Surface Tropical Ozone Trends (1998 to 2023): A Synthesis from SHADOZ, IAGOS and OMI/MLS Observations

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General comments

The manuscript assesses the ozone trends in tropospheric columns and the lowermost stratosphere based on ozone profiles obtained from ozonesondes (SHADOZ) and aircraft measurements (IAGOS) within the tropics (15°N-15°S). The authors extended the period previously analyzed in Thompson et al., 2021 (1998-2019) until 2023. SHADOZ stations were merged into five zones to improve representativeness and assess trends using a multi-lineal regression model and quantile regression, the latter as suggested by TOAR-II guidelines. Ozone trends in the tropics are relevant in terms of radiative balance and ozone precursor emissions, to mention some. In addition, the article addresses topics of high interest, such as declining convection in some tropical regions and the trends in the lowermost stratosphere. However, in my assessment, this manuscript needs major revisions before considering for publication.

Beyond the potential value of updating the previous work until 2023, the scientific motivation for the research is unclear. Neither the abstract nor the introduction clearly establish the research's contribution. Many aspects discussed in the paper are not developed in depth in the introduction, and instead, the references are widespread throughout the document. This option is certainly valid, but it lacks focus. The questions outlined (four bullets) at the end of the introduction require further elaboration. Also, in the introduction more literature and key findings need to be discussed. The figures require improvement (e.g., Figures 1, 2, 3, 5 and 7), and some of them can be moved to supplemental material to reduce the current number and emphasize the key message. In my opinion, the current version requires better organization (e.g., Methodology and Summary), and some sections must be refined to describe better the results (e.g., the first paragraph of section 3.3.1).

Minor comment

Lines 30-34: In the first lines, I expect the presentation of the scientific gap the main motivation. Instead, the authors included three references, which can be added later in the introduction.

Line 52: Why not include a reference here?

Lines 65-66: Key references are missing here.

Line 84: "TOAR-II decided…" Is that a TOAR-II working group decision or a TOAR-II suggestion?

Line 88 Which phases of TOAR?

Lines 109-119: These four questions need more elaboration and a clear statement of the scientific objectives. In what way could this comparison be different from other studies published in the frame on TOAR-II? Throughout the manuscript, the authors addressed other

publications such as Gaudel (2024) or Van Malderen (2024). However, at this point, we should be clear about the objectives.

Line 132: P-T-U is not defined for readers unfamiliar with this term.

133-134: The authors are referencing Quito station using three papers; the lat lon or something else changes within the publication of these papers?

Lines 135-137: What has changed in these three references?

Line 170: Note that LMS was already defined in line 145

Line 207 Complete: SOTC (ref)

Line 175: Define (P-T-U) earlier

262: Add ozone: tropospheric ozone trends.

277-279: The wording of this sentence is not clear to me: *"In the individual site analyses of HEGIFTOM observations (Van Malderen et al., 2024a), where all individual ozone records (L1) and monthly means (L3) were analyzed, annually averaged trends usually turned out to be the same within uncertainties".* Can L1 and L3 be defined explicitly in the main text?

Line 289: The mention of the Walker circulation is a good example of the kind of background needed to improve the introduction.

Line 299: This sentence is already mentioned in the methodology: *We assign TH to the altitude of the 380K potential temperature.*

Line 299: If trends of the LMS are one of the relevant topics addressed in this research, why is Randel (2007) not included in the background discussion in the introduction?

Line 314: I read cf. here and in other parts of the article. What is the meaning here?

Line 339-341: I understand that this interpretation is valid for 5-10 km. Is it similar to other portions of the troposphere (e.g., upper troposphere)?

Line 437: Instead of "sufficient" I suggest representative.

Line 463: I suggest defining OLR to avoid ambiguities

Lines 476-480: If these lines represent the authors' conclusions, shouldn't they also be paraphrased in the abstract? The word "definitive" isn't too strong?