We thank both reviewers for their thoughtful comments and have improved the manuscript following their helpful questions and suggestions. Please find a point-by-point response below, with reviewer comments in italics and author response in blue. -Michael Diamond

Anonymous Reviewer 1

The paper uses the change in cloud drop number concentration ratioed to the change in NO2 to assess how the IMO's implementation of sulfur-limiting regulations in 2020 affected cloud brightening due to ship tracks. The SE Atlantic is the area of focus due to recent increases in ship traffic as ships transit around the Cape of Good Hope rather than through the Suez Canal to avoid Houthi militia attacks. Comparing years 2019 and 2024, a large decrease in the ability of ship emissions to impact Nd is found. The paper is concise and well-written. The figures are of high quality and effectively display the results reported in the paper. I only have a few minor comments.

We thank the anonymous reviewer for their positive appraisal and constructive suggestions to improve clarity.

Line 35: "the estimated magnitude of the IMO 2020 effect ranges from ~15-75%". Is this based on the reduction in detectible ship tracks? Please clarify.

This is based on a combination of different studies and methodologies including ship track detection, air-mass tracking of ship locations, and analysis of the SE shipping corridor, as summarized in the assessment of Gettelman et al. (2024). The text has been revised to clarify this point (Track Changes lines 37-41).

Lines 67 – 68: "Because the 2018 NO2 values have a particularly low tail and Nd values have a high tail…" I could be interpreting this statement incorrectly but the 2018 high tail of the Nd values is not evident in Figure 2b.

Although both underlying NO2 and Nd distributions are approximately normal, the important point to make here is that the lower end of the 2018 NO2 values are particularly low (only year with substantial weight below 2.5%) and the upper end of the 2018 Nd values are particularly high (only year with substantial weight above 10%). The text has been revised to clarify this point (Track Changes lines 74-75).

Line 88: Does the "binary detection threshold metric" refer to relying on only two years to assess the cloud response to the decrease in ship sulfur emissions?

Apologies for the confusion — this refers to a detection metric which is binary in that it only has two options ("ship track detected" versus "no ship track detected"). The importance here is that a binary metric like this cannot provide useful information like "a ship track is detected but it's 67% weaker than before". Worse, the better the detection algorithm is, the poorer it will be at providing information about weakening but still-existent tracks. The text has been revised to clarify this point (Track Changes lines 97-103).

Jay Mace (Reviewer 2)

The study by Diamond and Boss examining the change in cloud droplet number (Nd) in the SE Atlantic region following the increase in shipping there during 2024 relative to the years since the fuel change and relative to the pre 2020 results shows that Nd increased in proportion to the increase in traffic. This allows the authors to quantify the efficiency with which Nd is influenced by shipping exhaust from before and after the 2020 change in sulfur content. Overall, I find the paper to be straightforward, concise, and compelling. I suggest only minor revisions.

We thank Jay Mace for his positive appraisal and constructive suggestions to more rigorously test for meteorological influences that could complicate single-year analyses and to improve the clarity of the manuscript.

The authors acknowledge the fact that examining a single year (2024) for the increase in traffic in their study region is somewhat fraught. They make a compelling and convincing argument that their findings are significant. However, I think they need to at least examine the large-scale atmosphere during 2024 to see if the months they consider are typical or perhaps anomalous relative to other years. The reason I think this is necessary is due to the role of drizzle in modulating Nd. While their data do not constrain drizzle occurrence or rate, were, perhaps, the marine inversion different from other years, drizzle may be more or less common, etc. While I think their results will hold up against this examination, it would be at least useful to examine this and report upon it

Thanks for the excellent suggestion, and we agree that accounting for the meteorological state, particularly in terms of drizzle, would increase the rigor of using single-year estimates.

See new Figure 4 and Track Changes lines 127-152 (Section 3.2) for a discussion of our analysis of CERES SSF NOAA-20 cloud fraction, cloud effective radius, aerosol optical depth, SST, EIS, and wind speed data. In brief, no concerning outliers are identified.

Lines 125-130 (first paragraph in the methods): It took me a bit to digest this and realize that the delta values in figure 2 were relative to the regression they discuss (do I have that right?). I think an illustrative figure would be helpful here to show their method. I think the approach is sound, but understanding their text would benefit greatly from a figure.

Thanks for another excellent suggestion (and yes, your understanding is correct). See new Figure A1 for an illustration of the approach using the September-October 2024 data.

Caption to Figure 2: Note that the region displayed is from SE Atlantic. This is somewhat obvious but it would still be helpful to casual reader who might just scan the figures.

Great point; clarification has been added to the caption of Figure 2.

With compliments to the authors,
Jay Mace