Comment on acp-2021-375
Anonymous Referee #1

Referee comment on "Opinion: The Germicidal Effect of Ambient Air (Open Air Factor) Revisited" by R. Anthony Cox et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-375-RC1, 2021

The Opinion article by Cox et al. is a valuable addition to the atmospheric chemistry scientific literature. I fully support its acceptance to ACP.

In particular, the goal of the article is to (re)introduce our community to observations and experiments conducted roughly 50 years ago on the “Open Air Factor” of the atmosphere, which refers to the germicidal properties of ambient air. The motivation for the publication of this Opinion is the ongoing pandemic. However, the article illustrates more broadly the impacts that atmospheric composition has on the viability of biological life forms that are exposed to the air. Although I have worked in the atmospheric chemistry field for roughly 35 years, I had never heard of the “Open Air Factor” and so I very much enjoyed reading the Opinion.

Specifically, this article describes research conducted on the viability of E. coli and Micrococcus albus bacteria when exposed either to ambient air or to controlled exposures of air containing ozone and alkenes. It was known at the time of these experiments that alkenes ozonolysis produces oxygenates such as Criegee intermediates, and a suggestion was made then that such species might be the molecular germicide. However, this chemistry has been explored in much more detail in the past few decades, and we now know that many oxygenates can form including peracids, multifunctional molecules containing -OH and –OOH groups, and organic peroxy radicals. The Opinion contends that highly reactive species, such as the Criegees and OH, are unlikely to be the active agents and suggests instead that these other oxygenates are more important. One potential mechanism of action, for example, is the formation of radicals from the decomposition of multifunctional oxygenates.

I have very few suggestions and corrections to offer. The Opinion is clearly written and topical, and it will likely motivate important follow-up studies. During the pandemic we have learned how hard it has been to fund interdisciplinary research of this type, i.e. which bridges the physical and biological domains. Papers like this will be valuable to encourage funding in this direction.

One suggestion: Secondary ozonides do not easily form in the gas phase, except perhaps from ozonolysis of endocylic alkenes (which apparently have strong germicidal properties). Could secondary ozonides be playing a role? Even if they are not produced in large amounts in the gas phase, they are known to form readily from multiphase
ozonolysis of adsorbed alkenes.

One correction: Line 48. The author name for the 1934 reference on olive oil ozonolysis is missing.