

Response to reviewers

Manuscript: Evaluation of angular resolution of the finite volume method on the predicted accuracy of wildfire thermal radiation

We thank both reviewers for their helpful comments. The responses below describe the completed revisions, with locations identified by manuscript section, figure and table. The revision includes quantitative surface statistics, additional angular-refinement calculations, measured memory use, clearer figures.

Reviewer 2

Comment: *This manuscript presents a sensitivity study of angular resolution in the so-called finite volume method for two fire configurations. In recent years, there has been some detailed work related to radiation modelling in fire conditions, such as the work coming out of the MaCFP working group (e.g., 10.1016/j.jqsrt.2024.109177). Therefore, this is a highly relevant work and has value to the research community. A rigorous study of the required angular resolution for fires is important. However, this manuscript needs to be revised significantly before it can be accepted. The two major concerns I have for this current version of the manuscript are the fact that there are no validated reference data available against which this numerical experiment can be judged, and the authors seem to make a highly generalized claim of required angular resolution of the FVM method based on just two cases and a couple of line plots.*

Response: We have added a published liquid-surface radiation validation, quantitative receiving-region statistics and two further 64-16 calculations. Section 3.4 and Table 3 assess all ground faces in geometrically defined near-source and far-field regions, complementing the line profiles. The additional refinement gives near-source/far-field differences of 1.88%/2.78% in the calm case and 0.46%/2.02% in the windy case relative to 64-12.

Figure R1 under Comment 10 presents the liquid-surface radiation comparison from Sun (2022) with English labels. The published 0.3 m methanol-pool study compares WSGG predictions with experimental data and the numerical results of Ahmed and Trouvé (2021). The WSGG calculation captures the measured heat-flux scale and the reduction towards the pool rim, supporting the use of non-grey gas radiation for surface heat-transfer predictions. Sun (2022) has also been added to the Introduction and References.

Comment 1

Comment: *This work focuses on ray effects, and there have been a significant number of studies on ray effects in various configurations over the past several decades. The manuscript would benefit from a very brief background on this. Additionally, some of the references in the introduction do not really point to an original or comprehensive source. For example, on line 32, there is a reference to Nmira et al., 2020 for the non-grey spectral model. While Nmira et al. discuss the non-grey spectral model, I doubt that it can be considered a comprehensive reference to the generic non-grey models used in radiation. Some review papers or even chapters from Modest and Mazumder or Howell et al's textbooks can serve as a more comprehensive reference here. Similarly, Sun et al. (2022) is not really a comprehensive reference for DOM, FVM, or MCRT. The introductory literature review needs to be revised appropriately.*

Response: The Introduction now includes a brief explanation of ray effects and their relationship to angular sampling and spatial numerical diffusion. General descriptions of non-grey models and RTE solution methods cite the textbooks by Howell et al., Modest, and Modest and Mazumder, rather than using individual fire studies as comprehensive references. The fire-specific papers are retained in the discussion of applications. The added references include Modest and Mazumder (2022) for the general methods and Sun (2022) for LES-based pool-fire radiation and liquid-surface heat-flux validation.

Comment 2

Comment: *The authors seem to simplify all RTE solution methods to the angular decomposition approach. RTE has both spatial and angular dimensions, and the deterministic methods essentially rely on various strategies to decouple the spatial and angular components. Stochastic methods work completely differently.*

Therefore, describing everything (including Pn and MC methods) as a form of angular discretization is misleading at best. This needs to be clarified and corrected throughout the text.

Response: The Introduction now distinguishes discrete directions in DOM, angular control volumes in the FVM, basis-function expansions in spherical harmonics methods, and stochastic sampling in Monte Carlo methods. Section 2.3 separately identifies finite-volume spatial discretization and the angular formulation studied here.

Comment 3

Comment: *Line 30: "... it needs to solve by linear algebra solvers over many directions...": This is somewhat an incomplete description. For example, for spherical harmonics methods, you do not really have "directions", and it is unclear why or what role linear algebra solvers play in this assessment.*

Response: The statement about solving the RTE by linear algebra solvers “over many directions” has been removed. The Introduction now describes the computational challenge in terms of the spatial, angular, and spectral dependence of three-dimensional radiative transfer. The intensity-field count is discussed only for the present FVM implementation.

Comment 4

Comment: *Line 32: "For the angular space of the RTE, four methods..." As already mentioned in #2, Categorizing various RTE solvers as simply methods of angular space discretization is arguably misleading.*

Response: The Introduction now describes numerical approaches for solving the RTE and explains their angular representations: discrete directions in DOM, finite angular control volumes in FVM, basis-function expansions in spherical harmonics methods, and stochastic sampling in Monte Carlo methods.

Comment 5

Comment: *Line 46: "CPU time for radiation solvers scales almost linearly...": This cannot be generalized across all RTE solvers. Even if one considers that the authors are referring only to DOM/FVM, the linear scaling will likely break down in the presence of scattering media and/or reflective surfaces. This should be carefully restated. In fact, in Table 1, the runtime more than doubles between 64-4 and 64-8 cases, meaning computational cost does not scale linearly.*

Response: The revised Introduction and Conclusions relate computational cost to the number of stored intensity fields, iterative work and parallel configuration. Tables 1 and 2 report measured wall-clock times together with the actual node and process allocations, while Section 3.3 reports measured total memory.

Comment 6

Comment: *Lines 56-58: The authors point to an important point about confusing terminology used: DOM and FVM. And later in line 124, they also point to another confusion regarding FVM. For this purpose, I would recommend that they consider choosing the terminology "Finite Angle Method" or "FAM" as proposed by Modest and Mazumder (Radiative heat transfer, 4th Ed.).*

Response: We have introduced “finite angle method (FAM)” as the alternative name for the finite-volume angular formulation and cited Modest and Mazumder (2022). We retain FVM in the manuscript but define it explicitly when referring to angular discretization. Finite-volume spatial discretization in the flow solver is identified separately in Section 2.3.

Comment 7

Comment: *The authors use the phrase "fire flame" repeatedly. What do they actually mean? What is considered a "fire flame"? How is it different from "fire" or "flame"?*

Response: The redundant phrase “fire flame” has been removed. The revised text uses “flame” for the emitting structure and “fire” for the overall case or process, as appropriate.

Comment 8

Comment: *On line 77, the authors refer to CO₂, H₂O, and soot as participating media and mention that radiative properties should be obtained from HITEMP. But soot properties are not available in HITEMP.*

Response: Section 2.1 now identifies CO₂ and H₂O as the participating gases for the non-sooting cases and specifies the Johansson et al. (2011) WSGG model for their radiative properties.

Comment 9

Comment: *In Sec 2.3, more details are required for the cases. For example, what is the fuel used? What are the boundary conditions? What modelling parameters are used in the original reacting flow simulations (the authors cite Sun et al., 2024, but a very brief description of parameters used in the underlying LES work that produced the flow and composition field will be useful)? Is there soot in the domain? If so, what is the level of soot there? etc.*

Response: Section 2.3 now identifies methane and the supply conditions actually stored in the frozen snapshots: a 0.01 m s⁻¹ fuel inlet at 298.15 K for the calm case and a 0.02 kg s⁻¹ inlet at 300 K for the windy case. It gives the windy inlet power-law profile, 101325 Pa reference pressure, thermodynamic/combustion/LES models, and the main flow and radiative boundary conditions. All radiation patches have emissivity one and use the prescribed patch temperature, with black radiative boundary conditions. The actual meshes contain 3,225,600 and 3,924,072 cells; the windy domain is 150×120×100 m. These replace the inconsistent original numbers. Radiation was disabled in the original LES for both cases, as clarified in Section 2.3; these simulations supplied the thermochemical fields for the subsequent radiation-only calculations.

Comment 10

Comment: *Using frozen fields or snapshots to assess radiation models is a valid approach, and such a strategy has been applied in various other works as well. However, it is important that, for such a numerical experiment, a well-validated result is used as a reference. Are there any radiation-related data - numerical or experimental - available that can be used as a reference for this numerical experiment?*

Response: We have included the published radiation-validation comparison from Sun (2022) as Figure R1 below. That study used LES and WSGG to simulate a 0.3 m methanol pool fire and compared the radial distribution of the absolute mean radiative heat flux at the liquid surface with experimental data and the numerical results reported by Ahmed and Trouvé (2021). The WSGG prediction captures the measured heat-flux scale and its decrease towards the pool rim, with substantially closer agreement than the grey-gas approximation. This comparison provides experimental support for the non-grey gas-radiation treatment used in surface heat-transfer calculations.

The published benchmark uses a cylindrical domain 1.6 m in diameter and 3 m high, a liquid-surface elevation of 0.15 m, a methanol supply of 0.0010692 kg s⁻¹ at 338 K, and time averaging from 5 to 30 s. Figure R1 reproduces the published liquid-surface quantity as the magnitude of the time-averaged radiative heat flux. For the present prescribed methane-fire fields, Section 3.4 and Table 3 quantify angular sensitivity using the additional 64-16 calculations and near-source/far-field surface statistics.

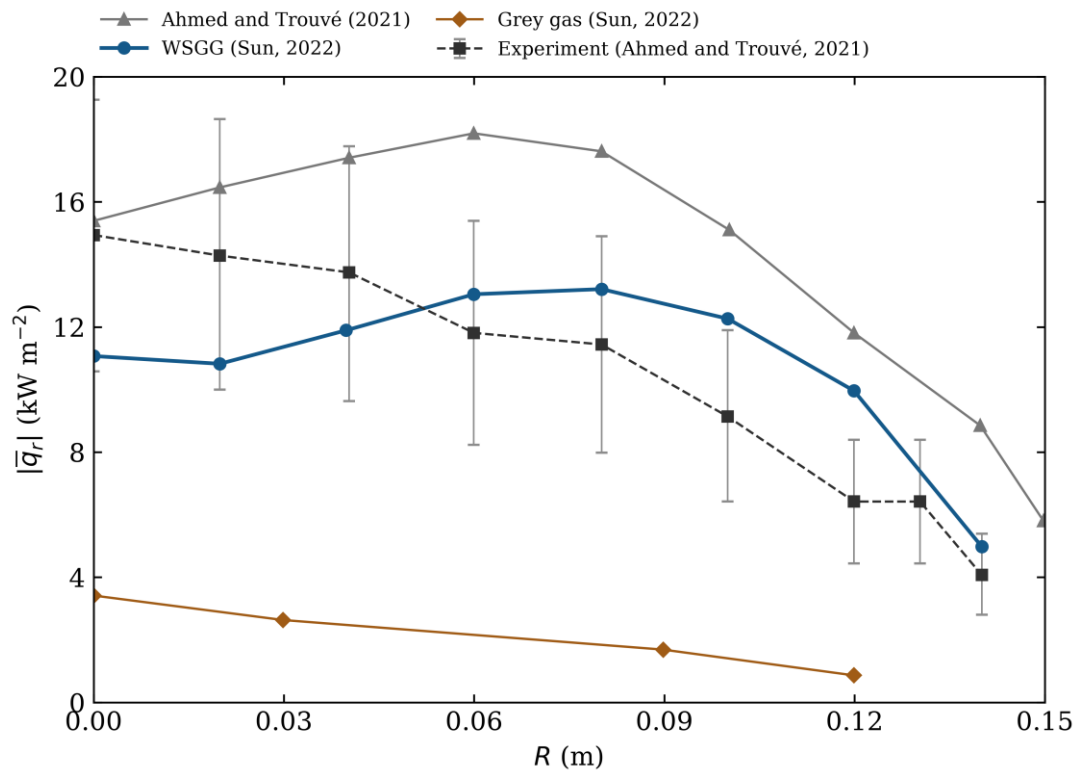


Figure R1: Radial distribution of the absolute mean radiative heat flux at the liquid surface of a 0.3 m methanol pool fire. Adapted from Fig. 9 of Sun (2022) with English labels. The WSGG and grey-gas results are from Sun (2022); the experimental comparison and the additional LES curve are reproduced there from Ahmed and Trouvé (2021). Markers and experimental error bars were digitized from the published figure.

Sun, Y.: Transient radiative heat transfer of pool fires based on large eddy simulation, China Safety Science Journal, 32(4), 65–71, <https://doi.org/10.16265/j.cnki.issn1003-3033.2022.04.010>, 2022 (in Chinese).

Ahmed, M. M. and Trouvé, A.: Large eddy simulation of the unstable flame structure and gas-to-liquid thermal feedback in a medium-scale methanol pool fire, Combust. Flame, 225, 237–254, <https://doi.org/10.1016/j.combustflame.2020.10.055>, 2021.

Comment 11

Comment: The authors discuss the temperature and velocity fields of the two cases. The velocity field has no direct relevance to the current study (and the authors sort of allude to that). The velocity field will be relevant for coupled simulations, which is beyond the scope of this work. However, the concentration fields (not just temperature) of participating media are very important, and the authors mention that concentration fields of CO₂ and H₂O are identical due to the unity Lewis number approximation. I am not convinced that, for a reacting flow, a unity Lewis number means that temperature, CO₂, H₂O, soot- everything- has the same distribution. This needs to be clarified.

Response: The velocity discussion in Sections 3.1 and 3.2 now provides concise context for the flame geometry. Section 2.3 states that temperature and the CO₂ and H₂O mass fractions are supplied as separate LES fields. This explicitly identifies the thermochemical inputs used by the radiation calculation.

Comment 12

Comment: Are the results based on one single snapshot of the flame? Or are they time-averaged?

Response: The study uses one instantaneous snapshot per case, at 60 s for the calm fire and 220 s for the windy fire. Section 2.3 now states this explicitly and explains that the radiation solver reads T and the species fields, with no averaging of the resulting radiation solutions.

Comment 13

Comment: Lines 174-175: Please mark in Fig. 5 where exactly these three different features (three local maxima?) are seen.

Response: Figure 5 marks A, B and C using the archived ground-patch field: A denotes the high-radiation region adjacent to the fuel edge, B a weak intermediate shoulder and C an outer local maximum. Section 3.1 describes these surface features consistently with the figure.

Comment 14

Comment: Lines 176-177: "These three levels of incident radiation correspond to the three levels of vortex structures in Fig. 4." How can the authors be sure that these three levels are related to the three vortex structures?

Response: Section 3.1 now describes A, B and C directly as features of the ground-surface radiation field: the fuel-edge high-value region, an intermediate shoulder and an outer local maximum. The revised interpretation is based on the surface field and the controlled angular-refinement comparisons.

Comment 15

Comment: Lines 177-178: "However, this is also physically false and caused by the ray effect in the zenith direction..." How can the authors separate which ray effect is due to zenith angles and which is due to azimuthal angles by looking at the contour on the ground surface?

Response: Section 3.1 uses controlled comparisons: 32-4, 40-4 and 64-4 vary the azimuthal count at a fixed zenith count; 64-4, 64-8 and 64-12 vary the zenith count at a fixed azimuthal count. Section 3.4 adds the 64-12 to 64-16 zenith-refinement comparison for both cases. Comparisons that vary both counts are described as their combined angular-refinement response.

Comment 16

Comment: Throughout the results section, for both cases, the authors make their point by looking at the contour plots at the ground surface and two line plots. Any quantitative assessments made on the basis of contour plots should be done with utmost care, as interpretation of colors in contour plots is very subjective. The line plots are better suited for quantitative assessment, but the location of the sampling line can influence the findings strongly. Therefore, the authors need to justify why the two sets of lines were chosen at the locations they indicated as opposed to any other locations. Furthermore, how can the authors make a generalized assessment of the radiation solution just by looking at the incident radiation at these two locations? There is no reference data as to which solution is correct. Additionally, a smooth incident radiation plot at one location does not automatically indicate the correct solution of the radiative source term inside the flame, which can have a critical role in the fire dynamics.

Response: Sections 3.1 and 3.2 explain the centreline/off-centre and near-source/farther-downstream sampling locations. Section 3.4 and Table 3 now complement those profiles with all-face statistics in fixed regions: $r \leq 0.6$ m from the calm fuel centre, and horizontal distance $d \leq 4$ m from the actual windy fuel rectangle, define the near-source regions; the remaining ground defines the far field. Fuel inlet faces are excluded. We calculate $D_A = 100 \frac{\sum_f A_f |q_f - q_{ref,f}|}{\sum_f A_f |q_{ref,f}|}$ from the original incident-radiation fields, with 64-12 as the explicitly stated comparison baseline. The same regions and physical receiving faces are used for every angular configuration.

At 32-4, the near-source/far-field differences are 9.17%/7.74% for the calm case and 6.22%/9.90% for the windy case. Increasing the zenith count from 12 to 16 at 64 azimuthal divisions changes the near-source and far-field incident radiation by 1.88% and 2.78% in the calm case, and 0.46% and 2.02% in the windy case, using the area-weighted measure relative to the archived 64-12 result. These are reported as differences relative to the selected numerical baseline. The assessment consistently uses ground-surface incident radiation. The published methanol-pool liquid-surface validation is presented in Figure R1 under Comment 10.

Comment 17

Comment: *Does the spatial resolution of the mesh used in the flow and concentration field not play any role in the ray effect? A discussion on the effect of CFD mesh size on the angular resolution requirement of FVM needs to be presented.*

Response: Section 4 now discusses the interaction between spatial and angular discretization. It explains that the spatial mesh resolves the temperature and composition structures, while numerical diffusion in the transport calculation can smooth angular patterns. Every angular comparison uses the same physical spatial mesh and frozen fields for its case, providing consistent spatial conditions for assessing angular sensitivity.

Comment 18

Comment: *Fig. 1: Please show the direction of theta and phi*

Response: Figure 1 has been redrawn with black angular arrows. The θ arrow follows a meridian from +Z, and the ϕ arrow follows the equatorial circle from +X towards +Y. Both arrows use the same spherical geometry as the grid, and the caption defines the angle conventions.

Comment 19

Comment: *Figs. 2-5, 7-9: Please show the coordinate system and spatial scales.*

Response: Figures 2–5 and 7–9 have been regenerated from the archived meshes and fields with named coordinate axes and physical scales. Figure 2 now uses orthogonal mesh sections and enlarged near-source views to resolve individual cells. Figures 3 and 7 identify the $y=0$ m and $y=40$ m planes and use equal-length arrows for flow direction, with colour indicating velocity magnitude. Figures 4 and 8 use tighter views and complete three-dimensional coordinate frames. The two-dimensional figures are exported at 600 dpi, and the volume renderings at 4800×4400 pixels. Figures 6 and 10 have also been regenerated at 600 dpi from the archived fields.

The local geometry checks also identified inconsistent domain dimensions and cell counts in Section 2.3; those values have been corrected from the actual meshes, as detailed in our response to Comment 9.

Comment 20

Comment: *Figs. 5 and 9: Please mark the locations where the sampling lines (for Figs. 6 and 10, respectively) are selected.*

Response: The 16-2 panel in Figure 5 now marks $y=0$ and $y=0.3$ m, and the 8-2 panel in Figure 9 marks $x=45$ and $x=54$ m. The captions state that the same sampling locations apply to all angular configurations. Axis directions were checked against the mesh coordinates and saved ParaView views.

Comment 21

Comment: *Line 24: What is a "false ray effect"?*

Response: The term “false ray effect” has been removed. We use “ray effect” consistently and explain it in the Introduction as a non-physical numerical artifact associated with inadequate angular representation.

Comment 22

Comment: *The discussion related to the computational cost needs to be revised for clarity. First, different cases seem to have been simulated under different parallelization. Therefore, the time presented does not allow for a one-to-one comparison of computational cost. Second, the discussion of memory requirements is confusing, and actual memory usage (in MB, GB, TB, etc.) is needed instead of how many CPUs on each node.*

Response: Tables 1 and 2 now report logged wall-clock times with the corresponding node and process counts. Section 3.3 gives the timer definition, decomposition method and solver settings. Runtime is interpreted together with the parallel allocation; for example, windy 40-6 used 64 processes and 48-8 used

128. The measured peak total memory of the two representative 64-12 cases is reported separately in physical units. Further details are given under Reviewer 1, Major Comment 4.

Measured peak total memory at 64-12 is 1324.85 GiB for the calm case and 1606.54 GiB for the windy case. This is the maximum simultaneous total memory of the solver processes, sampled every 2 s. Section 3.3 reports these values.

Comment 23

Comment: *There are several typographic and grammatical/editorial mistakes. Please review the entire manuscript to correct those. Below are some examples: (a) Line 33: There is nothing called "sphere harmonics method" - it is "spherical harmonics method" (b) Lines 27 and 39 - repeating "However" (c) Line 44: "... meshes exceeding 2000 rays ..." The authors should be careful about consistent terminology. It seems that they use direction or angles to describe DOM/FVM discretization not rays. The term "ray" is often used in Monte Carlo terminology. (d) Line 45: "high-resolution accuracy" should be "high accuracy" (e) Line 106: "chose" should be "chosen". (f) Line 144: "buoyant driven" should be "buoyancy-driven" (g) Line 177: "which has not 2 mesh" - this seems like a fragment of a sentence. Please correct this sentence. Please note that this is an incomplete list of such typographic/grammatical/editorial errors. Please review the entire manuscript thoroughly.*

Response: We have reviewed the manuscript for grammatical and editorial issues. (a) The method is called "spherical harmonics". (b) The repeated "However" constructions have been removed. (c) The numerical discussion distinguishes spatial cells, discrete directions, and angular control volumes; the ambiguous "meshes exceeding 2000 rays" phrase has been removed. (d) "High-resolution accuracy" has been removed. (e) The sentence now reads "The choice of the numbers ...". (f) "Buoyancy-driven" is used consistently. (g) The incomplete sentence has been removed and replaced by the controlled angular-refinement comparisons described in our response to Comment 15.

Figure 10 was resampled from the corresponding archived qin field, and both legends were corrected.