

# ***Interactive comment on “Estimation of NO<sub>x</sub> and SO<sub>2</sub> Emissions from Sarnia, Ontario using Mobile-MAX-DOAS and a NO<sub>x</sub>-Analyzer” by Zoe Y. W. Davis et al.***

## **Anonymous Referee #1**

Received and published: 19 August 2019

The manuscript by Davis et al. describes mobile multi-axis DOAS observations of industrial and urban emissions around the town of Sarnia, ON. The study is based on 3 days of observations of UV-vis absorption spectra in 30, 40, and 90 degree elevation viewing angles from a moving car downwind, and sometimes upwind, of the emission sources. The spectra were analyzed to retrieve NO<sub>2</sub> and SO<sub>2</sub> column densities. These were then converted into fluxes using 10m wind data. In addition, an in-situ NOx monitor installed on the vehicle was used to convert NO<sub>2</sub> to NOx columns. The use of the NOx monitor is a nice touch as it reduces one of the main uncertainties when using DOAS for NOx flux measurements. The manuscript is thorough in its discussion of the methodology, and the authors should be commended for the detailed discussion of

Printer-friendly version

Discussion paper



the uncertainties of their observations. The authors provide a preliminary comparison of their fluxes with those from a 2015 power plant emission inventory and a 2017 industrial facility emission inventory. The comparison is reasonable, considering the various uncertainties entering the determination of both emission rates.

Overall, this is a very good manuscript, although I am wondering if it would have been better suited for Atmospheric Measurement Techniques, since most of the manuscript is dedicated to explaining the use of the MAX-DOAS technique to measure emissions. Maybe it would be worth discussing the emission results in more detail, i.e. a comparison with some other studies that show whether the agreement with the emission inventory is similar to other locations and/or addressing the overall question of the accuracy of emission inventories. This would make this study more valuable for ACP readers. Aside from these general comments I have a few other minor comments that should be addressed before the manuscript can be published in ACP.

- I am a little confused about the SO<sub>2</sub> emission estimates. It seems those are only reported for day 1. What about the other days? Since NO<sub>2</sub> data is available I assume that SO<sub>2</sub> is available as well. If the SO<sub>2</sub> was not above the detection limit it should be reported as upper limit emission estimate.
- Section 2.2: Was HCHO included in the fit of SO<sub>2</sub>? And more generally, why was HCHO not retrieved? HCHO column densities would provided information on VOC's that are discussed later in the manuscript.
- Section 3.4.1: Are the emissions used for the NPRI comparison also scaled up to account for not measuring the entire plume?

[Printer-friendly version](#)[Discussion paper](#)

- Lines 270-271: "...thus offsetting."? I do not understand this sentence.
- Lines 478 – 480, and other locations in the manuscript: Please clarify that one needs both the vertical wind profile, as well as the trace gas profile, for accurate flux determination. Wind profiles alone, while improving the calculation, are insufficient. Have you considered the change of wind direction in the boundary layer (Ekman spiral)?
- Figures 2, 5, and 7: Add error bars, or at least list the errors in the caption.
- Figure 2: What is the horizontal line in panel 2e?
- Figure 7: it is difficult to identify the shading on the left as pink. Maybe choose a different color.

---

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-442>, 2019.

[Printer-friendly version](#)[Discussion paper](#)