The Berkeley High Resolution Tropospheric NO_2 Product

Response to Anonymous Referee #1

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We appreciate the reviewers careful reading and suggestions to improve the readability of the paper. We realize that there is a great deal of information here and that organizing better will to minimize the strain on the reader. We believe that incorporating many of the reviewer's suggestions help in this matter.

Regarding the overall comment about the complexity of the paper's organization, we have taken this to heart and tried to address these concerns throughout (adding titles to figure panels, introducing entire figures where appropriate, and moving some figures from the supplement into the main paper). The most significant change we made was to add a new section, "Paper structure", after the methods and before the results. Here we outline the remainder of the paper, directing readers to the relevant sections if they are more interested in the changes to the algorithm, usage recommendations, or product description, and introducing the two main figures and tables that most of the discussion refers to.

Responses to specific comments follow. The reviewer's comments will be shown in red, our response in blue, and changes made to the paper are shown in black block quotes. Unless otherwise indicated, page and line numbers correspond to the original paper. Figures, tables, or equations referenced as "R*n*" are numbered within this response; if these are used in the changes to the paper, they will be replaced with the proper number in the final paper. Figures, tables, and equations numbered normally refer to the numbers in the original discussion paper.

The paper describes updates to V3.0A and V3.0B but it wasn't clear to me why there are two versions to discuss. Why not just discuss version 3.0B (presumably the most recent version)? Have people already published with V3.0A? Also, it is not mentioned in the paper what version is provided in the data files

v3.0A was available on our website for a 9-month period between November 2017 and July 2018. Just in case anyone downloaded that version and publishes results with it, we want there to be a record documenting the changes since v3.0A. We have added a paragraph clarifying this to the introduction:

> "v3.0A was available on the BEHR website (`behr.cchem.berkeley.edu`) between November 2017 and July 2018; v3.0B replaced v3.0A on the website and the static repositories (Laughner et al., 2018a,b,c,d) in July 2018. Therefore, in this paper, we will separate changes implemented in v3.0A from those in v3.0B,

so that the differences between v3.0A and v3.0B can be accounted for if any results are published using v3.0A."

The title: This paper describes an algorithm update to the product for one specific satellite instrument (OMI). I think it would be more useful to give a more descriptive title, as there already exists a former "Berkeley High Resolution Tropospheric NO2 Product" and perhaps there will be more in the future. Something like: "The Berkeley High Resolution Tropospheric NO2 Product: Updated version 3.0 for the Ozone Monitoring Instrument" for example.

We appreciate the desire for clarity; however we envision taking advantage of ESSD's living data philosophy to update this paper to describe future versions of the product, so tying the paper title to a specific version is counterproductive, especially as our version numbering system is such that "3.0" indicates the version of the NASA Standard OMI NO_2 product ingested as the base, if NASA updates their version, our version will also increment.

Abstract: Does this paper and dataset only include data over the US? OMI is global but the paper only shows US region so tell the reader early on. It's not clear to me what years of data are available. You use 2012 as an example throughout the paper but don't talk about other years. I only get that other years other than 2012 are available because it's mentioned to use some years with caution. Be specific in abstract and text. You discuss V3.0 here but V3.0A and 3.0B in text. Is this V3.0B that is actually listed and discussed in abstract, and provided on repository?

Correct, the data only includes data over the US and parts of Canada. We have clarified the geographic region and time periods covered in the abstract:

> "...BEHR v3.0B builds on the NASA version 3 standard Ozone Monitoring Instrument (OMI) tropospheric NO_2 product to provide a high spatial resolution product for a domain covering the continental United States and lower Canada that is consistent...
>
> ...The subproducts using monthly profiles are available from January 2005 to July 2017. The subproducts using daily profiles are currently available for years 2005–2010 and 2012–2014. 2011 and 2015 on will be added as the necessary input data are simulated for those years."

Further, at the end of the introduction, we also clarify why only certain years are available, and explicitly state that we are using only 2012 here as an example:

> "Because of the computational resources required to simulate daily a priori NO_2 profiles, BEHR v3.0B is produced for all years from 2005 on using monthly average NO_2 profiles, and for as many years as possible with daily NO_2 profiles. The latter is available for 2005–2010 and 2012–2014, with the remaining years following as the simulations of the necessary NO_2 profiles are completed. In this paper, we focus on the 2012 data as an example to understand the effect each change to the algorithm has on the final VCDs."

Page 1, Line 19: While I guess anything in the atmosphere could have some effect on the radiative balance, the radiative effects of NOx of any significance are indirect, not direct.

Section 4 of Kiehl and Solomon (1986) indicated that NO_2 direct heating of the stratosphere is usually a few percent, but may be more under certain conditions. However, since our focus is tropospheric NO_2, we have deleted this sentence.

Page 1, Line 23: You give other thorough details, but then end with "NOx itself is harmful". Be more precise – not clear what this means. Harmful in what way?

We have added:

> " Additionally, NO_x itself is harmful, **as, for example, exposure causes bronchoconstriction and associated difficulty breathing, especially for those affected by asthma** (Kagawa, 1985; Chauhan et al., 1998; Wegmann et al., 2005; Kampa and Castanas, 2008)."

Page 2, Line 4: actually, GOME-2 came after OMI. Reword to make sense. For context, give approx spatial resolutions of various resolutions to clarify OMI's ability to look at urban and point sources.

We removed GOME-2 from the "early" instruments and added resolutions:

> "The spatial resolution available with early instruments (i.e. the Global Ozone Monitoring Experiment, GOME, 40×320 km^2, Burrows et al. 1999b; the SCanning Imaging Absorption SpectroMeter for Atmospheric CHartographY, SCIAMACHY, 30×60 km^2, Noel et al. 1998) allowed inferences at the scale of entire continents or entire metropolitan regions, including cities and their surroundings. More recent instruments have much higher resolution (e.g. the Ozone Monitoring Instrument, OMI, 13×24 km^2, Levelt et al. 2006; the Tropospheric Monitoring Instrument, TROPOMI, 7×7 km^2, Veefkind et al. 2012), allowing inferences about individual point sources and urban cores."

Page 2, Line 17: "also" and "as well" are repetitive

Removed "also"

Page 2, Line 24: Lots of molecules are detected using remote sensing in the UV. If you want to mention NO, give specific reason it is not detectable.

As NO measurements are not important for understanding our product, we have modified this to simply state that NO is not measured by the current sensors in orbit:

> "The current fleet of space-based sensors measures NO_2, not total NO_x, but due to the rapid daytime equilibrium between NO and NO_2, this allows inferences about tropospheric NO_x to be made from NO_2 measurements."

Page 3, Line 8: It's not clear to me why the surface elevation is necessary for a radiative transfer model. Is this something in your particular setup?

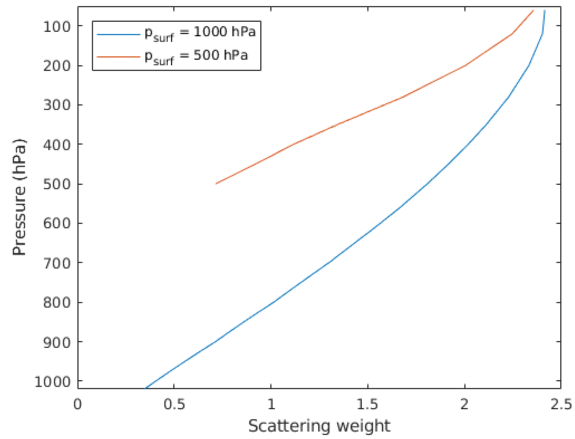

Figure R1: Two vectors of scattering weights for solar and viewing zenith angles of 30°, a relative azimuth angle of 180°, and a surface albedo of 0.02.

To our knowledge, all satellite NO_2 products need to know the surface elevation (or equivalently surface pressure) in order to get the lower boundary condition for radiative transfer correct. If there was a dark surface at a high altitude, for example, sensitivity would decrease more at higher altitudes because fewer photons would be scattered back to space before reaching the ground, where they would likely be absorbed (see Fig. R1). As this is standard in satellite NO_2 retrievals, we elected not to add any text explaining this to the introduction.

Page 3, Line 15: You discuss the effects of increasing the resolution in different studies, but relative to what original resolutions?

We have noted the resolution of the NO_2 profiles used in the global retrievals that these papers compare to:

> "Russell et al. (2011) found that increasing the resolution of the NO_2 profiles **from 2.5° × 2°** to 4 km altered the retrieved VCDs by up to 75%, primarily by capturing the urban-rural gradient in surface NO_2 concentrations. McLinden et al. (2014) found that increasing the a priori profiles' resolution **from 3° × 2°** to 15 km resulted in a factor of 2 increase in NO_2 column over the Canadian oil sands."

Page 3, Line 21: "restricted to a region of the world" is awkward phrasing. Any region is a region. Be more specific.

We have clarified:

> "The current trade off to obtain such high resolution profiles is that the resulting product **is only available over a subset of the world, rather than globally.**"

Page 3, Line 22: I don't think you should use the word "retrieval" here to describe your product. That implies you are doing a retrieval, which is really the spectral to slant column step (performed by NASA)

Our experience has been that "retrieval" is often used as a shorthand for the entire NO_2 algorithm or one of the components (SCD fitting or AMF calculation). Nevertheless, we have changed to "product" here and elsewhere in the paper.

Page 3, Line 24: Is v2.1C described elsewhere? Reference it.
Added:

> "Here we describe the updates from v2.1C to v3.0B. (For information on v2.1C, see Russell et al. 2011 and the changelog at `http://behr.cchem.berkeley.edu/Portals/2/Changelog.txt`.)"

Section 1 in general: I think you should list the science studies that have used the BEHR product specifically so that you can show importance of this product.
We have added the following near the end of the introduction:

> "The Berkeley High Resolution (BEHR) Ozone Monitoring Instrument (OMI) NO_2 retrieval is one such regional product that provides tropospheric NO_2 VCDs over the continental United States using high resolution a priori inputs. **The BEHR product has been used in numerous studies covering areas of research such as NO_x trends (Russell et al., 2012; Kharol et al., 2015; Pusede et al., 2016; Parker et al., 2017), anthropogenic emissions (de Foy et al., 2015; Jiang et al., 2018), soil emissions (Hudman et al., 2012), land use regression modeling (Bechle et al., 2015), and model evaluation (Canty et al., 2015; Travis et al., 2016).**"

Page 3, Line 27: The use of the word "visible" to describe an AMF here and elsewhere is confusing, but particularly in this section, where you have just discussed the visible (spectral) absorption of NO2. I had no idea what you were talking about until later in the paper.

We see how that could be confusing. Per one of your other suggestions, we moved discussion of the change to the visible-only AMF to the supplement, which should distance it from the discussion of the visible spectral range. Therefore, we removed this point in the itemized list, and added the following paragraph after the list:

> "These changes all affect the tropospheric VCDs. BEHR also provides a "visible-only" VCD, that is, the VCD excluding NO_2 below clouds for users interested in e.g. cloud slicing methods (Choi et al., 2014). These visible-only VCDs are computed by dividing the tropospheric slant columns by the corresponding visible-only AMF BEHR v3.0A implemented a more physically intuitive form of the visible-only AMF than that in v2.1C. This change is described in the supplement for interested users."

Page 3, Line 31: What were the old emissions? These update descriptions don't make sense without context.

We have added:

> "Monthly profiles use 2012 emissions, **instead of 2005 emissions used in v2.1C and prior**...Daily profiles, **with year-specific emissions**, used for as many years as possible"

Page 3, Line 2: vague statement

We are unsure which statement this refers to.

Page 4, Line 20: is g(p) mixing ratio or partial column? Specify

Mixing ratio, specified in text

Page 4, Line 24: Confusing to me what is provided in data until I looked at it.

Assuming this comment is directed to the regular VCDs vs the visible-only VCDs, we have specified what the variable names are in the data:

> "This method produces VCDs that include an estimated below cloud component, and thus can be considered a total tropospheric column. This is desirable for applications focusing on near-surface NO_2, **and are stored in the BEHR data as "BEHRColumnAmountNO2Trop"**. Other applications...
>
> ...yields a visible-only NO_2 column as the output, **stored in the variable "BEHRColumnAmountNO2TropVisOnly" in the BEHR files.** The form of this visible AMF changed from v2.1C to v3.0A; please see Sect. S1 in the Supplement for details of the old calculation."

Page 10: I'm not sure why so much detail is provided on RAA here. Is the definition different from what is standard or included in the OMNO2 files? If not, I would suggest you move it to the supplement.

We have found that different communities use different conventions for RAA; some define 0 as the sun and viewer are on the same side, others define it as the sun and viewer are directly opposite each other. We prefer to be explicit about how we define RAA so there is no confusion, and this seems the logical place to do so.

Line 18: Give reference for this empirical correction

Added:

> "A temperature correction, $\alpha(p)$ **(Bucsela et al., 2006, 2013)**,..."

Page 6, Line 6: Useful to know spatial resolution here for MODIS products

Added:

> "The coefficients, f_{iso}, f_{vol}, and f_{geo}, are taken **at 30 arcsec resolution** from the MODIS MCD43D07..."

Page 6, LIne 28: Mention this is modeled resolution

We have tried to clarify this. This line now reads:

> "The ratio of upwelling to downwelling radiation was simulated for 18 solar zenith angles (0° to 85° at 5° increments)."

Table 1: What does "atmospheric profile" mean? (p,T, O3?)

In the table, we have clarified that "MLS" means "mid-latitude summer" and not "Microwave Limb Sounder", in the caption we describe what quantities this concerns:

> ""Atmospheric profile" refers to the distribution of total precipitable water, O_3, CO_2, and CH_4."

Page 7, Line 2: For what is tropopause pressure used? Is it to define the height of the troposphere for equation 2?

We have added the following to the beginning of Sect. 2.4:

> "**For the upper integration limit in Eq. (2)**, BEHR v3.0A and prior versions used a fixed tropopause pressure"

Page 8, Line 6: You give a lot of other details. But what is source of OMI geometric and radiance cloud fractions? Is cloud fraction from the O2O2 algorithm cloud pres- sure? Why is there a MODIS cloud fraction included? I don't see this MODIS data mentioned anywhere else so not sure why it's here. It's from a different satellite which seems like a dodgy cloud product for use in OMI analysis.

We had specified that the OMI cloud products are the same as those in the NASA standard product. We have expanded this to identify them directly:

> "BEHR contains several cloud fraction products: **a geometric cloud fraction derived from the O_2-O_2 algorithm (Acarreta et al., 2004), a cloud radiance fraction calculated by NASA from the O_2-O_2 product,** and a geometric cloud fraction derived from the Aqua MODIS instrument (which currently makes observations ∼ 8 min before OMI), and cloud pressure from the OMI O_2-O_2 algorithm (Acarreta et al., 2004)."

The MODIS cloud fraction is included because Russell et al. (2011) found it to be less succeptible to errors caused by high surface reflectivity. We retain it in the product as an alternate method of cloud filter for users running into similar issues. The Aqua satellite precedes Aura by about 8 minutes currently, so they are reasonably coincident in time. However, for the AMF calculation we use the OMI cloud fractions, because the MODIS cloud data is not available for every pixel, and because we need a cloud radiance fraction, not just a geometric fraction, for the AMF calculation. We have added text explaining this:

> "Russell et al. (2011) found that the MODIS cloud product was less likely to give erroneously large cloud fractions due to high surface reflectivity over the California and Nevada desert, and concluded that this more than offset any error caused by the small separation between the overpass times (currently ~ 8 min) of OMI on board the Aura satellite and MODIS on board the Aqua satellite. We continue to provide the MODIS cloud product for cloud filtering; however, because it does not cover the full OMI swath, we use the OMI cloud fractions in the AMF calculations."

Section 2.6: Again, I'm confused about what years are analyzed, what years are modeled with WRF-Chem and why. Why is MOZART not available 2005-2006?

We added a paragraph in response to your earlier comment about what years are analyzed in this paper vs. available in the BEHR product. We have also added the following text at the beginning of section 2.6 that details why there are different sets of a priori profiles:

> "From v3.0A onward, BEHR is divided into two subproducts which differ in the temporal resolution of the a priori NO_2 profiles. Based on the results in Laughner et al. (2016), using a priori profiles specifically simulated for each day of BEHR observations is preferable; however, the computational cost of doing so limits the time periods that such profiles can be simulated for. Therefore a second subproduct using monthly average profiles derived from the 2012 a priori profiles is available that covers all years of the OMI data record. This assumes that monthly average profiles are applicable to years other than that for which they were simulated; while not a perfect assumption, it has successfully been used in previous NO_2 products (e.g. Bucsela et al., 2013).
>
> In this section, we describe the model configuration used to generate the a priori profiles. General model settings will be described first, followed by information specific to the implementation of daily and monthly average profiles in the BEHR algorithm."

As to why MOZART is not available, we are not running MOZART, NCAR provides MOZART output from 2007 on to give chemical boundary conditions for WRF-Chem. Why 2007 is the starting point isn't clear, but since we do not have experience with MOZART and *do* have experience with GEOS-Chem, we elected to use the latter for the two years of the OMI record that MOZART data isn't available for.

Page 8, Line 28: Is MOZART only used for boundary conditions? This sentence is confusing.

Yes, we have clarified:

> "**Chemical boundary conditions for WRF-Chem are taken two different global models.** For model years 2007 and later, **chemical concentrations** from the Model for Ozone and Related chemical Tracers"

Page 8, Line 31: What is GEOS-Chem resolution?
Added:

> "...the chemical data is taken from the GEOS-Chem model v9-02 **(at 2.5° × 2.0° resolution)**, with updates from... "

Page 9, Line 14 and Line 21: what does "when possible" mean? What years are available and what determines this?

Computational cost is the limiting factor as each year costs about 300,000 core-hours to simulate. We have clarified:

> "We make use of daily profiles for **as much of the OMI data record as it is computationally feasible to simulate these profiles**....
>
> As of this writing, daily profiles have been simulated for 2005 to 2010 and 2012 to 2014. Profiles for 2011 are in progress, and profiles for 2015 and later years will be simulated as time and computational resources permit....
>
> Given the computational cost in producing daily a priori profiles, we continue to use monthly average profiles as well to cover years **for which daily a priori profiles have not yet been simulated.**"

Page 9, Line 22: I'm not sure I understand what you are doing here. Is this making a single climatology for the entire month? Why not make 24 1-hour climatologies for each month and take the nearest profile of each observations?

Correct, this creates a single climatology for the entire month. Your suggestion is also a good way to do that, but not one we considered when designing the algorithm. This method of creating a single monthly climatology is in line with the heritage method used in earlier versions of BEHR.

Figure 1: Caption needs to mention what this is relative to, and version (ie., changes between V2.1C and V3.0A or whatever).

As mentioned at the beginning, we added a new section on the paper structure that describes this:

> "In sections 4 and 5, we evaluate the effect each change to the BEHR algorithm between v2.1C and v3.0B had on the tropospheric VCDs. In order to provide a clear history, changes introduced in v3.0A will be discussed first (Sect. 4), followed by changes introduced in v3.0B (Sect. 5). V3.0A incorporated all changes up through the introduction of the new gridding algorithm; the remainder are added in v3.0B. Changes to the visible-only VCDs (i.e. those excluding the below-cloud column) are discussed in the supplement (Sect. S1). Following this the overall difference between v2.1C and v3.0B will be presented in Sect. 6. Recommendations for the use of the product are given in Sect. 7. A description of the data format is given in Appendix A.

For the discussion of how changes to the algorithm affect the NO_2 VCDs, figures 1 and 2 and Tables 3 and 4 are the central focus. Each panel shows the change in the BEHR NO_2 VCDs resulting from a specific change to the algorithm. To generate these figures, BEHR VCDs were computed after adding each change to the algorithm incrementally. Each panel in the figures and line in the tables shows the percent change in VCDs due to the corresponding change to the algorithm. These are computed relative to VCDs with one fewer change to the algorithm; for example, Fig. 1b is the percent difference between VCDs using the new NASA SCDs and the new MODIS BRF surface reflectance versus VCDs using just the new NASA SCDs. The (a) panels in Figs. 1 and 2 and the first lines in Tables 3 and 4 are relative to BEHR v2.1C.

Figure 1 shows the percent change of average BEHR tropospheric VCDs due to each algorithm improvement for the subproduct using monthly average NO_2 a priori profiles, while Fig. 2 shows the changes to the subproduct using daily NO_2 a priori profiles. (Figure 2 has fewer panels than Fig. 1 as daily profiles were only possible in increments after the change to the algorithm to introduce the new a priori profiles was implemented.) Both figures are for summer (June–Aug.) 2012. Winter changes are presented in the supplement.

Table 3 gives the mean and median changes for each incremental improvement shown in Figs. 1 and 2; that is, it gives the domain-wide mean and median values of the time-averaged changes shown in the figures. Table 4 is similar, but is the statistics for individual pixels, rather than the time-averaged changes."

While this seems complex, it was much more sensible during development to add each change incrementally, rather than implement each change separately then merge them all together.

Since the VIS AMF is actually a separate AMF product in the datafiles, I find it very confusing to have it included in the changed parameters. Could it be moved to its own figure and discussion? It feels confusing to have it brought up in the middle of the other discussions of input parameters.

Good point. We agree and have moved this discussion to the supplement, so that the majority of readers who will be interested in the regular VCD/AMF will not be burdened by it.

Page 10, Line 7: Here and elsewhere: Figure and table referred to without contextual introduction. I think Figure 1 should be introduced and discussed concisely before a panel is mention in brackets with other tables).

As mentioned two comments ago, we've added a section before the existing results sections that introduces Figures 1 and what was Figure 4. Figure 4 has been moved to be Figure 2 so that both can be introduced together, along with Tables 2 and 3.

Table 2: Mention data version and change relative to what.

This is included in the addition from three comments prior.

Tables 2 and 3: Probably unnecessary significant figures after decimal place?

Adjusted so that no number has > 1 significant figure after the decimal place (though that significant figure is sometimes in the hundreds) and the 1σ or quantile value adjusted to end at the same place as the main value. These are expressions of variability, not uncertainty, so it does not make sense to truncate the main value at the first digit of the 1σ or quantile value.

Page 13, Line 1: Specify what is the other approach

Note: this is now in the supplement. Specified as:

> "Although both approaches to calculating a visible-only AMF (i.e. Eq. S1 and Eq. 3) are conceptually valid..."

For reference, Eq. S1 is:

$$A_{\text{BEHR,vis}} = (1 - f)A_{\text{clear,vis}} + fA_{\text{cloudy,vis}}$$

and Eq. 3) is

$$A_{\text{BEHR,vis}} = \frac{(1-f)\int_{p_{\text{surf}}}^{p_{\text{trop}}} w_{\text{clear}}(p)g(p)\,dp + f\int_{p_{\text{cloud}}}^{p_{\text{trop}}} w_{\text{cloudy}}(p)g(p)\,dp}{(1-f_g)\int_{p_{\text{surf}}}^{p_{\text{trop}}} g(p)\,dp + f_g\int_{p_{\text{cloud}}}^{p_{\text{trop}}} g(p)\,dp}$$

Page 14, Line 28: Reference to v3.0B but still in V3.0A section.

Corrected.

Figure 4: First reference is to panel c on page 18, but I think need an introduction to general figure for context. Include some titles on figures panels for readability. Also, mention change is relative to what. And what is the version?

This has been addressed in the new "Paper structure" section.

Page 15, Line 6: I find this discussion a bit hard to follow, with references to a previously undiscussed figure (I need a sentence to help me interpret it first) and to the supplement. Also, "it" on line 6 refers to what? What is "order of averaging"?

We have rewritten this section to help with clarity. In the revised version, we do not discuss the hypothesis involving cloud fractions, since it turned out to not be true. The new discussion here focuses on the qualitative difference between monthly and daily profiles, while a mathematical line of reasoning is left to the supplement for readers seeking a deeper understanding. The new section is:

> "Figure 4 shows the difference in v3.0A of the average total tropospheric NO_2 columns when using daily NO_2 profiles rather than monthly average profiles. Figure 4a is the summer (JJA) average, and shows a significant increase in VCDs along the eastern US, which is not present in the winter (DJF) average (Fig. 4b). The timing and location suggests that this difference is due to lightning, as the southeast US especially has very active lightning (Laughner and Cohen, 2017; Travis et al., 2016; Hudman et al., 2007).

Ultimately, the fact that lightning is an intermittent but significant NO_x source in the upper troposphere (UT) is the cause of this difference. Figure 5a shows the statistical distribution of NO_2 in the UT for two regions in the US: the southeast, which has significant lightning activity, and the northwest which has very little lightning. The distribution is highly skewed with a long tail in the southeast US due to the lightning activity, but not in the northwest US. Because of the nonlinear nature of the AMF calculation, this skewed distribution translates into different average VCD values.

Figure 5, panels b and c show average shape factors derived from monthly averaged and daily a priori profile for the southeast and northwest US. A shape factor is a profile divided by its integral:

$$S(p) = \frac{g(p)}{\int_{p_{\text{surf}}}^{p_{\text{trop}}} g(p)\,dp}$$

A shape factor can be intepreted as the relative vertical distribution of NO_2. It appears implicitly in the AMF calculation (Eq. 2).

Here we see how the skewed UT NO_2 distribution affects the southeast US AMFs through the shape factor. Figure 5b shows that the statistically skewed UT NO_2 distribution causes shape factors calculated from the monthly average a priori profiles in the southeast US to have a larger fraction of the column NO_2 in the UT than that calculated from the daily profiles. Through Eq. (2), this leads to systematically greater AMFs (and therefore smaller VCDs) in the southeast when using the monthly profiles if the scattering weights ($w(p)$ in Eq. 2) are greater in the UT than near the surface, which is usually the case. In contrast, Fig. 5c shows no difference in the monthly or daily shape factors for the northwest US. For interested readers, a more mathematical argument is given in Sect. S2 of the supplement.

The implication is that, for regions with long-tailed statistical distributions of NO_2 concentrations, there will be systematic differences between a product using monthly average and daily a priori profiles. It is likely that the VCDs calculated using the daily a priori profiles are more accurate, because in theory daily a priori profiles should properly account for that long tail on days when it is relevant, whereas monthly profiles will average in the extreme values.

Finally we note that this difference between daily and monthly profiles may change in the future. Laughner et al. (2018e) found that the simulation providing the NO_2 profiles had too much lightning in the southeast US. Correcting that may reduce the skewness of the UT NO_2 distribution. Work is underway to improve the representation of lightning for the southeast US NO_2 profiles. ”

Figure 5: What is data version here?
v3.0A—added to caption.

Figure 6: Need some intro discussion in text. I found I was with a reference to this figure but not much assistance on how to interpret it.

This figure has been simplified, and in the rewritten section above it is discussed:

> "Figure 5a shows the statistical distribution of NO_2 in the UT for two regions in the US: the southeast, which has significant lightning activity, and the northwest which has very little lightning. The distribution is highly skewed with a long tail in the southeast US due to the lightning activity, but not in the northwest US....
>
> ...Figure 5, panels b and c show average shape factors derived from monthly averaged and daily a priori profile for the southeast and northwest US....Figure 5b shows that the statistically skewed UT NO_2 distribution causes shape factors calculated from the monthly average a priori profiles in the southeast US to have a larger fraction of the column NO_2 in the UT than that calculated from the daily profiles. Through Eq. (2), this leads to systematically greater AMFs (and therefore smaller VCDs) in the southeast when using the monthly profiles if the scattering weights ($w(p)$ in Eq. 2) are greater in the UT than near the surface, which is usually the case. In contrast, Fig. 5c shows no difference in the monthly or daily shape factors for the northwest US."

Page 8, Line 15: Not sure what is (2) at end of sentence.
Was meant to be a reference to a table—fixed.

Section 4.1: You give 6 points early. Break up this section for better organization.
Done.

Page 19, Line 10: I believe you mean "overage average difference" instead
Overall average difference, yes

Page 19, Line 30: Just to clarify, these scattering weights are from the OMI SCD product?
No these are scattering weights used in BEHR. We have clarified:

> "...an array of scattering weights **used in the BEHR AMF calculation** is included..."

Page 20, Line 3: I'm not really clear why users could expect to reproduce at all without cloudy scattering weights and surprised it's so small, unless really low cloud fractions are being considered. Seems like this should not be attempted as obviously information is missing. I'm not sure you really need to go into the details here. Just saw you need to provide it for reproducibility/completeness. Line 5: If people start using their own cloud fractions, will they be consistent with the cloud top heights? Not sure this is something that should be encouraged for any except most advanced users.

The old scattering weights were not just the clear sky scattering weights, they were, as we said, the weighted average of the clear and cloudy scattering weights, so that users only had to deal with a single vector per pixel. The cloud fractions and pressures were intrinsic to the published scattering weights, because the cloudy weights were set to 0 below the cloud pressure, and because the published weights were this cloud radiance fraction weighted sum. We have added some equations to clarify this:

> "In BEHR v3.0A and prior, these scattering weights were the cloud radiance fraction weighted average of the temperature-corrected clear and cloudy scattering weights:
>
> $$w'(p) = (1 - f)w_{\text{clear}}(p)\alpha(p) + f w_{\text{cloudy}}(p)\alpha(p)$$
>
> where $\alpha(p)$ is defined by Eq. (6) and $w_{\text{clear}}(p)$ and w_{cloudy} set to 0 below the surface and cloud pressures, respectively.
>
> Using these scattering weights along with the published a priori profiles, users could reproduce BEHR AMFs well, to within $0.5 \pm 1.9\%$, using:
>
> $$A' = \frac{\int_{p_{\text{surf}}}^{p_{\text{trop}}} w'(p)g(p)\,dp}{\int_{p_{\text{surf}}}^{p_{\text{trop}}} g(p)\,dp} \qquad \text{(R1)}$$
>
> "

Indeed, this is something that only very advanced users should attempt, and the issue of inconsistent cloud fractions and cloud pressure is definitely a concern, but not one that can be rectified easily without a user running the full algorithm, since a different cloud pressure would change the scattering weights by more than just where the cloudy ones are cut off. We have added cautions about this:

> "Very advanced users may wish to recalculate custom AMFs using their own NO_2 profiles but with the scattering weights used in BEHR....
>
> ...The primary purpose is to allow users to replace the BEHR NO_2 profiles with their own for a custom AMF calculation. In theory, this also permits advanced users to use different cloud fractions in their custom AMF calculations, but doing so would require careful attention to possible errors, as the scattering weights are tied to the cloud pressure used in BEHR."

Page 22, Line 13: Why are some years available and others not? (Can mention earlier in paper.)

We have addressed this in the intro and methods

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