Dear Editor,

We like to thank the reviewers for their constructive and helpful comments, which helped us to improve the manuscript. We followed most of the reviewers' suggestions and made the according changes to the manuscript. Below we answer the reviewers' comments in detail and explain the changes made to the manuscript. We are convinced that the manuscript is now adequate for publication in AMT.
In the following we reproduced the reviewers' comments (in black) together with our responses (in red).

With very best regards
Stephan General

Anonymous Referee #1

Received and published: 27 March 2014

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.

Response:

(i) We changed the text describing the justification of the geometric approximation (see response to comments on section 3.2, below).

(ii) In our response to comments on section 3.2 we also briefly discuss the error for the VCDs derived by using the geometric approximation. We also included an according remark in the manuscript. A more detailed analysis and comparison with RTM would be beyond the scope of this paper from our point of view. This would be an independent research topic itself for our measurement geometries. However, for typical measurement conditions the error in the derived VCD is small, as described in the response below. Additionally, we especially highlighted now that the geometric approximation is only valid under special circumstances and a precise analysis would include extensive RTM modeling.

(iii) Our opinion is, that a detailed discussion of the servo motor control and the georeferencing procedure would be beyond the scope of this paper as it is a relative complicated technical issue. Discussing further details would easily add several pages to the manuscript. Therefore, we added a reference to the corresponding PhD thesis (*General (2014), see response to comments on section 2.3*), which contains these details.

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.

The suggested references were added in the introduction (Berg et al. 2012 is quoted in Sec. 3.2 Geometrical approximation of the light path)

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.

Cross references were added to the last paragraph of the Introduction and the text was also slightly changed in order to cover all sections of the article. In this context, the Measurement Software section (now Sec. 4) was moved in front of the Measurement Platform section (now Sec. 5).

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.

A table (Tab. 8) with approximate weights and dimensions of the different HAIDI configurations was added in section 5.

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.

Corrected.

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.

The following reference on the polarization dependence of aluminum coated mirrors (angle of incidence for a 90° deflection: 45°) was added:

Williams, M. D. Laser reflection from oxide-coated aluminum Appl. Opt., 1982, 21, 747-750

In addition, the following text was added to the manuscript to clarify the lower polarization sensitivity of a prism in comparison to a mirror: *"However, for almost normally incident light, like in case of the nadir scanner prism, the polarization dependence of the Fresnel reflection vanishes."* An anti-reflection coating is not used by HAIDI's prisms. The corresponding passage in the text was therefore removed.

The prism is a fused silica right angle prism from Edmund Optics with a side length of 25 mm. However, the article already states that a quartz prism is used and we think that additional information is not relevant.

Information about the servo motor was added at the end of Sec. 2.3. The corresponding sentence already contained a short explanation of the servo control and a reference to Sec. 2.5.2: *"For this reason, the PWM signal that controls the motion of the servo motor (HS-7940TH from HiTEC) is synchronized with the recording of the scans (Sec. 2.5.2).".* Since a detailed discussion on the servo motor control would be beyond this article, we also included a reference to the PhD thesis about the development of HAIDI at the end of the section: *"A detailed description of the servo motor control and the georeferencing procedure can be found in General (2014)."*

p.2196 ‘ground projected instantaneous field of view (GIFOV) of the telescope will be about 40m x 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.

The sentence was changed to: *“Thus, for an assumed flight altitude of 1500m, the ground projected instantaneous field of view (GIFOV) of the telescope is about 40m in diameter.”*

Figure 4 may look very theoretical but shows an accurate calculation of the GFOV including bow tie effect (even though it is not really visible for a scan range of +-25°). Pitch and roll angles can of cause be handled by the plotting algorithm but are not considered in Fig. 4 for the sake of simplicity and as variations are typically very small during normal flight. Therefore, in the caption of Fig. 4 we include “pitch = 0°, roll = 0°”.

The corresponding text in the section was modified to clarify this:

“To calculate the actual shape of the nadir scanner’s ground projected field of view (GFOV), including perspective distortion due to off-nadir viewing directions, requires a much more complex algorithm. Figure 4 illustrates such a calculated GFOV for typical flight conditions on smaller aircraft. The plots contain two consecutive swaths each, marked in red (1 st swath) and blue (2 nd swath) to show the areal coverage of the scanner. To get the best areal coverage, the gap between consecutive swaths should be as small as possible. The distance between two swaths depends on a number of parameters, e.g. flight altitude above ground level, aircraft speed, integration time and number of scans per swath. However, in general the distance covered by the aircraft during the time for one swath should be equal to the extent of the GIFOV in flight direction, d_{GIFOV} *, in order to minimize the gap and avoid overlap:*

$V_g \times t_{int} \times n_{scan,total} \overset{!}{\approx} d_{GIFOV}$ *(3)*

Because the aircraft speed usually cannot be influenced and the integration time per scan depends on the intensity of incident light, the distance between two swaths is typically adjusted by choosing a different number of scans per swath (1 to 128). For an accurate reconstruction of the GFOV also pitch and roll angles of the aircraft have to be taken into account, which can be handled by the used algorithm as well.”

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.

For clarification we changed the text to: *“This viewing geometry yields very long absorption paths, especially close to the flight altitude (see Sec. 3.2.3). For this reason, the forward-looking* *telescope is well-suited for the profile retrieval of trace gases in the lower troposphere on smaller, low flying aircraft. ”*

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.

We corrected the text to read:

“Due to technical reasons in the presented aircraft, the forward-looking telescope could only be realized with the pushbroom technique. However, the potential to realize a wider swath with a whiskbroom scanner would not give any significant advantage for the forward-looking telescope to derive trace gas profiles.”

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.

As section 2.5 is mainly about the instrument rack, the section title was changed to “Instrument rack” and the first sentences were slightly changed to read now: *“HAIDI’s imaging spectrographs and CCD cameras are housed in the so-called instrument rack (Fig. 6), which is a modified 19-inch aluminum rack with a height of 6 rack units (19×20×10.5in). In the current state, the instrument rack can store up to three spectrograph-detector units that are of identical design to be applicable for the whiskbroom scanner setup and the pushbroom setup as well”.* The following sections changed their numbers accordingly.

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)

The aberration and flat-field correction of the grating were already mentioned in the text (p. 2200 L. 6-10). The relevant sentences were slightly rewritten to clarify this: *“For instance, aberration effects like astigmatism are significantly reduced by a special groove pattern of the used holographic grating. This also improves the imaging on flat surfaces (flat-field correction), resulting in an almost homogeneous imaging quality over the whole surface of the chip (see Fig. 8b).”*

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)

Sentence changed: *"The spectral resolution (Full Width Half Maximum (FWHM) of emission lines) achieved by the system is about 0.5nm (≈ 5 pixel), as can be seen in Fig. 8b."*

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 '

As suggested we changed the sentence: *"Also HAIDI's CCD cameras are custom made in cooperation with khs-instruments (Munich, Germany) in order to optimize their size and weight. This makes it possible to install the whole system also on very small aircraft, e.g. ultralight planes."*

Section 2.5.3 Linearity

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

We added the following reference:

T. J. Fellers and M. W. Davidson, Concepts in Digital Imaging Technology - CCD Saturation and Blooming, http://hamamatsu.magnet.fsu.edu/articles/ccdsatandblooming.html, 2014

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

We included the information in brackets: *"(Cree XP-E Royal Blue)".*

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

We changed the sentence to read: *"Afterwards, the measured signal (in Counts/ms) was normalized to the value at 30000 Counts for each pixel and then plotted versus the corresponding intensity level (detector counts)."*

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.

Intensity level of a pixel /= light intensity. Thus, the given description is correct. For clarification we included: "… intensity level (detector counts) …"

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.

We changed the sentence to read: *"Starting at about 10000 scans (equivalent to 5×10 9 photons) other noise sources (e.g. readout noise, offset noise, dark current noise etc.) become dominant and the signal-to-noise ratio can not be improved any further by co-adding larger numbers of scans."*

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)?

We added the following explanations to the text:

".. an inverse FRS (to reduce the offset due to stray light in the spectrometer), an additional Ring spectrum that is multiplied by lamda^4 (to account for the wavelength dependency of the ring effect)…" Multiplying by lamda^4 or dividing will only change the fit coefficients of this spectrum, but not the other results.

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?)

We included on p.2205 l.29: *"The measurement errors (1 sigma) and detection limits (2 sigma) are calculated from the DOAS fit results according to Stutz and Platt 1996. Accordingly, the fit error is multiplied by 2 to obtain the 1 sigma measurement error."*

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.

The spectra analysis is the same for limb and nadir measurements. We specially selected the nadir measurements, as these spectra show typically a much lower measurement signal than for the limb measurement (shorter absorption path), and stronger spectral features from the surface reflection. Thus, it is much more difficult to achieve a good signal-to-noise level for nadir than for limb measurements. We therefore show nadir measurements to demonstrate the quality of the instrument.

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 effect on the AMF. This is probably a good start to study the uncertainty on the geometrical approximation.

The section introduction was shortened and some references were added at the end:

"The result of a DOAS analysis is the so-called differential slant column density (dSCD), S', which is the concentration of the trace gas integrated along a particular light path through the atmosphere minus the integrated concentration already existing in the FRS. As the light path

through the absorber depends on the respective measurement conditions (e. g. measurement geometry, solar zenith angle (SZA), cloudiness), the dSCDs are usually converted to vertical column densities (VCDs),V , which give the concentration along a vertical path through the atmosphere and therefore are independent of the used viewing direction. This is commonly done by applying radiative transfer models (RTM). However, for certain conditions, like high surface albedo, cloud free sky, SZA <80° and low flight altitude, the light path can also be estimated by a more simple geometric approximation. These conditions are largely met by the nadir measurements presented in this article. The following thus shows a geometric consideration of the nadir scanner's light path, as well as an approach for the conversion of dSCDs to tropospheric VCDs. Even if the geometric approximation is a strong simplification which may cause errors (e. g., Berg et al., 2012; Baidar et al., 2013) and RTM could give more precise results, it still has some advantages. It is very fast and relative simple to calculate, it does not depend on a priori values and gives a robust output."

We want to mention that the errors from geometrical approximation can be small for special geometric configurations (e.g. nadir) and measurement conditions (high surface albedo, cloud free, SZA <80°). These properties are mostly achieved for the presented measurements. Thus we think a geometric approximation is also due to its given advantages a useful tool. A detailed discussion and comparison with an RTM model would be beyond the scope of a technical instrument paper. Furthermore, RTM may produce additional errors due to several retrieval parameters and thus a geometric approximation can even be more precise.

At the end of section 3.2.2 we also added:

"As mentioned above the geometric approximation (Eq. (16)) is only valid for certain conditions, which are fulfilled, however, for the majority of the presented measurements. If in these cases, the error from the stratospheric correction and the tropospheric AMF are relative small in comparison to the error of the slant column density ∆S 0 , then the error of V trop can be approximated by::

*D Vtrop ~ D S' / S' * Vtrop*

For a more precise error estimation RTM calculations are necessary."

Section 3.3.2 Conversion of dscds to tropospheric vcds

P 2209 I.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.

We included the following text: *"(which is a realistic approximation for BrO and NO2 at SZA <80°)"*. We agree with the referee that this assumption is not generally valid.

Section 3.2.3 Limb observations

P 2210, I. 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.

Some references for airborne measurements (Dix et al., 2013; Merlaud et al., 2012; Baidar et al., 2013; Heue et al., 2011; Prados-Roman et al., 2011) were added to the first sentence. Since the references already included (Sinreich et al., 2005; Wagner et al., 2004, 2011; Frieß et al., 2006; Irie et al., 2008; Clémer et al., 2010) describe the general approach for the retrieval of trace gas and aerosol profiles from scattered light DOAS measurements, we would prefer not to remove them from the manuscript.

"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.

We agree that the exact term is 'maximum a posteriori solution' and that the sentence might need some more specific information on the general approach. However, Clive Rodgers certainly does not state in his book that optimal inverse methods do not exist (the chapter you are referring to is indeed entitled 'Optimal Linear Inverse Methods'), but he states that the maximum a posteriori approach is one of several possible optimal inverse method (P. 65 of his book). Clive Rodgers uses the term 'Optimal Estimation' in several publications, e.g. [Connor, B, and C. D. Rodgers, A comparison of retrieval methods: Optimal estimation, onion peeling, and a combination of the two., Adv. Remote Sensing Retrieval Methods, 271-281, 1988]. We would like to keep the expression 'optimal estimation' since it is well known and frequently used in the remote sensing community. To provide some more specific information on our approach, we modified the sentence as follows: *"Based on the Bayesian approach, our retrieval algorithms determine the maximum a posteriori solutions of the given inverse problems based on the well-known optimal estimation method (Rodgers, 2000)…"*

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

Information about the campaigns conducted with the setup was added.

"HAIDI was also installed on the Airborne Laboratory for Atmospheric Research (ALAR) during two campaigns in Indiana and Alaska (USA) (Secs. 6.2 and 6.3). ALAR is a twin-engined aircraft (Beechcraft Model 76 Duchess) operated by the Purdue University and has a normal capacity of one pilot and three passengers."

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.

We do not agree with the reviewer in this point. The design and construction of the HAIDI instrument is based on the requirements for HALO. The instrument will fly in the future on HALO, therefore we think it is appropriate to keep this chapter in a technical manuscript of the instrument. From our point of view it makes no sense to have this section only in a publication of HALO measurements in the future or only in the conclusion section. We therefore like to keep the section on HALO at its present position within our manuscript. See also response to comments of reviewer #2, below.

Section 6.2 Mapping of air pollution

P 2217 'the total NO2 emission of the probed area calculates to 3.95(62) t/h' 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?

Uncertainties are now given with +/-. Furthermore, an explanation of the calculation was added.

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…)

The English language was improved, but was also already checked by the native English speaking Co-Authors. We also made a large number of corrections in response to the comments of reviewer #2.

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

Text was changed to 'with high accuracy'. According to the achieved detection limits presented in Sec. 3.1.

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.

The font size of the mentioned figures was enlarged.

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 HADI configuration used e.g with the CTLS.

The first sentence of the caption was changed to: *"Schematic showing the measurement principle of the HAIDI system in the configuration for low flying aircraft (e.g. CTLS, see Sec. 4.1), which uses a whiskbroom scanner in nadir direction and a pushbroom scanner in forward direction."* In order to clarify that the schematic shows a specific configuration of the HAIDI system.

Figure 19 'NSEC' should be expanded.

'Location of the NSEC' was changed to 'Location of Mt. Etna's New South East Crater'

Anonymous Referee #2

Received and published: 22 March 2014

General comments:

The paper by General et al. presents a new instrument designed for a flexible usage in different aircraft types, combining the pushbroom and whiskbroom method for retrieving spatial information of a variety of species. It gives an overview over existing techniques and previous studies in that field and motivates the usage of such measurements for atmospheric science. The paper includes a discussion of the advantages and disadvantages of the pushbroom and the whiskbroom technique, which are both implemented in the instrument. The main focus of the paper is the technical setup of the instrument, making it suitable as reference for future publications. First results of already performed campaigns demonstrate the usability of the instrument. Therefore, the paper's subject is well within the scope of AMT. In general, the paper is scientifically correct and well written, therefore I recommend a publication after some minor changes.

Specific comments:

Structure of Section 1 (Introduction):

The introduction gives an overview over existing techniques, instrumentation and measurements (especially airborne), then the new HAIDI system is introduced (with features and benefits for chem. models). Afterwards, again previous studies are mentioned (I-DOAS), than again the advantages of the HAIDI system (with some repetitions). Therefore, I recommend to put the I-DOAS part (Page 2189 Lines 21..26, "I-DOAS instrumentation has ... from cities (Beirle et al., 2011)") somewhere before the presentation of HAIDI.

The section was rearranged and also partly rewritten to read:

"The aim of these measurements ranged from studies of stratospheric chemistry to tropospheric point source emissions, but only a few of them used Imaging-DOAS (I-DOAS) techniques to acquire 2-dimensional spatially resolved trace gas distributions (e.g., Heue et al., 2008; Kowalewski and Janz, 2009; Popp et al., 2012). I-DOAS techniques are well established in remote sensing and were successfully used in many applications in the past. For instance, BrO formation in volcanic plumes has been studied using ground-based I-DOAS measurements (e.g. , Louban, 2005; Bobrowski et al., 2007). On a larger scale, satellite data have been used to

quantify the strength of ship emissions based on SCIAMACHY NO2 distribution patterns in the Indian Ocean (Beirle et al., 2004) or emission from cities (Beirle et al., 2011).

Here we present the…"

Approaches in Section 2.2:

The two approaches for vertical profiles you title with "Multiple whiskbroom scanners" (Page 2192 Line 26) and "Pushbroom scanner" (Page 2193 Line 9). In my opinion, the main difference between these approaches is not whiskbroom vs pushbroom, but downward vs forward direction. Therefore, I recommend to title "Multiple downward scanners" and "Forward scanner" instead.

We made the changes to the text as suggested.

Section 3, Page 2208 Line 11,12:

"In addition there can be a spatial offset in dependency on the solar zenith and azimuth angles." What do you mean with that sentence?

The sentence was changed to read: *"In addition, the changed measurement geometry (see Fig. 14) can cause a significant spatial shift of the measured signal as a function of the solar zenith and azimuth angles, because the light traverses the absorption layer far from the surface reflection."*

Section 4.3 (HALO setup):

You could add a hint to other planned remote sensing instruments onboard HALO, especially the HALO mini-DOAS instrument. What is the current status of the mini-DOAS, and are there plans for comparison or validations between those instruments?

We modified the section to mention some other HALO instruments:

"In the near future, HAIDI will also be installed on the German research aircraft HALO. HALO is a modified Gulfstream G550 ultra-long range business jet, which is equipped with a wide range of instruments for atmospheric research. Beside HAIDI there are also some other optical remote sensing instruments to cover different scientific objectives, e.g. the HALO mini-DOAS (Hüneke et al., 2011) or APEX (Popp et al., 2012). As typical flight altitudes of HALO (~10km) are too high for the investigation of tropospheric trace gases with a limb system, HAIDI's forward-looking telescope will not be used in this case. Instead, three whiskbroom scanners will be applied in different directions: one in nadir direction and the other two tilted by 45° into and against the flight direction."

However, we believe that additional details on these instruments are not within the scope of our manuscript.

Section 6.2 (NO_2 emission), Page 2217 Line 6ff:

Except a literature reference and that you converted to trop. VCD, you do not tell how you obtained your emission estimate. Which wind direction and strength did you assume, and where does the information about the wind come from? It also should be mentioned whether the given error estimate already includes the uncertainty of the wind field. Furthermore, a note about the conversion between NO and NO_2 should be given.

The text was supplemented with an explanation how the emission was calculated (equation and used values for wind speed and direction, including the considered errors):

"Assuming a stratospheric NO_2 VCD of 2.0×10^{15} molec/cm ^{2} (Wenig et al., 2004) and negligible NO_2 transport towards Indianapolis, the total NO_2 emission F_{NO2} of the probed area can be calculated by the following equation (Ibrahim et al., 2010):

$$F_{NO2} = \Sigma_i V_{trop,NO2}(s_i)\cdot W\cdot\sin(\beta)(s_i)\cdot n\cdot\Delta s_i\ , \quad (17)$$

where $V_{trop,NO2}$ is the tropospheric VCD of NO_2, s_i the location of the i-th measurement, W the average measured wind speed in the boundary layer, β the angle between the wind direction and the heading angle of the aircraft, n the normal vector parallel to Earth's surface and orthogonal to the current heading angle, and Δs_i the distance traveled during the i-th measurement. The average wind speed ((7.7±1.9)m/s) and direction ((309±15)°) were determined with an air flow measurement probe (Best Air Turbulence probe) installed on the aircraft. Plugging in these values in Eq. (17) then yields a total NO_2 emission of (3.95±0.62)t/h."

Furthermore a note about the conversion between NO and NO_2 was added:

"In particular, the distribution of tropospheric NO_2 was investigated, which can cause enhanced ground level ozone, the so-called photosmog, during sunny days and also contributes to acid rain, resulting from its oxidation to HNO_3. Tropospheric NO_2 mainly originates from anthropogenic sources such as the burning of fossil fuels, where it is primarily emitted as NO. The NO then rapidly reacts with O_3, forming NO_2. At the same time NO_2 is photolyzed back to NO. Thus, the concentrations of NO and NO_2 are typically in a photostationary state (Leighton, 1961). However, in heavily polluted areas high levels of peroxy radicals can lead to a deviation from the photostationary state."

Figure 1:

As you mention in the figure's caption, the x- and y-axis represent the spatial dimension. However, the vertical axis in the left figure is not I(lambda) but lambda (which is depicted on the horizontal axis on the right side). Also in the caption, this should be stated clearer, e.g. "..., while the third axis represents the spectral dimension (i.e. the wavelength lambda)". Furthermore, the rainbow color scheme does not harmonize to the lambda axis (red color at small lambda, blue color at large lambda). Please reverse the rainbow colors or reverse the direction of the lambda axe or replace lambda by nu. Additionally I would replace "from a very narrow" by "from one narrow" in the last line of the caption in order to emphasize the 'one' (the width of that one range depends on the filter and therefore isn't necessarily 'very' narrow).

Text and figure corrected

Variable v_air:

You use "v_air" as symbol for the aircraft speed relative to ground, introduced on Page 2196 Line 20 as "the aircraft speed". Despite your correct introduction, the symbol "v_air" could be misunderstood as "airspeed" (i.e. the speed of the aircraft relative to the air). Therefore I recommend to use e.g. "v_aircraft" or just "v" instead of "v_air".

We introduced symbol v_g for the ground speed of the aircraft

Details and technical corrections and suggestions:

- several places: "aicrafts" -> "aircraft" (plural of "aircraft" is also "aircraft") Corrected
- Page 2188 Line 12: "Here we report" -> "Here we present" or "give" Corrected
- Page 2189 Line 27: "independent sources" -> "individual sources" Corrected
- Page 2190 Line 16: "to take hyperspectral images" -> "to retrieve hyperspectral images"; "take" sounds like the instrument would directly record 4-dimensional data (3 spatial, 1 spectral), but actually, the instrument only records 1 spatial an 1 spectral dimension at once. Corrected
- Page 2190 Line 17: "each pixel contains additional spectral information" – additional to what? -> Omit "additional" or reformulate, e.g. "each pixel contains a high-resolved spectrum" Corrected
- Page 2191 Line 4: "Various methods exist" - which other ones beside whiskbroom and pushbroom do you mean? There are a number of Snapshot (or "non-scanning") hyperspectral imaging techniques (e.g. computed tomographic imaging spectrometry (CTIS), fiber-reformatting imaging spectrometry (FRIS), integral field spectroscopy with lenslet arrays (IFS-L), integral field spectroscopy with image slicing mirrors (IFS-S), image-replicating imaging spectrometry (IRIS), filter stack spectral decomposition (FSSD), coded aperture snapshot spectral imaging (CASSI), image mapping spectrometry (IMS), and multispectral Sagnac interferometry (MSI)).
- Page 2191 Line 6 to 8: "Both techniques are based on the detection of light dispersed by a prism or grating ...". This applies in general for spectroscopic methods and is not specific to whiskbroom or pushbroom imaging. Therefore, you can omit this sentence or change its begin, e.g. "Like other spectroscopic ..." Corrected
- Page 2191 Line 9: "Each pixel of such a hyperspectral image is in principle a 3-dimensional dataset ..." - I think, you mean 'one spectrum per pixel', which is 1-dimensional. -> Omit "Each pixel of" at the begin of the sentence? Corrected
- Page 2191 Line 13: "... a third spectral one (I(lambda))" -> "... a third spectral one (lambda)"; cf. my comment to Figure 1 Corrected
- Page 2192 Line 7: "... HAIDI can operate ... DOAS instruments ..." - Isn't HAIDI the DOAS instrument? -> e.g. "... the HAIDI rack can operate ..." The name HAIDI refers to the overall system
- Page 2195 Line 3: "consist" -> "consists" Corrected
- Page 2195 Line 7: "with maximum density" -> "tightly packed" ? Corrected
- Page 2198 Line 1: "technique spectral" -> "technique, spectral" (comma) Corrected
- Page 2198 Line 13: "(31 _m x 190 _m)" -> "(31 x 190 _m)" - 31 is the number of fibres Mistake does not exist in original manuscript
- Page 2198 Line 26: "mountings that withstand" -> "mountings, which withstand" Corrected
- Page 2199 Line 13: "5K) but" -> "5K), but" (comma) Corrected
- Page 2199 Line 14,15: "... also ... at the same time" - redundant, remove "also" or "at the same time" Corrected

- Page 2200 Line 10ff: " ... advantage of producing ... and that ..." - I'm not sure whether this is grammatically correct. Suggestion: "As further advantages, concave holographic gratings produce less stray light than ruled gratings (Palmer and Loewen, 2005), and the zero order of the diffracted light can easily be guided to a light trap with small aperture, because it is also focused." Corrected
- Page 2200 Line 18: "... set from ..." -> "... set to ..." Corrected
- Page 2200 Line 20: "... optical resolution ..." -> "... spectral resolution ..." Corrected
- Page 2201 Line 4: "Apart from ... design the" -> "Apart from ... design, the" (comma) Corrected
- Page 2201 Line 5: "can also generates" -> "can also generate" Corrected
- Page 2201 Line 19: "characteristics" -> "characteristic" Corrected
- Page 2201 Line 25: "(Sect. 3) it" -> "(Sect. 3), it" (comma) Corrected
- Page 2202 Line 4: "ADC" -> "analog-to-digital converter" (you had not introduced the acronym before, and you use this term only here) Corrected
- Page 2202 Line 8: "linearity" -> "nonlinearity" Corrected
- Page 2202 Line 14: "In comparison to" -> "In contrast to" Corrected
- Page 2203 Line 18: "... change due to e.g. ...." -> "... change, for example due to ...." Corrected
- Page 2204 Line 16: put "(Graininger and Ring, 1962)" directly after "a Ring spectrum" Corrected
- Page 2205 Line 26: "varying" -> "various" Corrected
- Page 2206 Line 10: "minus the concentration" -> "minus the integrated concentration" Corrected
- Page 2206 Line 12: "like e.g." -> remove " e.g." Corrected
- Page 2206 Line 17: "atmosphere instead and are therefore independent" -> "atmosphere and therefore are independent" Corrected
- Page 2206 Line 18: "For this," -> "For this conversion," Corrected
- Page 2206 Line 25: "In addition" -> "Additionally," Corrected
- Page 2207 Line 12: "to to" -> "to" Corrected
- Page 2209 Line 21ff: "This selection can be e.g. if ... VCD." -> Reformulate, e.g. "A candidate for such a FRS can be a spectrum taken upwind of an emission source. Also a zero VCD obtained from the forward-telescope is an indicator for a potential FRS." Corrected
- Page 2210 Line 17: "... O4, which has a constant ..." -> "... O4, which has a well-known ..." or "... O4, which has a horizontally constant ..." (with height, O4 concentration decreases) Corrected
- Page 2210 Line 21,22: "... algorithms are ... and uses ..." -> remove singular s in "uses" Corrected
- Page 2210 Line 27: "performance" -> "quality" or "accuracy" or "goodness" ("performance" sounds more like 'efficiency of an algorithm with respect to computational time') Corrected
- Page 2211 Line 10: "However, with ..." -> "With ..." Corrected
- Page 2213 Line 22: "CCD's" -> "CCDs" Corrected
- Page 2216 Line 10: "performed a three" -> remove "a " Corrected
- Page 2216 Line 13: "originate" -> "originates" Corrected
- Page 2218 Line 10: "with the area of Prudhoe Bay" -> "within the area around Prudhoe Bay" Corrected
- Page 2218 Line 15: "maps the" -> "maps, the" (comma) Corrected
- Page 2219 Line 8: "like e.g." -> remove " e.g." Corrected
- Page 2220 Line 4: "ODEs" -> add "(Ozone Depletion Events)" Corrected

- Page 2232 Table 6: variable "c" not introduced, is it dSCD/(molec/cmˆ2) ? Variable c replaced by dSCD in molec/cmˆ2
- Page 2242 Fig. 8: For easier orientation, you could number the frames of part (b) with 1..6 or A..F and add markers on top of part (a) indicating the corresponding columns - like you have done it on the right side for indicating the columns. Markers were added
- Page 2243 Fig. 9: Probably you can see more details in the graph, if you replace the crosses by simple dots in part (a)-(d). Crosses replaced by small dots
- Page 2244 Fig. 10: y-axis: "Noise" -> "RMS of Noise", cf. Fig. 11 The noise is not only given as RMS in this figure, but also as Peak-to-Peak (PtP)
- Page 2248 Fig. 14: "... path is ..." -> "... path inside the absorber is ..." Corrected
- Page 2249 Fig. 15: Perhaps the graph would become clearer if less elevation angles were plotted, and if the colors would be chosen in an intuitive order (e.g. rainbow colors like in Fig. 21). Graph replotted with less elevation angles and colors like in Fig. 21
- Page 2255 Fig. 21: unit of O4 correct? Corrected
- Page 2256 Fig. 22: "The background shows ... (NASA, 2012)." - Well, I see a nearly homogeneous gray background. What you see is the snow cover, which is indeed very homogeneous. At least, the satellite image gives you the information that there are no open leads or other variations in the surface albedo.
- Page 2257 Fig. 23: 3rd line: "As can be seen the" -> "As can be seen, the" (comma) Corrected