The authors would like to thank the reviewers for their helpful and insightful comments. Each comment has been addressed below, and the manuscript has been revised based on each comment.

Reviewer #1: Overall the paper is very useful and valuable to the fire science community. One of the important factors that needs to be considered is the method that numbers and data are presented. On most of the plots, data points are not very clear and there are no error bars for the measured and calculated points. It is very important to take those errors mentioned in the paper into account when presenting the data points. When presenting the calculated velocities and measured velocities, it is important to use curve fitting to fit a linear curve and compare the slope and the intercept of the fitted curve with the straight line.

The manuscript states that the estimated experimental uncertainty was 10 % (as reported by Sippola), and the model underpredicted the measured aerosol deposition velocity by 45 %, on average. In an earlier version of the manuscript, error bars were incorporated in the aerosol deposition velocity plots. However, the error bars do not display well on the plots because they are very small values on a log-log scale.

Regarding the linear fits, the authors do not feel that it would be useful to fit arbitrary curves through the data unless we can derive what the true functional form is. A curve fit implies a statement about the underlying physics. However, additional edits have been made to the plots to clarify the comparison of measured vs. predicted aerosol deposition velocities. More information on the modifications to the plots in the revised manuscript are provided below in the response to Comment #8 in Reviewer #2’s comments.  
  
Reviewer #2: In this study soot deposition on surfaces is modeled with FDS. Three deposition mechanisms are discussed and introduced into the FDS code. A simple aerosol deposition experiment, that doesn't involve one of the deposition mechanisms (thermophoretic process), provides data for model validation. The study is simplified and, as the authors mention, needs to be eventually extended to include soot sources producing particles characteristic of fires. Thermophoretic deposition, which is an important phenomenon, should be validated before this model can be applied for practical applications.  
  
Following is a list of concerns, addressing which should help make the manuscript publishable in the Fire Technology journal:  
  
1.      On page 3, first paragraph, "Soot agglomeration …. deposition rates": does this phenomenon occur outside the fire, i.e. the hot region? Can the authors provide a reference(s) in support of this claim? The references will help differentiate between agglomeration mechanisms in soot models included in combustion modeling and the agglomeration of "cold" soot transported outside fires.

Agglomeration can occur at any location where aerosol particles are present. If two particles collide, then there is some chance they will stick together. The probability of collision is a function of the particle number density. In a fire, the largest particle number densities will be located in the flaming region and in the near-field plume, and particle number densities will decrease as air is entrained (particle mass density decreases) and as particles agglomerate. In far-field region, soot agglomeration continues to occur at any location where aerosol particles are present. The deposition mechanisms are similarly affected. In addition, thermophoretic forces on a particle are greatest for small particles at high temperature conditions, and gravitational forces are greatest as the average particle size increases from agglomeration. Soot agglomeration occurs at different scales and can increase the size of soot particles and affect soot deposition rates in both the flaming region and the post-flame environment. In the revised manuscript, this text has been added and additional references have been provided.

2.      On page 3, second paragraph: Ref 3 is not a peer reviewed publication. Can the authors replace it with a peer reviewed publication?

In the revised manuscript, the reference has been replaced with references to peer reviewed publications that justify the need to account for soot deposition in compartment fire scenarios.

3.      On page 11, end of first paragraph, "… allow for the flow …. developed": did the experimental setup involve flow straighteners at the 180deg bend? If not, the selected computational domain needs to be extended upstream, or a non-uniform/turbulent inlet condition needs to be applied.

In the model, an additional 3 m of duct section (equal to 20 duct diameters) was included upstream of the instrumented portion of the duct to allow for the flow to become fully developed. This has been clarified throughout the revised manuscript.

4.      On page 11, second paragraph: why isn't a mesh sensitivity study reported and how was the 1 cm mesh resolution decided? Also, why is it necessary to select/mention 1 mm duct wall thickness? How does the wall thickness affect deposition modeling?

To assess the impact of different grid resolutions, multiple simulations were run with grid cell sizes of 0.5 cm, 0.75 cm, 1 cm, and 2 cm at the smallest and largest air speeds and particle diameters. The aerosol deposition rates for each case are shown in a table that has been added to the revised manuscript. As expected, the predicted aerosol deposition velocity changes along with the grid resolution because the velocity profile is resolved differently at different grid cell sizes.

The duct walls were specified as a 1-mm thick, smooth material. The wall thickness did not impact the aerosol deposition rate, but is a required input parameter and is included for completeness and reproducibility. This statement has been included in the revised manuscript.

5.      On page 11, third paragraph: a sensitivity study of the aerosol concentration (100 mg/m3 selected) needs to be conducted as the experiment doesn't provide estimates. The selected concentration can be considered to be arbitrary.

6.      On page 11, second to last paragraph: the 45% under-prediction supports the need for a sensitivity study with the inlet concentration increased/decreased. Would using a higher inlet concentration help?

To assess the impact of different inlet aerosol concentrations, multiple simulations were run with inlet aerosol concentrations of 50 mg/m3, 100 mg/m3, 200 mg/m3, and 1000 mg/m3 at the smallest and largest air speeds and particle diameters. The aerosol deposition rates at the ceiling, wall, and floor for each case are shown in a table that has been added to the revised manuscript. In summary, the effect of different inlet aerosol concentrations on the predicted aerosol deposition velocity is relatively small because the aerosol deposition rates are a linear function of the aerosol concentration.

7.      On page 13, first paragraph: Why is the concentration at the ceiling lower than at the floor? Deposition velocities for this case (Test 16) are identical at the floor and ceiling. If the reason for the difference is the role of gravitational settling at the floor, it should be clearly described.  Contributions from turbulent deposition and gravitational settling should be compared.

In the simulation of Test 1, gravitational settling and turbulent deposition accounted for about 98 % and 2 % of the total aerosol deposition velocity to the floor, respectively. In the simulation of Test 5, gravitational settling and turbulent deposition accounted for about 100 % and 0 % of the total aerosol deposition velocity to the floor, respectively. In the simulation of Test 12, gravitational settling and turbulent deposition accounted for about 93 % and 7 % of the total aerosol deposition velocity to the floor, respectively. In the simulation of Test 16, gravitational settling and turbulent deposition accounted for about 49 % and 51 % of the total aerosol deposition velocity to the floor, respectively. In general, the increased aerosol concentration near the floor in these cases is mostly due to gravitational settling. The asymmetric shape (i.e., stratification) of the aerosol concentration gradient for Test 16 is a result of the increased contribution of gravitational settling to the overall aerosol deposition rate.

8.      On pages 15-17, Figs. 5-7: the plots are cluttered; it would be good to separate each plot into five (5) subplots. This would make it easier for the reader to compare the model performance against the experiments. Also, why were intermediate velocities not selected in the simulations? FDS runs really fast; this reviewer's recommendation is to run few more cases with the intermediate velocities and report the FDS results with solid curves (along with symbols) for comparison against experimental data. It is also suggested that higher than 9 m/s and lower than 2 m/s velocities are included in the simulations to demonstrate the model's capabilities.

The creation of 15 separate subplots might inhibit the ability to see how the overall performance shifts with particle size. Sixteen FDS cases were run with the same scenario parameters as the Sippola Aerosol Deposition experiments. This allows for a direct comparison of the measured and predicted aerosol deposition velocities, and is part of the model validation process presented in this study.

If model simulations were run outside of the experimental parameter ranges (air velocities of ~2 m/s to ~9 m/s), that would be part of the model prediction process, which requires the propagation of model uncertainty to quantify the uncertainty of the model predictions. Therefore, this study focuses on an initial validation step of quantifying the performance aerosol deposition in nonreacting conditions (i.e., a direct comparison of measured and predicted aerosol deposition velocities).

9.      On page 19, Fig. 9: why are the concentrations at x = 0 different for the three cases? Wasn't 100 mg/m3 specified as the input? The assumption would be that the concentration at x = 0 was uniform.

An aerosol concentration of 100 mg/m3 was specified at the duct inlet, which includes 3 m of duct length (about 20 duct diameters) upstream of the instrumented duct section. Figure 9 indicates the relative impact of the different particle sizes and air velocities on the aerosol concentration along the instrumented section of the duct. The instrumented section of the duct is 3 m downstream of the inlet, and thus we are seeing effects of deposition and settling prior to the instrumented duct section. This clarification has been added to figure caption in the revised manuscript.

10.     On page 20, Fig. 10: why are the profiles not symmetric? Is velocity affected by aerosol deposition?

An error was identified in the grid cell output method for velocity from the FDS simulations, which was causing inaccurate velocity results to be plotted and resulted in the asymmetric velocity profiles. This error has been corrected by including the cell-centered velocity values, and the new velocity profile plot is included in the revised manuscript.  
  
Comments from the Editor in Chief:  
Thank you for considering Fire Technology for publication of your manuscript. It is a nice contributions. In your revised manuscript, please provide a detailed explanation of how you have addressed/rebutted each of the reviewers' comments and these comments too:

- I would expect that the related journal publications of Riahi et al. would also be cited with a higher importance than the PhD thesis, which is not gone thought peer review and citation cannot be tracked. This includes 10.1007/s10694-012-0273-x.

In the revised manuscript, the reference to Riahi’s work on soot deposition has been updated to reference the peer-reviewed version of his study.

- An additional recent paper that is related to this work would be: "Smoke Damage Potentials in Industrial Fire Applications" (10.1007/s10694-013-0358-1). Please consider including it.

This reference has been added to the revised manuscript as related/pertinent work.