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Review of Christensen et al.

Anonymous Referee #2

Referee comment on "Opportunistic experiments to constrain aerosol effective radiative forcing" by Matthew W. Christensen et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-559-RC2, 2021

This paper aims at reviewing the advantages and limitations of various kinds of "opportunistic experiments" that nature and/or mankind offer to those studying aerosol radiative forcing of climate change. The paper is focused on aerosol-cloud interactions and their rapid adjustments. The authors highlight the important scientific findings that the "opportunistic experiments" have allowed, but caution that scaling those findings to the global scale remains difficult.

The paper is well written and its aim of providing a "common footing" in which to understand and interpret the opportunistic experiments is laudable. However, the paper could fulfil that aim more sharply. For that reason, I recommend revisions to address the weaknesses listed below. This could involve a sizeable restructuring of the paper, so major revisions may be needed.

Main comments:

- The paper does not give a tight definition of what an "opportunistic experiment" is. There is an attempt at lines 34-35, but it is unclear whether that definition covers all the types of experiments that are discussed in the paper. Some types do not allow to "know the unperturbed state", for example – more on that below. In addition, that definition is immediately thrown away "for convenience" in lines 38-39. It should be possible to use a consistent vocabulary throughout.
- The paper presents two main types of opportunistic experiments. The first type is essentially based on in-plume/out-of-plume comparisons (sections 2.1, 2.2, and to some extent 2.3 and 2.4). The second type covers events that are either much larger or comparing situations that are very distant in time (sections 2.5, 2.6, 2.7, 2.8). But the two types sit uneasily together. There is a clear tension between ability to determine the unperturbed state (that would favour the first type) and relevance to climate scales (that would favour the second type). Figure 10, the only figure that

attempts to mix the two types of results, illustrates the tension well. But the paper never attempts to resolve that tension. Tellingly, the very good section 5 is mostly based on the first type of experiments – it is the ability to determine the unperturbed state that has brought the best insights. Are some experiments too large in scope? Is there a sweet spot somewhere? The paper could offer guidance there.

- Section 4 on Methods is the weak point of the paper and compares poorly to the very good sections 2 and 5. I think that is because it lacks a clear focus and too often reads like an advert for upcoming campaigns/instruments/models. I am not sure that the section is needed. Pointing to key datasets is the role of section 3. Listing key insights brought about by specific campaigns or models should be done within the context of sections 2 and 5.

Other comments:

- Lines 11-12: I would move that sentence before the previous one, to have the statement on instantaneous forcing preceding that on rapid adjustment.
- Line 83: I would replace "as evident in" to "according to", because EDGAR is not an observational dataset.
- Line 122: Missing word between evolution and several.
- Line 145: I do not understand the "therefore". Is a large fraction of the ship tracks simulated by Peter et al. undetectable?
- Lines 161-180: This paragraph lacks a clear conclusion. The difficulty of model-data comparison is one thing, but how does that relate to the use of corridors as opportunistic experiments?
- Lines 238-239: That statement does not add much and could be deleted.
- Lines 253-257: Are those two sentences in the right place? The section would end better without them. (And the sentence on lines 254-255 is grammatically incorrect.)
- Lines 266-267: That statement seems to miss the point that ships increase aerosol loading over a normally low baseline.
- Lines 273-274: Could refer to section g3 of the Global Climate section of the BAMS State of the Climate 2020 report https://doi.org/10.1175/BAMS-D-21-0098.1 to support that statement.
- Line 312: It would help readers to clarify the link between economic restructuring and cloud reflectance.
- Line 312: "co-incident upward trend" combined with the previous sentence suggests that changes in cloud reflectance caused the trend in surface radiation. That is not so sure. Trends in surface radiation are much more robust in cloud-free sky than in all sky conditions.
- Line 316: "CMIP6 emissions database" needs a reference, especially as different versions of CMIP6 emissions have very different histories for sulfur dioxide.
- Lines 381-383: Would need references that attempted such approaches to support the claim that they hold promise. They sound very hard to do in practice.
- Lines 439-440: What caused that shallowing? Anomalous meteorology?
- Lines 447-449: Is that result linked to previous discussions that variability in cloudiness can "hide" substantial radiative forcings?
- Line 458: It would be useful to link contrail reductions to the subject matter of the paper, aerosol ERF. Perhaps a short reminder of the mechanisms of contrail formation?
- Lines 471-475: Aren't there ship track databases for the Southeast Atlantic stratocumulus deck? There are a few studies that focused on that region and some of that data seems to be used in Figure 10.
- Line 522: "can be performed" – reference needed there.

- Line 576-577: What are the implications of these findings for aerosol ERF?

Technical comments:

- Page 3, line 48: human -> human-caused
- Line 512: Delete “because”
- Lines 586 and 656: Character encoding problems.