bridge this gap, the STIC methodology was developed as a novel thermal-based biophysical scheme for directly estimating E over terrestrial ecosystems by leveraging the combined strength of T_R observations and physically-based models

(Mallick et al., 2014; 2015). In addition to physically integrating T_R observations into a combined PM-SW framework, STIC1.2 also establishes of a feedback loop describing the relationship between T_R and E, coupled with canopy-atmosphere components relating E to aerodynamic temperature (T_0) and vapor pressure (e_0) (in STIC1.2). **By blending T_R with standard SEB principles and vegetation-atmosphere exchange biophysics, STIC formulates multiple state equations in order to eliminate the need of exogenous parametric submodels for the surface and aerodynamic conductances, aerodynamic temperatures, and land-atmosphere coupling. Instead these 'internal states' are numerically retrieved.** Originally designed for application to thermal remote sensing data from Earth observation sensors, the STIC framework exploits observations of $\mathbf{T_R}$, radiative, and meteorological variables including net radiation (R_N), ground heat flux (G), air temperature (T_A), relative humidity (R_H) or vapor pressure (e_A) at a reference level above the surface, and can be applied over any ecosystem, provided the necessary input variables are available.

We hope this extended summary will help expanding R1's constrained judgement on STIC.

(2) STICs is misguided because it ends up with an aerodynamic conductance that does not depend on wind speed and introduces a soil moisture stress term that only depends on atmospheric variables.

Summary response: R1 claims that STIC is misguided due to two reasons. According to R1, the first reason should be that aerodynamic conductance does not depend on wind speed (W_s). **It should be noted that, in one of the hallmark papers by Choudhury and Monteith (1986), it is clearly stated that 'aerodynamic conductance determined by wind speed and roughness is assumed to be unaffected by buoyancy'. Strictly, the aerodynamic conductance should be replaced by a term which accounts for radiative as well as convective heat transfer'.** Although incorporation of W_s data has almost become a dogma (Foken, 2006) in the field of land surface energy balance modelling, there are several widely accepted evapotranspiration estimation approaches that do not incorporate W_s, for example, maximum entropy production approach (Kleidon et al., 2014), evaporative fraction approach (Jiang and Islam, 2001; Batra et al., 2006), complementary relationship approach (Venturini et al., 2008) etc.
On the second claim of R1 regarding the estimation of soil moisture stress that only depends on atmospheric variables, **the claim is not substantiated because the water stress factor was estimated by combining the radiometric surface temperature (T_R) with air temperature (T_A), dewpoint temperature (T_D), and near surface dewpoint temperature (T_{SD}) as explained in Mallick et al. (2015). The procedure is also briefly explained in the appendix of the current manuscript**

Detailed response: **Given the importance of g_A for evapotranspiration (E) estimates there are overriding cases for getting this 'right' in the surface energy balance models (Prihodko et al., 2008; Hong et al., 2010; Gibson et al., 2011; Holwerda et al., 2012; Gokmen et al., 2012; Morillas et al., 2013).** However, if the empirical g_A models currently provide accurate estimates of E for the wrong reasons then this status quo has to be questioned, especially as errors like this might become important when predicting E under future boundary conditions. **Furthermore, it is not obvious that W_S-based models currently provide accurate estimates of g_A, in particular at the grid-scale (e.g., 1 km and above) where bundles of site specific parameters are required (which cannot be measured).**

We would like to bring forward the following arguments concerning W_S-based g_A estimation.

(a) As highlighted in several studies (Monteith and Unsworth, 2008; Holwerda et al., 2012), the momentum transfer equation for g_A estimation based on the Monin-Obukhov Similarity Theory (MOST) only holds for an extended, uniform, and flat surface (Foken, 2006). MOST tends to fail over rough surfaces due to breakdown of the similarity relationships for heat and water vapour transfer in the roughness sub-layer, which results in an underestimation of the 'true' g_A by a factor 1-3 (Thom et al., 1975; Chen and Schwerdtfeger, 1988; Simpson et al., 1998; Holwerda et al., 2012). Despite some of the boundary layer studies based on parameterized friction velocity (u*) demonstrated the validity of MOST (subjected to tuning and calibration) (Harman and Finnigan, 2007; 2008), a considerable number of studies have casted scepticism on the validity of u* parameterization in the framework of MOST (Foken, 2006; Holwerda et al., 2012; van Dijk et al., 2015). **It is imperative to mention that g_A is one of the main anchors in the PM-SW model because it not only appears in the numerator and denominator of these models, g_A also provides feedback to g_C, aerodynamic temperature, and vapor pressure (seminal paper of Jarvis and McNaughton, 1986).** Therefore, the estimates of E and interception evaporation (Ei) in the PM-SW framework are robustly sensitive to parameterization of g_A and stable E estimates might be possible if g_A estimation is unambiguous (Holwerda et al., 2012; van Dijk et al., 2015). Consequently, our aim was to find analytical solution of g_A, and through algebraic reorganisation of surface energy balance equation we are able to do so. **Given the lack of consensus in the community on the 'true' g_A, we treat STIC1.2 derived non-parametric g_A to be the aerodynamic conductance that satisfies the PM-SW equation for estimating evaporative fluxes.**

(b) **In the state-of-art E modeling, the parametric g_A sub-models are stand alone and empirical**, **and do not provide any feedback to the canopy (or surface) conductances (g_C), aerodynamic temperature (T_0), and aerodynamic vapor pressure deficit (D_0)**. However, **g_A** is an internal state that provides physical feedback to E and H by influencing **T_0**, **D_0**, and **g_C**. Large g_A indicates **small gradients of vapor pressure deficit between the air and canopy boundary layer** and hence strong coupling between canopy and atmosphere (Jarvis and McNaughton, 1986). **These biophysical interactions are entirely overlooked in the land surface parameterizations of g_A (but are included in STIC). STIC1.2 consists of a feedback describing the relationship between T_R and E, coupled with canopy-atmosphere components relating E to T_0 and e_0. The equations are explicitly stated in the Appendix (eq. A2 to A8) of the manuscript and the detailed descriptions are in L595 to L627.**

(c) **Additional challenges** in grid-scale or spatial-scale g_A estimation are the **requirements of numerous site specific parameters** (e.g., vegetation height, measurement height, vegetation roughness, leaf size, soil roughness) and **coefficients** needed to correct the atmospheric stability conditions (Raupach, 1998). **These informations are required to fulfil the set of assumption established around 1960's**, that can and should be questioned by the community if we want to make science advance in the field of surface energy balance modeling.

(d) The enhanced errors in E estimates in water-limited regions due to uncertain g_A parameterisation (Gibson et al., 2011; Timmermans et al., 2013; Morillas et al., 2013; Castellvi et al., 2016) and repeated adjustment of different vegetation as well as soil parameters in the conductance equations to obtain a better E validation (Gokmen et al., 2012) questions the validity of wind driven non-stationary g_A parameterisations. **It solicits**

for revisiting the state-of-art g_A parameterisations and rethinking to develop a calibration independent g_A modelling framework.

(e) The credibility of STIC1.2 g_A estimates is shown in the figures (Fig. 1 and Fig. 2). While Fig. 1a in the manuscript illustrates the differences in the g_A magnitude between forest and pasture, Fig. 2a displays an independent comparison of STIC-g_A versus u*-based g_A. Fig. 2e and 2f showed that T_R, vapor pressure deficit (D_A), and net available energy (ϕ) (difference between net radiation, R_N and ground heat flux, G) can explain 42% to 83% variability of the u*-based g_A. **These correlations and scatterplots between u*-based g_A with radiative and meteorological variables clearly emphasize the explanatory power of these variables to characterise wind-driven g_A and the appropriateness of deriving an analytical g_A without wind speed. <u>This also supports the findings of Villani et al. (2003) which stated that during unstable surface layer conditions the major source of net available energy is located at the canopy top and drives the convective motion in the layers above.</u> Hence, the value of g_A is not only controlled by wind speed as advocated by R1.**

<u>Regarding R1's claim on characterising the water stress (M) as a function of atmospheric variables</u>, **we would like to draw further attention that the stress function was estimated as a function of T_R, air temperature (T_A), dewpoint temperature (T_D), and near surface dewpoint temperature (T_{SD}) (Mallick et al., 2015) which is clearly stated in the Appendix (L617). In STIC1.2, T_{SD} was estimated in an iterative mode to establish a feedback between the water stress, T_R, T_{SD}, and evapotranspiration.** Since R1 acknowledges to be aware of the published STIC methodology, the immediate fact supposed to be reflected in R1's understanding are the titles of the two previous manuscripts '**A surface temperature initiated closure of the surface energy balance fluxes**' and '**Reintroducing radiometric surface temperature into the Penman-Monteith formulation**' published in ***<u>Remote Sensing of Environment and Water Resources Research</u>***, respectively.

(3) The paper then uses Amazonian micrometeorological data to compare a range of gA and gC terms. No measurements of gC are used to provide verification.

Response: This exercise could not be performed as direct canopy-scale g_C observations are not possible with current measurement techniques. Although leaf-scale measurements of g_C are relatively straightforward, these values are not comparable to values retrieved at the canopy-scale. However, assuming u*-based g_A as baseline aerodynamic conductance, we had estimated canopy-scale g_C by inverting the PM equation ($g_{C\text{-}INV}$) to evaluate $g_{C\text{-}STIC}$. The comparison between $g_{C\text{-}STIC}$ and $g_{C\text{-}INV}$ over forest and pasture is illustrated in Fig. 3a, and the results are discussed in L305 to L315 of the manuscript.

We shall make this point explicit in revised version of the manuscript.

(4) A large number of plots are then presented where STICS variables are plotted against meteorological variables in a host of different ways. I am not surprised to see that these dependencies exist as all of them are intrinsic to the model. Also, because all of them are interdependent I am not sure how much realism there ultimately is in the findings.

<u>Summary response:</u> We do not agree with this statement of R1. Firstly, comparing different g_A estimates, linking the wind driven g_A estimates with some <u>independent</u>

variables (Fig. 2), and STIC driven g_A estimates with some interdependent variables (Fig. 6 to Fig. 8) is not a matter of choice, but a necessity, as it evident to any reader. The same is applicable to Fig. 6 to Fig. 8 for g_C. Secondly, despite the transpiration and evaporation estimates are interdependent with g_C and g_A (as shown in **Fig. 6 to Fig. 8); the figures reflect the credibility of the conductances as well as transpiration estimates by realistically capturing the hysteretic behavior between biophysical conductances and water vapor fluxes which is frequently observed in natural ecosystems (Zhang et al., 2014, Renner et al., 2016). Fig. 8 (a, b) also affirms that the conductance-transpiration-vapor pressure deficit relationships are compliant with the stomatal feedback-response theory earlier postulated from observational evidences (Monteith, 1995).**

Detailed response: Fig. 2 illustrates the diagnostic potential of thermal (T_R), radiative (ϕ), and meteorological (D_A) variables to explain the wind driven g_A variability (wind driven g_A is independently estimated).

Fig. 6 and 7 explains the 'hysteresis' between transpiration and conductances which shows the degree of hysteresis was larger in the dry season than in the wet season. These results are compliant with the theories earlier postulated from observations that the magnitude of hysteresis depends on the radiation-vapor pressure deficit lag, while the soil moisture availability is a key factor modulating the hysteretic transpiration-vapor pressure deficit relation as soil moisture declines (Zhang et al., 2014; O'Grady et al., 1999; Jarvis and McNaughton, 1986). **This shows that despite independent of any predefined hysteretic function, the interdependent conductance-transpiration hysteresis is still captured in STIC1.2 (which are generally observed in natural ecosystems).**

Fig. 8 (a and b) confirms the 'stomatal feedback-response' hypothesis as postulated by Lt. John Monteith (Monteith, 1995), which states that a decrease in stomatal conductance with increasing vapor pressure deficit is caused by a direct increase in transpiration (Monteith, 1995) and stomata responds to the changes in the air humidity by sensing transpiration, rather than vapor pressure deficit. This feedback mechanism is found because of the influence of vapor pressure deficit on both stomatal conductance and transpiration, which in turn changes vapor pressure deficit by influencing the air humidity (Monteith, 1995).

Fig. 8c shows the complex interaction between g_C, radiometric surface temperature (T_R) and vapor pressure deficit (D_A). This also answers why different parametric g_C models produce divergent results.

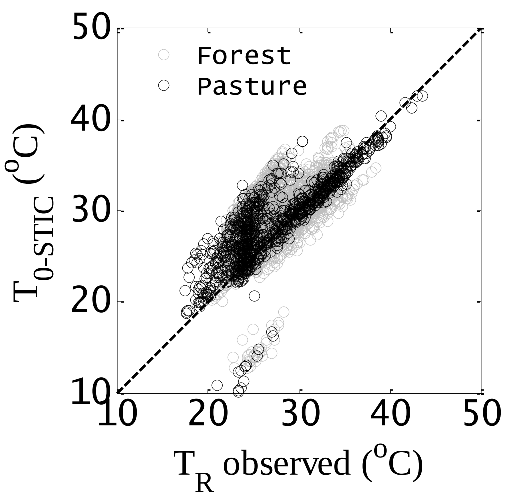
Fig. 8d emphasizes the behaviour of g_A according to existing theory that under extremely high atmospheric turbulence (i.e., high g_A), a close coupling exists between the surface and the atmosphere, which causes T_R and T_A to converge (i.e., $T_R - T_A \rightarrow 0$).

(5) Furthermore STICS assumes that T0 = TR and yet the manuscript does not mention the potential implications of this assumption, nor the fact that considerable errors can be made when measuring TR.

Summary response: This comment by R1 is incorrect. **In STIC1.2, T_0 is analytically estimated by integrating T_R into a combined PM-SW framework. The analytical**

<u>expression of</u> T_0 <u>is dependent on M and the estimation of M is based on</u> T_R <u>as described in the Appendix of the current manuscript.</u> <u>T_0 is a nonlinear function of T_R in STIC and they are not assumed equal.</u> To further address R1's point on the assumption that T_0=T_R, we show here an intercomparison of retrieved T_0 versus T_R for forest and pasture (figure below). This indicates the distinct difference of the retrieved T_0 from T_R for the two different biomes, which proves that R1's claim to be invalid.

We will include the figure of T_0 versus T_R and necessary descriptions in the Appendix of revised manuscript. **We will also address this point explicitly (i.e., $T_0 \neq T_R$) in the theoretical section (section 2.1) of the manuscript.**



<u>Figure:</u> Aerodynamic temperature obtained from STIC1.2 ($T_{0\text{-STIC}}$) versus radiometric surface temperature (T_R) over two different biomes in the Amazon basin. The regression equation of line of best fit is $T_{0\text{-STIC}}$ = 0.67(±0.10)T_R + 10.59 (±2.79) with *r* = 0.65

<u>Detailed response:</u> One of the core objectives of the original STIC formulation was to physically integrate T_R into the PM model to constrain the conductances. **This is done by estimating an aggregated surface moisture availability (or water stress factor) which is an emphatic function of T_R.** A detailed description of the STIC state equations is given in Mallick et al. (2015) and novel part of STIC1.2 is described in the Appendix of the current manuscript.

(6) In the end I feel I have learned little new and what has been presented is tentative and therefore potentially misleading. This is underlined by sentences (line 325-329) such as "The evaluation of the conductances and surface energy fluxes indicates some efficacy for the STIC derived fluxes and conductance estimates ..... As a result we feel some justification for exploring the canopy-scale biophysical controls on ET and EE generated through the STIC framework".

Response: We do not agree with the reviewer's impression. The major novelties of the present manuscript are as follows:

The bafflement of evapotranspiration originates from a supply-demand chain reaction where net radiation and soil moisture represents the supply side and the atmospheric vapor pressure deficit represents the demand side. This supply-demand chain reaction accelerates the biophysical feedbacks in evapotranspiration and understanding these biophysical feedbacks is necessary to assess the terrestrial biosphere response to water availability. In this context, the entire manuscript is about understanding the canopy-scale biophysical controls on transpiration and

evaporation over the Amazon basin. The two critical biophysical state variables (i.e., g_A and g_C) in the PM equation are the unobserved components which cannot be measured directly. Therefore, we explored the radiative (net radiation and ground heat flux), meteorological (air temperature and relative humidity), and thermal (radiometric surface temperature) information, and developed the STIC framework to analytically estimate these variables in an internally consistent manner (as described in the manuscript). However, before understanding the controls of g_A and g_C on transpiration and evaporation, some indirect evaluation of these two biophysical states was necessary, and hence the sentences in line 325 to 329 are justified.

Detailed comments:

Line 81-82: "An intensification of the Amazon hydrological cycle was observed in the past two decades characterised by increased temperatures and more frequent droughts and floods" How are increased air (?) temperatures directly linked to hydrological cycle? If it is surface temperatures then say this, but this would mean a decreased ET (hence the floods?)

Response: We agree. We will make the necessary correction in the revised manuscript as "An intensification of the Amazon hydrological cycle was observed in the past two decades characterised by increased air and land surface temperatures and more frequent droughts"

Line 86: "the Amazon forest may become an increasing carbon source". Should this be "increasingly become a net source of carbon?

Response: We will clarify this in the revised version on the manuscript.

Line 97-104: I disagree with the final point made in this section: GC does not include the conductance relating to bare soil. If you would have called it the surface conductance instead and defined it via the PM Big leaf equation I would have agreed.

Response: **We do not agree.** For a dense canopy, g_C in the PM equation represents the canopy surface conductance. Although it is not equal to the canopy stomatal conductance, it contains integrated information of the stomata. For a heterogeneous landscape, g_C in the PM equation is an aggregated surface conductance containing information of canopy and soil.

Lines 111-126 are stating the obvious. Where is this going?

Response: Line 111 – 126 explained the unresolved challenges and problems associated with $\mathbf{g_A}$ and $\mathbf{g_C}$ parameterisations. **If these are obvious, R1's previous claims on $\mathbf{g_A}$ appear to be unfounded.**

These statements are needed to recognize the need of a non-parametric $\mathbf{g_A}$ and $\mathbf{g_C}$ modeling framework.

Line 136: Why is the partitioning between soil evaporation and transpiration deemed so important in the Amazon? Soil evaporation must only make up a small part of total ET. Will this soil term affect flooding, atmospheric circulation etc. I highly doubt this.

Response: **We intended to address 'evaporation', not 'soil evaporation'**. In the Amazon forest, although the soil evaporation has negligible contribution, it is the interception evaporation that has substantial contribution in the total evaporative fluxes, and, therefore the partitioning of 'evaporation (λE_E)' and 'transpiration (λE_T)' is significant.

Line 141-143: "Given the persistent risk of deforestation, the ecophysiological changes of different plant functional types (PFTs) are expected to be reflected in gA and gC and EE and ET". I really do not understand what is meant by this sentence.

Response: Deforestation alters the radiation interception, surface temperature, surface moisture, associated meteorological conditions, and vegetation biophysical states. Conversion from forest to pasture will change the g_A/g_C ratio of the ecosystem and dry-wet evapotranspiration partitioning.

Necessary changes will be incorporated in the revised version of the manuscript.

Line 154-157: The surface temperature is already implicit in the PM equation as it combines the energy balance with bulk transfer equations.

Response: The surface temperature (T_R) was eliminated from the derivation of the PM equation by expressing the slope of the saturation vapor pressure at ambient air temperature. However, in the seminal paper titled 'Evaporation and Surface Temperature' (Monteith, 1981), Lt. John Monteith described the role of leaf temperature in constraining the biophysical conductances. Although T_R is implicit in the net radiation (Rn), which appears in the numerator of the PM equation, it may be noted that Rn has a relatively weak dependence on T_R (compared to T_R sensitivities of soil moisture and E). No universally agreed formulation is available that physically constrains g_A and g_C by using T_R. Development of STIC is based on the assumption that the intrinsic link between the PM-SW model and T_R emanates through the first-order dependence of the biophysical conductances on aerodynamic temperature (T_0) and soil moisture (through T_R). Hence, the conductances are explicitly constrained by using T_R information as described in the manuscript.

Line 179-181: "The retrieval of gA, gC, and E are based on finding a 'closure' of the PM equation using the STIC framework". In my opinion, the PM is already closed, see my point above. It calculates ET from Rn-G, and H is implicitly in there. Please study books such as those by Hamlyn Jones to see how PM equation is derived.

Response: The PM equation is 'closed' upon the availability of canopy-scale measurements of the two unobserved biophysical conductances (g_A and g_C) and if we assume the empirical models of g_A and g_C to be authentic. However, neither g_A nor g_C can be measured at the canopy-scale or at larger spatial scales. Furthermore, as shown by several recent studies (Matheny et al., 2014; van Dijk et al., 2015) a most appropriate or correct g_A-g_C model is currently not available. This implies that **a true 'closure' of the PM equation is only possible upon analytical estimation of the conductances.**

Line 184: This should be 'radiative temperature'.

Response: Necessary changes will be incorporated.

Line 203: You have now tacitly assumed that T0 = TR. There is a host of literature references that will tell you otherwise.

Response: The explanation is already provided above; there is no assumption on the equality between T_R and T_0. **We will address this point explicitly (i.e., $T_0 \neq T_R$) in section 2.1 of the revised manuscript.**

Line 204-205: PM equation is already closed. This assumption of energy balance closure is implicit in its derivation. But maybe I do not understand what you mean by this statement.

Response: As mentioned earlier, the PM equation is closed if measurements of the two unobserved biophysical conductances (g_A and g_C) are available. However, g_A and g_C cannot be directly measured at the canopy-scale and there is no universally agreed g_A and g_C model. By the term 'closure', we mean **actual 'closure'** of the PM equation by finding analytical solutions of g_A and g_C. This was done by solving 'n' equations and 'n' unknowns as described in equation 2 to 5 in the manuscript. The derivation of these equations is already explained in Mallick et al. (2014; 2015). We will briefly explain their derivation in the Appendix of the revised manuscript.

Line 225-227: "The estimates of EE in the current method consists of aggregated contribution from both interception and soil evaporation, and no further attempt is made to separate these two components". This is a considerable weakness in the approach seeing leaf area index and hence interception is so large for large parts of the Amazon and soil evaporation will be negligible. You are making this point yourself a few sentences later (line 232) Also: these two types of evaporation fluxes take place at very different source heights, so their GA will be very different, further weakening your approach.

Response: We do not agree. This is not a considerable weakness, but a fact which is clearly stated instead of withholding it. At the outset, the biophysical controls on evaporation and transpiration are mentioned, and no claim is made on understanding soil evaporation, interception evaporation etc.

We agree that different g_A exists for soil-canopy, sun-shade, and dry-wet conditions; which is currently integrated into a lumped g_A (given the big-leaf nature of STIC). From the big-leaf perspective, it is generally assumed that the aerodynamic conductance of water vapor and heat are equal (Raupach, 1998). However, for obtaining partitioned aerodynamic conductances, explicit partitioning of evapotranspiration is needed, which is beyond the scope of the current manuscript. We will mention this fact in the revised version of the manuscript.

Line 285: "The conductances showed a marked diurnal variation expressing their overall dependence on net radiation, vapor pressure deficit, and surface temperature". What conductance are you referring to here? gA or gC? Or both? Note that gA generally does not depend on net radiation or VPD etc., although it does in STICS.

Response: Here, we are referring to both g_A and g_C as clearly stated in Fig. 1 and the related descriptions as stated in line 279 to 288. The role of g_A is associated with the role of convection (Choudhury and Monteith, 1986) according to the surface energy balance principle as follows.

Neglecting horizontal advection and energy storage, the surface energy balance equation is written as follows:

$$\phi = \lambda E + H \qquad (1)$$

Where $\phi \cong R_N - G$, with R_N being net radiation, and G being the conductive surface heat flux or ground heat flux, H is the sensible heat flux and λE is the latent heat flux.

The sensible and latent heat flux can be expressed in the form of aerodynamic transfer equations (Boegh et al., 2002; Boegh and Soegaard, 2004) as follows:

$$H = \rho c_P g_A (T_o - T_A) \qquad (2)$$

$$\lambda E = \frac{\rho c_P}{\gamma} g_A (e_0 - e_A) = \frac{\rho c_P}{\gamma} g_C (e_0^* - e_0) \qquad (3)$$

Where T_A is the air temperature at the reference height (z_R), e_A is the atmospheric vapor pressure (hPa) at the level at which T_A is measured, e_0 and T_0 are the atmospheric vapor pressure and air temperature at the source/sink height, or at the so-called roughness length (z_0), where wind speed is zero. They represent the vapor pressure and temperature of the quasi-laminar boundary layer in the immediate vicinity of the surface level (Figure A1), and T_0 can be obtained by extrapolating the logarithmic profile of T_A down to z_0. e_0^* is the saturation vapor pressure at T_0 (hPa).

By combining eq. 1, 2, and 3 and solving for g_A, we get

$$g_A = \frac{\phi}{\rho c_P \left[(T_o - T_A) + \left(\frac{e_0 - e_A}{\gamma} \right) \right]} \qquad (4)$$

Equation 4 clearly portrays the dependency of g_A on net available energy and vapor pressure.

Given R1's disposition on the wind speed dependent empirical g_A models based on the Monin-Obukhov Similarity Theory (MOST), it is important to mention that the Monin-Obukhov Length (L) is a function of evapotranspiration (E) (Brutsaert, 1982), and E is strongly dependent on the net available energy as well as vapor pressure deficit. The functions below describes the dependence of g_A on net available energy (ϕ) (= net radiation – ground heat flux) and vapor pressure deficit in addition to T_0-T_A, despite g_A being generally estimated from wind speed information.

$$g_A = f\{L\} \qquad (5)$$

$$L = \frac{u^* \rho C_P T_A}{g(H + 0.61 C_P T_A E)} \qquad (6)$$

$$u^* = f\{L, E, specific\ humidity\ gradieant, wind\ speed\} \qquad (7)$$

$$E = f\{R_N, D_A, soil\ moisture, T_R\} \qquad (8)$$

Here u^* is the friction velocity (m s^{-1}), ρ is the air density (kg m^{-3}), c_P is the specific heat of air (1004 j kg^{-1} K^{-1}), T_A is the air temperature (K), D_A is the vapor pressure deficit (hPa). Rest all the variables are explained earlier.

According to equations 5 to 8, the dependence of g_A on net radiation and D_A is obvious. Wind is generated as a result of the differences in atmospheric pressure which is a result of uneven surface radiative heating. Therefore, the aerodynamic conductance (and wind as well) is an effect of net radiative heating and therefore, there should be a physical relationship between these two.

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