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Clarifying the meaning of mantras in wildland fire behavior modeling: reply to Cruz et al.

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

Brief summary: The validity of a number commonly held beliefs regarding fire behavior models is discussed with an emphasis on physical (or physics-based) models.

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

Introduction

In a recent commentary on fire behavior 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. 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, 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 favor empirical models for prediction while not recognizing the capabilities of physical models, especially those based on computational fluid dynamics, for improving our understanding of the mechanisms and their role in driving fire behavior.

While we appreciate the motivation and goal of Cruz et al., our intent in this response is to provide a constructive critique of Cruz et al. by clarifying the particularly confusing elements and providing viewpoints from the engineering and management perspectives. In Cruz et al., 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 behavior.

Mantra 4 (M4): Physical models give a better understanding of how fuel treatments modify fire behavior.

Mantra 5 (M5): Physical models can be used to derive simplified models to predict fire behavior operationally.

The discussion regarding physical models is flawed

The discussion related to the mantras for the physical models displays a limited understanding of modeling approaches that attempt to include (explicitly or implicitly) physical processes driving wildland fire. In the first paragraph of Cruz et al., the authors define a physical modeling 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 behavior 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 recognized 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 recognized processes driving fire behavior. 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 behavior" 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) don't measure radiation and, therefore, do not compare radiative and convective heat fluxes. Butler (2010) finds that convective and radiative heat flux can be comparable in magnitude at certain times, and do 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) (this study was not referenced in Cruz et al.) 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. 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 focused on the challenges facing CFD based physical models, including the need for some empiricism and more model validation. While space limits a comprehensive response, some statements are notably incorrect and demonstrate a limited understanding of CFD modeling. 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. do not convey is that the reason they can list challenges to CFD based modeling is precisely because these models are well characterized, both in their modeling approach and in areas needing improvement. CFD based fire behavior 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. 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., Mueller 2017a, Anand et al. 2017, Lamorlette et al. 2018).

In the last sentence of the M3 section Cruz et al. summarize their view of CFD based physical modeling: “Until a complete and robust understanding of the processes … we question how much is to be gained from pure modeling exercises ….” This statement is problematic for a number of reasons. Physical models have approximations and will not be “complete”, but they can be useful and their failings can be characterized and addressed, making this a specious criticism. In addition, the suggestion that the developers of CFD based physical models are in some way focused on “pure modeling 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. 2017b). 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 acknowledgment or discussion of how the particular wording of any given mantra, which impacts 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. for M2, the following text can be found (note, Cruz et al. do not provide these excerpts):

Catchpole and de Mestre (1986): "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."

Morvan and Larini (2001): "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."

Balbi et al. (2009): "… 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."

Mell et al. (2010) "… strictly speaking, their application to environmental conditions outside of those for which they were derived is not justified."

Pastor et al. (2003): "These are only applicable to systems in which conditions are identical to those used in formulating and testing the models." 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."

At first glance, these quoted statements seem to be well represented by M2 of Cruz et al. However, most of the statements allow for the possibility of applying an empirical model outside its original data set, but with appropriate caution. Thus, the wording of the Cruz et al. version of this mantra is *stricter* than that of the authors cited because Cruz et al. 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. do in their discussion of M2.

Cruz et al. 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.

While 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. 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. Figure 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. Figure 1 shows the time histories of the gas and vegetation temperatures and the contribution of the convective ($\nabla \cdot q_{\mathrm{CONV}}$) and radiative ($\nabla \cdot q_{\mathrm{RAD}}$) 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 (Figs. 1a and 1b). At the z = 0 cm on top of the fuel bed (Figs. 1c, 1d), radiation dominates until near ignition ($T_{\mathrm{veg}} \approx 350$ C, 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 and 1d (i.e., at the top of the fuel bed). Their results are similar to Figs. 1a and 1b because their imposed wind increases the unsteady behavior of the flame. Simulations with WFDS give similar results with an imposed wind (not shown).

Regarding M4, we believe that Cruz et al. 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 behavior”, which we believe is valid. It is not clear why Cruz et al. did not write M4 this way, especially since their opening sentence introducing M4 does. CFD based models have been used to simulate the influence of the spatial heterogeneity of vegetation on fire behavior (e.g., in addition to the references in Cruz et al.: 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 recognized by physical modelers and the community would be better served if Cruz et al. discussed the need for well-designed experiments to support model development and current activity. Instead, Cruz et al. 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 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. 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 page 1590 in Butler et al. 2004). Thus, the Cruz et al. use of Butler et al. (2004) has no relevance to M5.

While 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” formulas. 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 characterization 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 behavior lag far behind the science of fire-atmospheric 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-characterized confidence, the limits/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 modeling 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-atmospheric feedbacks, multiple fireline development, and canopy induced flows on planned ignitions. There are simply too many management tactics and decisions that involve critical fire behavior phenomena outside the domain of empirical inference. Because managers are themselves empirical modelers, tools that operate at conditions and fire behavior at the edge of their experience are the most critical for enhancing decision making in operational contexts.

Using CFD or other physical modeling tools is needed for the evaluation, either retrospectively or proactively, of processes and mechanisms that generate unexpected fire behaviors. 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 modeling tools approach the limits of their applicability. If skepticism of CFD and trust in empirical models is the ultimate point of Cruz et al., 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 all 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 behavior are not fully understood, physical modeling should be discouraged or held suspect. History and the scientific method have shown that progress in physical modeling is made with initial simplifying approximations to be tested against well designed experiments. The idea that the two approaches (experimental and theoretical/numerical) are complementary is widely shared in the scientific community (as, notably, stated in Cruz et al 2011).

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

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 behaviors not already self-evident through their own observations. The critical targeting of new approaches based on physical modeling, especially CFD based, by Cruz et al. runs the risk of undermining innovation and opportunities for managers to learn from this branch of fire research.

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Conflicts of interest

The authors declare no conflicts of interest.

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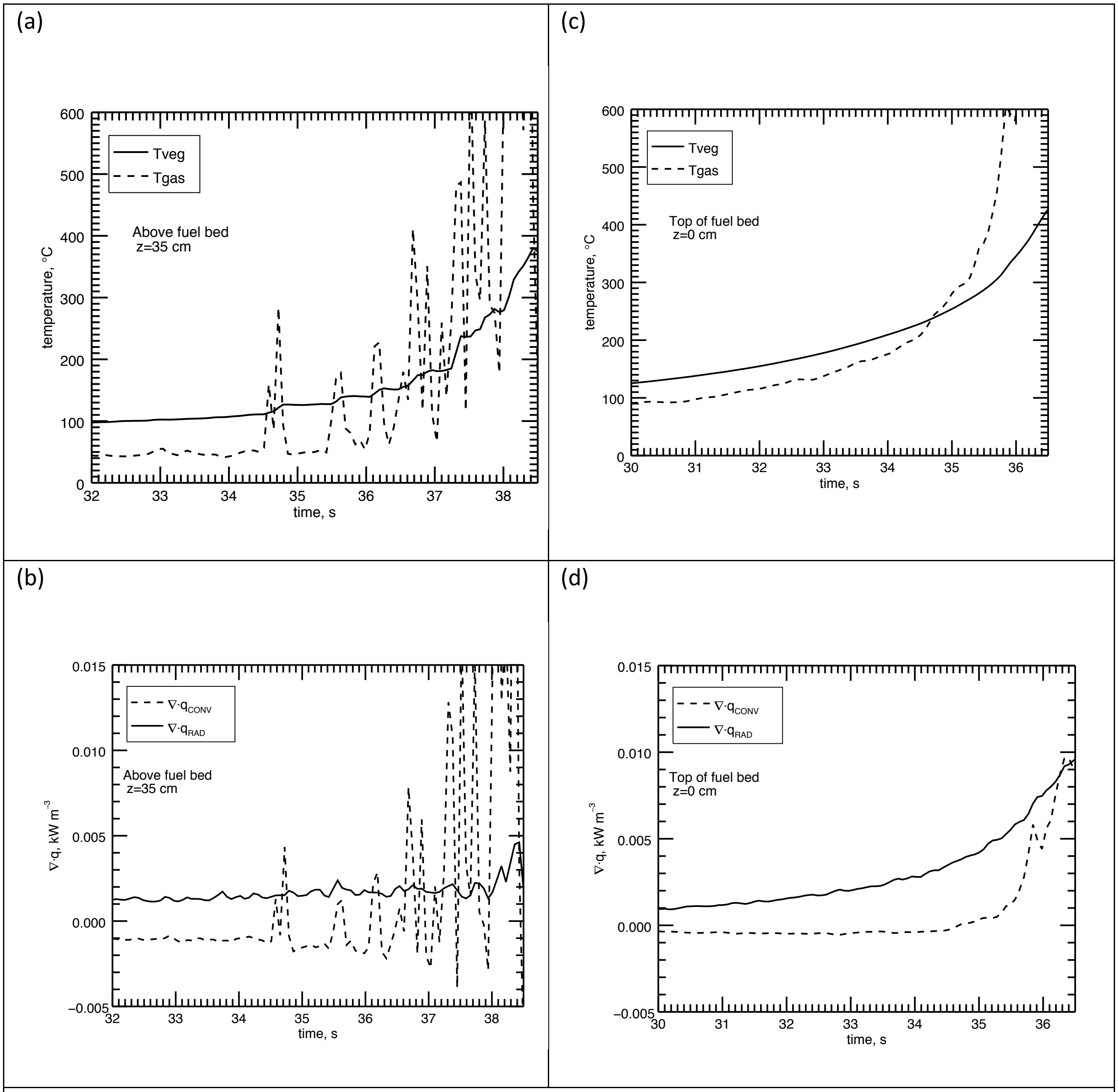

Figure 1: Results from a CFD based physical model simulation of a fire spreading through an excelsior fuel bed in the absence of an ambient wind. The gas temperature, the vegetation temperature, and measures of the convective ($\nabla \cdot q_{\text{CONV}}$) and radiative ($\nabla \cdot q_{\text{RAD}}$)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