

Interactive comment on “Monoterpene chemical speciation in the Amazon tropical rainforest: variation with season, height, and time of day at the Amazon Tall Tower Observatory (ATTO)” by Ana María Yañez-Serrano et al.

Anonymous Referee #1

Received and published: 27 October 2017

General:

The paper presents in-canopy, speciated BVOC measurements by GC-FID over a period of three days. These are compared PTR measurements and to longer time series of GC measurements in different seasons at the same site. The data is analyzed and discussed in terms of emission height, diurnal cycle, reactivity, and seasonality in comparison to the longer data series in different seasons. Observations are compared to a canopy model which contained detailed chemistry for some species and rudimentary chemistry for new others. The purpose of the comparison is to demonstrate that

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speciated measurements are a good test if the overall key features and processes are captured by the model. The manuscript presents interesting data and data analysis. The text is well structured. The manuscript could be published in ACP after considering the following major and minor comments.

My major concern relates to the discussion of the role of deposition on wet leaf surfaces. From the presented material I do not see proof - from the model comparison - that deposition on wet leaves is a significant process. This may be in parts owed to the fact that description and interpretation are not detailed enough. The authors may address the following questions and issues.

(I think a paragraph should be started in line 406 where the leaf wetness is discussed.)

1. Did you observe the leaf surface wetness in the two nights? If yes, was it the same or was it different?
2. You suggest that the model has a strong missing sink in the night from the 17. to the 18. Is this the only possible explanation for the strongly overpredicted α -pinene mixing ratio? Or could be there more reasons for the model showing so high α -pinene in the night from the 17. to the 18.? Only if the source and the chemical sink of α -pinene are about the same in the two nights then the depositions sink must have been also similar as the mixing ratios are about the same. Were source and chemical sink the same in both nights?
3. How can deposition on wet leaves affect so strongly the mixing ratio of non-water soluble compounds like α -pinene?
4. I understand there was no RH data for the 18.. How could the model derive a reliable surface wetness then?
5. Why do the different MT respond so differently to the ill predicted surface wetness on 17.: α -pinene and β -pinene too high, limonene ok, myrcene in between, and terpinolene too low. Insofar I think the statement in line 527 is not justified by the presented

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material and analysis.

Major comments:

line 514-518: I do not understand what the authors are discussing here. I cannot understand how a “potential” aerosol growth could have a dimension of molecules per cm³ and an order of 10⁻⁵ -10⁻⁶. Moreover, I did not find such notations in Bonn et al. 2014. This part must be clarified or deleted.

line 519f: This a strong statement. Do you have proof for that or a reference? Moreover, potential aerosol growth and formation rate are not the same. Indeed NO suppresses nucleation (Wildt et al. ACP, 2014) but not so much the yield (Sarrafzadeh et al. ACP, 2016)

Minor comments:

Starting with line 219 the authors give errors/uncertainty ranges/standard deviations in form of “+/-yx” at many instances (including Table 1) without explaining the specific meaning.

line 156: “142 Td”, Td is not explained

line 186f: The formulation “selected” suggests to me that the initial EA and the fractional EA for individual compounds were used as adaptable parameters. Is this case? If so, then the overall good agreement between modelled and observed concentrations in section 3.5 (line 398f) and Figure 6 is not surprising. If not, explain the in more detail the rationale for these selections.

line 386f: Similar is true for the formulation about the “assumed” leaf area index and leaf area density. What is the rationale for these choices? Did you estimate it from observations on the site?

line 256, first §: These percentages are averages over day and night hours and the three measurement days? They should have standard deviations.

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line 286, first §: I suggest to define what you mean by reactivity. e.g. OH reactivity = $k_{OH} \cdot [MT]$.

line 303, Table 2: The header of the third column is misleading. It should read “normalized reactivity for 1 ppb [s⁻¹]”, or so. Otherwise confusion with the use of reactivity later in the text.

line 489: Is Hallquist 2009 (a review) a good reference for this statement?

line 542f: That is not new and with the given formulation the conclusion in its generality does not make sense. The authors did not show that there is no suited representation of MT, which considers also their reactivity. Moreover, the degree of tolerable simplification depends also on the purpose of the model calculation.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-817>, 2017.

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