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.  
  
  
  
  
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.

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. This has been clarified and references have been added to the revised manuscript.

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 in 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?

XX

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?

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.

XX

The increased aerosol concentration near the floor is most likely due to the effect of gravitational settling, which results in the asymmetric shape of the aerosol concentration gradient for Test 16 that demonstrates stratification of the aerosol near the duct floor. The increased aerosol deposition velocity in Test 16 (compared to Test 1) is due to the increased contribution of turbulent deposition and gravitational settling to the overall aerosol deposition rate. These clarifications have been added to the revised manuscript.

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.

XX

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), this is 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.

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. The measured aerosol concentrations were not given in the test reports; therefore, an aerosol concentration of 100 mg/m3 was specified at the inlet (upstream) duct boundary. Figure 9 indicates the relative impact of the different particle sizes and air velocities on the aerosol concentration in the duct.

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

- 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 work.