RESPONSE TO REVIEWERS COMMENTS

"The Community Fire Behavior Model for coupled fire-atmosphere modeling: Implementation in the Unified Forecast System" by Pedro A. Jiménez y Muñoz, Maria Frediani, Masih Eghdami, Daniel Rosen, Michael Kavulich, and Timothy W. Juliano.

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Reviewer #2

General Comments:

The paper presents the development and implementation of the framework that allows coupling between the surface fire model and the Unified Forecasting System (UFS). The proposed framework/coupler is presented as designed to enable coupling with various atmospheric models using the Earth System Modeling Framework (ESMF). The fire model itself is generally a copy of the WRF-Fire code packaged so that it can be run in standalone mode or coupled with atmospheric models like the Unified Forecast System (UFS). It includes features such as a dynamic fuel moisture content model, fire propagation based on the level set method driven by terrain and wind conditions, simplified fuel consumption, and emission modules that mimic the existing capability of the fire code in WRF.

The paper describes the model's structure, its coupling with UFS, and the results of simulations of the Cameron Peak Fire for cases initialized from a point ignition and fire perimeter.

RESPONSE

We would like to thank this reviewer for the time she/he devoted to revise the manuscript. We have reproduced below each of the comments raised by this reviewer followed by a detailed answer indicating how we have changed the manuscript to accommodate the comment. Our main purpose with this contribution is to 1) present the model, the first fire model designed for interoperability with atmospheric dynamical cores (via the ESMF libraries, a standard approach for coupling Earth system components), and 2) to show the adequacy of our implementation by comparing WRF-Fire and the UFS coupled to the Community Fire Behavior model (CFBM). This last aspect is possible because we purposely follow WRF-Fire methods in our current implementation. We would not call CFBM a copy of WRF-Fire, we see it as a new model and we have highlighted why in this revision. We do, on purpose, rely on WRF–Fire methods, in this current version, to evaluate the adequacy of our implementation by comparing WRF-Fire and UFS-CFBM results. The WRF-Fire methods are mostly confined to the physics directory in Figure 1. The rest of the directories have mostly new code which is necessary to create the CFBM and make it available as a NUOPC component for coupling with other Earth system components. Also, most of the WRF-Fire methods are implemented as parameterization that can be easily changed, and we aim to do so in future versions of the model.

COMMENT 1

At a fundamental level, the paper does not appear to meet the publication standards of GMD. The fire model presented is essentially a refactored version of an existing code, which replicates its shortcomings and issues without introducing substantial novelty. Additionally, the description of the fire model's structure lacks clarity, *which would hinder community-based development. It is crucial to clearly separate the fundamental processes involved, such as the representation of the fire front and its propagation, fuel consumption, emissions, fuel moisture, and rate of spread. Unfortunately, these aspects are not adequately addressed in the paper, undermining its potential as a foundation for a community model.*

RESPONSE

The work in this paper provides novel functionality not seen in the previous WRF-Fire model: we introduce an agnostic coupling layer, based on ESMF, that allows for a variety of atmospheric coupling capabilities including and not limited to UFS coupling, which currently includes the FV3 dynamical core and CCPP physics package seen in this paper. We indicate this in the abstract "*we have created, for the first time, a fire behavior model that can be connected to other atmospheric models without the need of developing specific low-level procedures for the particular atmospheric model being used*". The CFBM is the first fire model designed for interoperability with atmospheric dynamical cores. We would now downplay the value of this achievement.

Section 2.1 provides the information mentioned by the reviewer. We start this section with a brief introduction to the main concepts of CFBM. Then we describe the fire ignition, how it is represented, and how it is propagated. During this description we describe how the fuel is represented and how the rate of spread is calculated. We also describe how the fuel is consumed and the feedbacks to the atmosphere which include the emission of smoke. Hence, we describe all the aspects mentioned by the reviewer. We have carefully reviewed the section again and we have clarified the more obscure parts. To facilitate isolating the description of the fire model, we have also moved the description of the offline coupling with the atmosphere to the new section 2.2 (see answer to next comment). We believe all the relevant information is there, and readers should be able to follow the methods used to represent the fire and its evolution, but if the reviewer still thinks we should clarify any particular aspect we will be happy to do so.

COMMENT 2

The description of the modeling system is challenging to follow and often appears disorganized. A more structured explanation, beginning with a high-level overview and then detailing specific components, would greatly assist the reader in understanding the overall architecture of the proposed system, including the coupling strategies. Additionally, a fundamental diagram illustrating the relationships between key components such as UFS, ESMF, NUOPC, CFBM, and the data flow in UFS, CFB, UFS-1way, and UFS-2way would be beneficial. The current use of diagrams needs improvement, as they do not effectively clarify the presented developments.

RESPONSE

We have restructured Section 2 to better describe the fire model (see answer to previous comment), and the coupling to the atmosphere. To this end Section 2.2 is now called "Coupling with the atmosphere" and includes subsections 2.2.1 and 2.2.2 that describe the offline and online couplings, respectively. We have also moved some part of the discussion to Section 2.3 dealing with "Compilation and testing". We have also clarified our explanations. The first 3 Figures of the manuscript show the structure of the code and the data flow between UFS and the fire model. We have enlarged our explanations of the NUOPC which is essentially a lightweight file, the cap, to connect models. NUOPC is part of ESMF. We have clarified these aspects in the reformulated Section 2. We believe our description should be much easier to follow now.

COMMENT 3

Furthermore, the file system structure is incomplete; for example, it does not specify the location of configuration files. The coupling diagram also fails to indicate the data flow for the experiments presented. It is unclear how the one-way coupled simulations are performed and which components are used in their execution.

RESPONSE

The only configuration files needed to run a simulation are the Geogrid file and the namelist. There are examples of the namelists in the testing directory (e.g., tests/test7/namelist.fire). This information was missing in the previous version of the manuscript. We have added the following to clarify this aspect: "*Besides the Geogrid file, the only other file needed to run the model is the namelist (i.e., namelist.fire). Examples of the namelist are provided in the tests directory, inside the directories for each test.*"

We now provide a better description of the data flow in the coupling diagrams (see more detailed information below).

With respect to our unclear explanations regarding the one way coupled simulations, we have clarified this in our extended and rearranged Section 2. More specifically, Section 2.2.1 describes the offline model that reads the WRF output to drive the CFBM, and Section 2.2.3 describes our wrapper to create the WRF-Data NUOPC component and how we test the NUOPC implementation. Please, refer to the answer to the previous comment as well.

COMMENT 4

The language in the document requires some revisions. There are inconsistencies in the tense used throughout the writing, and many sentences could be rephrased for clarity. Additionally, shifting the focus from "we developed..." to "the module... has been developed..." would improve readability.

RESPONSE

We have revisited the language and grammar with especial emphasis from one of us that has English as the first language. We believe the language should be appropriate for publication standards.

COMMENT 5

At the technical level, the paper faces several significant issues, including a flawed design in its numerical experiments, a lack of scientific rigor in validation and coupling strategies, and insufficient detail for replication. Critical elements, such as domain sizes and placement, are omitted. Additionally, the plots depicting fire simulations are missing scales and axis labels.

RESPONSE

We have revisited our configuration of UFS and WRF to have a very similar set up (same set of parameterizations, number of vertical layers, and time step), and we have included quantifications in the comparison of model results (see details below), improved most of the figures, and provided the sizes and locations of the domains to facilitate replication of results. We believe the experimental set up and the results, including quantifications now, are sufficient to illustrate the consistency of WRF-Fire and UFS-CFBM models which is our main purpose in this manuscript. Our main emphasis in model intercomparison was not clear enough in our previous version of the manuscript, and thus our explanations of this aspect have been improved in the new version of the manuscript.

COMMENT 6

Although UFS and WRF are different models, the parameterizations used in UFS are essentially a subset of those available in WRF. Therefore, it is important to focus on creating a comparable setup. Key aspects of the simulation, such as the time step and the number of vertical levels, could have been adjusted in WRF to align more closely with UFS, which would help reduce inconsistencies between the model configurations.

RESPONSE

We have revisited our experimental set up and we now have the same set of parameterizations, time step, and number of vertical levels. This has helped to produce results from both models, UFS and WRF, that are in better agreement with each other.

COMMENT 7

More attention must be paid to the ignition strategy. The issue of integrating fire observations into coupled fire-atmosphere models has been studied, and there are existing solutions that enable both smooth ignition and selective fire activation see for example Kochansky et al. 2023 (https://doi.org/10.3389/ffgc.2023.1203578). The instantaneous ignition method used in this study is shown to negatively impact model stability and although widely used for uncoupled models is not optimal for coupled simulations. Another issue is the fuel moisture. It remains unclear how it was preconditioned and why the fuel moisture model implementation was not evaluated.

RESPONSE

We use the same fire initialization procedures in UFS-CFBM and WRF-Fire to check for consistency between results of both fire behavior models. The procedure to initialize the fire from a perimeter is the only option available in WRF-Fire. This methodology has been successfully used in previous research and thus supported by publications (e.g., DeCastro et al. 2022; Turney et al. 2023). We understand there are other initialization methods that can be explored in future versions of the CFBM (e.g., Kochanski et al. 2023) where our intention will be to go beyond current WRF-Fire methods. We clarified these aspects in the new version of the manuscript when describing the fire initialization methods: "*This fire-perimeter initialization method is the only available in WRF-Fire and has been successfully used in previous research with the WRF-Fire model (DeCastro et al. 2022; Turney et al. 2023). More sophisticated methods (Kochanski et al. 2023) could be explored in future versions of the model that will go beyond WRF-Fire methods.*"

Note that the simulations we present in the paper starting from an observed perimeter are one-way simulations in order to compare with the simulation we performed with the standalone version of CFBM that reads WRF data offline. The fire and atmospheric dynamics do not play a big role here. This experiment aims to check for the consistency of UFS and WRF-Fire when starting from a given fire perimeter, and our results support the consistency between the models.

We set the FMC constant to 8%. This is the default in WRF-Fire. We did not activate the FMC model in our simulations because it is not activated by default in WRF-Fire. We do activate it in one of our consistency checks in the test directory which confirms the adequacy of our implementation of the FMC model. We added the following: "*The FMC model was not activated in the experiments since this is the default option in WRF-Fire. Instead, FMC was set to its default constant value, 8%.*" and: "*Currently, there are two tests implemented with and without activating the FMC model"*.

COMMENT 8

The issue of horizontal resolution is significant. A resolution of 3 km is inadequate for testing a coupled fire-atmosphere model. This level of resolution causes a dilution of the heat flux across an atmospheric box of that size, leading to under-resolved buoyancy and compromising the two-way coupling effect. Surprisingly, the authors did not reevaluate their choice of horizontal resolution after observing virtually no impact of fire feedback on the atmosphere. For proper validation of a two-way coupled model, it would be beneficial to use in-situ wind observations or airborne vertical velocity observations and plume top height data. Alternatively, model-to-model comparisons could be utilized, provided that the benchmark model is appropriately configured to resolve the processes involved in fire-atmosphere coupling. To effectively resolve fire-atmosphere interactions, coupled models like WRF-Fire are typically run at resolutions of hundreds of meters. For example, WRF-Fire in COFPS operates at approximately 111 m resolution. Consequently, a 3 km resolution is insufficient. If the UFS (Unified Forecast System) does not support a higher resolution, it becomes essential for model developers to create a coupler that can bridge the resolution gap necessary for a coupled fire-atmosphere model. Regarding observational data for model validation, utilizing datasets from experimental campaigns such as FireFlux, CALFIDE, or FIREX-AQ is recommended.

RESPONSE

We now use 1 km grid spacing in our simulations which is about the maximum grid spacing you can use with UFS at this time. We still see relatively small impacts as a result of activating the feedback from the fire to the atmosphere. However, the impacts of UFS with CFBM are consistent with WRF-Fire (Figs. 9-13) which is the main emphasis in this manuscript. We added a new figure showing impacts in the vertical velocity (Fig. 14), and again we see consistency between the models. We believe it is OK to show a small impact if both models, UFS and WRF, agree, and this is the case.

This is a model to model inter-comparison work and thus our emphasis is not on comparing results against observations, besides the comparison against observed perimeters. Comparing against observations is a logical next step for CFBM now that we are confident in the adequacy of our implementation.

COMMENT 9

One of the most alarming practices highlighted in the paper is the decision to manipulate the reference height solely to ensure consistency in the simulations. There is no scientific justification for this adjustment. The authors should consider the potential impact this decision may have on future model results, particularly when the wind overestimation in the Unified Forecast System (UFS) is resolved, or in instances where the UFS model accurately captures wind patterns. In such cases, this adjustment could lead to unrealistically low rates of spread (ROS) for fires.

RESPONSE

We have resolved the wind inconsistencies. It was related to the roughness length which was different in WRF and UFS due to the use of different land surface models. We now use the same parameterizations which resolved the issue. See the updated Figures 7 and 9 and their related descriptions.

COMMENT 10

Additionally, the rationale for interpolating winds from arbitrary levels to the fire wind height is also unconvincing. The Rothermel model was developed based on laboratory data where the prescribed wind was situated close to the fireline. The experimental data relied on handheld devices deployed at the surface, which were relatively close to the fireline -certainly closer than the described model's horizontal resolution of 3 km. This coupling strategy is integral to WRF-Fire, and altering such a critical component of the model without proper scientific justification is unacceptable. Fire-atmosphere interactions lead to low-level jets, resulting from the inflow into the base of the pyroconvective column, driven by the heat of the fire. This phenomenon is documented, for example, in Benik et al. (2023) https://doi.org/10.3390/fire6090332. Raising the reference height above the layer influenced by the fire contradicts the core principle of coupled fire-atmosphere modeling.

RESPONSE

This wind interpolation option is from WRF-Fire. We did not change it. Note we did not activate this option in the experiments we presented in the manuscript as described in the experimental set up (Section 4.1).

COMMENT 11

Another significant shortcoming is the lack of quantitative assessment in the validation section. The paper contains many unsubstantiated and generic statements, such as "The results show consistency between CFBM, UFS, and WRF-Fire, indicating proper implementation and potential for further development." However, the values of the heat fluxes in uncoupled simulations differ by as much as 100% (see Figure 9 at the end of the simulation). Additionally, the simulated fire perimeters are not qualitatively analyzed. It would be advisable to use the Sørensen coefficient for this analysis.

RESPONSE

We understand the comment about being more quantitative in our evaluation. We now provide quantitative comparisons of the agreement between the simulated fire perimeters. We have calculated the Sorensen coefficient and Heidke skill score and both provide virtually identical results. Figure 1 of this document (Fig. 6 in the new version of the manuscript) shows the Heidke skill score comparing the fire area from UFS-CFBM and WRF-Fire, or the offline CFBM against WRF-Fire. The Heidke skill score is always larger than 0.80 which is large.

We now provide quantitative information regarding the wind speeds. The correlation, bias, and mean absolute error between the wind speed from UFS and WRF is 0.69, -0.3 m/s, and 0.5 m/s, respectively. For the comparison between WRF-1way and CFB-1way the MBE and MAE are 0.1 m/s and 0.2 m/s, respectively; and the correlation is 0.87.

The agreement between the simulated series of the fluxes from WRF and UFS has improved after using the same set of parameterizations, time step and number of vertical layers. We do not see very large spikes now. We also provide quantitative information of the simulated fluxes from the fire. For example, the correlation, bias, and mean absolute error between the heat flux from UFS and WRF is 0.77, 0.15 10^{-4} W/m^2, and 1.3 10^{-4} W/m^2, respectively. The correlation, bias, and mean absolute error between the moisture flux from UFS and WRF is 0.77, 0.01 10^{-4} W/m^2, and 0.12 10^{-4} W/m^2, respectively. The correlation is high and the bias and mean absolute error are small compared to the values of the fluxes (see new Fig. 10). Note that the peaks do not need to perfectly align because UFS and WRF use different dynamical cores and the atmosphere is nonlinear which affects the winds and the fire evolution. However, we would expect to see good resemblances in the lower frequency evolution of the fire fluxes from both models, and that is what we see in the new version of the figure (correlation 0.77) with the time series of the fluxes (Fig. 10).

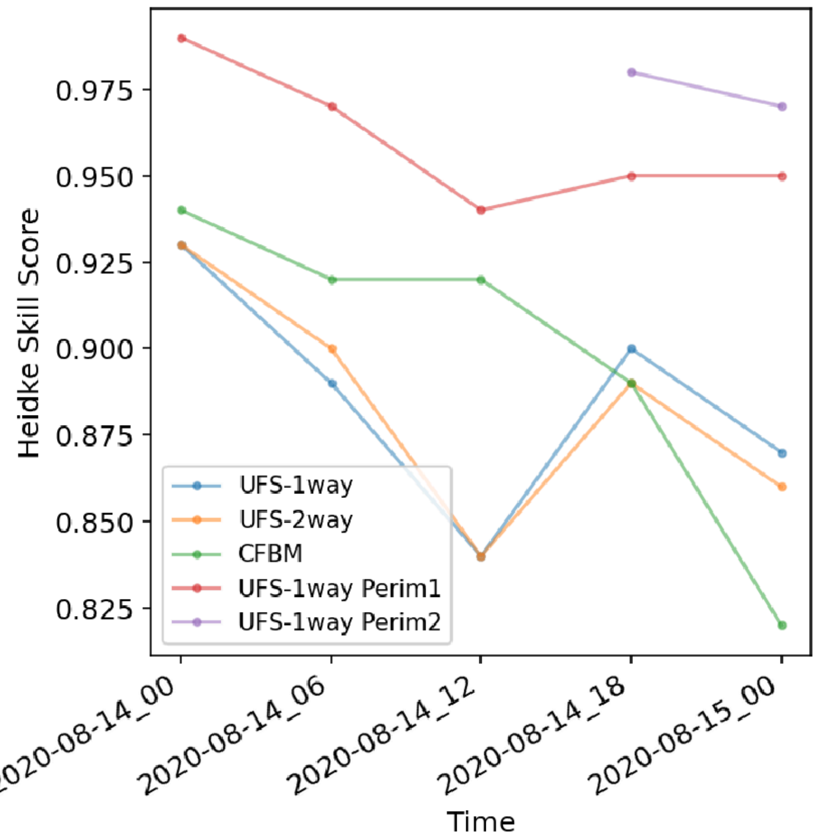

Figure 1: Heidke skill score comparing the simulated perimeters from UFS to the WRF-Fire results. The simulation labelled as CFBM is the standalone run driven by WRF data and has been compared against WRF-Fire results

COMMENT 12

The authors assert that "The CFBM is expected to facilitate joint developments and improve the accuracy of wildland fire simulations, ultimately contributing to better fire management and mitigation strategies." However, the results indicate very poor model performance, and using the uncoupled model still requires WRF and WPS, as the processing tools for the fire data have not yet been developed. This dependency complicates the creation of a community model, given the significant effort needed to build the libraries and set up WRF to generate the WPS.

RESPONSE

The sentence is just our vision. The main component we highlight in the manuscript is the coupled model. Indeed, we indicate that the standalone model was not the main goal, but it is useful to have. Note that this is a model to model intercomparison to ensure consistency of WRF-Fire and UFS-CFBM and this is what we see in our results. Improving the performance of the model is a logical extension of our work. Herein we highlight the interoperability of the fire model: CFBM is the first fire model available as an Earth system component for coupling with Earth system models via ESMF libraries which are standard for coupling models.

The CFBM relies on the WRF Preprocessing System (WPS) in this first implementation but could be decoupled from WPS. Indeed, our plans are to go beyond WPS in future versions of the model. Although WPS was originally developed for WRF, its reliability, parallelization, and flexibility to manage output variables via namelist or tables make it appropriate for using it for other models. In its simplest version WRF requires Netcdf libraries which is sufficient to compile Geogrid. WRF is also a community model with extended documentation and support. We do not anticipate the compilation of WPS being an issue. Actually, it is already being used by other models. For example, it is used by the Energy Research and Forecasting (ERF, Almgren et al. 2023), and the Model for Prediction Across Scales (MPAS, Skamarock et al. 2012). This last one, MPAS, will be incorporated into UFS and thus WPS will not make an extra requirement for the UFS-CFBM coupling.

Also, relying on WPS in this initial version of the CFBM helps to ensure consistency with WRF-Fire, a major objective in this manuscript, since both models rely on the same preprocessing system. We have clarified in the new version of the manuscript that it is expected that future versions of the CFBM will not rely on WPS: "*In the current version of CFBM, we also rely on the Geogrid program to define the fire grid and interpolate the static datasets, but, since there is no atmosphere component, only the fuels, and elevation (including slopes) in the fire grid are used. It is expected that future versions of CFBM will have its own preprocessing system.*"

COMMENT 13

Regrettably, this paper cannot be accepted for publication in its current state. While the effort to develop a coupling framework between the fire model and the Unified Forecasting System (UFS) is commendable, the work suffers from significant flaws that fundamentally undermine its scientific validity and contribution to the field.

RESPONSE

Our emphasis is to check for consistency between UFS-CFBM with WRF-Fire and we now provide an experimental set up of both models that is nearly identical. Hence, the experimental setup is sufficient for testing the consistency of UFS-CFM and WRF-Fire results. The coupling herein presented is the first time a fire behavior model has been made available as an Earth system component available for coupling in Earth system models.

COMMENT 14

The lack of sufficient innovation in the fire modeling component, poorly designed coupling strategy and numerical experiments, inadequate validation, and poorly organized presentation all contribute to a body of work that does not meet the standards required for publication. Despite the potential value of a robust community fire behavior model and coupling framework, this paper does not provide the necessary advancements or rigor to support such an outcome.

This recommendation is not given lightly. Rejecting a paper is always a difficult decision, especially when the effort invested by the authors is evident. However, the issues identified are too substantial to be resolved through even major revisions and require a complete rethinking of the methodology, validation strategies, and presentation. It is hoped that with further refinement, the authors can address these challenges and eventually produce a stronger contribution to the field.

RESPONSE

The main innovation is that CFBM is the first model available as an Earth system component available for coupling with Earth system models. Not such a model exists. We would not downplay this achievement. We understand this comment as a result of our unclear explanations of the coupling that we have clarified in the new version of the manuscript. The experimental setup is much improved with nearly identical configuration of both models, UFS and WRF, and is sufficient to show the consistency of our implementation with WRF. We have improved the presentation, experimental set up to compare both models, and quantified results.

This is a model description paper and from the aims and scope of the journal website we believe we align well with among other things "*Model description papers are comprehensive descriptions of numerical models which fall within the scope of GMD.*", or "*describe model components and modules, as well as frameworks and utility tools used to build practical modelling systems, such as coupling frameworks or other software toolboxes with a geoscientific application*"; and, again, we are original with our contribution because there is not a fire behavior model available for coupling as a NUOPC component. We believe the manuscript is now suitable for publication. Please, see a more detailed description of the changes introduced in our answers to the following comments.

COMMENT 15

Specific Comments:

L4 "WRF-Fire is a state-of-the-art fire behavior model". Other models like CAWFE or NesoNH-ForeFire offer a similar fictionality. It is unclear what specifically makes WRF-Fire a leading model in this field as suggested by the authors. There are other models built upon WRF that provide a higher level of coupling, including chemistry, as well as more advanced fire parameterizations.

RESPONSE

We now say the "widely used" model.

COMMENT 16

L76, L82, 85 The absence of perimeter processing functionality and reliance on WPS are significant shortcomings of a model aspiring to become a community fire model.

RESPONSE

The perimeter processing is the same as WRF-Fire. This methodology has been successfully used in previous research and thus supported by publications (e.g., DeCastro et al. 2022; Turney et al. 2023). See also answer to comment 7.

Please, see our previous answer to comment 12 for the reliance on WPS.

COMMENT 17

L105 The current wind interpolation appears to be incorrect. The Rothermel model requires wind speed measurements at mid-flame height. To achieve this, the wind speed should either be interpolated to 20 feet (6.1 meters) and then adjusted using the fuel-specific reduction factors, or it should be directly interpolated to the mid-flame height based on those reduction factors.

RESPONSE

The explanations are correct, this is how it is done in WRF-Fire. WRF-Fire does not have wind adjustment factors. The user needs to impose the height for the winds that drive the fire evolution. We understand this is something that should be enhanced in the future and we are exploring this possibility (Eghdami et al. 2025). We do say now: "*In the future, we would like to alleviate this subjective choice and implement the use of wind adjustment factors to automatically identify the height that drives the fire evolution based on the fuel characteristics Eghdami et al. (2025).*"

COMMENT 18

L153 It is unclear where the 2% comes from. Emission factors available in the literature should be used instead. Additionally, the moisture content in the fuel should be accounted for when the moisture fluxes are computed.

RESPONSE

The fuel moisture content is accounted for in the moisture flux. Please, see the sentence after Equation 5. The 2% is just the default value and follows Coen (2013). We now cite this reference. The factor can be changed in the namelist.

COMMENT 19

L157 The reorganization of the code should be discussed in more detail. A diagram of the code could be used to support this argument and strengthen this point.

RESPONSE

We have a diagram illustrating the organization of the code (Figure 1). We have changed the name of the headers to better describe the three categories (the columns in Figure 1). We have also improved our description of the code in Section 2. Please see answers to comments 1, 2 and 3 above. We have also added: "*The code was divided into fire behavior physics, input/output, Earth system model state, driver code, a directory with shared modules, NUOPC coupling code, build infrastructure, and test infrastructure.*"

COMMENT 20

L163 I recommend using more formal language; instead of "facilitate understanding of what is being done," ,"improve code clarity by relating parts of the code to its physical properties and specific functions".

RESPONSE

Thanks, we have removed that portion of the text and added a brief sentence saying: "*We have also improved code clarity.*"

COMMENT 21

L172-175 it would be helpful to explain how this stand-alone code relates to the original stand-alone version of WRF-Fire .

RESPONSE

This portion of the code is now in Section 2.2.1, the offline coupling Section. After restructuring section 2, it should be clear to understand the standalone, or offline, code. The standalone model was implemented as a way to run the fire behavior model independent of any system. At this time it utilizes existing WRF data to force the fire behavior model and this allows for comparison to the WRF-Fire application, which is the baseline.

We do have in CFBM an idealized version of the code that does not need an atmospheric component and we now show results of this idealized simulations to illustrate the adequacy of our implementation (see new Section 4.2.1).

COMMENT 22

L175 Change certain to required

RESPONSE

We have changed this word as suggested.

COMMENT 23

L182-183 "testing sensitivities in model parameters and methods" consider rephrasing and providing more specific information. For example "testing model sensitivity with respect to XYZ"

RESPONSE

We now say "*testing sensitivities in model parameters and methods defined in the namelist*".

COMMENT 24

L183-184 It would be beneficial to provide more details about how the model variables are coupled, the re-mapping strategies used, and so on. The fire code in the examples presented operates at a 100 m resolution, which leads to issues with the handling of fuel and slope data. How is it ensured that the fuel properties are preserved when integrating from a 30 m resolution to a 100 m resolution?

RESPONSE

We have clarified details about how variables are coupled and the regridding process in the updated Section 2 (see also answer to comment 33).

The interpolation from the fuel/terrain grid spacing to the target 100 m grid spacing is done following methods in Geogrid. There are a number of interpolation options available in Geogrid for doing this and being WRF a community model there is ample documentation about it.

We have added the interpolation methods used to interpolate from the WRF grid to the fire grind*: "The interpolation of variables from the WRF atmospheric data into the fire grid uses a nearest-neighbour interpolation. We plan to include other interpolation methods in future versions of the model."*.

COMMENT 25

L185 "directories with the fire code", change to directories including the fire code, then modules "used to define"

RESPONSE

We now say modeling code which is the header of this column in the new Figure 1. We also say "*modules used to define*" now.

COMMENT 26

L189-190. Rephrase and clarify what "extended with WRF-Fire approaches" means

L190 Consider rephrasing to say modules supporting writing standard [...] and reading […].

L193 Consider rephrasing to say "module that supports reading of NetCDF files".

L194 Change to module defining…, initializing, updating etc.

L194 Instead of "There is also", consider rephrasing to say "the last two modules X and Z are used to decompose the domain into tiles…and …"

L200 Consider changing to "when linked to the atmospheric model using two-way coupling".

RESPONSE

We have clarified all these aspects.

COMMENT 27

L215. Are these three-dimensional winds – U,V,W or only horizontal winds U,V on the 3D grid? How are winds projected to the terrain plane?

RESPONSE

Horizontal winds, we have clarified this. Like WRF, the W component is not considered when interpolating to the fire grid.

COMMENT 28

L217 Capitalize nuopc

RESPONSE

This is the name of a directory in the code using lowercase. We prefer to use lowercase so it is the same as in the code.

COMMENT 29

Figure 1 just says nuopc which isn't very informative a higher-level diagram showing modeling components should be used here. Details about the file organization should be presented later.

RESPONSE

These are the directories in the code. We say this in the caption of Figure 1: "*Each box corresponds with a directory in the code"*. In particular, the nuopc directory contains the coupling code. We have clarified this in Figure 1 (the header of column 2 says: coupling code). The explanations of what is inside of the directories are provided in the text. Adding all this information into the figure will result in a figure heavy on text. See the new organization of Section 2. We believe it is clearer now but if not we will be happy to clarify accordingly.

COMMENT 30

L220 The description of the EXMX is not sufficient. How does it work, is it similar to the WRF Registry mechanism? More explanation is needed here.

RESPONSE

We have improved our explanations of the ESMX coupling strategy in Section 2: "*The Earth system model connecting the fire and the WRF-Data NUOPC caps that is built for testing uses ESMF or the Earth System modeling eXecutable (ESMX). The ESMF build is the standard, and requires additional code with the NUOPC driver that connects the components, and this code is available in the nuopc directory (Fig. 2). The ESMX application is a community oriented application that builds a NUOPC coupled system, the Earth system model, containing NUOPC compliant components, the CFBM and WRF-Data components in this case, without creating or maintaining the NUOPC driver. Components are added to or removed from an ESMX build through configuration files written in YAML therefore eliminating the need to write code. Hence, using the ESMX extension reduces the amount of code to build the Earth system application, and thus allows for faster developments and easier maintenance. The ESMX build includes an extra test that utilizes an ESMX Data component to prescribe meteorological forcing values. The forcing values are exported to the CFBM which then returns kinematic sensible heat flux and the kinematic latent heat fluxes to the ESMX Data component for validation. These tests, as well as all the offline tests, run nightly to ensure the CFBM and CFBM cap do not break during development. Furthermore, any system utilizing ESMX as its application can embed the CFBM component. Our fire NUOPC component has been an early adopter of the ESMX technology and both coupling strategies, ESMF and ESMX, are being tested.*"

COMMENT 31

L224 How does "cap" defines the WRF-data grid? Is there a namelist/configuration file that defines variable names, grid location and such?

RESPONSE

Both the CFBM cap and WRF-Data cap share grid information read from the Geogrid file. This has been clarified.

COMMENT 32

L226 Figure 1 lists ESMF as a part of the CFBM code, while according to the text says "The main difference is that this offline coupling requires ESMF whereas the standalone code described before does not " This is confusing. Isn't the standalone code using offline coupling? This part needs some clarification.

RESPONSE

This is a typo and should not contain the word 'offline'. We have removed this word in the new version of the manuscript.

COMMENT 33

L227 Figure 2 does not effectively illustrate the coupling mechanisms. It is important to differentiate the paths and connections between the modules in offline, online, and standalone scenarios to clarify the data flow and coupling mechanism for each case. Additionally, the coupling between ESMF and ESMX should be explained more clearly.

RESPONSE

We have clarified our explanations of Figure 2: "*The diagram shown in Figure 2 summarizes the coupling strategy between the Earth system model coupling the WRF-data NUOPC component and the CFBM NUOPC component which is used for testing purposes. NUOPC caps have been written for each model, which allows data to pass from one component to another through NUOPC connectors (arrow). NUOPC connectors are built into NUOPC and redistribute or regrid data between connected pairs of components.*". In order to better explain the offline and online coupling, Section 2 now has 2 subsections, one dealing with the offline coupling (Section 2.2.1) and another with the online coupling (Section 2.2.2). Please refer to the answer to comment 30 for the clarifications between ESMF and ESMX builds.

COMMENT 34

L239 This line is confusing, did the authors mean that only testing doesn't support MPI, but the rest of the code does support MPI?

RESPONSE

The CFBM model can be integrated into a MPI parallelized application but at this time the CFBM model runs serially on a single processor. The CFBM model includes MPI library linking for future development. We have added these clarifications to Section 2.3 that deals with the compilation.

COMMENT 35

Organizing Figured 2 and 3 as a two-panel plot to highlight the similarities and differences between the coupling strategies would be beneficial.

RESPONSE

We considered joining the figures. In the end we kept them separately because they belong to different sections of the manuscript. Figure 2 is described in Section 2 and Figure 3 in Section 3. We have added some text to describe similarities and differences. Similarities being that a single NUOPC cap written for CFBM is agnostic to the external atmosphere being coupled. Differences being that a completely different atmospheric component, whether data or not, can be coupled therefore creating an entirely different coupled Earth system model. These explanations were included in Section 3.

COMMENT 36

L243. How is the machine environment specified? What information is included there? How would users add other machines?

RESPONSE

To ensure reproducibility and ease of use the environment used to build executables and run tests can be automatically configured, via adding --env-auto to the compile script. This flag then locates a bash script that loads modules and defines environment variables for defined systems. At this time the only defined system is 'derecho'. New systems can be defined by adding a <system_name> directory containing bash configuration files. The new system must then be added to the 'auto environment' section of the compile script. We have added these explanations to Section 2.3.

COMMENT 37

L253, Where is the fire namelist? Shouldn't it be listed in Figure 1?

RESPONSE

Yes, but we do not want to go deeper in the directory structure. There are examples inside the tests directory. We now say this in the text: "*Examples of the namelists are provided in the tests directory, inside the directories for each test*."

COMMENT 39

L258-259 Is there any specific reason why the original coupling strategy from WRF with the extinction depth was abandoned? How would the model be kept stable under rapid fire growth and an instantaneous initialization of the fire from perimeters?

RESPONSE

It makes sense to release the heat from the ground fire in the first model layer, like the land-surface model fluxes. And we did not see any instabilities with this UFS implementation. It is possible that when UFS allows for a finer grid spacing we have to revisit this implementation, but should not be a concern at this moment.

COMMENT 40

L273-274. This sentence needs rephrasing. It is unclear how "WRF-Fire procedures" are related to the comparison between the results from the models.

RESPONSE

Our implementation of CFBM closely follows WRF-Fire methods as described in Section 2.1. We now point to Section 2.1 in the sentence: "*The version 0.2.0 of the CFBM herein presented closely follows WRF-Fire methods (see description in Section 2.1) in order to compare results between the CFBM NUOPC component implemented in UFS and WRF-Fire results.*"

COMMENT 41

L274. More justification is needed for selecting this specific fire for comparison. Given the poor performance of WRF-Fire in this fire event, this choice is confusing.

RESPONSE

We selected this fire because it is the largest wildfire perimeter in Colorado's history. We say this in the manuscript. Our main emphasis in this manuscript is to show consistency between WRF-Fire and UFS-CFBM and for this purpose this case is sufficient. Hence, our main emphasis is in the model intercomparison and this is indicated in the title of the section 4: "4 Model inter-comparison: …".

COMMENT 42

L278. Considering the wide selection of parameterizations in WRF it should be possible to reduce the setup differences to the dynamics core alone. Such a comparison would be much more meaningful.

RESPONSE

Yes, the new version of the manuscript now uses the same set of parameterizations in UFS and WRF.

COMMENT 43

L280 A wind-driven fire like the one described here is not the best test case for a coupled fire-atmosphere model. Under strong wind conditions, evaluating the role of fire-atmosphere coupling processes is challenging.

RESPONSE

A wind driven case is good enough for our purposes of model intercomparison. If something is wrong in the coupling of the winds from the atmosphere with the fire it should be very evident. And it is also sufficient to compare the magnitude of the fire fluxes and the impact in the atmosphere which is what we show in the manuscript (Figs 9-14).

COMMENT 44

L293. This is a significant modification. Are there any observational data supporting the need for that? Do experimental data from experiments like FireFlux and FireFlux II suggest that the wind profile before and after changed significantly due to the roughness change?

RESPONSE

The WRF roughness length changed from the first time step to the next one and then remained constant. WRF-Fire used the first roughness length only. With this change we ensure the atmosphere and the fire "see" the same roughness length all the time steps. It is a change to ensure model consistency. We now say: "*First, we update the atmospheric roughness length used by the fire model every time step since it varies between the first time step and subsequent ones.*"

COMMENT 45

How was the roughness change estimated? Was it modified in both models?

RESPONSE

Yes, both models, WRF-Fire and CFBM in standalone mode (running offline reading WRF data) use consistent roughness lengths. Please see the answer to the previous comment.

COMMENT 46

Correct the tense "we updated"

Consistency in the tense. The text is hard to follow and should be rephrased.

L302 Consider changing to "Consistency between simulations"

RESPONSE

We now say we "fixed" the Table, and indicate this is a fix in the official WRF release. We changed the old line 302 as suggested.

COMMENT 47

L305- The ignition procedure requires a more detailed description, especially regarding the coupled simulations. Sudden ignition can disrupt the interaction between fire and atmospheric processes, leading to poorer model results. Greater care must be taken to ensure that the integration of fire perimeters is conducted in a manner that keeps both the fire and atmospheric model components in sync at the start of the simulation.

RESPONSE

The simulations starting from a fire perimeter are one way simulations to compare with the standalone version of the code which only runs in one way. Results for the coupled simulations are shown in the section dealing with the point ignitions.

COMMENT 48

L320 It is puzzling why the WRF time step and the number of levels have not been simply adjusted to match those in UFS. The study would benefit from a more careful experimental design.

RESPONSE

We now use the same number of vertical layers and the same time step in both models.

COMMENT 49

L327. Why 20 minutes? This creates inconsistencies between the uncoupled WRF simulation and the new implementation. Are the wind variables averaged over the 20 minutes or are instantaneous values used?

RESPONSE

We saved WRF output, which is always instantaneous, every 20 min. We understand that having more frequent outputs should provide better agreement between WRF-Fire and our offline simulations with CFBM, but the agreement is good enough to suggest the adequacy of our implementation. For the comparison between the wind speed from WRF-1way and CFB-1way the MBE and MAE are 0.1 m/s and 0.2 m/s, respectively; and the correlation is 0.87. In anycase, our main emphasis is agreement between UFS-CFBM and WRF-Fire and in these simulations the atmospheric state is updated every time step. And again, we see consistency of the results from both models.

COMMENT 50

L330- What is the scientific basis to believe that at 3km a coupled fire-atmosphere model could successfully resolve fire-atmosphere interactions? It is over an order of magnitude coarser mesh than COFPS, or other fire studies using coupled models.

RESPONSE

We now use 1 km grid spacing which is on the limit of what can be done with UFS. Other coupled fire-atmosphere modelers have used similar or coarser grid spacings (e.g., Kochanski et al. 2019, Michael et al. 2022). The 1 km grid spacing is fine enough to test the consistency between UFS-CFBM and WRF-Fire. This is demonstrated with the consistency in the time series of the fluxes and the impacts in the atmospheric variables that albeit small are sufficient to compare WRF-Fire and UFS-CFBM (Figs. 9-14).

COMMENT 51

L335 It must be explained what the purpose of running a coupled model at such low resolution is, and what exactly the authors expected to achieve by running simulations in such a configuration.

RESPONSE

We now use 1 km grid spacing which is sufficient to test the consistency between WRF-Fire and UFS-CFBM: *"However, the impacts are sufficient to assess the consistency between WRF and UFS fire simulations which is the main objective of this experiment.*". See also answer to the previous comment.

COMMENT 52

L338 It is unclear based on what the authors made this decision. Rothermel internally uses wind speed at the mid-flame height. The midflame winds are specified using reduction factors that are used to convert winds from 6.1 (20ft winds) to the midflame height. Such changes invalidate the simulations.

RESPONSE

We closely follow WRF-Fire methods in the manuscript. WRF-Fire does not have wind adjustment factors. The user needs to impose the height for the winds that drive the fire evolution. We understand this is something that should be enhanced in the future and we are exploring this possibility (Eghdami et al. 2025). We do say now: "*In the future, we would like to alleviate this subjective choice and implement the use of wind adjustment factors to automatically identify the height that drives the fire evolution based on the fuel characteristics Eghdami et al. (2025).*"

COMMENT 53

L350. That is not true. There is no quantitative analysis so this claim is subjective. Also, the fact that the authors had to adjust the fire wind height from 5 to 2.5 meters to get there is alarming.

RESPONSE

We now have a new Figure (Figure 6, and Fig. 1 above) that shows the Heidke skill score comparing UFS against WRF-Fire. The Heidke skill score is around 0.9, and always larger than 0.8 (perfect agreement is 1.0), which suggests consistency of the simulations. We now use the same wind height in the simulations.

COMMENT 54

L351. The term "underestimate" is not appropriate in this context. The simulation is completely unrealistic, likely due to a configuration error in the WRF-Fire model, which then affects the proposed model. Additionally, without proper validation of the basic weather variables, it is impossible to determine whether the issue lies with the fire or the weather aspects of the model. Overall, it is evident that both models have failed, but this does not provide any conclusive evidence regarding the implementation of the model.

RESPONSE

We have redone the simulations at 1 km grid spacing. There is no configuration error, the simulations underestimate the fire growth. We show the observations of the perimeter as a reference since our emphasis is on the model intercomparison. Hence, we do not focus on the comparison with observations, we focus on comparing WRF-Fire and UFS-CFBM. We now provide a quantitative comparison of the simulated perimeters, the Heidke skill score (Figure 6, and Figure 1 in this document), which shows large values, around 0.9, when comparing the CFBM simulations with WRF-Fire pointing to adequate behavior of CFBM with respect to WRF-Fire.

COMMENT 55

L358 This kind of discrepancy should be minimized through careful planning of the model setup. Running a standalone Fire code with UFS forcing would better serve the purpose of this experiment.

RESPONSE

We now use a very close set up of the models with the same parameterizations, same time step, and same number of vertical levels. We have updated this description of the new Figure 7. It is not possible to run the standalone fire code using UFS at this point. The standalone code uses WRF data to compare it with WRF-Fire to ensure we are able to reproduce WRF-Fire results with CFBM.

COMMENT 56

The mean wind speed varies by as much as 1 m/s, which represents a significant difference of 20-25%.

RESPONSE

We have updated the figure comparing the wind speeds since we now use the same height in both models, and a very similar model set up. There is good agreement now, and most of the time the differences are below 1 m/s. See the quantification in the answer to comment 11. There are differences between UFS and WRF because they are different models with different dynamical cores, but we would expect an overall good agreement as shown in the figure.

COMMENT 57

On a "hot, dry, and windy day," as mentioned in L281, why are the wind speeds only around 4 m/s, which is less than 9 mph? Previously, gusts were noted to reach 71 mph.

RESPONSE

This is the wind speed near the ground, at 2.5 m above ground level. The wind speed at the first model level, around 24 m above ground level is larger than 6 m/s in both models.

COMMENT 58

The difference between the CFB and UFS-1-way models is alarmingly high, especially considering their resolution.

RESPONSE

There is better agreement in the winds in the new set of runs using 1 km grid spacing and the same parameterizations, time step, and number of vertical layers. The correlation, bias, and mean absolute error between the wind speed from CFB and UFS-1-way is 0.74, 0.4 m/s, and 0.5 m/s, respectively. Again, differences are expected because UFS and WRF use different dynamical cores.

COMMENT 59

L361 The "consistency" should be quantified. The plot suggests something opposite. There is no consistency – there is no systematic bias. The WRF winds are higher or lower than the CFB or UFS-1-way depending on the time.

RESPONSE

We have updated the figure with results from the new simulations. We now quantify the agreement between the simulations to provide a better understanding of the consistency between the models. The correlation, bias, and mean absolute error between the wind speed from UFS and WRF is 0.69, -0.3 m/s, and 0.5 m/s, respectively.

For the comparison between WRF-1way and CFB-1way the MBE and MAE are 0.1 m/s and 0.2 m/s, respectively; and the correlation is 0.87. There is sufficiently good consistency now.

COMMENT 60

L365-366. "As has been already mentioned, to obtain consistency in the simulated winds from WRF and UFS we had to use a different height for the winds driven the fire in the models". This is unacceptable, especially because after the adjustment between 8/14/ 03 to 8/14/13 UFS showed lower values than WRF. Change "driven" to "driving"

RESPONSE

We now use the same height, 2.5 m, in both models.

COMMENT 61

Figure 5. The actual wind conditions at the same altitude should be presented. Additionally, validation against observed data is necessary. If the Unified Forecast System (UFS) is unable to realistically simulate winds and requires such corrections, its suitability as a platform for a community fire model is highly questionable.

RESPONSE

We now show the winds at the same height. It is not our main focus to evaluate the realism of the simulation but the consistency between UFS-CFBM and WRF-Fire. This manuscript focuses on a model inter comparison. In the new experiments, the correlation, bias, and mean absolute error between the wind speed from UFS and WRF is 0.69, -0.3 m/s, and 0.5 m/s, respectively. This is good consistency considering UFS and WRF are different models. We now provide the quantification of the agreement between the wind speed simulated by UFS versus WRF.

COMMENT 62

Figure 6. Explain how the fire front is defined here, based on the heat flux threshold, fuel fraction, air temperature?

RESPONSE

The fire front is defined as the grid cells that are burning. We no longer have this figure in the manuscript but we have added the description in the caption of Figure 7 that also shows time series at the fire front.

COMMENT 63

L372. The consistency hasn't been quantified or analyzed yet. At that point, we only know that after creating an artificial adjustment to the wind height, the results are comparable. For example, we don't know if the fuel moisture is consistent, if the fluxes at the fire mesh are comparable, whether fuel consumption is comparable, etc.

RESPONSE

Now we use the same height to calculate the winds that drive the fire evolution. The results are comparable as well, and we now provide quantifications of the agreement between UFS-CFBM and WRF-Fire using the Heidke skill score. See new Figure 6 (Fig. 1 in this document). The skill score is around 0.9, and always larger than 0.8, being 1.0 a perfect comparison. The fuel moisture content is set to the default values, 8%, and we now say this in the new version of the manuscript. In one-way simulations (Section 4.2.2) there are no heat and moisture fluxes. These are ultimately a function of the fuel consumption and are analyzed in this section (Section 4.2.3) dealing with 2-way simulations. We have clarified the opening sentence: "*After ensuring consistency of the perimeter evolution in one-way coupled simulations, we turn our attention to the two-way coupled experiments (WRF-2way and UFS-2way) that also allow for inspecting fire fluxes and atmospheric impacts.*"

COMMENT 64

L374. The claim "Again, we see an underestimation of the observed burned area with consistency between the simulated perimeters using WRF and UFS" is not supported.

RESPONSE

The underestimation is supported with the results shown in the figure. The consistency of the perimeters is also shown in the figure and now we have the quantification of the agreement of the simulated parameters calculated with the Heidke skill score shown in Figure 6 (Fig. 1 above). We now indicate that the Heidke skill score is around 0.9.

COMMENT 65

There is no quantitative analysis to support this claim. Interestingly, the backfire rate of spread (ROS) differs significantly between the models, with the WRF model showing a more rapid backfire propagation compared to the UFS. Since the backfire ROS is essentially derived from the no-fire, no-wind ROS from the Rothermel model, one would expect them to match. These discrepancies should not be affected by the modeled winds.

RESPONSE

The backfire rate of spread is the same in WRF-Fire and CFBM. What the reviewer indicates is backfire, it is actually not backfire but the winds moving forward the fire front in this part of the perimeter.

COMMENT 66

L375-376 "This is the first evidence of a relative small impact of the heat and moisture fluxes from the fire for these experiments that use 3 km grid spacing." Unfortunately, this is also evidence that the modeling experiment wasn't carefully planned and is unsuitable for validating coupled fire-atmosphere models.

It should also say here and in a few other places "relatively" not "relative".

RESPONSE

We now use 1 km grid spacing and the experimental setup is sufficient to test the consistency between UFS-CFBM and WRF-Fire. There may be a small impact in the atmosphere but both UFS-CFBM and

WRF-Fire are consistent in the magnitude of the fire fluxes (Fig. 10) and the impacts in the atmospheric variables (Figs. 11, 12, 13 and 14).

We have changed “relative” for “relatively”, thanks for pointing this out.

COMMENT 67

L376 Rephrase “In this direction, we further show the evolution of the burnt area and the wind speeds calculated with the 1way and 2way experiments using WRF and UFS (Fig. 8)”

RESPONSE

We have improved the sentence.

COMMENT 68

L380. “The consistency is also evident in the time series of the fire heat and moisture fluxes and the smoke emissions (Fig. 9).” The plots actually show something different. The fluxes from on-way simulations differ by as much as 50% on 08/14-03, or 08/14-05. At the end of the simulation, the WRF-1way heat fluxes are twice as high as the ones from the UFS-1way run. In the two-way simulations, we see identical heat fluxes with UFS-1way and UFS-2way, which is surprising considering that on 08-14 12 the couped winds are visibly weaker than uncoupled. A careful explanation of these issues is needed.

RESPONSE

This Figure is new since we now use 1 km grid spacing, the same parameterizations, time step, and number of vertical layers. There is an overall good agreement with the evolution of the fluxes (correlation 0.77) with some alternating peaks in the models that do not necessarily coincide in time. This is ultimately related to differences in the winds which is expected considering UFS and WRF use different dynamical cores. We have clarified these aspects in the new version of the manuscript. We also provide quantification of the agreement between the times series based on the correlation, bias and mean absolute error (see answer to comment 11).

In the new simulations the fluxes are similar but not identical, like the wind speed.

COMMENT 69

If the time step was an issue, the data should be time aggregated to the same intervals. However, I’m not convinced that WRF heat fluxes are averaged in time. More explanation is needed about why the instability appears at the ignition time. See also the comment about the ignition procedure. At what rate is the ignition implemented? The ignition needs a more thorough explanation and more careful planning.

RESPONSE

We have redone the simulations using the same time step and we do not see the large spikes in UFS. There are some alternating peaks. Note that the peaks do not need to perfectly align because UFS and WRF use different dynamical cores and the atmosphere is nonlinear which affects the winds and the fire evolution. However, we would expect to see good resemblances in the lower frequency evolution of the fire fluxes from both models, and that is what we see in the new version of the figure (correlation 0.77) with the time series of the fluxes (Fig.

10). Note that these are point ignition simulations. Both models are consistent in the ignition process and we now see consistency in the fluxes (See new Fig. 10).

COMMENT 70

Figures 10 and 11. The header should be changed. It is unclear what "WRF-Fire x UFS-Fire" means. It suggests a multiplication between the two, which is misleading.

RESPONSE

Yes, we agree, we have changed the title according to this comment.

COMMENT 71

L386. If the timestep is to blame why didn't the authors run WRF with the same time step? This is again the problem of inconsistency between the model configuration that could have been avoided if the numerical experiments were carefully planned.

RESPONSE

We now use the same time steps and we have resolved the problem of the large spikes. We have removed the part describing the issues related to using different time steps.

COMMENT 72

L393. "The WRF differences are positive because WRF has a dedicated variable for the fire fluxes in the atmospheric grid and the fact that the fluxes are zero in the one-way experiment." Actually, WRF also has an integrated variable GRNHFX that should be used here to keep it consistent with UFS since it provides only aggregated fluxes. Alternatively, a new diagnostic variable for fire heat fluxes could be added to UFS.

RESPONSE

We were not clear with this sentence. We wanted to say that WRF has a dedicated variable for the fire flux but UFS does not. The fire flux is incorporated into the surface flux variable in UFS. It is not as easy as in WRF to add variables into UFS. We have clarified the sentence.

COMMENT 73

L395. This claim is not supported by the presented results. The UFS simulation, particularly in the bottom right of Figure 10, shows a hot plume downwind from the fire, which is absent in the WRF simulation. This discrepancy should be investigated further. Additionally, a comparison of the overall heat fluxes from WRF and UFS displayed as the difference (WRF - UFS) would provide more informative insights. The same applies to temperature comparisons.

RESPONSE

This figure shows the fire heat flux from WRF and the fire heat flux plus the surface heat flux from the land surface model for UFS (see answer to previous comment). The fire signal in the heat flux is only valid at the location of the fire as we indicate in the text. The discrepancies mentioned are outside of the simulated fire perimeter valid for 12 h into the simulation. The discrepancies are going to grow due to nonlinearities in the atmospheric evolution, that is why the plots at 3 h and 6 h into the simulation look better. It is true that the UFS fire heat flux is more spread across atmospheric grid cells than WRF. We have clarified the sentence. We now say "*However, at the location of the fire (Fig. 7) both models show positive differences of similar magnitude, with the UFS impact spread across more grid cells, at the three times shown.*". For the case of temperature, we will expect the impact from the fire to be in better agreement between the models at the beginning of the simulation before the nonlinear effects grow and this is why we see better agreement 3 h and 6 h into the simulation that at 12 h into the simulation.

COMMENT 74

What is the size of the domains used for testing? Why are the data shown on a coarse 21x17 mesh? Is it the native atmospheric mesh?

RESPONSE

The atmospheric grid in WRF has 629 by 599 grid points and the grid in UFS has 599 by 570 grid points. We have added this information to the manuscript. We show the atmospheric grid around a small region around the fire. We have incorporated the size of the atmospheric grid in the new version of the manuscript.

COMMENT 75

L405 The change in nomenclature is confusing. The switch from UFS-1 (one-way coupled) and two-way coupled UFS and CFB makes the plots difficult to analyze. Additionally, the lack of axes and labels in Figures 7 and 10-13 is unacceptable, especially since a figure depicting the domain configuration is missing.

RESPONSE

The nomenclature was introduced in the section with the experimental setup (Section 4.1) and the label of the experiments is shown in Table 3. We have put axis with the geolocation in the new version of Figure 7, now Figure 8, that allows one to geolocate the location of the fire. This helps with the geolocation of the new version of Figures 10-13 (now Figures 11-13) since these figures include the observed fire perimeter like in Figure 8.

COMMENT 76

L406 The sentence, "This spread is simulated accurately in the northern and eastern parts of the fire, but results in a clear overestimation towards the northeastern portion of the perimeter," needs rephrasing as it is self-contradictory. It suggests that the northeastern progression is both accurate and overestimated. The reality is that the progression does not appear to be particularly good. This is particularly concerning in light of findings from the ignition point analysis, which suggested a significant underestimation of fire growth, while now we see a notable overestimation. I recommend that the authors review their approach to representing fuel moisture. The fact that the model overpredicts the rate of spread (ROS) at 10:00 in the morning but performs considerably better at 16:30, when fuel moisture reaches its minimum, indicates that fluctuations in fuel moisture are not represented accurately. Proper conditioning and modeling of fuel moisture are critical for analyzing diurnal fire activity.

RESPONSE

We have rewritten these portions of the manuscript to describe results from the new simulations. There is a remarkable good agreement between the modeled perimeters which is quantified in terms of the Heidke skill score (New Figure 6, and Fig. 1 above). We do acknowledge limitations with respect to observations but highlight the agreement between UFS-CFBM and WRF-Fire which is our main objective here to test the adequacy of our implementation of the fire model in UFS. After ensuring consistency between the models, our next logical step is a more detailed verification with observations in future studies.

COMMENT 77

L407 The issue of representing fire activity based on infrared perimeters has been previously studied. A potential solution to this problem is discussed in: https://doi.org/10.3389/ffgc.2023.1203578. The authors should consider employing such a method to enhance the realism of their simulation.

RESPONSE

Thanks for the suggestion, we will keep it in mind for subsequent studies where we will focus on comparing CFBM with observations. Herein we focus on the model inter-comparison.

COMMENT 78

L411 The differences between UFS and CFB are significant, and it is crucial to investigate why uncoupled simulations show faster progression. Generally, fire-induced winds tend to accelerate the fire, so we would expect the opposite effect. If the variations in the frequency of inter-model communication result in such significant differences, the proposed system cannot be considered robust.

RESPONSE

The simulations starting from fire perimeters are all one-way simulations since the standalone CFB can only run in one way. We have redone the simulations with the same parameterizations, time step, and number of vertical layers, and now there is a much better agreement (Figure 14 in the revised version of the manuscript). The Heidke skill score comparing the agreement between UFS and WRF perimeters for the simulations starting from an observed fire perimeter is high, around 0.95. There is consistency in the simulated perimeters.

COMMENT 79

L420 The simulated wind should be validated to provide more insight into these differences. Wind speed alone is not sufficient; wind direction is also critical. The authors should consider investigating this aspect as part of their analysis.

RESPONSE

The focus of the manuscript is model intercomparison. We now provide a quantification of the agreement of the simulated perimeters using the Heidke skill score. A comparison with observations is our next logical step after we have confirmed the adequacy of our implementation of the fire model.

COMMENT 80

L421-423 The plots indicate the opposite: CFB systematically overestimates fire expansion compared to UFS, which also consistently overestimates fire growth along the active parts of the fire. The runs are only consistent regarding backfire propagation and propagation along the northern and northwestern flanks.

RESPONSE

We have reformulated our explanations because we now have a remarkably good agreement between the models for the simulation starting from perimeter number 2. Again, we emphasize the emphasis in the model intercomparison that was not clear enough in the previous version of the manuscript.

COMMENT 81

L439 Refer to comment L4.

RESPONSE

We now say the "widely used" model.

COMMENT 82

L446 The work presented contradicts this statement, as it highlights significant inconsistencies between WRF and CFBM. There is underestimation when ignited from a point and overestimation when ignited from a perimeter.

RESPONSE

There is consistency between the simulated perimeters and this is now quantified using the Heidke skill score (Fig. 6, and Fig. 1 above). The Heidke skill score is around 0.9 and always larger than 0.8. We have added a reference to the new figure showing the Heidke skill score to support this statement.

COMMENT 83

L448 Unfortunately, this statement lacks substantiation. Fundamentally, it is impossible to evaluate coupled models using numerical experiments that are inadequate to resolve the processes that these models intend to represent.

RESPONSE

We do see a small impact of the fire feedbacks, but the feedbacks are consistent between UFS and WRF. This is exemplified in the manuscript by the comparison of the time series of the fluxes from the fire, as well as spatial plots with the heat flux that goes into the atmosphere, impacts in temperature and in moisture (see the updated Figures 9-13 and the new Fig. 14).

COMMENT 84

L454 The fire community would greatly benefit from a more modular construction of fire models, and greater attention should be given to this aspect.

RESPONSE

CFBM is designed with modules having orthogonal code in order to facilitate maintenance and future extensions, and maximize the interoperability with different atmospheric models. We have highlighted the interoperability part in the new version of the manuscript.

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