CSIRO PUBLISHING

International Journal of Wildland Fire **2021**, *30*, 221–223
https://doi.org/10.1071/WF20091

Response

A response to comments of Cruz *et al.* on: 'Simulation study of grass fire using a physics-based model: striving towards numerical rigour and the effect of grass height on the rate of spread'

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Keywords: Byram number, fuel height, grass fire, modes of fire propagation, rate of spread.

Received 16 June 2020, accepted 13 January 2021, published online 2 February 2021

In response to Cruz *et al.* (2020*b*) (hereafter: CSG20), we would like to clarify that we are committed to further research into the mechanisms that drive fire spread in grassland fuels, and are open to making the most of synergies that exist between various programs of research. As demonstrated by Filippi *et al.* (2013), Kochanski *et al.* (2013), Clements *et al.* (2019), etc., there is much to be gained by combining detailed experimental data with rigorous computational modelling.

CSG20 raise several concerns about the results of Moinuddin *et al.* (2018) (hereafter: MSM18), which indicate that a change in fire propagation regime (wind- *v.* plume-driven) can alter the way rate of spread (RoS) responds to changes in fuel characteristics. Here, 'plume-driven' refers to the dominance of buoyancy forces on flame behaviour, in comparison with the shear forcing of the ambient wind conditions – there is no link to pyrocumulus development. Specifically, CSG20 question the existence of two different modes of fire propagation and the finding that the RoS can decrease as grass height increases. They also question some of the stated implications of the findings for strategic decision-making. While our results show that the RoS of a plume-driven fire decreases with increasing grass height (proportional to fuel load), they also show that the intensity increases (fig. 11*a*, MSM18) with increasing grass height. Therefore, we are in no way suggesting that increased fuel load implies less of a hazard.

Central to the critique offered by CSG20 is the observation that practically all of their fires are characterised by a Froude number $Fr < 0.5$, and so all can be categorised as plume-driven fires under the threshold proposed by MSM18. However, despite the claim that they used the same methodology as MSM18, the Fr values calculated by CSG20 are not commensurate with those calculated by MSM18. Fireline intensity cannot be measured in field experiments (Alexander and Cruz 2019) and so CSG20 had to resort to computing Fr using the Byram intensity (I) correlation, which relies on estimating heat of combustion (corrected for moisture content) and assuming all fuel is consumed as the fire passes, and that I is constant over the extent of the fire line, both of which could lead to overestimated values of heat release rate (HRR). In MSM18, Fr was computed using simulated HRR that has been validated against laboratory-scale experiments (Perez-Ramirez *et al.* 2017). Overestimating I would yield systematically lower values of Fr and the proposed thresholds from simulation data would be inappropriate. In addition, CSG20 did not specify what surface temperature they used and how they obtained it. To use the same threshold, Fr must be computed consistently. To exactly match the range of Fr of MSM18, specific grassland experiments are required that 'match' the conditions of the simulations that were examined. By stating that 'It is not easy to disentangle the effect of…', CSG20 essentially state that they could not do it. Instead of trying to match Fr values, an alternative is to look for clear trends in Fr and relate them to trends in RoS as some parameter of interest changes, as done in MSM18.

We recognise that there are better Fr-like parameters. The Byram number, N_c (given as Eqn 1), is a more robust parameter in the context of wildfires, and one that has been more widely studied. N_c relies on the fire intensity (HRR per unit of fire line length), RoS, wind speed and constants such as ambient temperature. Other non-dimensional parameters that compare buoyancy with shear forces may provide better characterisations of wildfires, but this is an open question. A reanalysis of the data of

CSG20 (sourced from Cruz *et al.* 2020*a*) using N_c, which is better suited to directly incorporating Byram's intensity, indicates that their fires should mostly be characterised as wind-driven or 'mixed regime', with only 5 out of 45 fires qualifying as plume-driven, according to the thresholds given by Nelson (2015), and cited by Morvan and Frangieh (2018). The Byram number was computed as (Morvan and Frangieh 2018):

$$N_c = \frac{2gI}{\rho c_p T_a (U_{10} - RoS)^3} \quad (1)$$

where I is Byram's fireline intensity, which has been estimated using Byram's formula (Byram 1959; Cruz *et al.* 2020*a*), g is the acceleration due to gravity, $\rho = 1.225$ kg m^{-3} and $c_p = 1.0$ kJ kg^{-1} K^{-1} are the density and specific heat of air, respectively, and T_a is the ambient air temperature (assumed constant at 303 K).

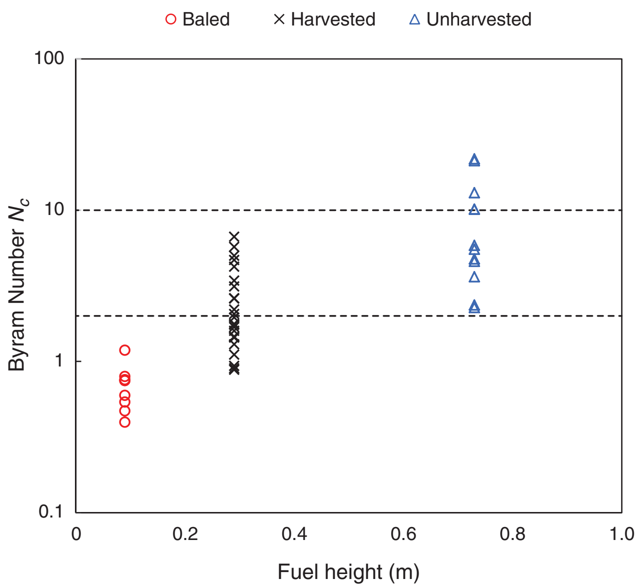

Fig. 1. Byram number (N_c) analysis of CSG20 data.

We used the 10-m wind speed U_{10}, RoS and intensity I listed in Cruz *et al.* (2020*a*; appendix 1). The N_c for each of the three states is presented in Fig. 1.

The values of N_c are consistent with the photographs presented by CSG20 (fig. 1) and Cruz *et al.* (2020*a*) (fig. 3). The flames for the baled crop fire (for which $N_c = 0.79$) are clearly lying over, suggesting wind-dominated spread. The angle of the flames for the harvested crop fire ($N_c = 1.44$) also suggests that the fire is more wind-driven, while the more upright flames in the unharvested crop fire ($N_c = 3.62$) could be interpreted as being more influenced by the buoyancy-driven flow (i.e. the plume was more influential on the fire's propagation). Based on this reanalysis, the behaviours of the fires discussed by Cruz *et al.* (2020*a*) are consistent with the simulation results reported by MSM18.

Fig. 2 combines the results of MSM18 with those obtained from additional simulations, in which the turbulence in the wind profiles was generated differently, i.e. with a combination of synthetic eddy model and surface roughness. The figure demonstrates that in the simulations, below a certain grass height (~0.24 m), RoS increases with height (and hence fuel load), consistent with the E1–E3 data of Cheney *et al.* (1993) and the crop fire data of Cruz *et al.* (2020*a*) (in which most of the fires were more wind-driven). However, it should be noted that the averaging involved in the statistical analysis of the data of Cheney *et al.* (1993) could obscure relevant dynamic effects, so the statistical analysis does not permit a direct comparison with MSM18.

CSG20 imply that the dependence of RoS on grass height is no longer a matter of debate (CSG20, p. 5). However, they state that Cheney *et al.* (1993) showed a direct and significant correlation between RoS and fuel height, while Cruz *et al.* (2015, 2018) found no such relationship. This discrepancy is at first ascribed to different grassland structures, but it is then suggested that grass height plays no role in fire spread. Taken together with the findings of MSM18, this suggests that a comprehensive understanding of the effect of grass height and fuel bed structure on RoS is still an open question with many factors.

Cruz *et al.* (2018) found an inverse relationship between fuel load and RoS, consistent with the (plume-driven) findings of

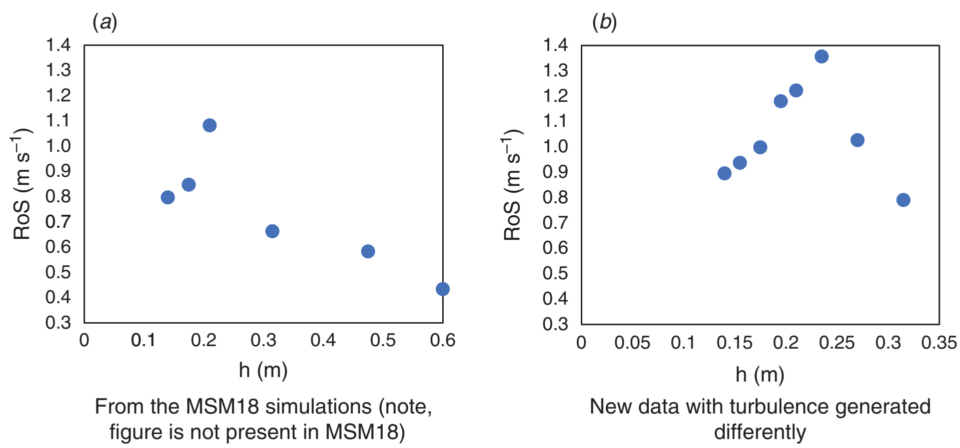

Fig. 2. RoS *v.* fuel height showing two modes of propagation.

MSM18. Therefore, without further qualification, CSG20 appears to be at odds with Cruz *et al.* (2018). This further supports the notion that the dependence of RoS on fuel height and fuel load cannot be simply characterised in general.

In their conclusion, CSG20 also level several criticisms that we believe are unfounded. They suggest that MSM18 did not mention the work of Cheney *et al.* (1993), when in fact this work forms the main foundation of the simulations, is discussed throughout MSM18 and is appropriately cited. CSG20 also noted Sneeuwjagt and Frandsen (1977) was not cited. While Sneeuwjagt and Frandsen present some useful RoS and fuel bed depth data, there are no measured intensity data, only those computed from the Byram correlation, so it is impossible to accurately determine the propagation regime for comparison with simulations. Given the small flame lengths (albeit unreliably estimated by eye, as Sneeuwjagt and Frandsen concede), these fires are likely to be wind-driven rather than representative of both modes of propagation.

We feel many of the apparent discrepancies highlighted by CSG20 can be reconciled through proper observance of the different parametric regimes that are represented in the various experimental and simulation results. There are sufficient degrees of freedom in the experimental and simulation parameters that allow apparently discrepant results to occur. To establish more robust (non-dimensional) Fr or N_c thresholds, a wider range of simulations and experiments with accurate measurement of fire intensity (with heat of combustion) and surface temperature is required, and the observational data (including the head fire spread rate, head fire depth, head fire width, fuel characteristics and georectified images) should be made publicly available, like the Wildland-Urban Interface Fire Dynamics Simulator (WFDS) source code, to facilitate detailed scientific analyses using a variety of methodologies. Sullivan (2007) states that such data exists for 120 fires. Until that is established and the data are made public, we firmly opine that two modes of fire propagation exist. Indeed, there is empirical evidence to suggest the existence and relevance of two modes of propagation in laboratory-scale experiments (Apte *et al.* 1991; Tang *et al.* 2017) and there is no reason to doubt that similar regimes exist in relation to wildland fires.

Conflicts of interest

The authors declare no conflicts of interest.

Acknowledgements

This research did not receive any specific funding.

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