Responses are given below in ***bold italics***

Referee #3 7th March 2012

General comments:
This paper presents a model that couples the evolution of the soil moisture calcultated by the Penman-Monteith energy balance and the stomatal conductance for O3. Four different methods are evaluated to estimate the biological control of the transpiration. The four methods are evaluated against field data describing a variety of soil water variables, stomatal conductances, and transpiration data for several forest trees. This evaluation is tested in parallel with a sensitivity analysis focused on the accumulated phytotoxic ozone dose.

The rationale and objectives of the paper are clearly laid out, the paper is well structured. I recommend this paper to publication c but, I have some suggestions in order to improve this paper:

- In the introduction, you should introduce a small paragraph on the SVAT models in order to examine your model in comparison with other models. For example, SurfAtm (Stella et al 2011), or PLATIN (Grunhage et al 1997, 2008), or the model of Tuzet et al (2011) are models for pollutant exchanges which integrate the O3 absorption and the energy and soil water balance : What are the advantages and disadvantages of your model compared to a few other similar models ?

We have taken this comment on board and propose to add a few sentences referring to other SVAT models, which stresses the uniqueness of the DO$_3$SE model. The text we suggest to add to the paper on page 33587, l. 25 reads:

'DO$_3$SE was one of the first O$_3$-related SVAT model which was first published in 2000 (Emberson et al., 2000 a,b) and has since been continuously developed. In comparison to similar models, such as PLATIN (Grünhage et al., 1997, 2008), SurfAtm (Stella et al., 2011) or MODD (Tuzet et al., 2011), DO$_3$SE has been extensively evaluated for various vegetation types (forest trees, crops, grasslands) and, as well as being used within the UNECE LRTAP Convention emission mitigation process, is also available as a stand-alone model for application on a site-specific basis (available in an interfaced form at http://www.sei-international.org/do3se) allowing easier access by O$_3$ experimental scientists; this benefits both model evaluation and subsequent model development.'

Note to editor: We kindly ask for some advice on whether or not it is appropriate to refer in the manuscript to a weblink of the DO$_3$SE model interface (see text above).

- In the chapter "Method", you should improve the use of the references: several references are not the "initial" references:
  for example, Eq 20 is attributed at Lhomme (2001) and Jones (1992) but it is Campbell (1974) who has introduced this equation. The same comments can be done for the table 3 which is the table for the "default soil parameters" of the model. The paper "Tuzet et al" is not focused on the determination of soil parameters and you use

these parameters as default parameters without find the origin of these parameters. I recommend to check these parameters.
(Other example : Eq 5 and value Ka = 0.5 ... same type of comments !) ...etc...

We have added the Campbell (1974) reference for Eq. 20 and refer to Campbell (1985) in terms of Table 3. We have added the Norman (1982) reference for Eq. 5.

- In the chapter “Method”, I don’t understand the time step of the model and the link between the daily processes and the hourly processes (see specific comments). In my mind, it is the most important correction you have to do.

We believe that our chosen approach is justified and propose to include the following description on page 33592, l.13 ff. to clarify the method we have used and the necessity for this chosen approach:

‘The DO$_3$SE model has to calculate ozone fluxes on an hourly time step to be able to capture the co-variation of environmental variables that influence stomatal O$_3$ uptake. Using this approach, the water loss from the plant-soil system is also calculated hourly. To estimate the water balance (driven by incoming precipitation and outgoing evapotranspiration), the respective variables are summed over the day and used to estimate the change in water balance at the end of each 24 hour period. This ensures that for each day, the initial soil water limitation is based on the previous day’s soil water balance allowing equilibration of the soil-plant system overnight. This prevents the occurrence of an overly sensitive plant response to frequent changes in soil water status that would occur if these too were modelled on an hourly basis; thereby accommodating lag effects that often occur in the response of leaf level g_{sto} to changes in soil water status.’

- In the chapters “Result / Discussion” : I feel that these two sections could be better organized : I “feel” that there is about the same information between the comments of the “Results” and the “Discussion”.... for example, “l26 p33607 to l8-33608” and “l9 p33609 to l16 33609” explain about the same things and mix results and discussion”... I have had the same “feeling” between the results “sensitivity analysis” and the discussions). In these two parts, the text and ideas are clear but I feel that it can be improved. It’s not a big problem but, when I read I have found about the same clear information in the two parts”

We propose to revise the respective sections as shown in the following:

Results section:
‘Table 5 shows that the fSWP and fPAW models almost always outperform the SS and NSS models. The fSWP and fPAW models fairly consistently achieve the highest proportion of variance (R^2- and IA-values of up to 0.94 and 0.97 respectively) and show the smallest absolute difference (fairly consistently low RMSE-values) between modelled and observed

data. In contrast, the NSS and SS models show, on average, the worst statistical agreement between observed and modelled data as indicated by low R^2 and IA values on the one hand and comparatively high values of MB and RMSE on the other. The poorer performance of the SS and NSS models is also mirrored by the much smaller number of days when fSWP is predicted to fall below 1 for these two models as compared to the fSWP and fPAW models (Table 6), suggesting a less pronounced effect of dry soil water conditions on g_{sto}.'

Discussion section:

'Table 5 provides summary statistics for the performance of all four models. Considering those sites and years for which soil water deficits occurred (defined as water deficits that resulted in some stomatal limitation for some part of the year as estimated by at least one of the models), the statistics suggest that a ranking of the models with regard to their predictive performance is fPAW = fSWP > NSS > SS.'

And

Results section:

'The results of the sensitivity analysis, performed for the Norunda site, are shown in Table 7. They reveal that a variation in the soil texture and g_{max} parameters lead to the biggest change in POD_1 regardless of the model used. Using the clay loam as compared to sandy loam soil texture resulted in a reduction of POD_1 of up to 31%. Changing the g_{max} by +/- 25% lead to an increase in POD_1 of up to 35% and a decrease of 46%. In comparison, changes in dr and LAI led to much smaller – and, depending on the model, sometimes contradictory – changes in POD_1. A reduced consistency in model predictions when using the SS and NSS model as compared to the fSWP and fPAW models also manifests itself in a larger variation in the number of days predicted with fSWP less than 1 for the two former models (Table 7).'

Discussion section:

'The analysis testing the models' sensitivity to key model parameters (Table 7) showed that for all four models the variation of g_{max} by 25 % leads to the largest change in POD_1, followed by, in order, soil texture, dr and LAI. As expected, an increase in g_{max} (increased g_{sto} and hence higher E_t) and dr (increase in accessible water and hence enhanced water supply from root to plant) resulted in higher POD1 values, whereas the change from a sandy to clay loam soil texture (less extractable water, hence reduced accessibility to soil water leading to enhanced drought effects) reduced the POD1. The effect of LAI on POD1 is comparatively marginal and inconsistent, which suggests that only pronounced changes in LAI (as can be found for deciduous trees as the growing season and thus foliage develops) might significantly affect the partitioning of the canopy into sunlit and shaded fractions with subsequent effects on the light penetration of the canopy and hence g_{sto}. These findings stress the importance of the accurate parameterisation of these key variables and especially g_{max}, as noted previously for Jarvis-type models (e.g. Büker et al., 2007).'

- In the discussion, you present some proposals as future model developments in relation with this study... When I read the discussion, I expected that the authors give priority to these new developments. It could be done.

Accordingly, the second paragraph on page 33617 will be changed as shown in the following:

'Future model developments should focus on further <u>evaluating</u> the various soil moisture modelling approaches, using both sap flow and eddy covariance techniques, as well as soil water content data which are starting to be made available from widespread, routine monitoring networks across Europe (e.g. FUTMON, www.futmon.org). This additional information should also allow optimisation of the <u>parameterisation</u> of the DO_3SE soil moisture module by introducing specific maximum g_{sto} values for sun and shade leaves. Finally, the model could be further developed by introducing <u>new formulations</u> that are able to account for i) direct effects of ozone on g_{sto}, ii) the effect of variable water holding properties by different soil layers, iii) a dynamic approach to estimate root depth and iv) consideration of how the interaction of multiple stresses influence water balance of forest trees. The prioritisation of these different model improvements will depend on data availability and the particular application for which the model is being developed.'

Specific comments :

- Line 12-16 p33593: In my mind, this hypothesis is particularly false few days after a Rain... How can you verify that this assumption is appropriate for your simulation ?.

Our text so far: "When soil water is limiting g_{sto}, such that the upper soil layers are likely to have dried through evaporative water loss, the soil evaporation is assumed to be negligible and hence the term Es is set to 0."

We believe this assumption is justifiable since the Es term is going to be almost negligible when the canopy is fully developed, as is the case when drought is so extreme that g_{sto} is limited. Under such situations most precipitation will leave the system through interception (E_i). However, we acknowledge that we may slightly overestimate the recharge by excluding the Es term and will investigate this as part of the future model development with respect to the introduction of a multiple soil layer approach.

- What's happen during the night ?(in particular for the use of the Penman-Montheith equation !!).

We propose to add the following text to make clear what is happening during the night-time:

'Evapotranspiration is usually very low during the night, due to the fact that only evaporation occurs from plant and soil surfaces since, in the absence of light, the stomata are assumed to be closed leading to zero transpiration. Because of the closed stomata, stomatal ozone fluxes are also predicted to be zero during night-time hours.'

- You use the equation 3, 4, 5, 6 ...11 with a hourly time step ...(Line 14 p33592) but you daily calculate the soil recharge (Line 6 p33594 + Eq 14)... The links between the time steps are not clear ... (The precipitations are daily known ... you mix hourly values with daily values ....?).

As described above, we will add an additional paragraph describing the link between hourly and daily time steps for the calculation of various model parameters at the beginning of section 2.2. In addition, we suggest changing the text from page 33594, l. 6 to page 33595, l. 7 as follows:

'The water lost through evaporation from wet plant surfaces (E_i) is estimated as (Monteith, 1965):

$$E_i = \frac{\Delta(\Phi_n - G) + \rho_a c_p \left(\frac{D}{R_{bH_2O}}\right)}{\lambda(\Delta + \gamma)}$$

(12)

Daily recharge of soil water is calculated by summing the hourly values of E_i, E_{at} and P to give E_{itotal}, $E_{attotal}$ and P_{total}. We allow for a fraction of P_{total} lost through interception by the canopy and subsequent evaporation (E_{itotal}) so that P_{input} is the fraction of P_{total} that results in soil recharge:

$$P_{input} = (P_{total} - S_c) + (S_c - min\{E_{itotal}, S_c\}) \qquad (13)$$

where S_c is the external storage capacity of the canopy that determines the amount of intercepted water. S_c (in m) is defined as 0.0001 LAI using the methodology of Sellers et al. (1996) developed for a range of land cover types including broadleaf and needle leaf trees.

Any water remaining on the canopy at the end of the day is assumed to enter the soil system. At the start of the year, when soil water calculations are initialized, θ (volumetric soil water content) is assumed to be equal to field capacity (FC). The volumetric FC defines the relative amount of water held by capillarity against drainage by gravity and is dependent on soil texture (Foth, 1984). At volumetric FC, the soil water storage (S_n) term, expressed over the entire root depth (S_n/d_r), is assumed to be at a maximum.

Daily estimations of S_n are made according to the mass balance formulation based on those used by Mintz and Walker (1993) where the S_n changes on a daily time step according to

$$S_n = S_{n-1} + P_{input} - E_{attotal} \qquad (14)$$

where S_{n-1} is the soil water storage of the previous day, $E_{attotal}$ is the daily water loss via evapotranspiration and P_{input} is water gained via precipitation; any excess P_{input} is assumed to be lost to run-off or percolation from the rooting zone.'

- Line 23-26 p33598 and 1-4 p33599, you explain that ψleaf = ψsoil is not always achieved after the night. There is a lot of explanation (Bruckler et al 1991, Personne et al 2003, ...), in particular in case of dry soil. What is the weight of this assumption in your approach?

We propose to further discuss this in the discussion section, adding the following text:

'This assumption mainly comes into effect under pronounced drought conditions, when plant and soil water potentials might not be in full equilibrium at dawn, usually due to low soil water availability and/or high atmospheric evaporative demand. During these periods, the assumption that ψleaf equals ψsoil might lead to an overestimation of g_{sto} and hence water loss and ozone flux, because ψleaf will in reality be smaller as compared to values modelled by DO_3SE due to a drought-induced reduced sap flow from roots to leaves. Under such conditions the model would tend to overestimate soil water loss.'

- The title "European" trees ... is not adapted in your case ...., no ?.

We have removed the word "European" from the title and following text of the paper.

- Can you argue the approximation G = 10% Φn.

This approximation is based on Norman (1994). We have hence changed the sentence on page 33601, l. 9f. to:

'Soil heat flux G (Eqs. 3, 4 and 9) was calculated as 10% of Φn (Norman, 1994).'

Norman JM (1994) Scaling processes between leaf and canopy levels. In: Ehleringer JR, Field CB (eds) Scaling physiological processes: leaf to globe. Academic Press, San Diego

- You can mention "run off" as other big omission in the hydrological cycle...

We appreciate this comment and have changed the respective sentence on page 33614, l. 27ff. to:

'Other limiting factors of the model include the omission of various elements of the hydrological cycle, such as surface run-off, snow water and groundwater storage terms.'

I appreciate the details which have been presented.
Technical corrections:

We believe that some of the comments below are related to the word processing software the reviewer has used. However, we will specifically ask the editor to check this before publication.

Table 2 and 6 are written too small for my eyes.

We assume this can be sorted out by the publisher? We submitted the table in a readable size.

Eq (2) needs to be rewritten with good typographical sizes of letters ("max")

We assume this can be sorted out by the publisher? The equation we submitted reads:

$$g_{sto} = g_{max}\, f_{phen}\, f_{light}\, \max\{f_{min}, f_T\, f_D\, f_{SW}\}$$

Eq (5), you miss "="

Tthe equation format appears to be fine to us, what we submitted and what can be found in the pdf of the discussion document is:

$$\Phi_{ns} = exp(-K_{\alpha}\; LAI)\Phi_{n}$$

Eq(9) : Why the brackets "("RbH2O")".

We have removed the brackets.

Line 3, p33602: "Et" needs to be change in good typographical size.

This has been changed from Et to E_t

some equations are sometimes not written consistently in a typographical point of view.

We have performed a last check for this and made sure that everything is consistent in terms of formatting and typography.

We would like to thank both reviewers for their useful comments!