Review of "Opinion: Establishing a Science-into-Policy Process for Tropospheric Ozone Assessment" by Derwent et al.

In this Opinion piece, Derwent et al. seek to highlight that the existential problem of increases in the abundance of tropospheric ozone could be addressed through a more formal science-to-policy framework – the likes of which has had success in protecting the stratospheric ozone layer and (hopefully) limiting the effects of anthropogenic climate change.

Clearly everyone is entitled to their own opinions and the authors are world renowned and respected for their work on tropospheric ozone, so their opinions matter. But I feel there are some significant issues with this piece that should be addressed before final publication. The major issues are discussed below alongside more minor issues latter.

Major issues:

1) **Do we not already have a "model" of tropospheric ozone?**

A key premiss of this paper is that as a community we lack a parsimonious model that can describe the processes that control tropospheric ozone. Although the level of simplicity is arguable, at least in my mind we have such a parsimonious model. Indeed, Figure 3 in the paper outlines such a model and this model has been the *de facto* model used within the community since at least the mid 2000s. In which case, what new insight is this Opinion piece adding?

At its simplest we can say that the model of climate change is a question of forcing and feedback:

$\Delta N = \Delta F - \alpha \Delta T$ **Equation 1**

The model of tropospheric ozone can also be written very simply (where ∇ is used to represent transport, $P(O_3)$ the production and L the first-order loss rate of ozone):

$P(O_3) = (L + \nabla)[O_3]$ **Equation 2**

However, these simple models are not practically useful. Complex problems require complex models. There is a good point to be made that the level of complexity of our model (Figure 3) is not fit for purpose but it's not clear how we as a community go about determining this. It seems to me, at least, that the model we have for tropospheric ozone (Figure 3) is fine. The main problem is the problem of who owns the challenge of tropospheric ozone (the air quality community or the climate community) and so who are we simplifying the model (Figure 3) for; this is an issue that is intimately linked with the choice of metric.

2) **Do we have a process for deciding which metrics for tropospheric ozone are policy relevant?**

Section 7, I think, is a key section for this Opinion piece. The authors outline some of the metrics used in the climate science and stratospheric ozone communities (GWP and ODP) and some of those used in the tropospheric chemistry community (OFP and POCP) but the authors don't go on to highlight the problems with the GWP and ODP metrics. A discussion on the problems with these metrics would be helpful as that would help underscore the

need for a process to develop the optimal policy relevant metric(s) for tropospheric ozone. See for example, Lynch et al. (2020) and Pyle et al. (2022).

The discussion about the UN FCCC is important (not necessarily interesting) but the UN FCCC deals with emitted species only, as these emissions can be regulated. Should the UN FCCC also consider OH as one of the gases it "controls"? Tropospheric ozone cannot be part of emission based policy metrics because it is not an emitted species. The UN FCCC does include methane and a significant fraction of the methane GWP comes from the impacts that methane has on tropospheric ozone. If tropospheric ozone were to come under the remit of UN FCCC then the fraction of GWP that is attributable to tropospheric ozone formation from methane would have to be removed. This would create a huge issue in terms of recent work that targets methane mitigation as a priority as the GWP-100 of methane would drop by about ¼. Again, a discussion of the impacts of the choice of policy metric would really help the community rally around a process to identify the right one(s).

Figure 2 highlights the alarming issue we have with metrics for tropospheric ozone. By my counting there are at least 4 different metrics being displayed. I think that an Opinion piece such as this should touch on this important aspect and draw on the literature which has discussed the choice of metrics at length. Through analysis of this literature it rapidly becomes evident that part of the problem with creating a "simple" model for tropospheric ozone is that the stakeholders for the impacts of tropospheric ozone are diverse and each want different things. A key and related aspect is which policy makers are the metrics being targeted at? Policy is a wide ranging world and many different tropospheric ozone metrics could be identified for different policy issues. This relates to my point about who owns the challenge of tropospheric ozone above.

Minor points:
L94: I suggest you delete the word "Interestingly" and let the reader make up their mind.
L115: The heading seems incomplete or at least it does to me. Delete "the" or add more words.
L129: I'm sure there are others but with my UK-centric hat on I would suggest you add AQEG to this list who have done fantastic work on tropospheric ozone for decades.
L183: See major comments above.
Figure 4: Methane emissions should top out at about 500 Tg/yr. Please check panel (a). The use of NMVOC and AVOC is confusing. Can you be consistent and define what you mean here. Also, please check the units for panels (b)-(e). Should there not be an area dimension?
L240: Fragment. Re-word.
L242: Replace the comma with a semi-colon or re-phrase the sentence here.
L255: Add "e.g.," to the reference as this was not the first study to point this out.
L260&266: What do the authors mean by "ozone air quality" and "air quality for ozone"?

References:
Lynch, J., Cain, M., Pierrehumbert, R. and Allen, M., 2020. Demonstrating GWP*: a means of reporting warming-equivalent emissions that captures the contrasting impacts of short-and long-lived climate pollutants. Environmental Research Letters, 15(4), p.044023.

Pyle, J.A., Keeble, J., Abraham, N.L., Chipperfield, M.P. and Griffiths, P.T., 2022. Integrated ozone depletion as a metric for ozone recovery. Nature, 608(7924), pp.719-723.