

Author's response to reviews

Title: Pediatric patient asthma-related emergency department visits and admissions in Washington, DC, from 2001-2004, and associations with air quality, socio-economic status and age group

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Response to Reviewer Holly Janes:

1. The reviewer says that the residuals in Figure 1 show a clear trend over time that looks like inadequately controlled seasonality.

In the previous review, this reviewer mentioned that they saw some structure that may indicate that the number of degrees of freedom was too small. In our previous response, we mentioned that we did look at adding degrees of freedom to the model, but found that this did not change the results. We agreed that the residual plot showed stationary noise but that this was due to changes in variance and not trending. We also mentioned how adding more degrees of freedom did not improve this distribution. Given this evidence and the fact that the other reviewers did not see a trend, it appears that any residual seasonality is likely insignificant.

2. The reviewer states that the description of the statistical model remains inadequate. The reviewer now suggests that the model controls for the seasonality of the outcome only, but not the pollution time series.

The selection of knots for our spline may have been based on the significant turning points in our outcome data, but note that there were 12 knots for 3 years of data, about 4 knots per year. In one of the plots in the original paper, one could see how these corresponded to meteorological seasons. In our paper, we mentioned how the peak ozone and PM occurred during the summer months while peaks in ED visits occurred in September-November and March-May. A comparison of the time series of the raw daily data showed that the ED visits went up when the pollutant concentrations went down. By removing seasonality in the ED visit data, we were able to adjust for environmental seasonality without minimizing peak ozone and PM concentrations that could have significant short-term health impacts.

In response to the reviewer's initial comments, we did add more detail to our description. We apologize if this seems inadequate to the reviewer. Our dataset for this pilot study was limited to 36 months of daily counts, substantially less than was used in some of the published studies, and our data contained missing values among some of the continuous predictor variables. We did consider using other models, but no model is perfect. We did find other published papers that used models very similar to ours. We chose a methodology that seemed justifiable for this dataset. Indeed, it is likely that the results would have not been significantly different had other models been used. We don't mean to imply that our method is superior. Every biostatistician has his or her favorite. The fact that our results are similar to those who used different statistical approaches further validates the conclusions of other studies of similar pollution effects.

3. The reviewer comments that we don't specify the question addressed by the SES analysis.

The goals of the EPHTP are not only to quantify the association between asthma exacerbations and air pollution, but to identify the populations and regions that may be more at risk. Our SES analysis was therefore addressing the latter. In the abstract, we stated that our goal was not only to quantify the associations of air pollution with asthma exacerbations, but also with SES and age group. Because we did not clearly reiterate this in the text of the paper, we have added some text to

the Background, Methods, and Results sections to make our scientific objectives and hypotheses more clear.