Replies to the reviewers' comments on

> **Virkkula A. et al., Overview of a prescribed burning experiment within a boreal forest in Finland, Atmos. Chem. Phys. Discuss., 13, 21703–21763, 2013**

RELEVANT CHANGES MADE IN THE MANUSCRIPT

The comments of the reviewers were very good and relevant. As a result we have now studied several papers on prescribed fires and found weaknesses in the manuscript. The most relevant changes made in the paper are

> 1) The title of the paper was changed. Reviewer 1 suggested the title
> "Emissions and Effects of Fire on Soil Properties from the Prescribed Burning of Logging Slash in the Boreal Forest of Finland"
> However, that suggestion did not take into account meteorological observations that play an important role in the paper so we modified the title to
> **Prescribed Burning of Logging Slash in the Boreal Forest of Finland: Emissions and Effects on Meteorological Quantities and Soil Properties**
>
> 2) improvement of the description of the progress of the experiment, including photographs of the fire at different times and description of the fire danger rating system in Finland as suggested by reviewer 1.
>
> 3) Recalculation of the VOC emission ratios as pointed out by reviewer 2
>
> 4) Comparisons of the aerosol data with results published from similar experiments.
>
> 5) The analysis of photographs revealed that the time in the figure (now Fig 4) that presents the sonic anemometer data within the burn area was wrong by one hour.The figure was corrected.
>
> 6) In figure 13 the number concentrations of flight 3 were corrected. In the original figure there were erroneously also data from above the city of Tampere, now only data from within 20 km distance from Hyytiälä.
>
> 7) In the conclusions we have emphasized new findings and those confirming earlier ones and presented the lessons learned for improving similar experiments.
>
> 8) We used the help of three people not earlier as coauthors and ask to add them as new ones, despite of the large number of authors.

Below are the detailed replies to all the reviewers' comments. The reviewers comments are written by bold and the replies indented.

REVIEWER 1

Compare the present results with previous studies such as FIRESCAN Team, 1994; Cofer et al., 1998.

..., while the experiment does not involve "natural forest fires" it would simulate to some extent a fire burning through a forested area containing clear-cut blocks of logging slash. However, in this case the ignition pattern used was not indicative of a wildfire moving through a forest area (see Walker and Stocks, 1972).

In the introduction references to several experiments were added, including Radke et al. (1991), SCAR-C (Hobbs et al., 1996), the International Crown Fire Modelling Experiment (ICFME) (Alexander et al., 2004; Stocks et al., 2004), the FROSTFIRE experiment (Hinzman et al., 2003), the Bor Forest Island Fire Experiment (FIRESCAN Science Team, 1996), and the Fire Effects in the Boreal Eurasian Forests (FIRE BEAR) (McRae et al., 2006).

I have two major concerns about the article in its present form. The first is that it tends to come off as poorly written.

We have now had the text checked

Secondly, the article does not provide a good description of the physical fire science aspects of the experiment.

We have now extended the description of the fire in section 3.1 including fire weather analyses and photographs with time stamps in Figure 2.

Table 1 – I gather that "Tree Biomass" refers to the roundwood material? What portion is needle foliage? Was any attempt made to differentiate the roundwood material (i.e. the load by diameter size class)?

The burned biomass is strongly dominated by slash (noncommercial wood) and the roundwood material consists of tree tops, diameter 7 cm or below, and branches typically smaller than 5 cm in diameter.
The mass portion of the needle foliage form the total burned biomass was 37 %.

SPECIFIC COMMENTS

Introduction

• Revise re comment made above regarding "natural forest fire".

In the introduction it was written:
"The prescribed burning therefore represents fires in a forested area, containing clear-cut blocks of logging slash. It does not represent fires in a full-grown boreal forest. The burned area is small compared with the experiments mentioned above "

Methods:

• What type of spruce?

Picea abies (L.) H.Karst. known as a Norway Spruce or European Spruce

• In terms of a burning prescription (see Alexander and Thomas, 2006), what in addition to wind direction was involved and how did it compare to what actually occurred (e.g., fure weather conditions, fuel moisture content, fire danger rating index)? This is important in case someone wanted to replicate your work. Suggest reviewing Alexander (2006, 2010). Does Finland use some form of a fire danger rating system. If so, then it would useful to indicate what the fire danger indices were during the burn.

In addition to wind direction the parameters that affected the decision of when to start the experiment were wind speed, soil moisture and cloud cover. There is a fire danger rating system in Finland, it is based on soil moisture (Vajda et al., 2013). Section “3.1 General description of the burning” now starts with the paragraph

> Whenever prescribed fires are conducted, parameters affecting fire weather, such as biomass moisture, temperature, relative humidity, wind speed, and rainfall should be measured and documented prior to and during the burning operation, as recommended by Alexander (2006). The measurement setup was ready at the beginning of May 2009, waiting for the proper conditions. For our experiment the required conditions were: 1) wind direction was to be in the range 175°–215° to blow smoke to our ground-based instrumentation, 2) wind speed had to be less than 5 m s^{-1} to keep the fire under control as suggested by the Finnish handbook of prescribed burns (Lemberg and Puttonen, 2002), 3) soil had to be dry enough so that it burns properly, and 4) sky was to be clear so that the smoke plume (i.e., the visible column of smoke) could be followed with the aircraft and possibly even from satellites. Fulfilment of requirement 3 was assessed from the forest-fire warnings issued by the Finnish Meteorological Institute. In Finland, forest fire danger is assessed by the Forest Fire Index (FFI), which is calculated from soil moisture, so that for very wet conditions FFI < 2 and for very dry conditions FFI = 6.0 (Vajda et al., 2013).

In section 3.2 there is discussion of the wind direction that changed during the day.

• Table 1 – I gather that “Tree Biomass” refers to the roundwood material? What portion is needle foliage? Was any attempt made to differentiate the roundwood material (i.e. the load by diameter size class)?

The burned biomass is strongly dominated by slash (noncommercial wood) and the roundwood material consists of tree tops, diameter 7 cm or below, and branches typically smaller than 5 cm in diameter. The mass portion of the needle foliage form the total burned biomass was 37 %.

• You indicated that “flaming was over within about 25 min” but elsewhere (e.g. the abstract) you indicate 2 h and 15 min). Active flaming in logging slash is typically 2 min (see Brown 1972) but isolated flaming in logging slash can last much longer.

The text was obviously not written clearly. The flaming lasted 25 min after closing the horshoe-like shape. The respective text now reads

> “The burning was performed against the wind as a backing burn; first the fire was ignited against the wind and then ignition slowly proceeded in both directions (Fig 1a). The idea was to slowly burn the edges of the site until a horseshoe-like shape was achieved and more than half of the area was burned (Figure 2a). This phase of our experiment took about 110 min. Then, the edges were rapidly ignited in both directions so that the edges of the site

were enclosed with the fire (Figure 2b). Thereafter, the fire proceeded rapidly downwind, and flaming was over within about 25 min. ”

• Any observations of average and maximum flame heights? Any ground photos during the flaming and smouldering phases?

This was a very good comment. We looked through photographs taken during the experiment and especially their time stamps. They showed that the time in the figure that presents the sonic anemometer data within the burn area was wrong by one hour.The figure was corrected. Some photographs have now been added to the paper as a new Figure 2. The photos can be used for estimating flame heights.

Did any spot fires development outside the cut block?

No, the fire was contained within the cut block.

TECHNICAL COMMENTS

The title is not indicative of the content of the article. I would suggest something along the lines of "Emissions and Effects of Fire on Soil Properties from the Prescribed Burning of Logging Slash in the Boreal Forest of Finland".

Ok.We can change it.

Table 2 caption – suggest this read ": : : during the prescribed fire experiment, the : : :"

Corrected

Given that so many abbreviations and acronyms are used throughout the article would it not be useful to include a summary list?

We have now added another table that presents the abbreviations and acronyms

.

REVIEWER 2

To be publishable, I believe the authors need to revise the paper by:

1) focusing on the successful measurements and discussing these in the context of previous studies. The paper needs an improved comparison of the VOC and particle measurements with previous studies, including correction of errors in Section 3.4 (see below)

We have now corrected the VOC calculation and compared the results and the particle data with those suggested by the reviewer.

2) identify a few findings that are new or confirm previous studies and summarize the importance of these findings with respect to air quality or climate,

We have now presented several findings that are new or confirm previous studies. Here are some of them

1) A new finding was that the particles emitted by the fire and transported to the measurement site 400 m apart from the fire were smaller during the flaming phase than during the smouldering phase. This is opposite to studies in which aircraft have been used. We cannot give any good explanation to this.

2) The aerosol optical properties were in agreement with other published data from wildfire smoke aerosols.

3) The emission ratios of most VOCs compared to carbon monoxide were larger than those published in other studies.

4) There were changes in soil physical and chemical properties, which influenced the soil greenhouse gas (CO_2, CH_4) fluxes for several years after the burning.

3) provide an expanded and improved description of what was learned from this experiment and how a future experiment would be conducted to successfully accomplishment the stated goals (e.g. studying a fire that is an appropriate proxy for a natural wildfire). The authors mention mobile platforms but should elaborate further, e.g. discussing instrument payloads and measurements requirements (response time, precision, what species to measure).

We have now added lessons that were learned from the experiment and how to make a better one.

SPECIFIC COMMENTS P3, L3: Grell et al. 2011 good reference linking fires to weather, but should replace Andreae 1991 and Penner et al. with more recent and more relevant references to support this statement, including the addition of a reference related to health effects.

The references to Andreae 1991 and Penner et al 1992 were replaced with a more recent one, Bowman et al. Fire in the Earth System, Science, 2009.
Two references to health effects were added:
Naeher &al, Woodsmoke Health Effects, Inhalation Toxicology, 2007
Johnston et al. Estimated global mortality attributable to smoke from landscape fires, Environ. Health Perspec. 2012.

P3, L7-9: Is transport from Europe to the Arctic important?

Yes, it is. According to the already referenced papers, for instance Shindell et al. (2008) and the AMAP 2011 report and very many papers Europe is a very important source of Arctic pollution.

P3, L9-11: The description of fire emission impacts on climate should be improved. Consider differentiating between short-term (aerosol, ozone, ch4) climate forcers and long-term climate forcers (co2, n2o) and consider using positive / negative radiative forcing rather than warming/cooling. See Shindell et al. (2009) for examples (Shindell et al. (2009) Science, 326, 716-718).

The corresponding text was rewritten as

> "Fires emit directly long-lived greenhouse gases for example carbon dioxide (CO2) and nitrous oxide (N2O) and short-lived greenhouse gases methane (CH4) and a large number of volatile organic compounds (VOCs) and nitrogen oxides (NOx) that are precursors for ozone (O3), a short-lived greenhouse gas (e.g., Andreae and Merlet, 2001; Akagi et al, 2011; Simpson et al., 2011; Jaffe and Widger, 2012). The particles emitted by fires are short-lived climate forcers that can have either negative or positive forcing effects, depending on their optical and cloud-forming properties and on the albedo of the underflying surface (e.g., Randerson et al., 2006; Quinn et al., 2008; Ramanathan and Carmichael, 2008). Black carbon emitted from wildfires may get transported and deposited to snow or ice where it has a positive radiative forcing due to the reduction of the albedo of the surface (e.g., Ramanathan and Garmichael, 2008)."

P3, L12: Reference(s) needed

Added

P3, L17-19: van der Werf et al. is emission inventory, should cite Giglio et al. (2010) instead (Giglio et al. (2010) Biogeosciences, 7, 1171-1186).

Corrected

P3, L 19-20: Not true. Satellite images provide information on burned area and fuels involved, they do not provide information on the amount of fuel consumed or smoke emitted. Emission inventories provide this information, e.g. van der Werf et al. (2010).

P3, L27: "satellite estimates" of what?

The text including lines 19-20 and 27 was rewritten as

> "Ongoing wildfires and burned areas can be observed from space by using satellite imagery (e.g., Flannigan and Haar, 1986; Lentile et al., 2006; French et al., 2008; Sofiev et al, 2009; Giglio et al., 2010). Satellite images give information on the area that is burning but not on the amount of fuel consumed or smoke emitted, for that othe information is needed. Van der Werf et al. (2010) used a biogeochemical model and satellite-derived estimates of area burned, fire activity, and plant productivity to calculate the total global carbon emissions due to deforestation, savanna, forest, agricultural, and peat fires. They estimated that the boreal region accounted for about 9% of total global carbon emissions from fires. To estimate the amount of aerosols and trace gases emitted, emission factors, defined as the amount of emitted aerosol or trace gases per mass unit of burned biomass, are needed. Recent reviews of emission factors include Andreae and Merlet (2001), Reid et al. (2005a, 2005b), Janhäll et al. (2010), Akagi et al. (2011), Simpson et al. (2011), and Yokelson et al. (2013)."

P4, L12: Is the experiment approach being assessed for use with wildfires or large managed burns?

No, there has not been any plan for that.

P4, L16-24: The wind is from the preferred direction only 10% of the time? Could a more favorable month have been used (July or August)?

We had made a reservation for May and June that were the best according to a climatological analysis. July and August were not possible months for several reasons: other measurement campaigns started at the beginning of July and summer holidays of key persons in August.

Section 2.3.1: The authors should include a relevant estimate of the measurement precision for the CO2 and CO instruments at the SMEAR II tower, e.g. the 30-s standard deviation while sampling a mid-range calibration gas. These are key measurements and it seems like the enhancementsin the diffuse smoke may not have exceeded the measurement precision very often. Please clarify. Also, the data is described as a 1 minute time step, but this appears to include 30-s flush time as the sampling rotates between levels. Please clarify, are the concentration data 30-second averages?

The signal standard deviation of the CO_2 analyser was 0.04 ppm while sampling calibration gas. This would be an estimate for the precision of any 1 min value. The signal standard deviation of the CO analyser was 6 ppb while sampling calibration gas. This would be an estimate for the precision of any 1 min value.

In general the averaging times were different for different analysers. And in the 1 min time scale exact averaging times can not be given for every analyser because of the combination of averaging caused by flushing of sample volumes, signal averaging and "sample measurement - reference measurement sequencing" of an analyser vs. the switching cycle of sample heights in the tower.

- For CO_2 analyser each 1 min signal is (approximately) the 30 s average.
- For CO analyser each 1 min signal is (approximately) the 1 min average.

P7, L9: what is the height above ground level of the inlet at REA cottage?

16 m, it is now writen in the text

P8, L31-32: Dust Tracks measure light scattering. Were these calibrated for aerosol from biomass burning?

No, they were not specifically calibrated for smoke aerosol. This is now written also in the text.

P10, L8-18: Please specify: 1) what period were the bi-weekly measurements taken, over the entire year, over the growing season, over the summer, etc. and 2) was temperature (or other variables) used to interpolate CO2 effluxes over hours of day and between days daily as soil temperature and/or soil moisture

1) Now, we have specified in the MS that the fluxes were measured every two weeks from early May to end of November for one year before and for three years after the treatment.

2) It is now specified in the MS that we approximated the cumulative release of CO_2 at each site after the treatments by interpolating between the days the effluxes from each treatment separately.

P10, L20-22: Please specify what time of year the VOC soil efflux measurements taken?

We have now added the dates for VOC soil efflux measurements in the revised MS.

P11, L4-6: It should be noted that MCE provides a measure of the relative mix of flaming and smoldering combustion with MCE approaching 1 for pure flaming combustion.

This has been noted and the discussion of MCE was shortened.

P11, L 19: Provide reference(s) for statement that single-scattering albedo = 0.3+/-0.1 for 'BC'.

This sentence has been changed to:

"For freshly-generated pure BC ω_0 has been measured to be approximately 0.2 ± 0.1 (e.g., Bond and Bergstrom, 2006; Cross et al., 2010; Bond et al., 2013). "

P14, L18-29: There can only be one fire-front passage, however this section refers to multiple fire-front passages. The heat flux measured at the surface (Q) that is plotted in Figure 3b shows the fire-front passage between 8:30 and 9:00. The first spike in sensible heat flux, which occurred just after 8:00, was a result of the plume passing across the instrumented 12 m pole.

Yes. We have modified the text to differentiate between plume passage and fire front passage. The total heat flux radiometer data shows when the fire actually passed the tower, while sensible heat flux increases are associated with the plume passing the 12 m mast. We have also corrected the times in figure 3 according to the now included photographs in fig. 2.

Section 3.2 Figure 2 should cover only region of interest, 6:00 to 15:00, this would make it much easier to read.

The end of figure time range was changed but not to 15:00 but to 18:00.
The reason is that the last research flight took place at 15:50 – 17:55. The simultaneous ground data should also be presented.

The gas instrumentation at the SMEAR II mast doesn't seem to have the proper temporal resolution (30 sec response) to measure smoke under conditions of this burn and the precision is not reported and may insufficient for the weak enhancements above background (especially CO). Please provide an estimate of the measurement precision for CO and CO_2 relevant for the 1-minute time resolution reported, e.g. 30-s standard deviation of measured mixing ratio while sampling a midrange calibration gas.

-The signal standard deviation of the CO_2 analyser was 0.04 ppm while sampling calibration gas. This would be an estimate for the precision of any 1 min value.

-The signal standard deviation of the CO analyser was 6 ppb while sampling calibration gas. This would be an estimate for the precision of any 1 min value.

The background was calculated "as the running first percentile of the one-minute averages during the 30 minutes before and after each measurement". This description is unclear.

Now it is written as:

"The concentrations of several trace gases vary also smoothly during the day due to biological processes such as photosynthesis, so the background was not taken as a constant value for the whole day. Instead, at every time step t all concentrations of a trace gas X measured in the time range [t-30 min, t+30min] were taken into account and the background was the lowest concentration during this time. This was applied both for the data obtained from the mast and for the PTR-MS data. For the trace gases measured from the mast at alternating altitudes the background was calculated without taking the altitude into account, i.e., by considering the time series as if it had been measured at one altitude only. ."

Section 2.3.1 states the gases were measured at 6 heights with a 1-minute time step - 30 second measurement and a 30 second flush time between levels, so there are 5 measurements per level each 30 minutes. Did you use the lowest of the 5 measurements as the background?

No. For the trace gases measured from the mast at alternating altitudes the background was calculated without taking the altitude into account, i.e., by considering the time series as if it had been measured at one altitude only.

Did you use the CO to identify the background time period or did you select for each gas separately?

The background was selected separately for each gas.

In panel F) of Figure 2 it is clear the CO2 background is changing. Please comment on: the source of this change, the rate of change during the burn, and if / how was accounted for in calculating CO2 background. I'm guessing the changing CO2 results from the growth boundary layer, initially shallow and having high CO2 from nocturnal ecosystem respiration, as it entrains air from above the canopy.

The variations in CO_2 concentration were mainly due to both photosynthesis and boundary layer growth during the day. There was only one 1-minute data point when CO_2 concentration was clearly above the baseline. This did not change even if the baseline was calculated for each altitude separately.

P16, L 24-25: There are 100's of organic gases produced in incomplete combustion (see Yokelson et al., 2013; Akagi et al., 2011), the relative importance of these gases is not fully understood and likely varies with ambient conditions (e.g. Crounse et al., Atmos. Chem. Phys., 9, 4929-4944, 2009).

This is now the introduction

P16, L33: What is the CO 30-sec measurement precision? Better than 40 ppb ?

The precision was 6 ppb.

P16, L32 – P 17, L23: I would recommend removing this section and Figure 5. It seems that the SMEAR tower did not receive enough exposure to the smoke plume to support this analysis. In fresh biomass smoke correlations between deltaCO and deltaCO2 should be very high. The lack of a strong correlation indicates the measurements are unreliable for such an analysis. I suspect the diffuse and spotty smoke, vary CO2 background, insufficient measurement precision, and sample times that were to large relative to smoke exposure all played a role in the inconclusive results.

This is true. The section was removed but from the associated figure only the correlations involving CO2 were removed. The scatter plots of ΔNOx vs ΔCO and ΔSO2 vs ΔCO are still shown.

P16, L25-29: I recommend focusing on only the time period before and during the fire 6:00 to 15:00. This would also improve the readability of Figure 2.

The end of figure and the discussion time range was changed but not to 15:00 but to 18:00. The reason is that the last research flight took place at 15:50 – 17:55. The simultaneous ground data should also be presented.

Section 3.3 Aerosol at SMEAR II: The authors should compare their results with previous studies, e.g. Hobbs et al. (1996) – similar fire type (slash from clear-cut), particle measurements for comparison.

There are now comparisons with the Hobbs et al. (1996) and the Reid et al. (2005) results.

P18, L6: specify if this (3.4 ug/m3) is the BC concentration.

Done

P20, L28-30: This interpretation is incorrect. BC production is associated with flaming combustion not smoldering combustion. One would expect to see high BC during flaming combustion but little during smoldering combustion. This is easily observed in the field: torching conifer tree crowns produce smoke that is visually very black while post fire front smoldering combustion produces smoke that is white in color. These common qualitative observations have been quantified in numerous laboratory studies (McMeeking et al. (2009) Volume 114, Issue D19; Hosseini et al. (2013) J. Geophysical Research – Atmospheres, Volume 118, Issue 17, 9914 – 9929).

True. Corrected.

P21, L33: Please note the height of the REA cottage inlet

16 m AGL, now given in text

P21, L34: Why were data not available after 12:00?

The instrument was removed for an other location. It was believed that no smoke arrives any more. That was obviously a mistake.

P22, L8 – 22: The correlation of deltaX with deltaCO seem reasonable for fresh smoke this suggest the poor correlation between deltaCO and deltaCO2 was related to the CO2 measurement. Emission Ratios When emission ratios (ER) are used to calculate EF the intercepts are usually forced to zero (e.g. Yokelson et al., 1999; Burling et al., 2010). Therefore one should use the forced zero intercept slopes for

comparison with published values literature values and given in Table 3 instead of the slopes with the fit intercept. The authors made an error in calculating emission ratios from emission factors. They did not account for the molecular weights of the species. The calculation should be made as: deltaX/deltaCO = (MWCO/MWX)(EFX/EFCO) e.g. for toluene: MWtoluene = 92 g/mol and A & M EFtoluene = 0.40 g/kg MWco = 28 g/mol and A & M EFco = 107 g/kg deltatoluene/deltaCO = (28/92)(0.40/107) = 0.00114 See Sect 2.2 of Andreae & Merlet 2001. The more recent EF review by Akagi et al. should be used as the basis for comparison, not A & M 2001, it is outdated. The authors should also include Simpson et al (2011) who measured boreal fire emissions.

This is all true. The calculations have now been redone

1) by taking into account the molecular weights

2) by forcing the linear regression through zero

3) The comparison now includes both Akagi et al. and Simpson et al. (2011).

P24, L 4-11: Please note the measurement response time, typical aircraft speed, and the typical sample length of each plume passage maximum (e.g. at 100 m/s speed and 1 sec measurement rate, each data point is roughly a 100 m sample segment).

With a flight speed of 38 ± 2 m s^{-1}, at about 100 m above ground level, the plume was crossed in about 3–4 seconds, yielding a plume width of about 120 m. At 1200 m above ground level, the plume width was 800 ± 100 m.

This is now explained in the text.

P 25, L12: awkward, needs to be rewritten

Corrected. Now it reads

> The rates of soil CO2 efflux at the sites were similar before the clear-cut and burning of slash. After the treatment, the efflux at the burnt site was approximately only a half of the flux at the control site (Kulmala et al. 2013).

P 25, L26-27: Please give examples/reference for this statement.
P 25, L 26-30: Can the authors comment on the magnitude of soil VOC emissions relative to foliar emissions and emissions from down dead wood?

Corrected. Now it reads

> The soil VOC emissions were generally low compared to the total VOC emissions from similar forest ecosystems. This result is also in line with the general finding that the forest floor makes up maximum ~ 10% of the total ecosystem VOC emissions in coniferous forest (Hayward et al., 2001; Hakola et al., 2006; Taipale et al., 2008).

P27, L 10-12: After correcting calculation errors please compare with more relevant/ recent papers (Akagi et al., Simpson et al., 2011 – see above)

Done

P27, L14-32: Consider comparing with Hobbs et al. (1996) and Reid et al. reviews (see comments above)

Done.

Specific comments on Figures

Figure 2: Focus on time period before and during the burn 6:00 to 15:00. Specify the temporal spacing of data points at each level (6 minutes?).

The time of the figures was changed to 07:00 – 18:00 because the last research flight took place at 15:50 – 17:55.

Figure 3. P14, L 31 states the TKE and Hs are 1 minute averages. Please clarify in caption.

Done

Figure 4. The water vapor mixing ratio should be rescaled

Unfortunately, the plotting package used to create this plot only allows a maximum of two scales (one left and one right). So, there can be no separate third scale for the water vapor. Seeing fluctuations in water vapor with a 0.1 g/kg resolution is simply not relevant to our argument, anyway. So, there's really no point in expanding the plot. No change.

Figure 5. This figure should be eliminated. See comments above.

The figure was not totally eliminated, just the ones including carbon dioxide.

Figure 6. Focus on time period before and during the burn 6:00 to 15:00. L6 change 'E)' to 'F)', specify where the mass concentration of particles <10um comes from (Dust Track?)

The time of the figures was changed to 07:00 – 18:00 because the last research flight took place at 15:50 – 17:55. The mass concentration comes from the particle number size distributions measured with the DMPS and the APS.

Figure 10. Focus on the 7:00 to 10:00 period since VOC measurements after this period are not discussed in the paper.

Done

Figure 13. Is the concentration in panel B average, median, or peak? Caption for panel A, should this read 'symbol color' ?

In B it is the maximum concentration in the plume crossing.
Corrected.

TECHNICAL COMMENTS

P3, L12: change 'have also' to 'also have'

Done

P3, L30-31: Awkward sentence should be rewritten.

Done

P3, L34: change 'wildlife' to 'wildfire'

Done

P4, L2: change 'burning forests' to 'managed forest burning'

Done

P4, L11: sentence starting with 'Nowadays' is awkward and be rewritten

Done

P4, L 14: suggest using 'managed burning' instead of 'controlled'
Done

P4, L15: delete 'controlled'
Done

P4, L34: Maybe change to 'Specific goals and objectives: : :' since the list includes both goals and objectives (i.e. clear and measurable targets such as measuring emission factors)
Done

P4, L12: change 'analyze' to 'assess' or 'evaluate' and delete 'used'
Done

P4, L24: 'frequently', need better description – is this 3 m/s average or median or what?
The text now reads
"Based on this climatology, wind directions of 180–200° occur 9.6% of the time with no particular preference for a specific time of day. When the wind occurs in this direction, the average wind speed is 3.1 ± 1.3 m s^{-1} and 86 % of wind speeds are less than the 5 m s^{-1} threshold required for a safe burn.

Insert 'threshold' prior to 'required'
P5, L6: change 'an extraction' to 'the difference'
Done

P5, L14: change 'an extraction' to 'the difference'
Done

P9, L28: "have a lack-time" doesn't make sense, please rewrite.
Done, it now reads
Also, although the soil conditions do change rapidly during and after the fire, many of the biological processes and responses to changing conditions have a time lag and therefore require several years of monitoring.

P10, L6: change 'burning' to 'burn'
Done

P14, L32: delete 'and' between 'fluxes' and 'associated'
Done

P16, L27: change '08.47' to '08:47'
Done

P 25, L12: awkward, needs to be rewritten
Rewritten, it now reads

The rates of soil CO_2 efflux at the sites were similar before the clear-cut and burning of slash. After the treatment, the efflux at the burnt site was approximately only a half of the flux at the control site (Kulmala et al. 2013).