Preliminary Comments from Dr. H. Christopher Frey on EPA's Integrated Science Assessment for Sulfur Oxides – Health Criteria (External Review Draft – November 2015) 1/27/16

Comments on Chapter 3

Responses to Charge Questions:

a. **To what extent is the discussion on methodological considerations for exposure measurement and modeling clearly and accurately conveyed, appropriately characterized, and relevant to the review of the SO_2 NAAQS?**

EPA should consider developing a glossary of terms that are used not just in this ISA, but in multiple ISAs, to help enforce consistent and correct use of terminology. For example, the term "exposure" is used correctly in many places, and is correctly defined in the context of inhalation exposure in Equation 3-1. However, "exposure" is later used incorrectly to imply that somehow it accounts for inhalation rate related to activity levels. This implied definition of exposure is not consistent with earlier text. Estimation of the product of exposure concentration and inhalation rate would lead to a 'potential dose,' rather than an "exposure."

A glossary would be useful in keep track of various terms that are complex yet used often in the text. For example, the term "exposure error" is used, but it was difficult to find a good definition of this term in Chapter 3. The term was eventually explained to the reader, but it would help the reader if there was a "go to" place to find the definition, given its repeated usage. Other terms that merit definitions include but are not limited to "exposure bias," "classical error," "Berkson error," "minimum detected level," "ambient concentration," "exposure concentration," "microenvironment," "indoor," and others.

The draft Chapter 3 is somewhat difficult to read. It is not clear in many places. It is repetitive. It is not well organized not only because of repetition but because there is not a clear linear flow of information.

The introduction to the chapter should define more clearly the scope in terms of the exposure scenario that is addressed throughout. In general, exposure can be via multiple routes (inhalation, ingestion, and dermal contact) and for each route there can be multiple pathways. It would help the general reader if this was acknowledged and if a rationale was given as to why the focus of this document is only on the inhalation exposure route.

The scenarios related to exposure pathways are mentioned in passing throughout the chapter, which implies that it would help to have an upfront section that clearly specifies the exposure pathways of interest. These include point sources with elevated stacks (e.g., power plants). The text occasionally refers to near-ground level releases from vehicle sources. Perhaps there are others.

Some context regarding the relationship of the selected exposure routes and pathways for the purpose of health effects assessment pertaining to ambient SO_x, with respect to secondary pollutants such as sulfate, would be useful. It would be helpful to the reader to understand, for example, that the formation of

secondary sulfate and the direct and indirect human and ecological exposures related to sulfate are not in the scope of this document or review but are handled in other reviews, such as for the primary and secondary PM standard and the secondary NO_x and SO_x review.

The introduction to the chapter should also lay out the relationship between exposure modeling and the overall risk assessment methodology. For example, to the extent that dose-response relationships are based on controlled clinical studies, the exposure assessment should focus on estimating exposure concentrations. To the extent that the dose-response relationships are based on epidemiological studies that are predicated on fixed site monitoring data, then the exposure assessment would typically focus on ambient concentration as surrogate. This context is not clearly stated and would help the reader understand the scope of this chapter.

A general comment is that this draft chapter often contains assertions that should be supported by reference to literature, but no reference is given. See detailed comments for examples.

The text frequently refers to "SO_2" when it should instead be more clear and specific: e.g., SO_2 ambient concentration, SO_2 exposure concentration.

When referring to a concentration (e.g., ambient, exposure), it is critical to clearly communicate the averaging time regarding the concentration. Specification of the averaging time should be done consistently throughout.

For Section 3.2.1.2 the reader would be grateful for a summary table of the detection methods, their precision, accuracy, averaging time(s), detection limits, interferents, and so on. Also, an indication of their state of development (e.g., well established, demonstrated, in development) would be helpful.

Section 3.2.2 on modeling should be more clear regarding the distinction between modeling ambient concentration versus exposure concentration versus exposure.

Table 3-1 should be revised to more clearly distinguish models for ambient concentration versus models for exposure concentration versus models for exposure. Models for ambient concentration are not typically considered to be "exposure models" but rather they are used in situation for which ambient concentration is used as a surrogate for exposure.

Section 3.2.2.1 should, with better clarity, distinguish between estimated ambient concentration versus estimated exposure concentration.

The discussion of source proximity models begs the question about quantification of emissions. The document should be more clear about the source categories of interest, the elevation at which emissions are released (e.g., tall stacks for power plants, near ground level for vehicles, and so on), and provide context regarding the apportionment of SO2 emissions among source categories. For example, there should be a table that summarizes the National Emission Inventory for SO_2 at a national level to help the reader understand the average relative contribution of key source categories. Additional example(s) of localized inventories, such as in proximity to a particular central site monitor, would help provide insight regarding the significance of elevated sources, ground level sources, etc. For example, on page

3-7, lines 21-35, the reader wonders what are the key sources? What is the role of averaging time, spatial resolution, source classification, and completeness of the emission inventory?

The document should always be clear as to whether a reported concentration is measured or estimated using a model (and regarding the averaging time and spatial resolution of the reported concentration).

The comparison of models is a good start but needs further development. For example, in the context of page 3-9 as an example, there should be a more extensive and structured framework for comparing modeling approaches that takes into account factors such as what sources are considered, what receptors are considered, spatial scale, averaging time, goodness of fit metrics, and so on.

Gaussian plume models (e.g., on page 3-10) and Chemical Transport Models (page 3-11) require input data regarding emission sources. As noted above, the document currently lacks adequate information regarding SO_2 emissions inventories and regarding source characteristics that are relevant to either the input or interpretation of various modeling approaches. For example, even though LUR models may not require input data regarding emissions, they are based on emission surrogates and interpretation of their results requires knowledge of emissions.

The discussion of CTMs should be more structured. CTMs typically have four main components: (1) emissions, (2) chemistry, (3) meteorology, and (4) initial and boundary conditions.

The text should be clearer to distinguish between models, measurements, and reality. Sometimes statements are made as if ground truth was obtained from a model or measurement, when this may not be the case. As an example, on page 3-12, lines 27-28, a statement is made about cloud-based reactions as if these are known with absolute certainty, but instead it needs to be mentioned that the results are based on a model, not absolute truth.

The text should be careful when referring to “any” – e.g., “so that any inherent biases are understood.” It may not always be possible to exhaustively identify, much less understand, “any” (and presumably, all) sources of bias.

With regard to CTMs, the text should be more clear that grid-based models generally underestimate localized peak concentrations because of spatial averaging within a grid cell. One approach to deal with this, plume-in-grid, is mentioned. However, another approach is the used of nested grids, which is not mentioned but should be. Furthermore, the role of the height at which emissions are released should be treated more clearly in the document. On page 3-13, first paragraph, the text implies (perhaps unintentionally) that emissions released in the ground layer of a CTM grid are perhaps of more significance to exposure than those released in higher layers of the grid, such as from stacks of power plants. However, the text does not mention that coal power plants typically release far greater SO2 emissions than other sources (e.g., diesel vehicles) and that the higher point of release also leads to longer range transport and dispersion. Thus, the plume from a power plant can affect a larger downwind population than a plume from a vehicle. These types of distinctions are important when interpreting data from air quality monitors and the results from models, and thus merit discussion.

The text could state or infer implications of the review of modeling approaches with regard to model selection.

CHAD is an extremely important resource and merits a more focused and a bit more detailed treatment than what exists in this draft. CHAD is mentioned in several places, but could instead be reviewed in one part of the document to avoid repetition. Instead of repetition, the treatment of CHAD should be more specific. It would help to have a table that lists the key studies that comprise CHAD and their relevant characteristics – e.g., dates, number of individuals, number of diary days, demographic characteristics of the individuals, etc.

The use of newer methods for obtaining diary data is an important topic and should not be part of a run-on paragraph that includes other topics. However, it would be useful to report whether there are data now available from these newer techniques that would be useful in the current review of the SO2 NAAQS. It is not clear whether data are sufficiently established from these newer techniques to be relevant (e.g., for the REA).

The discussion of air exchange rates is too narrow and not well motivated or organized. Entirely missing is mention of databases of measured air exchange rates. The text could be more systematic in reviewing air exchange rates not just for "buildings" but for other enclosed microenvironments, such as vehicles. The text on page 3-14, lines 25-32 seems to describe a very complex model that arguably does not actually exist. No references are cited to support this text. Some statements in this paragraph are dubious. Making a model more complex does not always, in practice, "reduce the uncertainty" and to state this as if it is a truth is misleading. Adding more variables increases input data requirements, and each variable may have associated uncertainty. The more complex model may also have model uncertainty.

The role of "microenvironmental exposure models" in the last review of the SO_2 NAAQS is mentioned only in passing. To the extent that models such as APEX and SHEDS are likely to be relevant to the current review, they merit more treatment than given in this draft. These are also referred to as "stochastic population-based models."

A statement is made that models such as SHEDS or APEX "are not used for exposure assessment in epidemiologic studies." While this is typically the case, the statement as given is false. There are some examples in which models such as SHEDS and APEX have been used to evaluate the utility of estimated exposure estimates, rather than fixed site monitoring or CTM estimates, in explaining variability in adverse effect endpoints. Much of this work has been done for PM rather than SO_2, but methodologically these PM examples are relevant. Two examples are Chang *et al*. (2012) and Mannshardt *et al.* (2013). There are other examples.

On page 3-15, the text states that "the state of science for stochastic population exposure models has not changed substantially since the 2008 ISA for Oxides of Nitrogen". This statement is very puzzling. APEX was used extensively in the most recent review of the O_3 NAAQS, and various updates to the input data and the model were incorporated during this more recent review process.

Similar to the comments on CTMs, the discussion of stochastic population based exposure models can be more clearly structured. The key inputs to these models include ambient concentration, population demographic data (i.e. location of population and distribution of population by census tract), activity diary data, and data regarding infiltration to enclosed microenvironments. The latter can depend also on

housing stock. Each of these merits discussion, but to the extent a topic is already treated in detail earlier in the document, that material can be cross-referenced without repetition.

The term "indoor" is a bit confusing in that it does not connote in-vehicle microenvironments but apparently is defined to include these.

There is no transition from

The overall organization of this chapter is hard to follow.

b. Please comment on the accuracy, level of detail, and clarity of the discussion regarding exposure assessment and the influence of exposure error on effect estimates in epidemiologic studies of the health effects of SO_2.

This charge questions appears to be aimed especially at Section 3.3 "Exposure Assessment and Epidemiologic Inference" and its constituent parts, including 3.3.5 "Implications for Epidemiological Studies of Different Designs". The title of this section is awkward. This section is itself awkward and hard to follow at times.

A glossary would help the reader with this section of the document, since many key terms are used frequently.

The content was generally reasonable, but the writing could be improved.

Section 3.3.1 defines exposure. As such, this material belongs earlier in the document.

Section 3.3.2 focuses on infiltration and as such could be combined with an earlier section that brings up this topic. The treatment of the infiltration factor would be improved by systematically dealing with evidence for Finf and with evidence separately and individually (to the extent such evidence exists) for P, a, and k. In particular, k is barely treated in this chapter but is quite significant for SO_2.

Section 3.3.2.2 the term "Personal-Ambient Relationships" is unclear.

Section 3.3.3.1 on "Activity Patterns" co-mingles two different issues. One is the amount of time spent in each microenvironment. The other is the activity level of an individual at a given time or location. These should be treated separately. Furthermore, the "level of activity" is not given proper context. Level of activity affects inhalation rate. The product of exposure concentration and inhalation rate is a potential dose. Up to this point, no modeling approaches have been discussed that attempt to estimate potential dose. Thus, the context of introducing "level of activity" is not very clear. It is also very misleading to imply that "activity pattern" includes both time in microenvironment and "level of activity" – i.e. it is not correct to state that Equation 3-3 includes both of these.

To the extent that the ISA should address level of activity to help provide insight regarding relationships between exposure concentration and adverse effect, then the ISA should treat this topic separately and more clearly. Inhalation rate is related to metabolic rate, and there are some modeling approaches that

are predicated on metabolic rate (e.g., see work of Tom McCurdy and others) that could be mentioned here. The concept of "potential dose" should be introduced. Potential dose may be a more accurate surrogate of adverse effect than exposure concentration. However, there can be errors in estimation of "level of activity" and inhalation rate such that an attempt to model the sensitivity of inhalation rate to level of activity can potentially introduce both random and systematic errors to an analysis.

Figure 3-1 is a very important figure, but the reader wonders whether this can be updated with the most recent version of CHAD and whether there are diary data in CHAD more recent than the 2001 publication date of the figure. My understanding is that CHAD does include more recent diary data. There have been some questions, for example, in prior CASAC reviews of other criteria pollutants as to whether activity patterns may be changing as a result of changes in lifestyle.

On Page 3-39, it is stated that "exertion level is an important determinant of exposure." This is not correct: exertion level is an important determinant of potential dose, but would not affect exposure concentration.

Page 3-41: CHAD is mentioned again. Instead, have one place in the chapter that focuses on CHAD and either cross-reference to that or augment it only as needed in other places. The information given here is quite out of date. For example, the number of diaries now in CHAD may be larger than mentioned here.

Exposure error is mentioned in many places. The reader would appreciate one clearly indicated section in which exposure error is addressed thoroughly. Other sections of the chapter could cross-reference to this. In general, repetition should be avoided. Exposure error is mentioned many times but a deeper attempt to define it does not appear until late in the document.

The summary was not very helpful. Less repetition and more integration/synthesis would be helpful.

Other Comments

Page 3-3, lines 21-38: some context regarding these measurement methods is needed. Which have actually been used in practice? Which are methods that are in development? The paragraph reads a bit like a laundry list and would benefit from more discussion of each technology raised and its context (e.g., microelectrodes using nanomaterials – are these actually deployed in use, are they in development but not yet demonstrated, etc.).

Page 3-8, line 7: "it" is unclear… LUR?
Page 3-8, first paragraph – please be more clear about averaging times.
Page 3-8, line 23, please specify the key source categories that are "industrial" sources.
Page 3-8, 34-35: please indicate the mean concentration so that the reader can put these numbers into context.
Page 3-8, line 37: when reporting that a distribution is skewed, it would be helpful to give supporting information such as the coefficient of variation and the skewness coefficient.

Page 3-9, lines 18-19: This statement is unclear with regard to epistemic foundation. Is the finding that SO concentration "was not influenced by" the sources mentioned based on a model or based on statistical analysis of measurements, or other?

Page 3-9, line 24: "when diesel fuel still contained sulfur" is a very odd statement. Diesel fuel continues to contain sulfur now. However, the allowable maximum sulfur content has changed. The ISA should be much more clear regarding what the allowable sulfur content was and is for highway diesel and nonroad diesel, and the implications of this. As noted in an earlier comment, there should be a section of this chapter that deals with the emission inventory.

Page 3-10, line 16: bicubic spline functions *for what*?

Page 3-10, line 19: IQR *of what*?

Page 3-10, line 33: "industrial/vehicle" is unclear.

Page 3-11, lines 3-4: the reported correlation with vehicle emissions requires some more explanation and interpretation. For example, were these onroad diesel or nonroad diesel vehicles? Where there some location-specific features that lead to the estimated correlation. It is also not very clear as to exactly what was correlated here and the averaging time that was the basis for the correlation.

Page 3-11, line 36: what averaging time?

Page 3-12, line 35: "vertical emissions" is an awkward term and not one that I have seen before. Perhaps this refers to emissions released at an elevated height from a stack. The meaning should be more clear.

Page 3-13, lines 21-22: This may be true, but what is the basis for this claim? Also, define "microenvironment" and provide support from the literature as to why time spent in microenvironments is important. For SO_2, what is known from prior work regarding the relative importance of microenvironments? If there are not good examples for SO_2, then give examples for other criterial pollutant(s) to at least motivate the importance of this topic. For example, explain how does microenvironmental concentration differ by microenvironment? An example or two would help.

Page 3-14, line 15: "could be"? is this actually being done?

Page 3-16, first paragraph: no references are cited here, but should be. This text cites lengthy tables, but lacks discussion or interpretation of these tables. Such discussion and interpretation should be included.

Table 3-2: consistently identify the averaging time and spatial scale for each approach. There is room to spell out the model names that are abbreviated in the first column. The term "exposure bias" is used in some of the commentary but this term is not clearly defined. How it is different from "exposure error"? The table should also cite references for examples of these approaches. The table should also be more clear as to whether the approach is aimed at ambient concentration (environmental concentration), versus exposure concentration, versus exposure.

Microenvironmental models are not applicable only to panel studies – see comment above about their role in epidemiological studies.

Table 3-3: please define/explain the difference between "local," "urban," and "regional." The column on exposure model type seems rather complicated but why not more clearly define the model types (i.e. in Table 3-2) and then use a more terse notation here that is also more clear.

Page 3-26, line 5: "this framework" – there is no prior framework given, so the antecedent here is undefined.

Page 3-26, line 17: be clear as to whether "indoor" includes all enclosed microenvironments such as vehicles.

Equation 3-3: check if the last term is $C_{o,i}$ or should it be C_o?

Equation 3-4 – the discussion here should be clearer that in this case C_o is the outdoor concentration in close proximity to the enclosed microenvironment or to the point of outdoor exposure.

Page 3-28, last paragraph. This paragraph is awkwardly worded and hard to follow. It is also seems to be inaccurate. Line 22 is particularly confusing. A negative concentration is not physically possible and cannot be explained with respect to a detection limit.

Page 3-29, line 13: how do personal activities affect air exchange rate? This may be possible but is not explained and, therefore, is unclear.

Page 3-36, first paragraph. The studies by Sarnat et al. that are cited here are not listed in Table 3-4. It is not clear as to why they do not appear in the table. What is the implication of the information in this paragraph for the ratio of indoor to outdoor concentration? Line 15: "noise" (random error?) in measurements would reduce the magnitude of the correlation between measured concentration and ambient concentration, but this point is not clear.

Page 3-36, second paragraph – is this about the ratio of indoor to outdoor concentration?

Page 3-37, line 2: "large" relative to the mean measured concentration? Or "large" in some absolute sense?

Page 37, Section 3.3.3, first paragraph, there are a lot of issues with this paragraph in terms of lack of clarity. For example, lines 8-9, it is not clear what is really meant here… does this intend to refer to the idea that variability in concentration to which people are exposed, even in locations near the monitor, compared to the measured concentration at the monitor, can differ? This could be clearer. Line 14: "indoor reaction rate" – this issue has not been addressed and needs to be – at least a paragraph at an appropriate location in the document. Lines 16-18: "dose" is mentioned here but the distinction between exposure and dose has not been clearly stated. Also, what is meant by "extent" of exposure (define in glossary). This sentence, however, seems out of place here.

Page 3-40, line 6: what are "minute ventilations"? why are no references cited in this paragraph?

Page 3-40, lines 8-14. References need to be cited in this paragraph. The term "gender" appears to be understood to refer to gender identity, and thus is not the right term to use here. Che et al. (2014) analyzed the effects of different diary sampling methods on exposure estimates and this work may be relevant here.

Page 3-41, section 3.3.3.2. Text here is often unclear, and lacks adequate citations to literature.

Page 3-42, line 7: "for SO_2" – this is unclear. Does this refer to SO_2 *ambient concentration*? Goodness of fit with respect to what?

Page 3-42, last paragraph – this is ahrd to read. How can *pollutants* have error? Error in what, specifically? (pollutant concentration?). "Bias in SO_2" .. bias in what, exactly? Bias in the estimated relationship of health effect to ambient concentration? This paragraph needs a rewrite to remove awkward phrases, and to be more clear and precise regarding the basis of statements made.

Page 3-43: distance to a monitor is useful but not sufficient. For example, downwind location of plume "touchdown" would depend also on stack weight, wind speed, wind direction, atmospheric stability

Figure 3-2 "from to" in caption is confusing.

Page 3-47: Variations among seasons is not exactly the same as "temporal variability". "Temporal variability" could imply hourly, diurnal, daily, weekly, etc. Seasonal is only one possible interpretation of this term. Inter-annual variability is probably not considered by many to be the same as "temporal variability" Line 18 "the two methods"? Prior text does not clearly define and identify each of two methods, so the antecedent here is unclear.

Page 3-48: Section 3.3.3.3 - the reader wonders why we have another section on infiltration since there was a prior section on infiltration.

Page 3-48, section 3.3.3.4 – "moderately correlated" needs to be defined quantitatively to avoid being vague. The discussion of implications of data below detection limit was also discussed quite a bit in the review of the draft ISA in the recent NOx review. Please be sure that the treatment of this issue is consistent between the SOx and NOx ISAs.

Page 49: up to 3.3.4 text here is sloppy. Concentration is not the same as exposure. Instrument error is not only because of interference, although this may be one type of error that is important in some cases. The text should distinguish between relative versus absolute errors as appropriate. The text lacks adequate citations to literature.

Page 49, Section 3.3.4 – "confounding" needs to be defined. The notion of effect modifiers should also be introduced and discussed. A clearer distinction between confounders and effect modifiers would be useful.

Page 50 – first paragraph, text switches between ambient concentration, exposure concentration, exposure, and effects – this is very confusing and these distinctions are not clearly made or well organized.

Page 50 – second paragraph: no references are cited (!). could write more about mechanistic expectation of collinearity or correlation, in terms of emissions, atmospheric processes. Averaging times need to be clearer.

Also, missing are some complex inter-relationships. For example, coal-fired power plants emit both SO_2 and NO_x in often significant quantities. Thus, there is a common general exposure pathway to both of these pollutants, although the specifics differ in terms of atmospheric transformations. On the other hand, SO2 and NOx may be emitted also from vehicles, although the SO2 emissions from vehicles are usually small in magnitude.

Page 3-51: a correlation of 0.41 is described as "moderate" here, calling into question the meaning of other statements about moderate correlations that are unquantified elsewhere in the document. A correlation of 041 might be considered by some to be "weak." Thus, definition of the meaning of these terms will help both the authors and the readers.

Page 3-51, lines 14-21: seems like this isn't likely to be adequately interpreted – e.g., howmuch of the data were below detection limits? What are the confidence intervals on the correlations? (Were the correlations statistically significant?).

Page 3-51, line 27: no basis is given (no reference is cited).

Table 3-9: Please clarify the averaging time – is this is what is meant by "duration"? What was the time frame of each of these studies? E.g., if they are based on daily averaging times, how many days of data were used? (or indicate sample size and the meaning of a sample – e.g., n=365 daily averages). To the extent possible, please indicate if reported correlations are statistically significant. Some insight regarding the proportion of data below detection limit would be helpful.

Page 3-58, section 3.3.4.2: what are the implications and relevance of this information?

Page 3-58, Section 3.3.5: Implication of *what* for epidemiologic studies? Here, we finally get a definition of exposure error and its components. Yet, some text here seems repetitive of earlier text. References should be cited for classical and Berkson error. More clarity on errors related to bias versus errors related to imprecision would be helpful.

Page 3-62, Table 3-10: This table is rather difficult to decipher. This appears to be a case study based on one paper. The meaning of the rows and columns should be clearer. For the columns, please include text descriptors of what is the intended purpose or meaning of each of these metrics. The rows labeled as averages are confusing- average of what for what location and time period? This table is not self-explanatory, but it should be. If all of this is model-based, what really is the point of including this in so much detail in the ISA?

Page 3-63, line 34: SO_2 is not a nonreactive pollutant.

Page 3-65, line 17: were these simulations for a reactive air pollutant? If not, how are these simulations "relevant to SO_2"? Not clear.

Page 3-66, line 4: clarify that the lack of spatial variability does not imply lack of variation over time.

Page 3-66, section 3.3.5.3: With regard to the first paragraph, while it is the case that stochastic population based models are "not designed" for comparison with panel studies, this does not mean that comparisons cannot be made. For example, comparisons could be made for the frequency distributions predicted by the model versus the frequency distribution measured in the panel study for the same averaging time, time period, and geographic scope. Such a comparison will be subject to imprecision. For example, the confidence intervals of the empirical frequency distribution from the panel study could be estimated using bootstrap techniques.

Section 3.4:
Page 3-69, line 12 – this is not correct.
Page 3-69, lines 18-27. Sloppy use of "SO_2" when the correct meaning may be "SO2 ambient concentration"

References Cited

Chang, H.H., M. Fuentes, and H. C. Frey, "Time Series Analysis of Personal Exposure to Ambient Air Pollution and Mortality Using an Exposure Simulator," Journal of Exposure Science and Environmental Epidemiology, 22, 483-488 (2012) doi:10.1038/jes.2012.53. Sept/Oct 2012.

Che, W., H.C. Frey, and A. Lau, "Assessment of the effect of population and diary sampling methods on estimation of school-age children exposure to fine particles," Risk Analysis, 34(12):2066-2079 (December 2014), DOI: 10.1111/risa.12238

Che, W., H.C. Frey, and A.K.H. Lau, "Comparison of Sources of Variability in School Age Children Exposure to Ambient $PM_{2.5}$," Environmental Science and Technology, 2015, 49(3):1511–1520

Mannshardt, E., K. Sucic, W. Jiao, F. Dominici, H.C. Frey, B. Reich, and M. Fuentes, Comparing Exposure Metrics for the Effects of Fine Particulate Matter on Emergency Hospital Admissions, Journal of Exposure Science and Environmental Epidemiology, 23:627-636 (doi:10.1038/jes.2013.39). (2013)