Answers to Anonymous Referee 2:

This is a paper presenting HCHO and NO2 MAXDOAS measurements along the TransBrom cruise during October 2009. This is an interesting contribution to the knowledge of NO2 and HCHO distribution over Pacific Ocean, where measurements are sparse, and gives a new estimation of the background concentrations for both gases over Pacific boundary layer. Tropospheric vertical distribution for NO2 and HCHO have been retrieved from MAXDOAS measurements and a comparison between MAXDOAS data and satellite retrievals have been given for stratospheric and tropospheric columns of NO2 and tropospheric column of HCHO with a good agreement. Validation of NO2 stratospheric columns between GOME2 and SCIAMACHY has been performed with a very good agreement between 0.7 and 1.1 %. The paper is good structured. Description of instrumentation and analysis settings and retrievals is generally well explained and results are showed clearly with well discussion although it is qualitative is some sections. I would recommend the publication of this paper in ACP after the consideration of the following comments and technical revision:
We thank the referee for the general positive comments. Below, we reply point-by-point to the specific comments . As far as possible, we have considered the suggestions in the revised manuscript.

Minor revisions.

1. Please consider to include in the title that GB measurements used for validation have been performed using MAXDOAS instrumentation.
In the revised manuscript, the title has changed to:
"Formaldehyde and nitrogen dioxide over the remote Western Pacidic ocean: SCIAMACHY and GOME-2 validation using ship-based MAX-DOAS observations"
(see also first comment of Referee 1).

2. Page 15978 L12-14. Please indicate that the increase estimated for NO2 in the stratosphere is during the day due to diurnal cycle of NO2.
Included in the revised manuscript.

3. Page 15980. L 19, substitute 'low' by 'under instrumental detection limit'.
Changed in the revised manuscript.

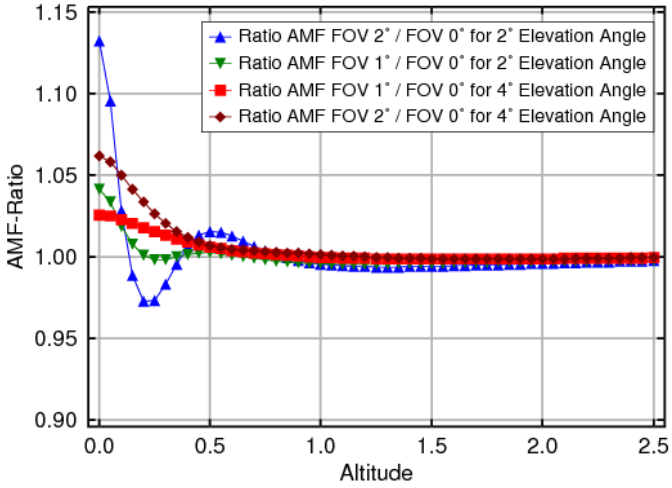
4. Page 15981 L 26. Please specify a little the characteristic of backtrajectories used for this study (i.e. potential temperature or altitude).
We described them in more detail later when explaining the backward trajectories in Fig. 15. The settings are identical to trajectories in Fig. 1. We rearranged the description so that the reader is provided with the information when trajectories first appear (in Fig. 1).

5. Page 15986 L9. It is said here that reference at zenith at the end of the every scan is takes as DOAS reference, but later on in the text it is said that this reference is used only for the retrieval of tropospheric columns whereas for stratospheric column of NO2 a single reference is used during all the period of measurements and a daily reference is used for HCHO. Please clarify.
That is a misunderstanding. The phrase in brackets on Page 15986 L9 should only state, that zenith spectra (in general) are used as reference, not that the specific zenith spectrum at the end of each scan is used. The way you described the use of reference spectra in the different trace gas retrievals is right. That is also explained in the individual sections. Therefore, we simply skipped the phrase in brackets on Page 15986 L9 to avoid further misunderstandings.

6. Page 15987 L4. Indicate temperature for NO2 XS used in this retrieval.
Included in the revised manuscript (for trop. NO2 at 294 K, for strat. NO2 at 220 K).

7. Page 15987 L13-14. Indicate temperature for both BrO and NO2 XS.
Included in the revised manuscript. NO2: 220 K to account for stratospheric NO2 (because for the HCHO fit, a fixed daily zenith spectrum has been used as a reference, so there are stratospheric NO2 contributions during twilight whereas tropospheric NO2 was shown to be under the detection limit for most of the cruise). BrO: 223 K (same reason).

8. Page 15988 L18. I suppose the instrumental FOV has been included in these calculations. How does the uncertainty in FOV affect to profile calculations for both NO2 and HCHO?
The FOV hasn't been included because block-AMF studies using the RTM SCIATRAN have shown that a FOV of about 1° (which is in the range of our instrument's FOV) affect only the bAMF calculations in the lowermost approx. 300 m by max. 5-6% relative to model runs without considering the FOV. (See picture below, which is the bAMF ratio as a function of altitude; the calculations were performed using SCIATRAN).



9. Page 15989 L5-6. I would appreciate more detail in the vmr used for the a priori profiles used for these calculations.
See also question 3 from Referee 1.
As mentioned in Sect. 3.5 (Data analysis) for both, HCHO as well as trop. NO2 the a priori profile is simply a linear decrease from the surface to 4km altitude. In the revised manuscript, we provide plots with retrieved profiles including the a priori (Fig. 10 and 16 in the revised manuscript).

10. Page 15990. Calculation of SC ref. It's not clear for me why satellite data from SCIAMACHY have been used to calculate the SCref for MAXDOAS measurement. Specially when the obtained final VCs are going to be compared with SCIAMACHY. There are methods to consider the diurnal variation of NO2 taking into account the photochemical behaviour of NO2 using modified LP (i.e., Lee et al., J. Quant. Spectrosc. Ra., 52, 5, 649–657, 1994 or Roscoe et al., J. Quant. Spectrosc. Ra., 68, 337–349, 2001) is there any reason for not to use them? Please clarify.
We decided to use a fixed reference spectrum over the remote ocean for the whole strat. NO2 analysis, because (in contrast to the beginning and end of the cruise) tropospheric contributions can be neglected. As our latitude-crossing speed was relatively fast (60° in 14 days) the strat. NO2 latitudinal variation would superpose the diurnal cycle. Therefore,

we didn't attempt to calculate the reference slant column taking into account methods for photochemistry, but used a fixed reference with known NO2 vertical column from simultaneous satellite observations. We agree that this is problematic when comparing the final results again to satellite measurements. However, we showed that even an uncertainty of 30% of the reference NO2 vertical column taken from the satellite can be neglected in the final results (see calculations in Sect. 4.1).
Further support for our choice of reference value is provided by Fig. 5 which shows the diurnal evolution of stratospheric NO2 on October 15. An inappropriate value of SC_ref would lead to large deviations of this curve around noon which is not the case.

11. Page 15991. Discussion of data showed at figure 5. As the x-axis of figure 5 is local time, it's difficult to me follow the discussion about the difference between vertical columns if figure 4. For an amount of 1.7x1014 molec/cm2, the difference between MAXDOAS vertical column time and the overpassing of satellite should be about two hours (and it's not, SZA 90_ it's at 6AM and overapssing at 9.30AM). As the used vertical columns used is an average between 90_ and 88_ SZA, I find that an extra axis with SZA would be very useful in figure 5. I would appreciate if the fit for calculation of rate of diurnal production of NO2 would appear in the figure as well.
After the morning minimum, the a.m. value (approximately the first blue data point) is reached again around noon, which is about two hours after the satellite's overpasses of 9:30 h, resp. 10:00 h. According to your request (here and in point no. 22) we changed the figure for the revised manuscript, plotting in addition a SZA axis and the trend line for the linear NO2 increase during the day.

12. Page 15991 L25. Discussion of differences between satellite and MAXDOAS NO2. It would be very useful to have a table in which would appear day, latitude, MAXDOAS NO2, SCIA NO2, difference MAXDOAS-SCIA, GOME2 NO2, difference MAXDOASGOME2. Please, consider to include it.
In the revised manuscript, we now provide such a table (Tab. 2). The first column is the day, the second column the latitude. Please note that all observations (MAX-DOAS a.m., p.m., SCIAMACHY and GOME-2) differ slightly from each other in measurement time and location. Therefore, in the second column the latitude of the ship's position at 12 h local time of the respective day (first column) is provided as a compromise for the measurement location. Please note also, that we recalculated the differences between the data sets (in the original manuscript, these are mentioned in the text only, now they are given in the text as well as in Tab. 2). The recalculated values differ slightly from the original ones, because we now used the values in Tab. 2 (originally, we interpolated due to the problem of different measurement times and locations). However, the changes are small.

13. Page 15993 L5. It's really difficult to observe the difference between SC for different elevations. Would be possible to modify the scale or the figure in order to see the described differences?
We tested, but a larger scale would cut-off the peaks. The message of this plot should be that larger trop. NO2 values occur only near the coasts where viewing angles split-up (this should be visible) and on some events (peaks at red arrows), while over the ocean tropospheric NO2 is mostly below the detection limit and subsequently no split-up is observed for most of the time anyways. We therefore decided to leave the figure as it is.

14. Page 15994 L20. It's not possible to distinguish when SC is abover detection limit or not by only looking at figure 6. I don't know if the line at y=0 is y=2x1015 molec/cm2 instead. At any case that should be explained in the figure caption.

The y=0 line is really the zero-line. For better visualization, we included the 2E15 molec/cm2 line in the revised manuscript. Over the remote ocean, only three events (at red arrows) are above this line, the rest scatter around the y=0 axis (this should be the message of this figure).

15. Page 15994 L 22-23. What difference would be if the method for estimating detection limit of Plat and Stutz 2008 were used instead?
The formula given in Platt and Stutz 2008 (also Stutz and Platt, Applied Optics, 35, 30, 6041ff, 1996) yields an even smaller detection limit than the intuitive approach we used. For example, using the values given in the text to derive the detection limit for trop. NO2 and a fitting window of 364 pixels, a DL of approximately 3E-5 for the optical density (corresponding to approx. 3E14 molec/cm2 for the slant column) would follow from Platt and Stutz 2008, which is almost one order of magnitude smaller than our estimate. Thus, the DL derived from the intuitive approach we used in the manuscript is an upper estimate (on page 15994, L20 of the unrevised manuscript, we stated that an upper limit was estimated). To clarify, we also give the DL according to Platt and Stutz 2008 in the revised manuscript.

16. Page 15995 L 15-17. I don't understand this sentence. MAXDOAS measurements have been performed at ship geolocation, what MAXDOAS measurements have been averaged and showed in figure 8?
This is an explanation of the brown data points in Fig. 8. In principle, these data points were spatially averaged MAX-DOAS vertical columns (blue data points). Therefore, no new information is added and we removed the brown data points and the confusing explanation, which easily leads to misunderstandings.

17. Page 15995 L 25 and Page 15996 L10. Please don't mention figure 11 before figure 10 in the text. Change the figures' order in the manuscript.
You are right, we changed the figures' order.

18. Page 15997 L22. Please, mention how the detection limit for HCHO has been calculated. If it's the same way than HCHO, indicate it. In that case, if Platt and Stutz 2008 is applied instead the definition of detection limit, is there any appreciable variation observed on the results?
We used here the same method as for NO2. This is indicated in the revised manuscript. Again, the formula of Platt and Stutz 2008 yields a smaller DL (about 0.1E14 for the VC), so our DL is an upper estimate, as was intended. (See also reply to point 15 and point 5 of Referee 1.)

19. Page 15998 L12. Regarding the observed diurnal cycle of HCHO peaking at noon, if oxidation of CH4 is the only mechanism to produce HCHO at a clean location, and destruction mechanism are reaction with OH and photodissociation, is this noon peaking expected by chemistry? Has this behaviour been previously observed?
Unfortunately, we were not able to verify the behavior of the diurnal HCHO cycle using a chemical model. However, the observed behavior appears reasonable to us. For example, Still et al. 2006 ("Ambient formaldehyde measurements made at a remote marine boundary layer site during the NAMBLEX campaign – a comparison of data from chromatographic and modified Hantzsch techniques", Atmos. Chem. Phys., 6, 2711-2726) observed a similar diurnal behavior of HCHO with elevated levels at noon/afternoon in the marine boundary layer at a remote station (Mace Head, Ireland). Also the results of MacDonald et al. 2012 ("DOAS measurements of formaldehyde and glyoxal above a south-east Asian tropical rainforest", Atmos. Chem. Phys., 12, 5949-5962, 2012) in the same tropical region as TransBrom show maximum HCHO concentrations around

noon/afternoon (but here, absolute values as well as the diurnal cycle is dominated by the emission and chemistry of precursors as the measurements were performed on Borneo, Malaysia). In the revised manuscript, we refer to these Papers when discussing our results.

20. Page 16011. Caption of figure 2. Please do not include information in the caption that is an explanation of results or methodology. In this caption I would keep only the first sentence.
Changed in the revised manuscript.

21. Page 16013. Caption of figure 4. Error margin plotted as grey-shaded area is not mentioned previously in the text. Please, describe it in the text when the figure is introduced. Change "gray" by "grey".
Included in the revised manuscript. (For explanation, see reply to point 2 of Referee 1.)

22. Page 16014. Figure 5. It would be very useful to have an extra axis with SZA in this figures and the line of the linear fit (mentioned in the text) over-imposed to data.
We changed the figure and included an SZA axis as well a linear for the linear NO2 increase during daytime (see also point 11.).

23. Page 16015. Caption of figure 6. Please do not include information in the caption that is an explanation of results or methodology. I consider that last sentence should be explained in the text, except the explanation what red arrows mean.
The figure's caption is changed in the revised manuscript and the information given in the text, as suggested.

24. Page 16017. Figure 8. Please indicate what grey-shaded area is.
See point 4 of Referee 1.

25. Page 16018. Caption of figure 9. Please do not include information in the caption that is part of the conclusions or of the discussion. Last sentence is explained in the text.
The last sentence of this caption has been deleted.

Technical corrections:
Included in the revised manuscript.