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

William Mell<sup>A,G</sup>, Albert Simeoni<sup>B</sup>, Dominique Morvan<sup>C</sup>, J. Kevin Hiers<sup>D</sup>, Nicholas Skowronski<sup>E</sup> and Rory M. Hadden<sup>F</sup>

<sup>A</sup>Pacific Wildland Fire Sciences Laboratory, US Forest Service, 400 N. 34th Street, Suite 201, Seattle, WA 98074, USA.

<sup>B</sup>Fire Protection Engineering Department, Worcester Polytechnic Institute, 100 Institute Road, Worcester, MA 01609, USA.

<sup>C</sup>Aix-Marseille University, CNRS, Centrale Marseille, M2P2, Marseille, France.

<sup>D</sup>Tall Timbers Research Station, 13093 Henry Beadel Drive, Tallahassee, FL 32312, USA.

<sup>E</sup>Northern Research Station, US Forest Service, 180 Canfield Street, Morgantown, WV 26505, USA.

<sup>F</sup>School of Engineering, University of Edinburgh, King's Buildings, Mayfield Road, Edinburgh, EH9 3JL, UK.

<sup>G</sup>Corresponding author. Email: wemell@fs.fed.us

**Abstract.** In a recent communication, Cruz *et al.* (2017) called attention to several recurring statements (mantras) in the wildland fire literature regarding empirical and physical fire behaviour models. Motivated by concern that these mantras have not been fully vetted and are repeated blindly, Cruz *et al.* (2017) sought to verify five mantras they identify. This is a worthy goal and here we seek to extend the discussion and provide clarification to several confusing aspects of the Cruz *et al.* (2017) communication. In particular, their treatment of what they call physical models is inconsistent, neglects to reference current research activity focussed on combined experimentation and model development, and misses an opportunity to discuss the potential use of physical models to fire behaviour outside the scope of empirical approaches.

**Additional keywords:** CFD, empirical models, physics-based models.

## Introduction

In a recent commentary on fire-behaviour models, Cruz *et al.* (2017) identify five statements, or mantras, they believe have gained ‘currency as facts – or truths’ regarding empirical and physical (sometimes called physics-based or process-based) wildland fire models. Cruz *et al.* (2017) are concerned that an unquestioning acceptance of the mantras will lead to poorly informed use of the models in question. They seek, therefore, ‘to discuss the validity’ of these mantras. We agree that model users should be aware of the strengths and weaknesses of a given model. However, inconsistencies between how the mantras are represented by Cruz *et al.* (2017), and how they appear in the literature, add confusion, rather than clarity, to a broader discussion. In some cases, the authors’ discussion of the mantras is not even consistent within their own framework. Regarding physical models, the largely negative critique is confused by inconsistent definitions, inaccuracies and falls short of understanding how model advancement in engineering science is coupled to appropriate measurements. The authors appear to favour empirical models for prediction while not recognising the capabilities of physical models,

especially those based on computational fluid dynamics, for improving our understanding of the underlying physical and chemical mechanisms and their role in driving fire behaviour.

Although we appreciate the motivation and goal of Cruz *et al.* (2017), our intent in this response is to provide a constructive critique of Cruz *et al.* (2017) by clarifying the particularly confusing elements and providing viewpoints from the engineering and management perspectives. In Cruz *et al.* (2017), empirical (as opposed to semi-empirical) models are the subject of the first two mantras and what they call ‘physical’ models are considered in the last three mantras. These mantras are:

- Mantra 1 (M1). Empirical models work well over the range of their original data.
- Mantra 2 (M2). Empirical models are not appropriate for and should not be applied to conditions outside the range of the original data.
- Mantra 3 (M3). Physical models provide insight into the mechanisms that drive wildland fire spread and other aspects of fire behaviour.

- Mantra 4 (M4). Physical models give a better understanding of how fuel treatments modify fire behaviour.
- Mantra 5 (M5). Physical models can be used to derive simplified models to predict fire behaviour operationally.

### The discussion regarding physical models is flawed

The discussion related to the mantras for the physical models displays a limited understanding of modelling approaches that attempt to include (explicitly or implicitly) physical processes driving wildland fire. In the first paragraph of [Cruz \*et al.\* \(2017\)](#), the authors define a physical modelling approach as one that ‘employs a mathematical description of fundamental physical and chemical processes underpinning combustion, fluid flow and heat transfer’. We take this to mean that the processes driving fire behaviour are explicitly accounted for in ‘physical models’. [Cruz \*et al.\*](#) then use the term ‘physical model’ for both simpler models that, for example, neglect the process of convective heat transfer (in M3 and M5) and for more comprehensive physical models based on computational fluid dynamics (CFD) that explicitly account for all the recognised driving processes (in M3 and M4), including convective heat transfer.

A consequence of this inconsistent use of the term ‘physical model’ is confusion and lack of completeness. For clarity, here we place physical models into two groups: one group uses CFD, and the other does not. Both have model equations that are the result of approximations based on physically motivated assumptions. To be more precise, we use CFD-based physical models to denote comprehensive approaches that explicitly model the recognised processes driving fire behaviour. This is consistent with references cited for CFD-based models and statements made by [Cruz \*et al.\*](#)

In [Cruz \*et al.\*](#), nearly all the cited non-CFD physical models do not explicitly model convective heat transfer. [Cruz \*et al.\*](#) appear to mistakenly assume that convective heat transfer was neglected because the model developers assumed it is not relevant to fire spread, which is clearly not the case. If one reads the cited literature, it is clear that the model developers are fully aware that convective heat transfer, in some environmental conditions, will be relevant; but these are not the environmental conditions for which they derive their model. The assumption of radiation dominance in these models is not, therefore, an ‘example of our ignorance of the fundamental processes governing wildland fire behaviour’ as stated in the third paragraph of the M3 discussion.

Adding to the confusion, [Cruz \*et al.\*](#) incorrectly interpret findings in the cited literature ([Anderson \*et al.\* 2010](#); [Butler 2010](#)) when they write (end of second paragraph of M3 discussion) ‘recent experimental evidence suggests it is convective heat transfer ... that is the dominant heat transfer mechanism determining wildland fire propagation’. [Anderson \*et al.\* \(2010\)](#) did not measure radiation and, therefore, do not compare radiative and convective heat fluxes. [Butler \(2010\)](#) found that convective and radiative heat flux can be comparable in magnitude at certain times, and did not state that convective heat transfer dominates. [Finney \*et al.\* \(2015\)](#) do state that ‘repetitive convective heating thus appears to be the critical heat transfer mechanism causing ignition and spread of these fires’. In addition, [Morandini and Silvani \(2010\)](#) (a study not cited by

[Cruz \*et al.\* 2017](#)) conducted five field experiments and found that, depending on the fire experiment, radiative heat transfer either dominated convective heat transfer, or they were of similar magnitude. [Morandini and Silvani \(2010\)](#) considered shrub fires, [Butler \(2010\)](#) considered full-scale crown fires, and [Finney \*et al.\* \(2015\)](#) considered laboratory-scale surface fire in highly uniform fuel beds. Clearly, more work is needed to determine why the findings of these experiments differ. This point is missed by [Cruz \*et al.\*](#)

The latter part of the discussion of M3 and most of the M4 discussion is focussed on the challenges facing CFD-based physical models, including the need for some empiricism and more model validation. Although our response is not comprehensive, some of [Cruz \*et al.\*](#)’s statements are notably incorrect and demonstrate a limited understanding of CFD modelling. For example, it is not possible to model buoyant flow driven by combustion while assuming (as stated by [Cruz \*et al.\*](#) in the M3 section) constant density, incompressible flow.

Significantly, what [Cruz \*et al.\* \(2017\)](#) do not convey is that the reason they can list challenges to CFD-based modelling is precisely because these models are well characterised, both in their modelling approach and in areas needing improvement. CFD-based fire-behaviour models are constructed from coupled numerical models, for the governing processes, that vary in their degree of maturity and proven physical fidelity. For example, the models for fluid flow (including buoyancy induced flow) and radiation are significantly more advanced and validated than models for the processes of thermal degradation and momentum drag in vegetation. [Cruz \*et al.\* \(2017\)](#) give an incomplete picture of the advances made and the state of activity (including new experiments) in pursuit of improvements to these models (e.g. [Anand \*et al.\* 2017](#); [Mueller 2017](#); [Lamorlette \*et al.\* 2018](#)).

In the last sentence of the M3 section, [Cruz \*et al.\* \(2017\)](#) summarise their view of CFD-based physical modelling:

Until a complete and robust understanding of the processes ... we question how much is to be gained from pure modelling exercises...

This statement is problematic for several reasons. Physical models have approximations and will not be ‘complete’, but they can be useful and their failings can be characterised and addressed, making this a specious criticism. In addition, the suggestion that the developers of CFD-based physical models are in some way focussed on ‘pure modelling exercises’ displays a lack of familiarity with fire engineering science. It is fundamental to the scientific method and well established in the fire engineering community that the development of physical models requires comparison with observations and experiments (see [Mell \*et al.\* 2007](#); [Tihay \*et al.\* 2008](#); [Mell \*et al.\* 2009](#); [Morvan \*et al.\* 2009](#); [Tihay \*et al.\* 2009](#); [Hoffman \*et al.\* 2016](#); [El Houssami \*et al.\* 2018](#)). The necessity to have detailed comparisons between numerical results and experimental data (i.e. not just rate of spread observations) often push experimentalists to use more and more sophisticated experimental diagnostic methods in the laboratory ([Marcelli \*et al.\* 2004](#); [Morandini \*et al.\* 2005](#); [Zhou \*et al.\* 2007](#); [Lozano \*et al.\* 2010](#)) and in the field ([Frankman \*et al.\* 2013](#), [Mueller \*et al.\* 2017](#)). This list of experimental studies, using advanced diagnostics, is only a sampling, many more exist.

## Mantra 2 is not representative of statements in literature

There is no acknowledgement or discussion of how the particular wording of any given mantra, which affects the mantra's meaning, required choices by the authors. For example, consider Mantra 2 which is stated to be 'likely the most commonly used fire behaviour modelling mantra'. In the literature cited in table 1 of Cruz *et al.* (2017) for M2, the following text can be found (note, Cruz *et al.* 2017 do not provide these excerpts):

While such models may be very successful over fuel and environmental conditions similar to those occurring in the test fires, their lack of a physical basis means that the use of such models outside of these conditions must be treated with caution. [Catchpole and de Mestre 1986]

The predicted values for the ROS [rate of spread] remain valid for conditions close to the experimental conditions which were used to gauge the parameters of the model... Unfortunately the results obtained with this type of approaches are not easily applicable for more general fire conditions. [Morvan and Larini 2001]

...but the model is only valid in the range of experiments for which it was validated. Peculiarly, the change from laboratory to field scale experiments is not supported, but involves a new calibration of the parameters. [Balbi *et al.* 2009]

...strictly speaking, their application to environmental conditions outside of those for which they were derived is not justified. [Mell *et al.* 2010]

These are only applicable to systems in which conditions are identical to those used in formulating and testing the models. [Pastor *et al.* 2003]

Later in the paper it is stated, regarding McArthur meters for dry eucalypt forest, that:

Nevertheless, the use of this model in landscapes with vegetation different from that of dry eucalypt forest in Australia should be done with caution. [Pastor *et al.* 2003]

At first glance, these quoted statements seem to be well represented by M2 of Cruz *et al.* (2017) However, most of the statements allow for the possibility of applying an empirical model outside its original dataset, but with appropriate caution. Thus, the wording of the Cruz *et al.* (2017) version of this mantra is *stricter* than that of the authors cited because Cruz *et al.* (2017) make no allowance for the possibility that an empirical model may work outside the original environmental conditions. This sets the stage for easily invalidating M2 by finding any case where an empirical model works sufficiently well outside its originating environmental conditions. This is what Cruz *et al.* (2017) do in their discussion of M2.

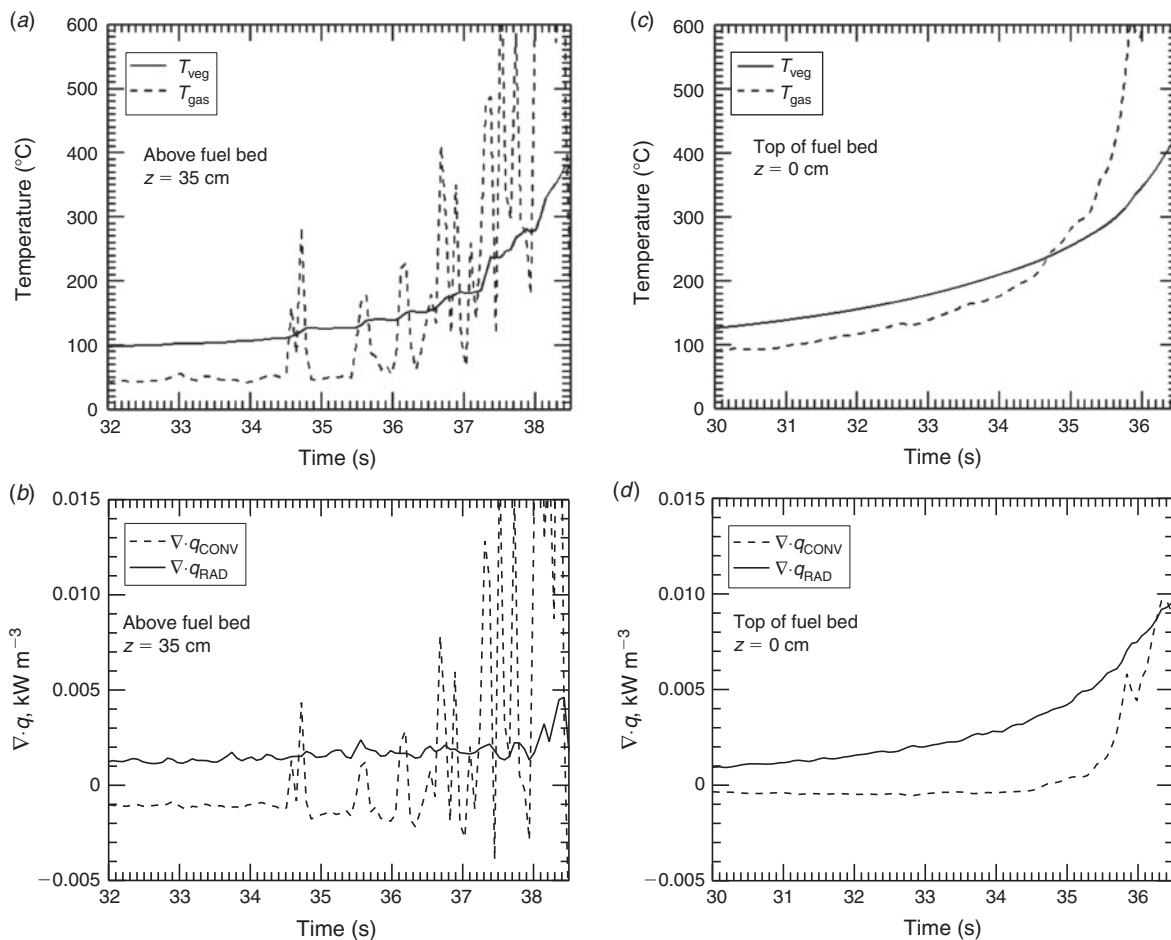
Cruz *et al.* (2017) go further and state that 'empirical models are likely to be valid for far drier and windier conditions than those involved in the model development'. But this statement *required* sufficient measurements in the new environment to show that the original model actually worked outside its dataset. Also, there are contrary examples. The work by Fernandes (2014) had the opposite finding: an empirical model could not be successfully extended to environmental conditions outside its original dataset unless it was recalibrated using the new data.

Although many scientists would allow that an empirical model may work for environmental conditions outside its originating dataset, they would also agree that, without measurements confirming it, there is no justification for asserting that the empirical model will do so with quantifiable confidence. Caution is inherent to the process of using empirically fit models beyond their domain of inference and is taught in basic statistics (Sokal and Rohlf 1995). In essence, Cruz *et al.* (2017) agree with this when they state, at the end of M2, 'evaluation should always precede the use of models within operational contexts'.

## Are Mantras 3 through 5 valid?

We agree that the wording of M3 is representative of the literature and believe it to be valid. As an example, we provide a simple demonstration of how of CFD based models can provide insight into the roles of convective and radiative heat transfer. Fig. 1 shows results from a three-dimensional, time-dependent simulation (using the wildland-urban interface fire dynamics simulator (WFDS); Mell *et al.* 2009; Perez-Ramirez *et al.* 2017 have model details) of a surface fire spreading, with no ambient wind, through a 10 cm deep, 80 cm wide, 1.8 m long excelsior fuel bed. Fig. 1 shows the time histories of the gas and vegetation temperatures and the contribution of the convective ( $\nabla \cdot q_{\text{CONV}}$ ) and radiative ( $\nabla \cdot q_{\text{RAD}}$ ) heat fluxes to the rate of change of the vegetation's temperature. These quantities are plotted at two vertical locations (both at a distance of 1 m from the ignition region):  $z = 35$  cm above the fuel bed (i.e. a location subjected to the combustion generated buoyant plume and intermittent flame) and at  $z = 0$  cm (i.e. top of fuel bed and subjected to a relatively slower and less variable flow and radiation from a continuous fire front). Consistent with the findings of Finney *et al.* (2015) (see their fig. 5A), the vegetation temperature at  $z = 35$  cm follows a 'stair-stepped' rise that is controlled by a varying convective heat flux (Fig. 1a, b). At  $z = 0$  cm on top of the fuel bed (Fig. 1c, d), radiation dominates until near ignition (i.e. the temperature of the vegetation,  $T_{\text{veg}} = \sim 350^\circ\text{C}$  at time = 36 s), at which point radiation and convection are comparable, at no point does convection exhibit the large oscillations seen at  $z = 35$  cm. The experimental configuration of Finney *et al.* (2015) is a surface fire and their measurement location is similar to Figs. 1c, d (i.e. at the top of the fuel bed). Their results are similar to Figs. 1a, b because their imposed wind increases the unsteady behaviour of the flame. Simulations with WFDS give similar results with an imposed wind (not shown).

Regarding M4, we believe that Cruz *et al.* (2017) chose a wording that is stricter than in the literature. This mantra should read: 'physical models *have the potential to* give a better understanding of how fuel treatments modify fire behaviour', which we believe is valid. It is not clear why Cruz *et al.* (2017) did not write M4 this way, especially because their opening sentence introducing M4 does. CFD-based models have been used to simulate the influence of the spatial heterogeneity of vegetation on fire behaviour (e.g. in addition to the references in Cruz *et al.* (2017): Pimont *et al.* 2011; Hoffman *et al.* 2015; Ziegler *et al.* 2017). The challenge is to evaluate how well these simulations represent reality, which requires well-designed experiments. This is well recognised by physical modellers



**Fig. 1.** Results from a computational fluid dynamics- (CFD) based physical model simulation of a fire spreading through an excelsior fuel bed in the absence of an ambient wind. The gas temperature, the vegetation temperature, and measures of the convective ( $\nabla \cdot q_{\text{CONV}}$ ) and radiative ( $\nabla \cdot q_{\text{RAD}}$ ) flux into a  $2\text{-cm}^3$  volume of excelsior are plotted v. time. The left-side column (a and b) show these quantities at a location  $z = 35$  cm above the fuel bed. The right-side column (c and d) are for a location at the top of the fuel bed,  $z = 0$  cm.

and the community would be better served if Cruz *et al.* (2017) discussed the need for well-designed experiments to support model development and current activity. Instead, Cruz *et al.* (2017) present an obstructive discussion on model approximations and the lack of model validation. Also, with their emphasis that the physical models are not ready for operational use, the discussion deviates from M4. The wording of M4 does not explicitly state that it refers to either CFD based physical models (which is the only type of physical model cited) or operational objectives.

We agree with Cruz *et al.*'s (2017) statement in M5 that models applied to operational objectives need to be properly used and their limitations known. However, their M5 discussion suffers from another inconsistent use of the term 'physical model'. In this section, they write:

...the physical model is an acceptable representation of the fire processes and that the only limitations for model implementation are extraneous to the modelling of the fire processes, such as numerical implementation issues and computational time demands.

This is followed by their declaration that Albini's model (Albini 1996, 2000) is a physical model of crown fire spread. But Albini's model does not meet the characteristics of a physical model as described above by Cruz *et al.* (2017). Instead, Albini's model is a simpler approach and Butler *et al.* (2004) combine four existing simpler models for different components of the problem (see bottom right of p. 1590 in Butler *et al.* 2004). Thus, Cruz *et al.*'s (2017) use of Butler *et al.* (2004) has no relevance to M5.

Although we do not find compelling evidence that M5 appears in the references cited, we agree with the mantra in the sense that it is possible to use ROS predictions from CFD-based models to develop 'empirical' formulae. For example, the study of Mell *et al.* (2007) found good agreement of the head fire ROS determined from numerical predictions and an empirical model based on field observations. This included predictions of fireline acceleration dependent on the head-fire width. Thus, these simulations could have been the basis of an empirical model. But model developers, as a matter of course, are reluctant to provide such empirical models without sufficient

characterisation of model performance, which requires a range of appropriate experiments. Examples of analysis leading to a reduced model from a more comprehensive physical model include the works of Simeoni *et al.* (2001), who use the approach of model reduction, and Margerit and Sero-Guillaume (2002) who use asymptotic analysis.

### Management implications

From the perspective of a land manager, the changing landscapes in which wildfires and prescribed fires are managed demand a more robust toolset for understanding the processes at play. Operational tools for predicting fire behaviour lag far behind the science of fire–atmosphere interactions, and a continued reliance on empirical models becomes less ‘predictive’ as managers face increasingly novel combinations of fuels (from non-native species), weather, climate and heterogeneity across landscapes (Kraaij *et al.* 2018). Furthermore, by definition, empirical models cannot capture, with well-characterised confidence, the limits or extremes of observed fires (see discussion of M2). This limitation creates the need for caution, which is often not adequately relayed to the management community, when employing empirical models beyond their domain of origin. Also, managing fire in conditions for which measurements are incomplete creates an important operational decision space for the use of CFD-based approaches for understanding the potential physical mechanisms in increasingly complex contexts. Empirical modelling focuses almost exclusively on the ROS. The use of ROS as a gold standard for validation further misses a critical management need to understand complex fire–atmosphere feedbacks, multiple fireline development and canopy-induced flows on planned ignitions. There are simply too many management tactics and decisions that involve critical fire-behaviour phenomena outside the domain of empirical inference. Because managers are themselves empirical modellers, tools that operate at conditions and fire behaviour at the edge of their experience are the most critical for enhancing decision making in operational contexts.

Using CFD or other physical-modelling tools is needed for the evaluation, either retrospectively or proactively, of processes and mechanisms that generate unexpected fire behaviours. Such lessons learned for fire reconstructions has proven useful in understanding rare events (e.g. Cunningham and Reeder 2009). It is equally important for managers to understand when CFD- or other physics-based modelling tools approach the limits of their applicability. If scepticism of CFD and trust in empirical models is the ultimate point of Cruz *et al.* (2017), then they sadly miss the opportunities that each approach provides as managers tackle a range of operational contexts.

### Conclusions

We believe that there is a need for many types of models for research and for operational purposes. We also firmly reject the assertion that because all the physical processes and their interaction driving fire behaviour are not fully understood, physical modelling should be discouraged or held suspect. History and the scientific method have shown that progress in physical modelling is made with initial simplifying

approximations to be tested against well designed experiments. The idea that the two approaches (experimental and theoretical or numerical) are complementary is widely shared in the scientific community (as, notably, stated in Cruz *et al.* 2011).

Recurrent in Cruz *et al.* (2017) is the recognised need for well-designed experiments for the development and evaluation of both empirical and physical models. We heartily agree and emphasise that for physical models, especially in the field, these measurements are challenging (e.g. Mueller *et al.* 2017, 2018) and require careful consideration of model needs in order to adequately provide information on vegetation, wind, and fire behaviour.

Uncertainty is and will always be part of a fire manager’s risk calculations, and most managers clearly understand that models are tools. Nearly all managers are also anxiously awaiting tools that provide insight into fire behaviours not already self-evident through their own observations. The critical targeting of new approaches based on physical modelling, especially CFD based, by Cruz *et al.* (2017) runs the risk of undermining innovation and opportunities for managers to learn from this branch of fire research.

### Conflicts of interest

The authors declare that they have no conflicts of interest.

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