**CSIRO** PUBLISHING

*International Journal of Wildland Fire* **2018**, *27*, 776–780
https://doi.org/10.1071/WF18161

**Communication**

# A response to 'Clarifying the meaning of mantras in wildland fire behaviour modelling: reply to Cruz *et al.* (2017)'

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**Abstract.** This paper represents our response to the questioning by Mell *et al.* (2018) of our interpretation (Cruz *et al.* 2017) of five generalised statements or mantras commonly repeated in the wildland fire behaviour modelling literature. We provide further clarity on key subjects and objectively point out, using examples from relevant scientific findings, that our discussion of the identified mantras presented in Cruz *et al.* (2017) was indeed not ill-conceived as suggested by Mell *et al.* (2018).

**Additional keywords:** energy transfer, model validity, physical model, rate of fire spread, wildfire propagation.

Received 19 September 2018, accepted 13 October 2018, published online 31 October 2018

We acknowledge the comments made by Mell *et al.* (2018) on our article (Cruz *et al.* 2017) and their desire to further the debate on the following mantras of wildland fire behaviour modelling:

- Mantra 1. Empirical models work well over the range of their original data (M1).
- Mantra 2. Empirical models are not appropriate for and should not be applied to conditions outside the range of the original data (M2).
- Mantra 3. Physical models provide insight into the mechanisms that drive wildland fire spread and other aspects of fire behaviour (M3).
- Mantra 4. Physical models give a better understanding of how fuel treatments modify fire behaviour (M4).
- Mantra 5. Physical models can be used to derive simplified models to predict fire behaviour operationally (M5).

The objective of Cruz *et al.* (2017) was to highlight how the necessarily incomplete science of fire behaviour modelling is conveyed in the literature and identify the tendency to repeat the above statements without necessary due diligence. It was not our intent to criticise any particular modelling approach *per se*. We believe physical-based modelling will continue to contribute to our knowledge and understanding of fire propagation processes, as has empirical fire behaviour research, including observations of laboratory fires, field experimental fires and wildfires.

In their comments on our paper, we found that Messrs Mell, Simeoni, Morvan, Hiers, Skowronski and Hadden were unable to contribute constructively to the debate raised by the mantras; instead, they provided a rather negative and misleading interpretation of our work. Mell *et al.* (2018) relied extensively on implicit language of inference of error rather than fact-based criticism of our work and inadvertently provided additional evidence for the existence of the specified mantras in their efforts to refute them.

Our aim here is not to produce a detailed point-by-point response to Mell *et al.* (2018), as this would not be in the best interest of the readership, but rather to confine our responses to only the most notable issues requiring clarification.

## The discussion regarding physical models is not flawed

Mell *et al.* (2018) state that our paper displayed a 'limited understanding of modelling approaches'. They base this claim on the argument that our definition of physical-based modelling incorporated a broad range of model types. Mell *et al.* (2018) contend that physical models should be subdivided into two groups – computational fluid dynamic (CFD) models and non-CFD ones, with the implication that CFD models represent a distinct, superior category. Mell *et al.* (2018) have missed the point that over the last 35 years, there has been an evolution in the field of physical-based modelling and CFD models are but the latest iteration of these efforts. The classification that we relied on is based on several published reviews (e.g. Weber 1991; Pastor *et al.* 2003; Sullivan 2009*a*). It is worth pointing out that Mell *et al.* (2018) failed to mention that Mell *et al.* (2007) followed exactly the same classification we used, explicitly classifying models such as those of Albini (1985) and de Mestre *et al.* (1989) as physical models.

The statement by Mell *et al.* (2018) that we 'appear to mistakenly assume that convective heat transfer was neglected because the model developers assumed it is not relevant to fire spread' is itself mistaken. We did not state that model developers assumed convective heat transfer was not relevant to fire spread, and here, as in other parts of their communication, Mell *et al.* (2018) produce a misleading statement to imply that we were in error. Most, but not all, of the published non-CFD physical modelling work referenced in Cruz *et al.* (2017) focused on radiative heat transfer as the main driver of fire spread, a fact not disputed by Mell *et al.* (2018). There might very well be various reasons for this focus, one of which is that it is clearly easier to model radiative heat transfer than convective heat transfer. In the latter situation, one would need to describe the flow field (e.g. fluid velocities, direction of flow and fluid temperatures) within and around the flame front as determined by the combustion environment and fire–wind field interactions. This was simply not feasible before the advent of CFD modelling. This does, however, not invalidate the example of incompleteness in earlier physical models.

Mell *et al.* (2018) also incorrectly interpreted our reading of the experimental results of Anderson *et al.* (2010) and Butler (2010). What Mell *et al.* (2018) misconstrued from those two studies was the important role of convective cooling in diminishing the effect of radiative heating until the fuel particles are bathed and then fully immersed in the advancing flame front. The effect of convective cooling was shown in a very simple, but compelling, experiment described by Finney *et al.* (2013) and Cohen and Finney (2014). Under free (i.e. non-forced) conditions, a strong radiative heat flux was unable to ignite a fine fuel particle (1-mm diameter, noted as being at the coarse end of fine fuels), despite coarser particles starting to release pyrolysates soon after exposure. Finney *et al.* (2013) attributed the difference in the outcomes between the two fuel particles to their different boundary layer structure and the associated effect of convective cooling during the experiment.

The importance of convective cooling is observed in the plotted results of Anderson *et al.* (2010) and in more detail in Finney *et al.* (2015). In plots of temperature vs time or distance, it is visibly obvious that as the flame front approaches an unignited fuel particle, its surface temperature increases slowly but remains well below ignition temperature. While the fuel particle is being heated radiatively and convectively cooled, its surface temperature is kept at approximately or slightly above the surrounding air temperature. It is only when a significant increase in fluid temperature is observed, associated with flame contact, that the surface of the fuel particle shows a rapid increase in temperature. Although Butler (2010) did not provide surface fuel temperatures, he measured fluid temperature and velocity, plus total and radiative heat fluxes, leading him to state that 'convective cooling can be significant before ignition and that convective heating at the immediately prior to and at the time of ignition is extreme'.

From a fire propagation perspective, Mell *et al.* (2018) were incorrect in their reading of the results of Morandini and Silvani (2010) that 'radiative heat transfer either dominated convective heat transfer, or they were of similar magnitude' because they failed to acknowledge the complexities of heat transfer measurements in a field setting, particularly the influence of the sensor package on the measurements of convective heat transfer. Any analysis of fire propagation mechanisms and in particular heat transfer to unburned fuels must take into consideration the fuels sustaining the process. We wonder if Mell *et al.* (2018) considered the intrusive sensor package used by Morandini and Silvani (2010) to represent the boundary conditions characteristic of a fuel particle, or assemblage of particles, at the top of the fuel layer and as a consequence, the heat transfer received and lost by fuel particles as the fire approached it. As an example, the heat transfer in the transducers (gauge type not described) used by Morandini and Silvani (2010) was measured in an ~2.5-cm-wide flat surface, in contrast to the millimetre width of the fine fuels present in their experiments (Silvani and Morandini 2009). Most importantly, the intrusive nature of the apparatus meant that any cooling inflow within the convergence zone, as measured by Butler (2010) and Clements *et al.* (2007), and simulated by Dupuy *et al.* (2011), would not be able to affect the instruments located in the lee of the instrument protection box as would happen to fine fuels on top of the fuel bed.

## Mantra 2 is representative of statements in the literature

In their critique of M2, Mell *et al.* (2018) start by dissecting how the wording does not exactly fit the excerpts for five of the references given in table 1 of Cruz *et al.* (2017). It is unclear to us if Mell *et al.* (2018) believe that each statement should be identical to warrant their mention as supporting the concept of M2. Contrary to the assertion of Mell *et al.* (2018) that our 'version of this mantra is stricter than that of the authors cited', our use of 'should' is in our view less strict than (i) 'the model is only valid in the range of experiments for which it was validated' (Balbi *et al.* 2009); (ii) 'strictly speaking, their application to environmental conditions outside of those for which they were derived is not justified' (Mell *et al.* 2010); or (iii) 'these are only applicable to systems in which conditions are identical to those used in formulating and testing the models' (Pastor *et al.* 2003).

Mell *et al.* (2018) attempt to falsify M2 by giving a partial and misleading statement (whether through ignorance or intentionally) from our argument and providing an example (from Fernandes 2014) where 'an empirical model could not be successfully extended to environmental conditions outside its original dataset'. What Mell *et al.* (2018) chose to omit in their argument is the clause at the beginning of our sentence: 'given the appropriate functional relationships and field data scaled in terms of fireline width'. As such, and contrary to the assertion by Mell *et al.* (2018), the cited study of Fernandes (2014) does not invalidate our discussion of M2. As explicitly mentioned by Fernandes (2014), the original data used for development of the surface fire spread model was based on small fire propagation segments and thus was not scaled in terms of fire size. Mell *et al.* (2018) also ignored the fact that Fernandes (2014) was not trying to validate his surface fire rate of spread model, but instead derive an adjustment factor so that it could be applied to wildfires.

## Mantras 3 through 5 although theoretically valid are not true given the current state of knowledge

We note with interest that in support of their view that M3 is valid rather than an unsupported mantra, Mell *et al.* (2018) do not reference any published scientific work but instead provide a sole example of where a simulation (fig. 1 in their paper) based on the Wildland–urban interface Fire Dynamics Simulator (WFDS) (Mell *et al.* 2007) 'can provide insight into the roles of convective and radiative heat transfer'. However, their interpretation of the results seems questionable. First, it is unclear why Mell *et al.* (2018) contrast their no-wind simulation with the observations from Finney *et al.* (2015) for a wind-aided laboratory fire, despite mentioning that they have results for a similar wind-driven fire simulation. Furthermore, the graph in Finney *et al.* (2015, fig. 5) reports measurements made at the top of the fuel bed that are quite different from the depiction of Mell *et al.* (2018) of simulations at $z = 0$ cm (their fig. 1*c* and *d*). What one can observe, however, are similar trends between the measurements at $z = 0$ in Finney *et al.* (2015) and $z = 35$ cm in Mell *et al.* (2018), thereby illustrating that their simulation does not replicate the observations well. It is also unclear why one would use a complex and computationally expensive model to produce simulation results but then analyse them in a qualitative way instead of undertaking a meaningful quantitative analysis of the veracity of the results. Their example provides no support for the argument that simulations using physical models provide insight into fire behaviour, or even convective and radiative heat transfer for that matter, and leaves open the question as to whether such insight would exist without previously being found in direct observation of a laboratory fire, *à la* Finney *et al.* (2015).

In questioning our view of M5, Mell *et al.* (2018) doubt that this mantra appears in the references cited in table 1 of Cruz *et al.* (2017). This may be dealt with by the following quotes from the cited works that Mell *et al.* (2018) were unable to discern:

> Progress toward a parametric version of FIRETEC that still incorporates the essential physical processes critical to fire behavior has begun. (Hanson *et al.* 2000)
>
> Of course, the numerical simulation of such models will provide to firemen the needed information as the position of the fire front. So as to perform tractable simulations, such complete physical models should be reduced. (Margerit and Sero-Guillaume 2002)
>
> In conjunction with numerical fire behavior models such as FIRETEC or WFDS it will be possible to more precisely study transitions from surface to crown fire and develop species-specific thinning spacing guidelines. … Correlative relationships observed through more intense numerical studies may be used to refine existing operational models. (Parsons 2006)

The assertion by Mell *et al.* (2018) that M5 is supported and validated by the results obtained by Mell *et al.* (2007) requires scrutiny. Mell *et al.* (2007) compared WFDS rate of fire spread simulations with those observed in two grassland experiments and an empirical model developed by Cheney *et al.* (1998) to scale observed rates of fire spread of experimental fires with measured fireline width (eqn 4 in Cheney *et al.* (1998), not to be confused with the model used operationally in Australia represented by eqns 11 and 12 in the same article). Mell *et al.* (2007) indicate that 'spread rates and fire behaviour were nearly identical when the grassland plot was centered in a 1500 m versus a 2700 m square domain', that 'several horizontal grid sizes were tested', and that 'as the grid resolution increased, the head fire spread rate decreased somewhat'. From these statements, it is safe to say that the WFDS simulation environment used by Mell *et al.* (2007) was adjusted to fit the experimental observations or model; hence, it is not surprising that the results are within an acceptable error. Such adjustments of WFDS numerical grid settings to conform to observed results appear to be standard practice (e.g. Menage *et al.* 2012; Perez-Ramirez *et al.* 2017), and suggest that grid size independence has been an issue with WFDS. It is worth noting that a recent publication by Moinuddin *et al.* (in press) identifies and attempts to overcome the issue of grid size dependence in WFDS.

Despite such adjustments to WFDS simulation settings to match observed rates of fire spread, there are several inconsistencies in their results, namely in the analysis of figs 15 and 16 in Mell *et al.* (2007). In fig. 15*b*, one can observe that the WFDS outputs again agree perfectly with eqn 4 in Cheney *et al.* (1998), with a spread prediction of ~70–80 m min$^{-1}$ between a time of 40 and 100 s. But a simple simulation using eqn 4 for the conditions of experimental fire C064 with a 2-m wind speed of 4.6 m s$^{-1}$ (16.6 km h$^{-1}$), dead fuel moisture content of 6.3% and a head-fire width of 70 m (according to fig. 15*a* of Mell *et al.* (2007)) indicates a spread rate of 46 m min$^{-1}$. This is quite a different result from the plotted value. It is unclear how such an exact fit was produced when the values are so different. A similar issue appears for experimental fire F19 in fig. 15*a* of Mell *et al.* (2007), with the plotted results associated with eqn 4 being much higher than what the equation produces. These results further suggest the observed model fit is more likely due to the model adjustment process described above than inherent model adequacy. It is worth noting that the process of curating simulations through numerical adjustments to make the models fit the intended fire behaviour, as done with WFDS in Mell *et al.* (2007) and Perez-Ramirez *et al.* (2017), for example, will make it difficult to properly quantify model veracity and thus identify areas for model improvement.

It is also worthwhile pointing out that several studies using WFDS have shown worrisome trends in rate of fire spread outputs that further question the model's validity as to its capability to accurately predict the movement of free-spreading wildland fires. As an example, Overholt *et al.* (2014), using WFDS, predicted rates of spread in wind-driven grassfires that were faster than the wind – clearly an impossibility according to available empirical evidence (Cheney *et al.* 1998), but a possibility in the modelling realm. Hoffman *et al.* (2016) also noted an overprediction trend in the WFDS with respect to crown fire propagation in conifer forests. This is clearly seen in the simulations by Ziegler *et al.* (2017) where $R/U_{10}$ ratios (i.e. the ratio between the rate of fire spread and the 10-m open wind speed) of 0.25 and 0.3 were produced by WFDS for crown fire simulations. In contrast, Pimont *et al.* (2017), from a reanalysis of wildfire rate of spread data contained in Alexander and Cruz (2006) suggest an average ratio of 0.09. In this case, WFDS results are 2.8–3.3 times greater than empirical evidence would suggest. In light of these results, it is clear that WFDS is not yet ready to produce accurate predictions of fire spread rates in wildland fuels without considerable *a priori* knowledge of what the model outputs should be. On the basis of these observations, and the statement by Mell *et al.* (2018) that the simulations of Mell *et al.* (2007) 'could have been the basis of an empirical model', one can contend that pushing models like WFDS to be used operationally based on their apparent high-level theoretical background and a few curated model output comparisons with observed data can at their best lead to end-user distrust in fire behaviour science in general. At the very worst, they could lead to direct detrimental effects in fire management and suppression operations, with potential deleterious consequences.

## Implications for wildland fire management

We agree with the general needs of fire managers for fire behaviour information as expressed by Mell *et al.* (2018). However, to our knowledge, physical models have yet to provide any fire behaviour insight that was not already known from practical experience or empirical fire behaviour field studies. Their view that empirical-based fire behaviour models have outlived their usefulness appears to be US-centric. Cruz *et al.* (2018) have, for example, demonstrated the value of continuous improvement in the empirical-based fire behaviour models used in Australia.

Although it is true that current operational models have not been developed to replicate dynamic feedbacks, it is easy to forget that wildland fire behaviour prediction has long been regarded as both an art and a science (Alexander and Cruz 2013*b*), and is likely to remain so. There are indeed many aspects of wildland fire behaviour for which we do not have a complete understanding (Cruz *et al.* 2014). Plucinski *et al.* (2017) has illustrated how compensation strategies can be utilised to overcome knowledge gaps when making operational fire behaviour predictions in order to ensure meaningful outcomes.

## Closing remarks

Mell *et al.* (2018) appear to conceive of an enmity or hostility towards physical modelling on the part of Cruz *et al.* (2017) that does not exist. At no point was this perspective expressed by Cruz *et al.* (2017). The overriding intent of Cruz *et al.* (2017) was to identify and highlight the deficiency with which the necessarily incomplete science of fire modelling is conveyed in the literature and the tendency towards repeating given statements without necessary due diligence in regard to checking the veracity of such statements.

Overall, we believe that Mell *et al.* (2018) largely corroborate our assessment of the existence of the identified mantras. We have shown that much of the commentary by Mell *et al.* (2018) was based on misleading statements or misconstrued interpretations of our reasoning, and not supported by any published work.

We agree that physical-based modelling, underpinned by sound experimental evidence and interpretation, will contribute to our understanding of fire propagation processes (Hoffman *et al.* 2018). We are in fact on record as having said that physical models hold great promise (Sullivan 2009*b*, 2009*c*; Alexander and Cruz 2013*a*). But improvement in physical models will first require a thorough understanding of their limitations and advantages, as clearly stated by Linn *et al.* (2002). Continued work is still needed to improve the description of processes that are not adequately characterised (e.g. Colman and Linn 2007; Pimont *et al.* 2011). Simulation models should be exercised to understand, and not to mask, their limitations, hence opening opportunity for further improvement, with the ultimate goal of producing models that will in the future accurately describe wildland fire processes, dynamics and behaviour.

## Conflict of interest

The authors declare that there are no conflicts of interest.

## Acknowledgements

This research did not receive any specific funding.

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