GENERAL COMMENTS:

In this work the author present a parameterization to predict the injection height of wildfire emission. They followed a statistical approach based on the MODIS Fire Radiative Power (FRP) and the MISR stereo-height products. The formulation they suggest gives rather good results as it predicts correct plume heights which differ from the observed MISR height by less than 500m, for 2/3 of the 2000 fires they collected in their data set.

The parameterization is developed to be easily embedded in a host model as it is based on a function (equation 10) only depending of the ABL height H_{abl}, the FRP, and the FT Brunt-Vaisala constant N_{FT} and so required no time integration. The constant of this function are evaluated via optimization algorithm to best match the model predicted and the MISR observed heights.

In order to improve the capacity of the parameterization to detect and evaluate the plume height of FT fires, the authors developed two algorithms based on Equation 10 which promote either Atmospheric Boundary Layer (ABL) or Free Troposphere (FT) fires. The choice between these two algorithms is left to the user.

Evaluating the impacts of fires on air quality and climate requires the use of transport models, which must simulate correct injection height. The development of such parameterizations is currently a matter of debate in the scientific community. In this context, the present manuscript is interesting and original.

However I think the quality of the manuscript could be improved if the authors could run a more comprehensive comparative study with current state of the art parameterization. According to the conclusion, the manuscript is aiming to compare the new approach with existing stack-oriented models and Plume Rise Model (PRM). However they did not consider the use of the PRM developed by Freitas et al 2007, 2010, or the mass flux formulation of Rio et al 2010. The only 1D PRM used in this study is the BUOYANT model which gets less attention than the Briggs equation in the section 2 on 'formulation' description. For example, I did not find any details on the parameterization of fire heat emission in BUOYANT. Furthermore the way the work on PRM comparison is carried along the paper is rather confusing: (i) the abstract did not mention it and (ii) rather quick assumption are made on PRM behaviour. For example at the end of section 5.3, the discussion on latent heat sensitivity of the Freitas model is unfunded. The effect of latent heat (responsible of a second updraft in the case of some fire as the one studied in Freitas et al 2007) cannot be used as the main reason for the over-estimation of the injection height. Other parameters like the fire heat emission or the entrainment scheme might be as much relevant.

In conclusion, I have no major concerns and recommend publishing the manuscript in ACP, however I think that in order to improve the quality of the paper, its structure should be rethought to make its claims and its results more consistent. The comparisons with 1D PRM formulation should be dropped or if the authors want to keep it further work need to be done.

SPECIFIC COMMENTS:

Abstract:
As stated previously, the abstract never refers to the work done on 1D PRM.

Section 2 - Existing Plume rise Formulation:
p27942 -l15: VSMOKE is not used in the reference paper Freitas et al 2007.

Section 4 – Methodology for injection ...
p27944-l3-6: The vertical updraft near the flame and several hundred meters above the fire is much more important than the atmospheric fluctuation. Riggan at al 2004 measure vertical wind speed of 15 m/s (8 m/s) over cerrado fire at an altitude of ~200 m (~1,000 m). At these altitudes the momentum is definitely not negligible and the entrainment of fresh air from the ambient environment plays an important role. A direct consequence of this mechanism is the 'puff' structures formed at the edge of the plume. I understand that the formulation described here is making some assumptions which are acceptable; however the author should consider reformulating the sentence of line 5.

Equation4: I did not understand the derivation of this equation. I could be wrong, but according to the text p27945-line 2 the first term describe the work against the buoyancy force. Therefore I was expecting a term similar to the one used in the common formulation of the CAPE, -g B dz. According to the structure of Equation 4, it seems that the first term is coming out of 1/v (d E/dz) using $E = \rho_a C_p w \theta$ which is the only contribution of sensible heat which is in contradiction with the definition of P_f in the formulation of Equation 3 (see also comment posted by Edward Hyer). However this doesn't explain the negative sign. Could the author give more details on the derivation of this equation? Furthermore, the presence of E_0 in the second term instead of E is also surprising. Does this means that the only parameter varying with altitude is the cross section S of the plume?

Equation 6: Could the author check that there is no missing factor '1/w' in the second term of the right hand side. Adding this term I ended up with a different formulation for equation 7,

$$-\beta\xi_p + (\beta-\alpha)\xi_p + \alpha = 0 \quad \text{with} \quad \beta = \frac{c_p\ \rho_a \theta\ N^2}{g 6\pi K} S_f w \quad \text{and} \quad \alpha = \frac{Pf}{S_f w}$$

which yields to $\xi_p = \frac{1}{2\beta}\left((\beta-\alpha) + \sqrt{(\alpha+\beta)^2 + 4\alpha\beta}\right)$

However I think this does not alter the discussion of page 27946.

Section 5 – Inter comparison
Equation 14: This equation refers to the Dozier (1981) algorithm and no reference is made to this previous work. Furthermore I think the equation reported here is wrong. The radiance L_i emitted by a

pixel at frequency ν_i is $L_i = B(\nu_i,T_{rad}) = p_f B(\nu_i,T_f) + (1-p_f)B(\nu_i,T_b)$, where p_f is the proportion of the pixel with fire and B the plank function $B = \frac{2h\nu^3}{c^2}\frac{1}{e^{\frac{h\nu}{kT}}-1}$.

The author should also give some more details on the nature of the MOSID data used here. Is it the MOD14 product? Did they apply any atmospheric correction to the brightness temperature T_{rad}? How the background temperature was evaluated?

P27949,l15: If Tf ~ Tb, it is more likely that the pixel won't be detected as a fire pixel than the Dozier algorithm would not converge. The authors can refer to the work of Giglio and Kendall (2001) for more details on this issue.

Equation 15: Would that be possible to give more details on the formulation of the buoyancy flux F. Variables r and v_s are not defined.

p27951,line 10: The reference to 'it' is not obvious.

p27951,line 16-17: Freitas et al 2010 show that ambient wind shear can have an impact plume height.
p27951,line 16-17: As stated in the 'general comment' section, such sentence does not present solid ground as it mainly relies on one single plume studied in Freitas et al 2007 that might not be representative of the 2000 plumes overlooked in this work.

Section6 - Discussion

To highlight the improvement of the new parameters (Eq. 21) in FT fire detection, it would be interesting to see a figure similar to Fig. 5 for (eq10,21).

TECHNICAL CORRECTIONS:

Typo:

Equation4 : the term ρ_a is not defined
P27952-l24 : 'repeated' is repeated 2 times.
P27953-l4 and l13: I think the reference to Eq. 12 is instead Eq. 13.
P27953-l5: '....this fit for ...' ?

Figures:

For sake of clarity, a reference to the algorithms used [ie (eq10;13), (eq10;19), or (eq13;21)] could be added to each caption.

REFERENCE:

Dozier, J. (1981), A method for satellite identification of surface temperature fields of subpixel resolution, Remote Sensing of Environment, 11, 221-229, doi: 10.1016/0034-4257(81)90021-3.

Freitas, S. R., Longo, K. M., Trentmann, J., and Latham, D.: Technical Note: Sensitivity of 1-D smoke plume rise models to the inclusion of environmental wind drag, Atmos. Chem. Phys., 10, 585-594, doi:10.5194/acp-10-585-2010, 2010.

Giglio, L., and Kendall, J. D., 2001, Application of the Dozier retrieval to wildfire characterization: A sensitivity analysis. Remote Sensing of Environment, 77, 34-49.

Riggan, Philip J., Robert G. Tissell, Robert N. Lockwood, James A. Brass, João Antônio Raposo Pereira, Heloisa S. Miranda, Antônio C. Miranda, Teresa Campos, and Robert Higgins. 2004. REMOTE MEASUREMENT OF ENERGY AND CARBON FLUX FROM WILDFIRES IN BRAZIL. Ecological Applications 14:855–872

COMMENT ON THE ANSWER TO THE NOTE POSTED BY EDWARD HYER.

'*the main message stays: during the calibrationof the formula, the value of FRP is directly related to the injection height Hp, thus taking into account the contribution of both sensible and latent heat. An implicit assumption behind this step is that the ratio of these contribution is about constant - or that the contribution of the latent heat is noticeably smaller than that of the sensible heat*.'

As reported by Rio et al 2010, the effect of the latent heat in the plume behaviour (e.g. pyro-convection) is due to the ambient water vapour entrained in the plume and not the water vapour injected by the fire (i.e. from the combustion).

'*Thereare two indirect hints that the second may be closer to reality. Firstly, computationswith BUOYANT (no accounting for humidity) appeared the second-best after our formula. The model failed only for few high plumes where the dry-plume assumption is indeed wrong. Secondly, the model of Freitas et al (2007), which shows comparable contributions of sensible and latent components, seems to over-estimate the heights, at least the fraction of the plumes reaching the free troposphere (we discussed it in the paper).*'

I think that the overestimation of the Freitas model is more likely related to the entrainment scheme which mainly controls the injection of water in the plume and so the contribution of latent heat. This scheme can be depend of fire characteristics and ambient condition. Therefore, a fix contribution of latent heat is certainly unrealistic.