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Interactive comment on "Nitrogen dioxide stratospheric column at the subtropical NDACC station of Izaña from DOAS, FTIR and satellite instrumentation" *by* Cristina Robles-Gonzalez et al.

Anonymous Referee #1

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Before proceeding with some minor comments, I suggest the authors address these two major, to my mind, issues:

1. Independent studies (Belmonte Rivas et al. 2014; van Geffen et al. 2015; Marchenko et al. 2015) show that, even after the appropriate diurnal correction the SCIAMACHY-nadir and OMI stratospheric VCD(NO2) systematically differ by 8-12*10ˆ14 molec*cm-3 in the contamination-free areas. Such differences do not show any discernible longitudinal dependencies, hence they could be applicable to the Atlantic-ocean area under consideration. However, this study provides the initial (un-

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corrected for the diurnal NO2 changes) estimates of OMI-SCIAMACHY ∼ 4*10ˆ14 (bottom sections of Fig. 4), i.e., far below van Geffen's et al. (2015, AMT, 8, 1685) evaluation: OMI-SCIAMACHY ∼13*10ˆ14. The authors should address the source of this ∼300% difference in the estimates.

2. I am uncomfortable with the idea of applying the diurnal correction via introduction of a fixed, purely geometric factor. This factor (effective SZA) is based on the 27.5 km estimate of the 'effective height' of the vertical NO2 profile. Firstly, for the tropical zone the SCIAMACHY profiles point to maxima in the NO2 profiles at H>∼30 km (Bauer, R., et al. 2012, AMT 5, 1059). Moreover, the effective height of the stratospheric NO2 shows ∼10% seasonal changes (Spinei, E., et al., 2015, AMT, 7, 4299). If the authors insist on introducing the 'effective SZA' correction, then it must be based on the most recent NO2 profile estimates (either models or observations), plus their seasonal changes, since such correction factor should be calculated with the weights provided by seasonal NO2 profiles extracted either from the SCIAMACHY data or the CTM output. In addition, such profile-weighted 'effective SZA' may result in slightly different corrections for the AM and PM observations, based on the differences in the morning/evening NO2 profiles. Besides this rather technical detailing which may introduce some relatively minor adjustments to the proposed correction, I question the validity of the 'effective SZA' approach. At the moment of a twilight zenith observation, the most probable light path (ch. 9, Platt & Stutz, 2008) is defined by two factors: the Rayleigh scattering and the trace-gas absorption, with dominance of the former in the particular NO2 case. True, the strength of absorption depends on the pathway-vise distribution of the absorbers (hence the proposed 'effective SZA'). However, the registered signal is weighted by the intensity of the Rayleigh-scattered light. It remains to be proved (presumably, by applying a single-scattering 2-component RT model) that the 'effective SZA' estimates have some merit. Any RT+CTM -based correction seems to be preferable over the proposed 'effective SZA' factor.

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