ACP-2022-782: Simulating impacts on UK air quality from net-zero forest planting scenarios

Response to Reviewer 1

Response: We thank Reviewer 1 for their time spent reading our manuscript and for the comments and suggestions to improve the manuscript. Below we respond to each comment individually (in blue font) and indicate the changes made to the revised manuscript.

This manuscript describes model simulations of the air quality impacts of four UK afforestation scenarios. The authors have improved on previous studies by using more detailed data on landcover suitability (placing limits on the potential area available for plantations), emission factors based on UK measurements of the specific species and a high-resolution chemistry and transport model (WRF-EMEP4UK). These upgrades have the potential to result in a more accurate simulation. The manuscript has detailed methods and results sections and there is some discussion of the results including a brief comparison to previous studies. The current manuscript does not discuss the accuracy of these results which may mislead readers into assuming that these simulations are accurate representations when they have not shown this to be the case.

The following three main points should be addressed before this manuscript is published:

Main points

1. The authors conclude that their simulations "demonstrate the need to use locally relevant data and atmospheric chemistry transport models to assess the impact of additional forest planting on surface atmospheric composition" but do not provide any evidence that this simulation is more accurate than any other simulation. Demonstrating that their improvements significantly improve the ability to model these scenarios requires comparing with alternative simulations and showing that they are more accurate.

Response: We agree that the statement the Reviewer quotes is not appropriate in that we do not demonstrate our simulations have greater accuracy; but since we are simulating possible future scenarios, it is not possible for us objectively to quantify the accuracy of our model simulations. This is true for any model simulation of the potential future. What we can do is demonstrate that our model set-up provides effective simulations of historic and present-day atmospheric composition over the UK, which we do by citation to several papers that evaluate model output against measurements (there are more such papers than those we currently cite), and then use the most relevant data available to us as the model inputs for simulating potential future scenarios. This approach should provide the best simulations possible at present. This is why we use data on local growing conditions to inform where particular tree species may be grown in the UK; and use locally measured BVOC emission potentials, given there may be local factors influencing the emission potentials from particular tree species, e.g. genetic variations (Bäck et al., 2012; van Meeningen et al., 2017; Duncan et al., 2001), locally relevant biotic influences (Rieksta et al., 2020; Blande et al., 2007) and climate and other meteorological-based influences (Bonn et al., 2019; Copolovici and Niinemets, 2010). We also simulated potential planting scenarios of four different tree species that encompass a range of types (deciduous, evergreen, etc.), and isoprene and monoterpene emission rates, so as to span a range of potential future impacts of large-scale planting on air quality.

Our intent with the statement quoted above was to highlight that our study had used locally-relevant tree species and emissions data, which should, in principle, yield more accurate simulations than using data derived from outside the UK. To take account of the Reviewer's legitimate point we have reworded the manuscript to make this clearer (line 982-986)

“We show how locally-relevant tree species data, BVOC emissions potentials and meteorology should, in principle, improve the simulations by atmospheric chemistry transport models of the complex interactions between additional forest planting and impacts on surface atmospheric composition”.

2. The authors conclude that widespread planting of trees will slightly increase UK ozone and decrease PM2.5 but there is no assessment of the uncertainty of these simulations. For example, if their BVOC emissions change is underestimated and/or the particle deposition rate changes are overestimated then not only would there be error in the magnitude of the change in PM but even the sign of the change could be wrong. The manuscript needs a comprehensive description (as quantitative as possible) of the uncertainties associated with each component of the model system (BVOC emission, ozone and particle uptake, chemical transformation, etc) and a discussion of how the uncertainties might influence these results. In particular, this should address whether the uncertainties are so large that it is not currently possible to accurately predict whether widescale UK tree planting will have a positive or negative health impact.

Response: In this work, we used a range of tree species – with a range of isoprene and monoterpene emission rates, leaf area indices, growing season, etc. – in order to explore a range of the potential sensitivities of atmospheric composition to large-scale tree planting in the UK. There are uncertainties in every part of these simulations, not just the uncertainties in BVOC emissions and particle deposition. It is not feasible to conduct a comprehensive uncertainty analysis on the entire set of model variables. Many variables are interconnected, so simulations changing a single variable at a time may not result in quantification of the uncertainty of a specific variable. We have effectively already conducted an indirect sensitivity analysis on BVOC emission and particle deposition by using different emission rates and trees with different leaf areas and annual leaf cover. As per our response to comment 1 above, we use a model that is well validated for its ability to simulate current and historic UK atmospheric composition, together with the UK-relevant data available to us for future tree planting scenarios. In our Discussion section we provide several paragraphs on uncertainties and limitations in our model simulations (which apply to any such simulation). Also as noted above, it is not possible to provide quantitative uncertainty bounds on a given simulation given the uncertainties in so many aspects of the simulations. As it turns out, however, the changes in surface ozone and $PM_{2.5}$ that we simulate are relatively small despite the very substantial additional tree planting areas that we include. The large uncertainties that undoubtedly there are on these simulated changes are therefore acting on relatively small changes in composition and are therefore not likely to change the ‘big picture’ of the extent of impact on surface composition of these very large additional tree plantings.

To further acknowledge the uncertainties in our simulations, particularly in relation to BVOC emissions, we have added text to the Discussion as detailed in other responses.

As an example, the BVOC emission factors used for their model simulations are based on measurements of trees growing in the UK which should be more representative. But what is the uncertainty of these enclosure measurements?

Response: The uncertainty of the enclosure measurements used during this study has now been added to the supplementary material and referenced in the text starting in line 862-872: “In order to mitigate uncertainties in the emission potentials of isoprene E_{iso} and monoterpenes E_{mtp}, as well as the temperature, light and humidity dependence of the BVOC emissions, we use data from UK-specific measurements to underpin the model simulations. The default emission potentials for landcover types in the model are not assigned an uncertainty as they are derived from a weighted sum of emission potentials of species based on literature values. All measurements of emission potentials are subject to uncertainties, and potentially more so when using plants grown and measured under field conditions. The uncertainties of emission potentials used in this study are given in the Supplementary Material S8. Detailed discussions of these individual uncertainties are given in Purser et al. (2021a) and (2021b)."

Have the emission rates been assessed by above canopy flux measurements?

Response: For the tree species used in this work there is a paucity of literature data for above-canopy fluxes. Our simulations are for monoculture plantings, not mixed species forests. For alder, aspen and *Eucalyptus gunni* there are no canopy flux measurements in the literature described as being from single-species plantation, which would be the relevant comparison. Some measurements exist for Sitka spruce in the UK but only for an extremely short duration (24 hours) (Beverland et al., 1996). The scope for comparison here is further limited as there were no measurements of monoterpenes and the flux technology and methodology used at the time was in its infancy. Consequently, the best emissions data currently available for our simulations derive from chamber measurements.

To make the lack of above-canopy measurements more explicit, we have now inserted the following text to our paper (line 324-328):
"No appropriate above-canopy flux measurements were available for the tree species in this study. The emissions were therefore based on chamber studies conducted on single-species branches. Further information on the methodology used to derive emission potentials, and a comprehensive comparison against other literature values, is given in Purser et al. (2021)."

This is especially important for monoterpene emissions which are well known to be disturbed by the process of enclosing the vegetation for measurements. How valid is the assumption that light, temperature and biomass density are the only important controlling factors?

Response: Currently few techniques can measure monoterpene flux measurements in the field. As already noted in response to the previous comment, chamber measurements are the most appropriate datasets currently available. For single-species monocultures, these chamber measurements are currently the only emission measurements available. We fully agree that many factors in addition to light and temperature may impact emissions under real-world conditions. Herbivory, disease, the effect of rain and genetic differences within a single species are all examples of the many variables that cannot be replicated in any of the wide range of atmospheric transport models that have been used in simulations of potential impacts of tree planting.

To address the Reviewer's comment we have added the following at line 872-881 for context:
"Both monoterpene and isoprene emission factors may also be impacted by a range of other variables in the field such as biotic factors e.g. herbivory or plant disease (Rieksta et al., 2020; Blande et al., 2007), effect of precipitation; genetic differences within each tree species (van Meeningen et al., 2017; Duncan et al., 2001; Bäck et al., 2012); flooding, drought and heat stress (Copolovici and Niinemets, 2010; Seco et al., 2015; Bonn et al., 2019). The full range of variables found in the field currently cannot be replicated in the necessarily simplified model environment. It is also possible that the collection of such emission data using the enclosure technique could have an influence on the measured emissions."

Many studies have shown that these BVOC emissions are highly sensitive to stresses which can influence both the emission factor measurements and the model extrapolation. Other processes, such as the particle deposition rates, may be even more uncertain and the total uncertainty is likely quite high. A finding that these model simulations are highly uncertain does not suggest that they are not a valuable activity but will emphasize the need to reduce these uncertainties before any robust conclusions can be made.

Response: We agree that our simulations are inherently uncertain, as is the case for all such simulations, and welcome the Reviewer's acknowledgement that this does not mean the simulations are not a valuable activity. As we have already noted above, there are uncertainties in all parts of the simulations, not just in the BVOC emissions on which the Reviewer concentrates their remarks. The emission potentials we use span three orders of magnitude for isoprene (0.03 to 22.8 μg C g_{dw}^{-1} h^{-1}) and an order of magnitude for monoterpenes (0.17 to 3.4 μg C g_{dw}^{-1} h^{-1}). This use of a range of BVOC emissions in our scenarios gives equivalent insight into the impact of uncertainty in BVOC emissions on model output as assigning some estimate of formal uncertainty to an emissions value. We believe that the additional text insertions to our paper made in response to all the Reviewer's comments confirm that we fully acknowledge these uncertainties in our study.

3. The manuscript would benefit from additional simulations to investigate model sensitivity and quantify the impact of the identified uncertainties. It would also be informative to have simulations where only emissions or uptake is changed to better understand the results. For example, this could be used to determine the relative contributions of increased tree particle uptake vs lower agricultural BVOC emissions in determining the reductions in PM2.5 mentioned in lines 764 -766.

Response: This comment is similar to those to which we have responded above. There are uncertainties in so many components of the simulations that any new model simulations would only be touching on one or two contributions to the uncertainty in model output. As mentioned, we already have model simulations that use different BVOC emissions and different particle depositions. What is relevant as 'policy' output is the net effect on atmospheric composition associated with the tree planting of both emissions and deposition changing simultaneously, since one will not change in isolation of the other in reality. As it turns out, the changes in surface ozone and $PM_{2.5}$ that we simulate are relatively small despite the very substantial additional tree planting areas that we include. The large uncertainties that undoubtedly there are on these simulated changes are therefore acting on relatively small changes in composition and are therefore not likely to change the 'big picture' of the impact on surface composition of these very large additional tree plantings.

Specific points:
Line 403: exsisting => existing

Response: Typographical error corrected, thanks.

Line 608-613: aspen is a higher per biomass emitter but is the area average emission (biomass X emission factor) of aspen and spruce about the same?

Response: We acknowledge the Reviewer's comment that the average emissions for aspen, 7501 μg C m^{-2} h^{-1}, is similar to Sitka spruce, 6747 μg C m^{-2} h^{-1}, during the summer months based on a multiplication of their biomass and emission factors. However, aspen is still the higher emitter in comparison with either Sitka spruce or eucalyptus. We have amended the text as follow to reflect that the difference is not as large as initially stated (line 584-589).
"Interestingly, the aspen planting scenario has a lower impact on ozone concentration changes in the summer, only 1 ppb, despite being a higher emitter of isoprene than eucalyptus and Sitka spruce (Table 3 and Figure 4). Both isoprene and monoterpenes are

precursors for the formation of tropospheric ozone, and aspen does not emit monoterpenes, whereas eucalyptus and Sitka spruce are significant emitters of monoterpenes (Table 3 and Figure 6)."

Line 613-622: it is surprising that a relatively small amount of monoterpenes would offset the ozone impacts of a much larger amount of isoprene. Some explanation should be provided of how this could be the case. For example, is it the difference in ozone formation potentials assumed by the model?

Response: Yes, the net impact is driven, amongst other things, by the different ozone formation propensities, which as well as being influenced by different rates of VOC oxidation are also influenced by NO and NO_2 concentrations. It's also important to recognise that net ozone concentrations are also influenced by the rate of dry deposition of ozone which will also vary across the different tree species. The following text has now been added (line 611-614):
"These net impacts on ozone concentration are driven not only by the different ozone formation propensities of isoprene and monoterpenes (which in turn are influenced by local NO and NO_2 concentrations), but also by the different rates of ozone dry deposition across the different tree species."

Line 646 : "than the loss than through" delete second "than"

Response: Spurious repeated word deleted.

Table 2. Specify the standard conditions for emission factors. Also, why is the monoterpene emission factor for deciduous woodland so high?

Response: An asterisk has been added to columns E_{iso}, E_{mtp} and E_{mtl} in Table 2 and a footnote added underneath the table to indicate that these emission potentials relate to emission rates at 30 °C and 1000 µmol m^{-2} s^{-1}. We have spotted that the monoterpene emission potential for deciduous woodland given in Table 2 is based on a more current version of the EMEP model, rv4.34 and its auxiliary files as downloaded from the GitHub (https://github.com/metno/emep-ctm/releases/tag/rv4_34).

The text has been updated on lines 257-259 to reflect this:
"The auxiliary files for this version can be downloaded from GitHub (https://github.com/metno/emep-ctm/releases/tag/rv4_34)"

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ACP-2022-782: Simulating impacts on UK air quality from net-zero forest planting scenarios

Response to Reviewer 2

Response: We thank Reviewer 2 for their time spent reading our manuscript and for the comments and suggestions to improve the manuscript. Below we respond to each comment individually (in bold font) and indicate the changes made to the revised manuscript.

In their paper "Simulating impacts on UK air quality from net-zero forest planting scenarios" the authors address a very timely and policy relevant issue for the UK. The authors present a first step towards a more complete understanding of the wider impacts of large-scale afforestation for carbon sequestration and bioenergy production in the UK.
The paper is certainly within the scope of ACP and my recommendation is that the paper is published, subject to correction / clarification on the following minor issues:

Response: We thank the Reviewer for this recommendation to publish in ACP following attention to minor clarifications.

Section 2.3.1 (lines 251 – 255): Could you clarify the model setup, is it running for the whole European domain but nested at 5 x 5 km resolution over the UK online? What is the resolution for the rest of the domain?

Response: Yes, the model operates over an 'extended Europe' domain, which is at ~50 km × 50 km horizontal resolution, in which is nested a British Isles domain of ~5 km × 6 km horizontal resolution. To clarify this in the revised paper, the text starting at line 252-258 has been slightly modified to the following:
"Simulations were undertaken at ~5 km × 6km horizontal resolution (and hourly temporal resolution) with EMEP4UK ACTM version rv4.34 (Vieno et al., 2016, 2010, 2014; Nemitz et al., 2020). This is a nested version of the EMEP MSC-W model described in Simpson et al. (2012, 2020) in which the higher resolution British Isles domain is nested within an extended Europe domain that is simulated at ~50 km × 50 km horizontal resolution."

Section 2.3.1 (lines 273 – 276): As the emission of BVOCs, dry deposition of gases and aerosols, and formation of SOA, are all important processes in this study it would be very useful to summarise here the approach taken (rather than just refer the reader to Simpson et al 2012). In particular, it will help with the later Results / Discussion to specify how SOA is formed i.e., via a fixed yield from oxidation of BVOCs? Do isoprene and monoterpenes both contribute to SOA? is a volatility basis set approach applied or does SOA condense irreversibly onto existing aerosol?

Response: The description of how BVOC emissions are derived in the model is provided in detail in Section 2.3.2, the section immediately following the section referred to in this comment. We feel that this description is better placed in this subsequent section alongside the description of other aspects of the BVOC emission data such as leaf area indices, biomass density etc., and which are encapsulated in Table 2.

The dry deposition of gases uses the standard 'resistance' formulation widely used in atmospheric chemistry transport models which includes terms for aerodynamic resistance, quasi-laminar layer resistance and surface (canopy) resistance. Dry deposition of ozone (and some other gases such as NH_3) also includes stomatal and non-stomatal contributions. The parameterisation of dry deposition of aerosols in the model is more complex than for gases and has algorithms with dependencies on both particle size and composition.

The SOA chemistry uses a volatility basis set approach. All primary organic aerosol (POA) emissions are treated as non-volatile, to keep emission totals of both PM and VOC components the same as in the official emission inventories, while the semi-volatile ASOA and BSOA species oxidise (age) in the atmosphere by OH-reactions, leading to decreased volatilities for the SOA.

The descriptions of all the above processes extend to several pages in the article by Simpson et al. (2012). It is not possible to summarise these parameterisations into a few sentences in this paper, which is why we direct the reader to Simpson et al. (2012) for the detail.

We also make two further points here. Firstly, the EMEP model is very widely used across Europe to simulate atmospheric composition and is subject to routine annual evaluation reports against measurements and to model improvements (www.emep.int/mscw/). These (and other) parameterisations in the model therefore represent current state-of-the-science for high resolution atmospheric chemistry transport models. Secondly, whilst the processes mentioned by the Reviewer are certainly key processes determining ozone and $PM_{2.5}$ surface concentrations, so are many other processes that the Reviewer doesn't mention, such as boundary layer height, wind speed and direction, solar flux and rainfall. There is also not space in our paper to provide detail on the modelling of these important processes either.

Section 2.3.2 (lines 297 – 298): The yield data is at 250 m x 250 m whereas the model land cover is at 5 km x 5 km, could you add a note here to clarify how the conversion is made? The underlying planting data takes into account the constraints from the Lovett et al 2014 study, are those constraints lost when scaled up to 5 km x 5 km or is just a % of a gridcell used?

Response: The original resolution of the Lovett et al. (2014) yield data are preserved, by aggregating to %/grid cell as the Reviewer suggests. The landcover data used by EMEP4UK is at a grid resolution of 0.01 x 0.01 degree (~1 km) resolution with values representing percent cover of each land cover type. We have converted the yield data to the same spatial resolution (0.01 degree) and projection system as the land cover data (as %/grid cell). The datasets were then combined to estimate a new land cover values. We have added the following text (line 302-306):
"The landcover data used by EMEP4UK is at a grid resolution of 0.01 x 0.01 degree (~1 km) resolution with values representing percent cover of each land cover type. The ECS-DSS yield data was converted to the same spatial resolution (0.01 degree) and projection system as the land cover data (as %/grid cell). These datasets were then combined to estimate a new land cover values." The EMEP4UK model internally interpolate or extrapolate the land cover values to the model resolution of for example ~5 km x 6 km.

Table 2: it would be useful to reiterate in the caption that the first four rows are based on field experiments from your previous study and the last four are based on the model algorithm used in EMEP

Response: A dagger symbol, †, has been added to the first 4 rows in column 1 and a footnote has been added to state:
"Based on measurements conducted by Purser et al.,(2021a, b)."

Figures 9, 11 and 13 are a little confusing. Showing the scenarios as a difference from the baseline makes sense but having the baseline concentrations on the same plot gives the (incorrect) impression that e.g. the baseline O3 is higher than all scenarios between Jan and

April. One option would be to show the baseline seasonal cycles of O3, SOA and PM2.5 on their own Figure and have the scenarios on their own Figure each for O3, SOA and PM2.5?

Response: We feel that having the information of the baseline and its changes on one graph is a useful in order to reflect how the baseline may change with a given scenario. The graph axes are appropriately labelled and additionally fully explained in each figure caption. We have, however, decided to add an extra label to the primary *y* axis to aid with interpretation; but do not believe these graphs need separation to be interpreted.

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