The study titled "*Integrating Fire-Induced Meteorological Changes into Plume Rise Modeling for Extreme Wildfire Simulations*" uses the Freitas et al. model to simulate plume rises during the Australian 2019/2020 wildfire season. The authors included fire-induced moisture releases and other fire-atmosphere feedbacks. Smoke simulations were improved when the ICON/Freitas model included radiative aerosol effects, and heat/moisture releases.

General comments:

Overall, the results of this study are interesting and are highly relevant for global/regional aerosol transport models and I believe this work will eventually be suitable for publication. The impacts of aerosol radiative effects on smoke lofting height is an especially neat result and separates this study from the existing literature. With that said, the methodology section of this manuscript could use significant improvements, and it's unclear how the Freitas model was incorporated within ICON, and how moisture fluxes and aerosols radiative fluxes are implemented, and the order in which these processes are calculated. A lot of these details need to be clarified in the methodology before this manuscript can be considered ready for publication.

The introduction felt a bit "long-winded," and I wonder if there's text that could be omitted. The authors may want to take a closer look at the content between lines 75–115 to see whether some material could be trimmed or written in a more concise and consolidated way.

For the methodology section, it was unclear how moisture fluxes and aerosol radiative effects were implemented within the Freitas et al. plume-rise model. For example, was an aerosol-radiative-effect subroutine or parameterization added directly into the Freitas model? Or is smoke first lofted by the Freitas model and then inserted into the 3D fields of the ICON model, where aerosol radiative effects within ICON further loft the smoke aerosols through "radiative lofting"? Along these lines, how were moisture fluxes incorporated into the Freitas et al. model? Were they used to modify the moisture content in the first vertical level of the Freitas model? This seems to be implied by the statement that "the moisture is added to the specific humidity tracer," but it's unclear whether this refers to ICON or Freitas. More details on how moisture fluxes were coupled with the Freitas model would be helpful. I also wonder whether Figure 1 could be used more effectively to describe the workflow and the order in which aerosol radiative effects and moisture fluxes are computed in the ICON/Freitas plume-rise framework. Figure 1 may also need to be reworked—it's a bit vague in its current form.

The abstract and introduction discuss how fire–atmosphere interactions were implemented within the ICON/Freitas plume-rise modeling framework. I believe the manuscript uses the term "fire-induced meteorological effects." However, I did not see any explanation of how fire–atmosphere interactions were included in this framework. When I think about fire–atmosphere interactions, I think of models like WRF-FIRE or WRF-SFIRE, where the fire generates a plume, and through mass continuity the near-surface winds accelerate, causing the fire to spread faster and release more energy, which then further strengthens the plume-rise updraft. I suspect many fire modelers interpret "fire–atmosphere interactions" in this way. If these types of feedbacks are not included in the ICON/Freitas framework, the authors may want to revise the abstract to more clearly define what is meant by fire–atmosphere or meteorological interactions.

The structure of the discussion section could also be improved. Some of the descriptions seem to bounce around a bit, and it may be worthwhile for the authors to give this section another pass to make it read more coherently.

Specific comments:

Line 20: While I suspect this # is not too far off, it's worth noting that these analyses were based on MISR data, which corresponds to satellite overpasses that occur during the late morning/early afternoon, when plumes are not fully developed. Granted, the PBL was also probably shallower during these times so the % above the PBL may not change a ton, but maybe just note that these overpasses occur in the late morning/early afternoon.

Line 40: Another issue, especially for the empirical-based Sofiev scheme is that it's trained off MISR data and does a poor job with extreme plume rise values, e.g., the tail of the distribution. It does better with smaller plumes, but it's usually not the small plumes we care about. The larger fires that emit orders of magnitude more smoke are generally the ones that generate the plumes that loft smoke further into the free troposphere.

Line 60: I would argue the Freitas model is computationally efficient. As a standalone model, it runs in less than 0.1 seconds on a single CPU core. ML/AI approaches could be faster, but in terms of computational time relative to the host 3D model my guess is this would not really have much of an impact on the model run time.

Line 160: replace "this limitation" with "cloud blocking", or something along these lines to make this clearer of what "this" is referring to.

Line 175: Slightly confused here, it was noted that geostationary satellites can observe the diurnal cycle, but it looks like an alternative cyclic function was used? It was unclear if this cyclic function was related to geostationary satellites observations or not.

Line 187: I am sure this OK, but why not use a mass flux approach to directly compute the smoke detrainment (see Wilmot et al. 2022)? This gets away from the parabolic emission profile assumption, which is a little "hand wavy", and uses an approach based on plume physics.

Wilmot, K., D. V. Mallia, A. G. Haller, and J. C. Lin, 2022: Wildfire plumes in the Western US are reaching greater heights and injecting more aerosols aloft under a changing climate. Scientific Reports, 12, 12400.

Line 209: Where did the 3.4 GFAS correction factor come from? It looks like this is based on Kaiser et al., so are we just assuming that since the smoke emissions are off by a factor of 3.4, this difference is related to a 3.4 difference in fuel consumption, which would be proportional to heat fluxes? Is it possible that this 3.4 might be the result of uncertainty with fire emission factors? These emission factors, especially for $PM_{2.5}$ can be quite large (see Urbanski 2014). It might be good to include some of the limitations of this assumption at the very least.

> *Urbanski, Shawn. 2014. Wildland fire emissions, carbon, and climate: Emission factors. Forest Ecology and Management. 317: 51-60.*

Line 209: In equation 6, what is the 5.5? Did you mean 0.55?

Line 310: See the general comment, but it is unclear how moisture fluxes and aerosol–radiation interactions were incorporated in ICON/Freitas, along with the order of these calculations. It is also unclear what is meant by a fire-induced heat flux? If I remember correctly, the Freitas et al. plume rise model will not run if the Heat flux is equal to 0. If the "fire induced heat flux" is 0, is the plume rise model just being forced by terrestrial radiative fluxes?

Line 345: Are heights reported in ASL or AGL? Might be good to add this when reporting heights here, i.e., 3.5 kmAGL.

Line 343: How does the fire destabilize the atmosphere in ICON? I do know that the Freitas model runs for X time steps to develop to build up the plume (I think this takes anywhere from 30-55 model minutes)? Is ICON reading output from each time step from the Freitas model and using that to modify the 3D weather/aerosol fields within ICON? Or are the emissions only released vertically once the plume has reached steady state? Along these lines, is the Freitas model one-way or two-way coupled with ICON, both in terms of meteorology/aerosols?

Line 424: The plume is rising-- is this after the smoke has been vertically lofted by the Freitas model, i.e., has the smoke been carried upward by the Freitas model, vertically distributed within the ICON vertical column, and then lofted up further due the aerosol radiative heating that is resolved within ICON?

Line 429-434: Near-surface aerosol layers promote stability by warming low/mid-level layers, which makes sense. But I am not entirely following the upper-level description? If the smoke is lofted further up, would there still be warming aloft and therefore a stable layer, which is just higher up in the atmosphere?

Line 455: Okay, I think this loosely answers my earlier question about the order of operations for the aerosol radiative feedbacks. However, this really needs to be explained more clearly in the methods section. I strongly recommend redesigning Figure 1 so it clarifies the sequence of steps: the heat-flux/emission calculation -> the Freitas model simulation -> injection of smoke into ICON -> the physics that ICON resolves once the smoke fields have been added. It seems that the same workflow applies to moisture as well. While this order makes sense for aerosol radiative feedbacks, I wonder whether releasing moisture after the plume-rise calculation is a potential limitation? Moisture fluxes could be relevant for the plume-rise calculation itself. For example, if the lower boundary conditions in the Freitas model contained more moisture, the rising plume would reach the lifting-condensation level sooner, allowing latent-heat release to occur at a lower altitude. That additional buoyancy could enable the plume to rise higher. Coupled fire–atmosphere simulations have explored this effect, and although some preliminary results suggest that moisture fluxes may not matter much for plume development, I still wonder if adding moisture only after the plume-rise step is an oversimplification. This is, of course, assuming I'm interpreting the order of operations in the Freitas/ICON framework correctly.

Line 470: Does ICON resolve SOA formation? Might be good to note this somewhere (maybe the methods, unless I missed this). Seems like there is no SOA based on the statement in line 563.

Line 535: This seems to be a common issue with plume-rise models and has been documented for the Freitas model as well. However, I'm not sure we can attribute the underestimation solely to the plume-rise model itself. The inputs to these models, namely heat flux and active fire area, are highly uncertain, and the model is very sensitive to heat-flux density. I'm not confident that we have a solid understanding of the actual heat-flux density for wildfires outside of a few field campaigns, since current satellite fire detection data is likely too coarse to definitively determine whether plume-rise models truly have a systematic underestimation problem. Maybe we are just systematically underestimating the heat flux density? This point goes beyond the scope of the paper, and satellite-derived inputs remain one of the best available option despite their limitations. Still, it may be worth noting this somewhere in the discussion or conclusion section. Along these lines, while the addition of moisture and aerosol physics within the Freitas/ICON framework appears to improve the results, a major caveat is that these enhancements may be compensating for systematic underestimation of how plume-rise inputs (heat flux and fire area) are computed. In any case, this isn't something I expect the authors to resolve—it's just a thought. I believe this idea is somewhat alluded to around line 546.