Responses to Referee #2’s comments

General comment: This study investigated the OH reaction rate of pure eugenol compound and its SOA yield with a custom-built oxidation flow reactor (OFR). The impact of NO_2 and SO_2 influence on SOA formation was also investigated. The results of the study might be very interesting to many, yet quite a few items need to be clarified before it can be accepted for publication.

Response to comment: Many thanks for the reviewer’s constructive comments and valuable suggestions, which would be much helpful to improve the scientific merits of this manuscript. The concerns raised by the reviewer have been carefully addressed in the revised manuscript.

Comment 1: Experiment design: The manuscript mainly deals with two topics: the rate constant of Eugenol-OH reaction and SOA yield of Eugenol. As for the 6 experiments listed in the Table 1, apart from the Eugenol, how about the combination of other species such like m-xylene/1,3,5-TMB/SO_2/NO_2. For experiments determines the rate constant for the reaction with OH, were SO_2/NO_2 added? And for SOA yield studies, what was the reference compound to derive OH? It seemed that here SO_2/m-xylene/1,3,5-TMB are all not appropriate to sever as the reference compound since m-xylene/1,3,5-TMB they themselves are also SOA precursors and SO_2 instead influence the SOA yield. It is quite confusing. The authors need to clarify in Table 1 what combinations (including reference compound for deriving OH) of species are prepared for obtaining the rate constant, and what combinations instead are for studying the SOA yield.

Response to comment 1: Thank you for your suggestion. When measuring the rate constant of eugenol-OH reaction, m-xylene or 1,3,5-TMB were used as reference in the absence of SO_2 and NO_2. This was pointed out in Table 1. When investigating SOA formation, both m-xylene and 1,3,5-TMB were not introduced into the reactor because they were also SOA precursors. The caption of Table 2 has been rewritten in the revised manuscript to make it more clear.

For SOA yield studies, OH exposure in the OFR was calculated using SO_2 as the reference compound in the separate calibration experiments. The repeat experiments showed relative low uncertainties (± 10.2%) about OH exposure. At the same time, OH exposure were also calculated using a box model (Peng et al., 2015). The modelled OH exposures were also in well agreement with the measured results. In order to investigate the effects of NO_2 and SO_2 on SOA formation, NO_2 and SO_2 were separately added in the OFR.

According to your valuable suggestion, more experimental conditions have been added in Table 2 in the revised manuscript, which is shown in Table R1.

Table R1. Experimental conditions and results for SOA formation.

Expt.	$[\text{eugenol}]_0$[a] (μg m^{-3})	△[eugenol][b] (μg m^{-3})	M_0[c] (μg m^{-3})	SO_2 (ppbv)	NO_2 (ppbv)	Y_{max}[d]	OH Exposure[e] (10^{11} molecules cm^{-3} s)	τ[f] (d)
#1	272	265	29	–	–	0.11	5.41	4.17
#2	351	339	54	–	–	0.16	5.41	4.17
#3	485	474	83	–	–	0.18	5.41	4.17
#4	636	625	145	–	–	0.23	5.41	4.17
#5	874	858	241	–	–	0.28	7.37	5.68
#6	1327	1304	399	–	–	0.31	8.91	6.87
#7	273	267	40	41	–	0.15	5.41	4.17
#8	273	266	35	–	40	0.13	5.41	4.17

[a] Initial eugenol concentrations.

[b] Reacted eugenol concentrations.

[c] SOA concentrations.

[d] Maximum SOA yields.

[e] Corresponding OH exposure of maximum SOA yields.

[f] Corresponding atmospheric aging time of maximum SOA yields, calculated using a typical [OH] in the atmosphere in this work (1.5×10^6 molecules cm^{-3}) (Mao et al., 2009).

Revision in the manuscript:

Table R1 has been added in the revised manuscript (i.e., Table 2).

Comment 2: From the study only the overall rate constant for reaction with OH was obtained. It seemed that not so much degradation kinetics are presented as indicated by the title.

Response to comment 2: According to your valuable suggestion, "degradation kinetics" in the title has been replaced by “rate constant”.

Revision in the manuscript:

Title, Change: “Degradation kinetics and secondary organic aerosol formation from eugenol by hydroxyl radicals” **To** “Rate constant and secondary organic aerosol formation from the gas-phase reaction of eugenol with hydroxyl radicals”

Comment 3: Line 28: The enhancement values need not to have 4 significant figures.

Response to comment 3: Four significant figures of the enhancement values have been reduced to three significant figures.

Revision in the manuscript:

Line 28, Change: “38.57% and 19.17%” **To** “38.6% and 19.2%”

Comment 4: Lines 77-84: What’s the overall concentration of eugenol in ambient air? How important does this precursor compared with other Methoxyphenols.

Response to comment 4: As a type of methoxyphenols, eugenol is a representative compound with a branched alkene group, which has been mentioned in the revised manuscript. Based on the previous work, the overall concentration of eugenol in ambient air is on the order of ng m^{-3} and is comparable to the other methoxyphenols (Simpson et al., 2005; Bari et al., 2009). This has been added in the revised manuscript.

Revision in the manuscript:

Lines 77-80, Add: “4-Allyl-2-methoxyphenol (eugenol) is a typical methoxyphenol produced by ligin pyrolysis with a branched alkene group. It is widely detected in the atmosphere with the concentration on the order of ng m^{-3}, which is comparable to those of other methoxyphenols (e.g., guaiacol and syringol)”

Comment 5: Line 105: The [O_3] was in the range of 0.94-9.11 ppmv while you want to explore the reaction between OH radical and eugenol. Can you clarify whether such high level of O_3 was just used to generate OH radical in the mixing tube, or they indeed existed in the flow tube? If it is the latter case as your supplement material shows, then an evaluation of the interference from the O_3 is needed.

Response to comment 5: O_3 with the concentration of 0.94−9.11 ppmv in the OFR was used to generate OH radicals, and its concentration decreased to 0.39−6.02 ppmv after the photo-reaction between O_3 and H_2O. In order to evaluate the possible decay of eugenol via the reaction with O_3 and the possible SOA formation from their reaction, the control experiments were conducted in this work. The results showed that the concentration of eugenol was not affected by O_3 and no SOA formation was observed by SMPS and HR-ToF-AMS. These results were mainly resulted from the short reaction time in the OFR and the low rate constants of O_3 with methoxyphenols (~10^{-19} cm^3 $molecule^{-1}$ s^{-1}) (El Zein et al., 2015). These descriptions have been added in the Supplement.

The variations in the normalized concentrations of eugenol and reference compounds (i.e., 1,3,5-trimethylbenzene and *m*-xylene) in the presence of 9.11 ppmv O_3 are shown in Figure R1.

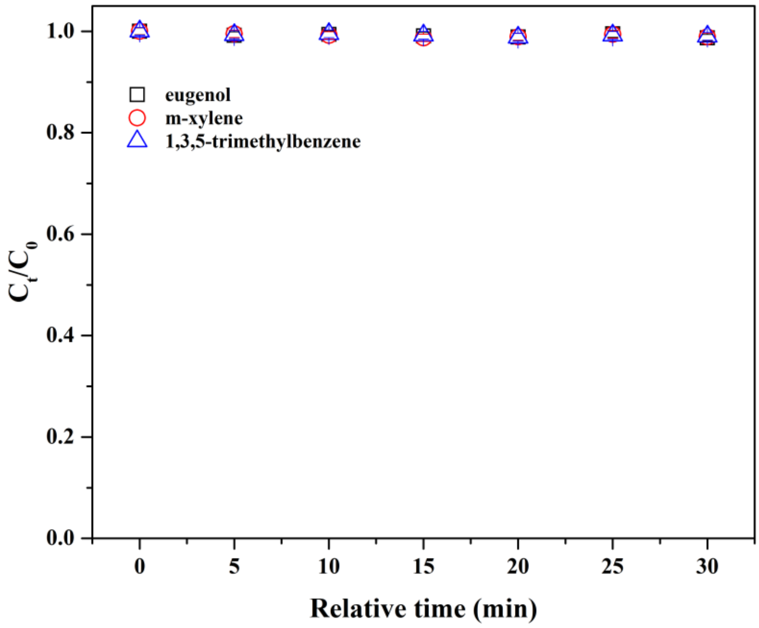

Figure R1. Variations in the normalized concentrations of eugenol and reference compounds (i.e., 1,3,5-trimethylbenzene and *m*-xylene) in the presence of 9.11 ppmv O_3.

Revisions in the manuscript:

Lines 105-107, Change "O_3 was produced by passing zero air through an O_3 generator (Model 610-220, Jelight Co., Inc.), and its concentration was in the range of 0.94−9.11 ppmv in this work measured with an O_3 analyzer (Model 205, 2B Technology Inc.)" **To** "O_3 with the concentration of 0.94−9.11 ppmv in the OFR was produced by passing zero air through an O_3 generator (Model 610-220, Jelight Co., Inc.), which was used to produce OH radicals."

Lines 140-142, Add: "The possible effect of O_3 on the decay of eugenol and reference compounds was investigated in this work. As shown in Fig. S3, their concentrations were not affected by O_3. Meanwhile, no SOA formation was observed by the SMPS and HR-ToF-AMS."

Supplement, Lines 24-34, Add: "Before photochemical reaction, the concentration of O_3 in the OFR was in the range of 0.94−9.11 ppmv, which decreased to 0.39−6.02 ppmv due to the consumption by H_2O with 254 nm UV light. In order to evaluate the possible decay of eugenol via the reaction with O_3 and the possible SOA formation from their reaction, the control experiments were conducted in this work. The results showed that the concentration of eugenol was not affected by O_3 and no SOA formation was observed by SMPS and HR-ToF-AMS. In addition, the possible effects of O_3 on the decay of reference compounds (i.e., 1,3,5-trimethylbenzene and *m*-xylene) were also investigated. The results showed that the decays of reference compounds by O_3 could be ignored in this work. The variations in the concentrations of eugenol and reference compounds (i.e., 1,3,5-trimethylbenzene and *m*-xylene) in the presence of 9.11 ppmv O_3 are shown in Fig. S3."

Figure R1 has been added in the revised Supplement.

Comment 6: Lines 109-110: This is not acceptable. The OH reactivity applied in this experiment is at least 80 s^{-1} to 380 s^{-1} with only Eugenol accounted (using OH reaction rate calculated in section 3.1), not mentioned the SO_2 (0-198 ppb) and NO_2 (0-109 ppb in line 27) added in the later experiment. It suggested the calculated OH exposure should be at least several times lower than the OH exposure calculated without considering the external OH reactivity (=0 s^{-1} assumed in original calculation) (Peng et al., 2015)

Response to comment 6: Thank you very much for your valuable suggestion. In this work, the OH exposures calculated by SO_2 decay in separate calibration experiments were taken as the original exposures without the external OH reactivity.

The OH suppression by the external OH reactivity has been recalculated in the revised manuscript. According to the concentration of eugenol in this work, the OH reactivity was in the range of about 85 s^{-1} to 410 s^{-1}, calculated using the method described by Peng et al. (2015). Subsequently, according to the OFR exposure estimator (v2.3) developed by Jimenez's group based on the estimation equations reported in the previous work (Li et al., 2015; Peng et al., 2015, 2016), the maximum reduction of OH exposure by eugenol in the OFR is approximately 90%, which has been corrected in the revised manuscript. The detailed calculation has been added in the Supplement.

Revisions in the manuscript:

Line 232, Change "30%" **To** "90%"

Lines 232-233, Add: "Its detailed calculation has been shown in the Supplement."

Supplement, Lines 59-74, Add:

3. Calculation of OH suppression

The OH suppression by external OH reactivity in the OFR was estimated according to the OFR exposure estimator (v2.3) developed by Jimenez's group based on the estimation equations reported in the previous work (Li et al., 2015; Peng et al., 2015, 2016). The concentration of O_3 required by this estimator was in the range of 7−70 ppmv. Thus, O_3 with the concentrations of 7.8 and 9.1 ppmv in this work was used for this estimator. In addition, RH and rate constant for eugenol with OH radicals were 44% and 8.01×10^{-11} cm^3 $molecule^{-1}$ s^{-1} used in this estimator. The external OH reactivity in this estimator was only taken eugenol into account, due to its much higher concentration than those of SO_2 and NO_2. The external OH reactivity was calculated to be in the range of 85 s^{-1} to 410 s^{-1}, according to the following equation (Peng et al., 2015):

$$\mathrm{OHR_{ext}} = \mathrm{k_{eugenol+OH}}[\mathrm{eugenol}] \tag{S3}$$

where $\mathrm{OHR_{ext}}$ is the external OH reactivity, $\mathrm{k_{eugenol+OH}}$ is the rate constant of eugenol with OH radicals, and $[\mathrm{eugenol}]$ is the concentration of eugenol.

According to the parameters mentioned above, the maximum reduction of OH exposure by eugenol in the OFR was approximately 90%.

Comment 7: Lines 116-117: Better add the assumed average [OH] and the reference as well.

Response to comment 7: The assumed average OH concentration and the reference have been added in the revised manuscript.

Revision in the manuscript:

Lines 119-120, Add: "which was calculated using a typical [OH] of 1.5 × 10^6 molecules cm^{-3} in the atmosphere (Mao et al., 2009)."

Comment 8: Lines 124-125: How do the authors calibrate their aerodynamic size distribution in AMS? If the authors consider the chemical-composition based particle density (Kuwata et al., 2012; Salcedo et al., 2006), how about it when compared to the effective aerosol density applied here? The aerosol size distribution of each experiment should be considered separately. The 100% full cut size of AMS lens is around 600 nm in mobility size (Nault et al., 2018). The effective density calculation from dva/dm could be biased if the aerosol in OFR grow beyond the AMS lens cut.

Response to comment 8: In this work, the HR-ToF-AMS is size-calibrated using NH_4NO_3 particles with the diameter between 60−700 nm selected by a DMA. This sentence has been added in the revised manuscript.

According to the equation based on chemical composition (i.e., H/C and O/C) (Kuwata et al., 2012), the calculated particle density (1.7−2.1 g cm^{-3}) is higher than the effective particle density estimated using the equation $\rho=d_{va}/d_m$ (DeCarlo et al., 2004). The discrepancy might be mainly resulted from the relatively higher oxidation state of SOA in this work. In addition, we agree that aerosol size distribution of each experiment should be considered separately. But, assuming that particles were spherical and non-porous, all effective particle density was calculated to be in the range of 1.42 to 1.59 g cm^{-3} in this work. Considering the insignificant discrepancy among all effective density, the average effective particle density of 1.5 g cm^{-3} was applied. In addition, the maximum diameters of SOA for all experiments were lower than 600 nm. Thus, the bias of the effective particle density should be reasonably insignificant, calculated using the equation $\rho=d_{va}/d_m$ (DeCarlo et al., 2004).

Revision in the manuscript:

Lines 131-132, Add: "The particle size for HR-ToF-AMS measurement was calibrated using NH_4NO_3 particles with the diameter between 60−700 nm selected by a DMA."

Comment 9: Lines 152-153: The decay of SO_2 was used to calculate [OH], so why was SO_2 not used as the reference compound in the relative rate method? As mentioned above, the reader should be informed in Table 1 what were initially added and what are the reference compound.

Response to comment 9: In general, the reference compounds are selected to have OH rate constants similar in magnitude to that of the test compound (Edney et al., 1986). The rate constant of SO_2 with OH radicals is 9×10^{-13} cm^3 $molecule^{-1}$ s^{-1} (Davis et al., 1979), which is about 2 orders of magnitude lower than that of eugenol with OH radicals obtained by the relative-rate method in this work. Thus, SO_2 is not suitably selected as the reference compound in the relative-rate method.

According to your valuable suggestion, more experimental conditions have been added in Table 2 in the revised manuscript, which is shown in Table R1.

Table R1. Experimental conditions and results for SOA formation.

Expt.	$[eugenol]_0$[a] (μg m^{-3})	△[eugenol][b] (μg m^{-3})	M_0[c] (μg m^{-3})	SO_2 (ppbv)	NO_2 (ppbv)	Y_{max}[d]	OH Exposure[e] (10^{11} molecules cm^{-3} s)	τ[f] (d)
#1	272	265	29	–	–	0.11	5.41	4.17
#2	351	339	54	–	–	0.16	5.41	4.17
#3	485	474	83	–	–	0.18	5.41	4.17
#4	636	625	145	–	–	0.23	5.41	4.17
#5	874	858	241	–	–	0.28	7.37	5.68
#6	1327	1304	399	–	–	0.31	8.91	6.87
#7	273	267	40	41	–	0.15	5.41	4.17
#8	273	266	35	–	40	0.13	5.41	4.17

[a] Initial eugenol concentrations.

[b] Reacted eugenol concentrations.

[c] SOA concentrations.

[d] Maximum SOA yields.

[e] Corresponding OH exposure of maximum SOA yields.

[f] Corresponding atmospheric aging time of maximum SOA yields, calculated using a typical [OH] in the atmosphere in this work (1.5×10^6 molecules cm^{-3}) (Mao et al., 2009).

Revision in the manuscript:

Table R1 has been added in the revised manuscript (i.e., Table 2).

Comment 10: Lines 135-137: This is confusing. Did the authors examine a full cycle of UV light applied in the experiment?

Response to comment 10: Yes. In order to investigate the possible photolysis of eugenol and reference compounds at 254 nm UV light in the OFR, the comparative experiments are performed with UV lamp turned on and turned off, when eugenol or reference compounds are introduced into the OFR. To describe accurately, this sentence has been supplemented in the revised manuscript.

Revision in the manuscript:

Lines 144-146, Add: "the comparative experiments were conducted with UV lamp turned on and turned off, when eugenol and reference compounds were introduced into the OFR"

Comment 11: Lines 140-141: What is the photon flux of 254 nm in OFR. How do the authors determine 254 exposure/OH exposure ratio?

Response to comment 11: The photo flux of 254 nm is 2.0×10^{14} photon cm^{-2} s^{-1}. According to the OH exposure calculated in this work, 254 nm photon flux/OH exposure is in the range of 1.6×10^2 to 1.7×10^3 cm s^{-1}, which has been added in the revised manuscript.

Revision in the manuscript:

Line 151, Add: "(1.6×10^2 to 1.7×10^3 cm s^{-1})"

Comment 12: Lines 163-164: Is it possible that the difference between your measurement and the theory was caused by the O_3 reaction?

Response to comment 12: Considering that the decays of eugenol and reference compounds by O_3 was negligible, the difference between the measured rate constant and the theory rate constant was not reasonably caused by O_3. In addition, inaccurate performance of the AOP WIN model has been widely observed (Coeur-Tourneur et al., 2010; Lauraguais et al., 2012), because AOP WIN model is an empirical model (structure activity relationship model).

Comment 13: Lines 164-167: Have the authors considered the potential wall loss of three species, which could result in different species decay ratios. Thus extra uncertainty on OH reaction rate coefficient of Eugenol could be introduced.

Response to comment 13: The possible wall losses of eugenol and reference compounds in the OFR in this work were investigated. But, the results showed that the insignificant wall loss (< 3%) was observed by HR-ToF-PTRMS. The uncertainty on the rate constant of eugenol with OH radicals has been shown in Table 1 in the original manuscript.

Revisions in the manuscript:

Line 188, Change "2.31" **To** "(2.31 ± 0.12)"

Line 188, Change "8.01" **To** "(8.01 ± 0.40)"

Comment 14: Lines 181-183: How about those reaction rate coefficients estimated from experiment when compared to those from the SAR method?

Response to comment 14: According to the US EPA AOP WIN model based on the structure activity relationship (SAR) (US EPA, 2012), the rate constants of the OH-initiated reactions of guaiacol, 2,6-dimethylphenol, and syringol are 2.98 $\times 10^{-11}$, 5.05 $\times 10^{-11}$, and 16.51 $\times 10^{-11}$ cm^3 $molecule^{-1}$ s^{-1}, respectively, which have been added in the revised manuscript. Their corresponding rate constants obtained from experiments are 7.53 $\times 10^{-11}$, 6.70 $\times 10^{-11}$, and 9.66 $\times 10^{-11}$ cm^3 $molecule^{-1}$ s^{-1}, respectively (Coeur-Tourneur et al., 2010; Thuner et al., 2004; Lauraguais et al., 2012). These differences among rate constants suggest that it is necessary to determine the rate constants of multifunctional organics through lab experiments.

Revision in the manuscript:

Lines 198-202, Add: "while their corresponding rate constants were calculated to be 2.98 $\times 10^{-11}$, 5.04 $\times 10^{-11}$, and 16.51 $\times 10^{-11}$ cm^3 $molecule^{-1}$ s^{-1}, according to the US EPA AOP WIN model (US EPA, 2012). These differences among rate constants suggest that the rate constants of multifunctional organics should be necessarily determined via lab experiments"

Comment 15: Line 207: The decrease have also been reported in references of (Palm et al., 2016; Palm et al., 2018)

Response to comment 15: These two references have been added in the revised manuscript.

Revision in the manuscript:

Lines 228, Add: "Palm et al., 2016, 2018"

Comment 16: Line 211: Should be larger than 30% based on Fig. S4 in (Peng et al., 2015). Please show the detailed calculation results.

Response to comment 16: Thank you very much for your valuable suggestion. We are very sorry to make this mistake about OH suppression estimation. According to the concentration of eugenol in this work, the OH reactivity was in the range of about 85 s^{-1} to 410 s^{-1}, calculated using the method described by Peng et al. (2015). Subsequently, according to the OFR exposure estimator (v2.3) developed by Jimenez's group based on the estimation equations reported in the previous work (Li et al., 2015; Peng et al., 2015, 2016), the maximum reduction of OH exposure by eugenol in the OFR was approximately 90%, which has been corrected in the revised manuscript. The detailed calculation has been added in the Supplement.

Revisions in the manuscript:

Line 232, Change "30%" **To** "90%"

Lines 232-233, Add: "Its detailed calculation has been shown in the Supplement."

Supplement, Lines 59-74, Add:

3. Calculation of OH suppression

The OH suppression by external OH reactivity in the OFR is estimated according to the OFR exposure estimator (v2.3) developed by Jimenez's group based on the estimation equations reported in the previous work (Li et al., 2015; Peng et al., 2015, 2016). The concentration of O_3 required by this estimator is in the range of 7−70 ppmv. Thus, O_3 with the concentrations of 7.8 and 9.1 ppmv in this work was used for this estimator. In addition, RH and rate constant for eugenol with OH radicals were 44% and 8.01×10^{-11} cm^3 $molecule^{-1}$ s^{-1} used in this estimator. The external OH reactivity in this estimator was only taken eugenol into account, due to its much higher concentration than those of SO_2 and NO_2. The external OH reactivity was calculated to be in the range of 85 s^{-1} to 410 s^{-1}, according to the following equation (Peng et al., 2015):

$$\mathrm{OHR}_{\mathrm{ext}} = \mathrm{k}_{\mathrm{eugenol+OH}}[\mathrm{eugenol}] \qquad (S3)$$

where $\mathrm{OHR}_{\mathrm{ext}}$ is the external OH reactivity, $\mathrm{k}_{\mathrm{eugenol+OH}}$ is the rate constant of eugenol with OH radicals, and $[\mathrm{eugenol}]$ is the concentration of eugenol.

According to the parameters mentioned above, the maximum reduction of OH exposure by eugenol in the OFR was approximately 90%.

Comment 17: Line 227: How about the wall loss of aerosols in the flow tube. The authors could examine the wall loss by directly injecting aerosols into the OFR.

Response to comment 17: According to our previous results reported by Liu et al. (2014), the wall loss of particles (< 3%) in this flow tube could be ignored, mainly resulted from the short residence time and a uniform velocity profile. The ignorable wall loss of particles in this flow tube has been added in the revised manuscript.

Revision in the manuscript:

Lines 218-219, Add: "The wall loss of aerosol particles in the OFR could be ignored, according to our previous results reported by Liu et al. (2014)."

Comment 18: Line 263: Have the author consider the $NH_4NO_3 \rightarrow CO_2$ effect, which could influence f_{CO2} and thus O/C and H/C ratio substantially. This bias could be larger especially in the later NO_2 + Eugenol experiment.

Response to comment 18: Thank you very much for your valuable suggestion. Considering that zero air was used as the carrier gas in this work, the residual NH_3 could be ignored. In addition, the amount of ammonium salts formed in the OFR measured by HR-ToF-AMS was very low (< 0.2 μg m^{-3}). Thus, NH_4NO_3 should have insignificant influence on f_{CO2} in this work.

Comment 19: Line 284-286: For saying this, OH exposure should be compared.

Response to comment 19: The comparison of OH exposure in smog chamber and PAM has been added in the revised manuscript.

Revision in the manuscript:

Lines 305-307, Add: "because OH exposure in the PAM reactor is about 1−3 orders of magnitude higher than that in smog chamber"

Comment 20: Line 294-296: We cannot definitely conclude that the decrease is due to the fragmented molecules formed through the oxidation of gas-phase species. So better add “probably” or “possibly” before “due to”.

Response to comment 20: “Possibly” has been added before “due to” in the revised manuscript.

Revision in the manuscript:

Line 316, Add: “possibly”

Comment 21: Line 311: Can the formed SO_4 be fully explained by the SO_2 decay in SO_2+ Eugenol experiment?

Response to comment 21: According to the law of conservation of mass, the mass concentration of sulfate could be fully explained by the SO_2 decay.

For example, the sulfate concentration formed at the OH exposure of 12.55×10^{11} molecules cm^{-3} s was about 51 μg m^{-3}, shown in Figure 4 in the orignial manuscript. Meanwhile, the consumption of SO_2 by OH radicals was about 14 ppbv when the initial concentration of SO_2 was 41 ppbv SO_2. Therefore, the mass concentration of sulfate could be fully explained by the SO_2 decay, according to the law of conservation of mass.

Comment 22: Line 323: Why does the eugenol can partition quickly under acidic aerosol condition?

Response to comment 22: We are very sorry to make an expression mistake. This sentence has been rewritten in the revised manuscript.

Revision in the manuscript:

Lines 345-347, Change “Under acidic conditions, the gas-phase oxidation products of eugenol would be partitioned more quickly into the particle-phase and further oxidized into low volatility products, or produce oligomeric organics by acid-catalyzed heterogeneous reactions” **To** “Under acidic conditions, the gas-phase oxidation products of eugenol partitioned onto the particle-phase would be further oxidized into low volatility products or produce oligomers by acid-catalyzed heterogeneous reactions”

Comment 23: Lines 360-362: In the paper of Finewax et al. (2018), it is phenoxy radical rather than OH-aromatic adducts that react with NO_2 or O_2. In fact, the formations of phenoxy radical and OH-aromatic adduct from phenols are totally different in reaction pathways. Lines 366-367: Still, the reaction pathway through the NO_2 addition on the phenoxy radical was neglected by the author.

Response to comment 23: Thank you very much. According to your valuable suggestion, "OH-aromatic adduct" has been replaced by "phenoxy radical" in the revised manuscript.

Revisions in the manuscript:

Lines 381 and 384, Change "OH-aromatic adducts" **To** "phenoxy radicals"

Line 388, Change "OH-eugenol adduct" **To** "phenoxy radical"

Comment 24: Line 375: The authors could still specify the NO_3 exposure compared to OH exposure by assuming thermo-steady state of NO_2 and O_3.

Response to comment 24: According to your valuable suggestion, the NO_3 exposure was estimated using a box model. The maximum exposure of NO_3 radicals was calculated to be approximately 1.7×10^{11} molecules cm^{-3}, using the maximum O_3 concentration of 9.11 ppmv in this work. This exposure was about one order of magnitude lower than the maximum OH exposure. In addition, the rate constant of NO_3 radicals with eugenol was reported to be 1.6×10^{-13} cm^3 $molecule^{-1}$ s^{-1} (Zhang et al., 2016), which is about 2 orders of magnitude lower than that (8.01×10^{-11} cm^3 $molecule^{-1}$ s^{-1}) for eugenol with OH radicals obtained in this work. Therefore, the decay of eugenol by NO_3 radicals was not predominant, compared to the reaction of eugenol with OH radicals.

Based on the discussion mentioned above, the detailed revision in the revised manuscript was pointed out as follows.

Revision in the manuscript:

Lines 396-401, Add: "Using the box model (Peng et al., 2015) and the maximum O_3 concentration (9.11 ppmv) in this work, the maximum NO_3 exposure was calculated to be approximately 1.7×10^{11} molecules cm^{-3} s. Compared to the rate constant of eugenol with OH radicals obtained in this work, the rate constant (1.6×10^{-13} cm^3 $molecule^{-1}$ s^{-

[1]) of eugenol with NO_3 radicals was about 2 orders of magnitude lower (Zhang et al., 2016). Thus, the contribution of NO_3 radicals on the decay of eugenol was insignificant."

Comment 25: Line 391: In this method, if the NO^+ and NO_2^+ ions from organonitrate were missed, the organic nitrate calculation was underestimated (Farmer et al., 2010). The authors can use the real time NO^+/NO_2^+, and NO^+/NO_2^+ ratio from NH_4NO_3 and organonitrate (a factor of 2.25) (Fry et al., 2018) to calculate $-ONO_2$ group concentration for organonitrates.

Response to comment 25: The fraction of organic nitrates has been calculated to be in the range of 25.64% to 82.05% in the revised manuscript, using the NO^+/NO_2^+ ratios obtained at different OH exposure, according to the method described by Fry et al. (2013). In addition, the calculation method has been added in the revised Supplement.

Revisions in the manuscript:

Lines 418-421, Add: "According to the method described by Fry et al. (2013) (shown in Supplement), the fraction of organic nitrate was calculated to be in the range of 25.64% to 82.05%, using the NO^+ / NO_2^+ ratios (3.98−6.09) obtained at different OH exposure"

Supplement, Lines 51-58, Add:

2. Calculation of organic nitrate fraction

The fraction of organic nitrate can be typically calculated according to the following equation (Fry et al., 2013):

$$\mathrm{RONO}_{2,\mathrm{frac}} = \frac{(R_{\mathrm{experiment}} - R_{\mathrm{NH_4NO_3}})(1 + R_{\mathrm{RONO_2}})}{(R_{\mathrm{RONO_2}} - R_{\mathrm{NH_4NO_3}})(1 + R_{\mathrm{experiment}})} \tag{S2}$$

where $\mathrm{RONO}_{2,\mathrm{frac}}$ is the fraction of organic nitrate, $R_{\mathrm{experiment}}$ is the ratio of NO_2^+ / NO^+ measured by HR-ToF-AMS in the experiments, $R_{\mathrm{NH_4NO_3}}$ (0.295) and $R_{\mathrm{RONO_2}}$ (0.13) are the NO_2^+ / NO^+ ratios for ammonium nitrate and organic nitrates, respectively (Fry et al., 2013).

Comment 26: Line 839: "ratioas" should be "ratio as".

Response to comment 26: "ratioas" has been replaced by "ratio as" in the revised manuscript.

Revision in the manuscript:

Line 897, Change “ratioas” **To** “ratio as”

References

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