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Atmospheric Measurement Techniques Discussions

Interactive comment on "The Heidelberg Airborne Imaging DOAS Instrument (HAIDI) – a novel Imaging DOAS device for 2-D and 3-D imaging of trace gases and aerosols" *by* S. General et al.

Anonymous Referee #1

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General comments

This paper describes a new airborne DOAS instrument, the Heidelberg Airborne Imaging DOAS instrument, and its use for the measurements of several trace gases (SO2, BrO, NO2) during three campaigns. The HAIDI instrument appears promising and its description fits well within the scope of AMT. Results of the three presented campaigns are also interesting on their own. Therefore, this paper should be published. There is, however, some room for improvement of the paper before final publication. Regarding the style, my main concern is that the paper contains several unnecessary or ill placed sections which makes reading the article difficult. The section introductions are often

too long and technical. Regarding the content, my main concern are: (i) the assumption of geometrical approximation for the light path, which seems rather optimistic and, as a consequence, (ii) the error budget on the VCDs, which is merely missing, and (iii) the information on how the georeferencing is done. It seems that the authors use the attitude measured in the plane by separate IMU instruments but this should be discussed within the data analysis part in a proper new section, also discussing the achieved pointing accuracy.

Specific comments

Introduction

The authors refer to many previous airborne DOAS experiment, which is useful and relevant. However, when presenting imaging airborne DOAS more specifically, they only mention the work of Heue et al. I suggest to add the references to other airborne iDOAS instruments, at least APEX:

Popp, C et al, High-resolution NO2 remote sensing from the Airborne Prism EXperiment (APEX) imaging spectrometer, Atmos. Meas. Techn., 5, doi: 10.5194/amt-5-2211-2012, 2012.

And possibly ACAM:

Kowalewski, M. G. and Janz, S. J.: Remote sensing capabilities of the Airborne Compact Atmospheric Mapper, in: Proc. SPIE 7452, Earth Observing Systems XIV, 74520Q, doi:10.1117/12.827035, 2009.

And SWING:

Merlaud et al., Small whiskbroom imager for atmospheric composition monitoring (SWING) from an unmanned aerial vehicle (UAV), Proc. SP-721 ESA ISBN : 978-92-9092-285-8

I also suggest to add the following reference, since the geometry is close to HAIDI, even if it's not a iDOAS system

Berg et al Ship emissions of SO2 and NO2: DOAS measurements from airborne platforms, Atmos. Meas. Tech., 5, 1085-1098, doi:10.5194/amt-5-1085-2012, 2012

As a general comment, these references should be more present in the methods sections when the authors build on these studies to develop their own data analysis. I have suggested some of these references below.

In the last paragraph, the authors should be more specific about what is included in which section. Just adding 'In setion x, we present…' and so on. This would be particularly useful as the paper is quite long.

Section 2: HAIDI

As a general comment : the total payload weight and size of complete HAIDI instrument is not given. Even if there are various setups it would be good to give indications of weight and size for one or two of the described setups.

Section 2.3 The nadir scanner

P.2193, L11-12. 'Instead one system is looking in forward direction, covering a range of elevation angles around 0°'. One comma is missing after 'Instead', as often (see technical corrections). By '0°' the authors seem to mean 'the horizon direction'. They should write that fully since this origin is not previously defined.

P.2195. The authors write that polarization sensitivity is reduced when using a prism compared to a mirror. This statement should be supported by a reference. The authors also write that (l.24 25) 'Fresnel reflection can significantly be reduced by the application of anti-reflection coatings'. However it is not clear whether or not there is such a coating on HAIDI's prism. Please clarify.

Regarding the scanner, some information is missing. What is the model and manufacturer of the prism? What is the servo motor model? How is it controlled? Part of this

information (pwm signal is created by the detector) is found in sect 2.5.2 but it should be mentioned quickly here with a reference to sect. 2.5.2.

p.2196

'ground projected instantaneous field of view (GIFOV) of the telescope will be about 40m$\times$40m'. This may be confusing since the reader might understand that the pixel is a square of 40 m side. The author should better write more simply that the GIFOV is 40 m.

The authors write that 'the exact GFOV …considering …pitch and roll…Figure 4 illustrates such a simulated GFOV…' However, Fig. 4 seems very theoretical and does not appear to take into account pitch and roll variations. If it does, please mention it more clearly with the ranges of considered pitch and roll. If it does not as it seems; Fig 4 should be described before explaining that pitch and roll have to be taken into account.

Section 2.4 The forward-looking telescope

This section should be rewritten.

p.2197, l.20 'The viewing geometry yields long absorption paths…For this reason the forward-looking telescope is suited best for … smaller, low flying aircraft'

I do not understand the implication here. If you have a limb channel on a larger aircraft, you can study the free troposphere, as was done by Merlaud et al., 2011, Dix et al. 2013, and Baidar et al. , 2013.These references should be added to this section when the authors mention the high sensitivity of this geometry. Please clarify the implication or remove it.

p.2197 l.25 'Due to space restrictions on these aircrafts…this could be achieved with the pushbroom technique' does not make sense either if you remove the aforementioned implication. Moreover, HAIDI has also a whiskbroom scanner on the smallest aircraft (CTLS). The authors should better write that the pushbroom is more appropriate for the forward looking since a wide swath (whiskbroom) does not bring anything for profile retrievals.

Section 2.5

P.2198 and 2199 The description of the temperature regularization system, mounting, rack dimension and so on is interesting and relevant. But It should be moved in a new section (such as before linearity) and not in the introduction of this section, which makes the reading awkward.

Section 2.5.1

P. 2199 Looking in the Jobin Yvon catalog, the grating in HAIDI are not classified in 'Holographic Concave (type I)' but in 'flat field and imaging gratings (type IV)'. They are as well concave and holographic but the information that these gratings are corrected for aberration is interesting and should be added (it is in the caption of fig. 7 but should also be in the body of the article)

P.2199 L.20 'The optical resolution ..is about 0.5 nm (5 pixel)' The authors should be more accurate. What is their definition of optical resolution (FWHM it seems from fig 8, but this should be clarified here)

Section 2.5.2 Detector

P.2200 l .24 'Compared to similar commercially available detectors' The authors should mention such commercial detectors with references to articles or remove this part of the sentence, just writing e.g 'To optimize the size and weight, we used custom built detectors '

Section 2.5.3 Linearity

P 2201 l 22 'Most detectors show a decreasing sensitivity. . .' This statement should be supported by a reference.

P 2201 l 27 'a temperature stabilized LED'. The authors should provide the model and manufacturer of the LED.

P 2202 l 1 'the normalized signal' normalized to what? This is explained in the caption of fig 9 but it should be explained here as well

P 2202 l. 4 'plotted vs the intensity level' The intensity does not change in the described experiment, only the exposure time, so the plot is vs the number of detector counts, as shows fig 9

Sect. 2.5.4 S/N ratio

P.2203, l.12-13 'Starting at about 10 000 . . . other noise (e.g.instrument noise) become dominant and the noise in the spectra can not be decreased any further. '

Can the authors be more specific about the 'instrument noise'. Do they mean 'readout noise'? The expression 'instrument noise' is too vague.

Section 3 Data analysis

P 2204 Can the author explain briefly why they use an inverse FRS? (to reduce the offset?) And an additional Ring cross section multiplied by lambda 4 (shouldn't it be divided by lambda 4)?

P 2205 What is the definition of the detection limit (is it based on the doas fit residuals? If so, is it 2 sigma, 3 sigma?)

Fig 12 and 13 both shows DOAS fits for the nadir looking channel. It would be more interesting to see one DOAS analysis of the limb channel.

Section 3.2 Geometrical approximation

Again, the section introduction is too long and technical.

The main problem here is that the uncertainty of the geometrical approximation is not discussed at all. This problem has already been discussed in recent previous studies that should be quoted in this section: Baidar et al.; 2013, Berg et al. 2012. A typical uncertainty of using the approx. should be estimated, either extracted from the aforementioned references, either investigated with a radiative transfer model by the authors. If the authors cannot find an estimated uncertainties in previous studies, this problem should at least be discussed more in detail in this section and stressed out again in the conclusion. The work of Popp et al (2012) should be read by the authors since with APEX, they are able to derive the ground albedo and calculate its effet on the AMF. This is probably a good start to study the uncertainty on the geometrical approximation.

Section 3.3.2 Conversion of dscds to tropospheric vcds

P 2209 l.11 12 'By assuming that the stratospheric VCD remains relatively constant during the flight (be aware that this assumption is not valid for all trace gases)'

The authors should be more specific and explain for which trace gases the assumption is realistic or not.

Section 3.2.3 Limb observations

P 2210, l. 12-14. 'Limb observations at different flight altitudes…are well suited for profiles …This is usually (e.g. Sinreich et al., 2005; Wagner et al., 2004, 2011; Frieß et al., 2006; Irie et al., 2008; Clémer et al., 2010).'

All these studies refer to ground-based measurements and are, by definition, not taken at different flight altitudes. The authors should better quote the airborne DOAS measurements of trace gases profile (Dix 2013, Merlaud 2011, Baidar 2013) , possibly mentioning that these work build themselves on the ground based studies they quote.

"Our retrieval algorithms are based on the well known optimal estimation method (Rodgers, 2000)"

Although many authors use this expression, there is no such thing as 'the optimal estimation method' in Rodgers, 2000. This expression is meaningless since, as Rodgers write p. 65 of the book quoted by the authors (Rodgers, 2000), there are many ways to choose an optimal solution. I guess the authors mean, like previous investigators, the

'Maximum a posteriori solution', which is widely described in Rodgers, 2000. Please correct. Another possibility is to remove this discussion on profile retrievals since the authors do not retrieve profiles later on.

Section 4.2 Setup for Beechcraft

The authors should mention where this plane was used, as they did in the previous section with the CTLS

Section 4.3 Setup for HALO

This section describes future experiments with the HALO aircraft, which has not been done yet by the authors. It could be merely skipped and mentioned in the conclusion. Section 6.2 Mapping of air pollution P 2217 'the total NO_2 emission of the probed area calculates to 3.95(62) th^{-1}'

I am not sure that this way of writing the uncertainty is appropriate for Copernicus, most of the papers use the symbol '+/-' . But the main question is how was this uncertainty calculated by the authors?

Technical corrections

The paper should be fully checked by a native English speaker. For instance, across the text (abstract, p.2190, l.2, etc…), 'custom build', should be 'custom-built' (p. 2190, l.16): 'system was build' should be 'system was built'. The author also use 'custom made' (p.2201, l.4-5) and 'custom-made' (P.2214, l.15), this is not consistent. In many places, the sentence structure sounds weird. For instance: 'Also vertical profiles of trace gases and aerosols can be derived' should be 'Vertical profiles of trace gases can also be derived', or 'Also, vertical profiles etc…'. This comma after the first adverb or proposition is almost always omitted in the paper and this should be corrected (e.g. 'In total the fiber optics bundle has a length of 5m' should be In total, the fiber …, 'To cover a preferably large area with one overflight the HAIDI system always uses a whiskbroom scanner in nadir direction' should be 'To cover a preferably large area with one overflight, the HAIDI system . . .)

P. 2189 L.16 ‘with improved accuracy.’ Very vague. . .Improved in relation to what?

Figures: most of the figures are pretty and useful, however, the axes labels are often too small to be easily readable. For instance, the authors should increase the y-axis label size on fig. 19 to 23. Fig. 16 to 18 should also be increased. Fig 18 should be removed if the author skip the description of the HALO setup as suggested above. Figure 2: the authors write in the caption that the scheme presents the ‘general measurement principle’ but in the text it says that different configurations are possible and that fig 2 present a setup ‘especially used for smaller, low flying aircraft’. The caption of fig 2 should mention this last point and states that the scheme is an example of Haidi configuration used e.g with the ctls Figure 19 ‘NSEC’ should be expanded.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 2187, 2014.