Review of “Radiative forcing due to shifting southern African fire regimes” by Tom Eames et al.

This study aims to provide an assessment of how changing fire regimes in southern African savannas affects global climate forcing. Moving beyond previous research that only examined CH_4 and N_2O emissions, the researchers evaluate the complete climate impact of prescribed early-season burning versus late-season fires by incorporating CO_2 emissions, aerosols, short-lived climate forcers, and surface albedo changes. WRF-Chem model is used as a primary tool to calculate the radiative forcing (RF) impacts due to aerosols and other short-lived climate forcers, while RF_{GHGs} are calculated separately. Their findings indicate that shifting fires earlier in the dry season generally produces a cooling effect (negative radiative forcing) of approximately -0.001 to -0.006 Wm^{-2}, with CO_2 emission reductions and albedo effects being major contributors. Conversely, late season burning tends to create a warming effect of smaller magnitude. The research aims to inform emissions mitigation programs currently operating in Australia and expanding into Africa, while emphasizing that the climate benefits of fire regime modifications must be evaluated carefully at local scales due to significant regional variations in effectiveness.

The study claims to be a novel effort in quantifying the impact of fire regime changes on Earth’s radiative forcing, which is valuable to the scientific community and within the scope of ACP. However, some major clarifications are required regarding set-up of modeling experiments and calculations of Radiative Forcing (RF).

Major Comments:

1. Further clarifications are required for the need to present the RF estimates as a global average rather than a regional average as calculated by WRF-Chem over the southern African domain. The projection or extrapolation of regional RFs to global scale could have been a section in addition to presenting regional mean estimates and distribution over the domain (which one would hope to have higher and more significant magnitudes). If indeed global estimates were the intended outcome, why use WRF-Chem as the tool for such a study and not use a global model rather to avoid the additional uncertainties in projection/extrapolation?

2. In section 2.1, authors say, “In this paper we only address changes to global RF because of shifts in southern African fire patterns, and all numbers presented should be interpreted in this context. We do not concern ourselves with the absolute forcing as a result of tropical fires as a whole, but rather the difference relative to a pre-defined baseline scenario.” Is the baseline scenario one of the WRF-Chem experiments? If so, please include or call it out in Table 1. Further details and

equations are required throughout section 2.3 and 2.4 to explain how RF of each component (aerosols, albedo change etc.) are extrapolated to a global scale and made commensurate with RF_{GHG} when presented together in Figure 8?

3. The model experimental set-up is confusing in terms of the choice of initial and boundary conditions. In Section 2.3.2, it says that Gas, NMVOC and aerosol initial and boundary conditions were adapted from CAM-Chem (Buchholz et al., 2019). When repeating model experiments for each year after 2019 for the seven-month period, were the "Gas and NMVOCs" (including CH4, N2O, and NMVOCs) initial and boundary conditions reset to 2019 values at the start of April every year? Please explain how the continuity of gas concentrations is maintained for the 20 years following the baseline year of 2019 to be able to calculate reasonable RF_{GHG} changes?

4. The AOD comparisons of WRF-Chem to MODIS and AERONET both show a substantial overestimation of model AOD during June month, which is also the peak of your EDS (Fig. 2a). Please add a discussion on how this uncertainty should impact the magnitudes of your results for RF changes due to aerosols.

5. Section 4.2 is long-winded and hard to follow. Consider making it concise labeling the sub-sections as well. More importantly, "location matters" subsection within 4.2 highlights the criticality of location of burning versus timing of burning and that the climate benefits of fire regime modifications must be evaluated carefully at local scales due to significant regional variations in effectiveness. Therefore, it would be valuable to complement this discussion with spatial maps of the region showing the magnitudes of RF changes for each component (GHG, aerosol and surface albedo) rather than just detailing in words that is hard to visualize and follow.

Specific Comments:

- Line 29-31: "The optical properties of pyrogenic aerosols, secondary aerosol effects (such as aerosol-cloud interactions....". There are specific terms describing these effects, called the "direct, indirect and semi-direct effects of aerosols". It could also be rephrased as Aerosol-radiation interactions (ARI) and Aerosol-cloud interactions (ACI) to be consistent with IPCC. Consider revising this sentence to include either of these set of terms to define the effects of aerosols. Moreover, please include suitable references that studied ARI and ACI, especially over the region of interest. The phrase "...well documented climate warming effect from (pyrogenic) GHGs" prior to only GHGs makes it sound like aerosol effects aren't documented, so consider dropping this phrase completely.

- Line 36-37: "…aerosol-climate interactions are less certain than those attributable to GHGs". Consider adding a sentence or two to explain why so?
- Line 44-46: Emission proportions are also heavily dependent on "type of burning" (i.e., flaming versus smoldering). Add that to this sentence with suitable refernces.
- Lie 264-65: Why are the other BB emissions from GFED4 while BA is from GFED5?

Editorial Comments:

Line 51: remove "e.g."

Fig. 2a: There is no labeling of the y-axis. Please add that.

Fig. 2b: Suggest changing the three different colored blue lines to "distinct" colors (e.g., RGB) for better legibility.