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Atmospheric Measurement Techniques Discussions

Interactive comment on "The development of a nitrogen dioxide sonde" *by* W. W. Sluis et al.

Anonymous Referee #2

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The manuscript describes design, test and first deployment of a small, lightweigth NO2 sensor for ballon-based measurements based on a partly new design. In principle this is a much needed type of instrument, also the manuscript is reasonably comprehensive, clear and reasonably well organized. Therefore it is basically suitable for and clearly worth publishing in AMT. However, there are several parts of the manuscript where the information given is incomplete and that are difficult to read and understand (see below).

Overall, the required changes amount to a major revision of the manuscript.

A potentially severe problem with the measurement technique chosen (liquid-phase chemoluminescence), which is not discussed by the authors, is due to the non-linear response of the instrument (see e.g. Kelly et al. 1990) at low NO2 levels (below about 2 ppb). Although the response is zero at zero NO2 there is a considerable change in the slope of the calibration curve up to a few ppb. Thus the calibration described in the manuscript (section 4) will give quite wrong results if this effect is neglected. Here the authors need to provide a better calibration function (equ. 4) or they need to explain why this effect is absent in their design. In addition a meaningful discussion of the overall measurement error is missing.

Other points:

1) Page 2806, line 25: Explain NOx

2) Page 2807, line 7ff: Here it would be good to state that these instruments are known as "gas-phase chemoluminescence NO detectors“

3) Page 2807: The discussion of existing measurement principles for NO2 is not very sytematic and also incomplete. It would be much better if the measurement principles were listed one by one and their advantages and disadvantages (with respecto to balloon-borne measurements) were briefly discussed. For example:

a) gas-phase chemoluminescence + NO2 – NO converter

b) LIF

c) liquid-phase chemoluminescence (i.e. luminol)

d) CRDS/CEAS

e) photoacoustic

f) ...

4) Section 2: give volume of luminol reservoir.

5) Page 2809, line 11: The sentence starting "the gas is leaving ...“ is obscure. where are the holes? In fig. 2 it appears that the gas (air?) is simply leaving the tube at its

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end?

6) Subsection 2.1: Here are several unclear points: From the stated Johnston noise of the feedback resistor it appears that the bandwith of the amplifiers is about 1 Hz, but this is stated nowhere in the manuscript.

7) Subsection 2.1: The sentence starting "the VTB8440 photodiodes produce ..." is unnecessary cryptic. Probably the electronics uses two independent amplifiers for the sseing and blind diodes and then the diference of both is formed. At least this is suggested by Equ. 4. Changing the temperature probably causes slightly different heating of the diodes thus temporarily causing a non-zero difference.

8) Subsection 2.1: It would be good to have a circuit schematic of the preamplifier system (see also point 7 above).

9) Subsection 2.1: An important factor determining the noise is the capacitance of the photodiodes. Assuming C=100 pF their impedance at 1 Hz (between common and amplifier input) would be around 1.5 GOhms. Combined with the feedback resistor of 50 GOhms the AC-amplification of the preamplifier would be around 30 (!), thus its noise contribution would be much larger than estimated by the authors. This point is not mentioned in the manuscript, but definitely needs to be addressed.

10) Page 2811, line 13: On which basis is the 5m vertical resolutilon estimated?

11) Section 3: there is no explanation of the actual chemoluminescence reaction, also no discussion of the non-linearity of the reaction (see e.g. Kelly et al. 1990) is given.

12) Section 3.1: the optimization of the solution composition is made at NO2 levels in the ppm-range, however actual measurements are made at ppb levels, how do the authors know that the composition is optimal for these low mixing ratios as well?

13) Section 4: Linearity of calibration needs to be discussed, see above.

14) Page 2816, line 17: There is a "third order polynomial in time" mentioned, is that the

description of the temperature sensitivity, perhaps a 3rd order polynomial in temperature combined with a measured temperature time series would be more appropriate to use?

15) Section 5: At which altituede were the comparisons made?

16) What is a "M200E Photolytic analyzer“, if this instrument uses a new measurement principle it should be discussed in the intro. (section 1).

17) Page 2819, line 2: Range of 1-100 ppb. Judging from the non-linearity of the detection principle it is probably very difficult to reach 1 ppb detection limit.

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