Preliminary Comments from Members of the CASAC Sulfur Oxides Panel on EPA's Integrated Science Assessment for Sulfur Oxides – Health Criteria (External Review Draft – November 2015)

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Comments on Chapter 2

Overall, Chapter 2 is informative and well-written. The responses to the three charge questions below point to areas that need additional verification with references provided at the end.

To what extent is the information presented regarding sources, chemistry, and measurement and modeling of ambient concentrations accurate, complete, and relevant to the review of the SO2 NAAQS?

Section 2.2 Sources of Sulfur Dioxide (Page 2-1)

Gaseous SO_x needs to be better defined. Besides S_2O, SO_3, and H_2SO_4 (Lines 7-8, Page 2-1), it should also include sulfur monoxide (SO; later acknowledged on Line 1, Page 2-18), SO_2, thiosulfate (S_2O_3), and heptoxide (S_2O_7). As atmospheric sulfur budgets are mainly composed of SO_2 and sulfate ($SO_4^{=}$), both gas and particle phases should be acknowledged.

Source categories in Figure 2-1 need to be clarified (labels should be added to both axes). Many of the source subcategories are not shown on the x-axis. What is the difference between the "Industrial Other" and "Industrial" subcategories? Does "Boilers-Oil" represent fuel-oil combustion (one would expect higher SO_2 emissions by residual oil combustion)? What source category covers smelter emissions? Figure 2-1 emphasizes the importance of coal combustion, especially the electrical generating unit (EGU) fraction, as the major source of U.S. SO_2 emissions. Since many of these source subcategory emissions are low compared to EGU, it may be more informative to replace this with a table containing the major sources from the 2011 National Emission Inventory (NEI; U.S.EPA, 2013a).

Do the counties (yellow-outline) in Figures 2-2 to 2-4 represent counties with a single 1-hr SO_2 concentration exceeding 75 ppb or the 99th percentile SO_2 concentrations and for what time periods? Emphasis is given to cement production as well as industrial chemical and allied products manufacturing, without acknowledging the smelter sources. Chemical emissions should be isolated in Figure 1 to correspond with these plots. What about a map showing locations and sizes of commercial marines, which is the third largest emission category in Figure 1?

"Major emitting facility" was defined as >100 tons/yr (Lines 4-5, Page 2-7), while Figure 2-4 plots facilities with >1,000 tons/yr SO_2 emissions, also calling them "major." Is this a mistake, or are the sources illustrated in Figure 2-4 a subset of >100 tons/yr facilities? Janoe and Craft (2015) note a 2,000 tons/year threshold for the 2013 attainment designations. For the consent decree, the limits are 2,600 tons/yr SO_2 with an annual average emission rate of at least 0.45 lb

per million BTUs or >16,000 tons/yr for a single plant. Figure 2-4 emission rate categories bracketing these ranges would be informative.

In Figure 2-5 it would also be useful to add years when the on-road and off-road diesel standards for sulfur content and when the North American Emission Control Areas for shipping were implemented.

Section 2.3 Atmospheric Chemistry and Fate (Page 2-18)

Figure 2-10 (Page 2-23) should reference Figure 7.19 of Seinfeld and Pandis, (2006), as the reference (Seinfeld and Pandis, 1998) cited is out or print. What are losses due to dry and wet deposition? Dry deposition of SO_2 is much larger than that of $SO_4^=$ (Clarke et al., 1997; Shadwick and Sickles, 2004; Sickles and Shadwick, 2007), while reacted $SO_4^=$ may be removed through occult deposition of the large fog droplets (Dollard et al., 1983; Lillis et al., 1999; Pandis and Seinfeld, 1989).

Section 2.4 Measurement Methods (Page 2-24)

A list of available Federal Reference Methods (FRMs), Federal Equivalence Methods (FEMs), and related citations (e.g., Blacker et al., 1973; Kok et al., 1989; Luke, 1997; Stelson and Bao, 1988; Trieff et al., 1968; West and Gaeke, 1956; Wright et al., 1989) should be given with potential interferences. A summary of SO_2 measurement comparison studies (e.g., Ferek et al., 1997; Kim and Kim, 2001; Leppanen et al., 2005; Luke, 1997; Medina et al., 2011; Terraglio and Manganelli, 1962; Vadjic et al., 1992) and potential reasons for discrepancies would be helpful. This would be important for determining the equivalence between standard monitors (1-hr averages) and new trace monitors (5-min averages with lower detection limit [LDL] <0.2 ppb) as well as within a given instrument type. It would also be helpful to show a time series of these comparisons and verify similarities and differences by season and time of day.

The performance specifications in Table 2-4 (Page 2-25) lists an LDL of 2 ppb, zero drift of 4 ppb, and interference of 5 ppb. These levels seem high given low ambient 1-hour average SO_2 concentrations of 4 ppb (Lines 12-14, Page 2-35) or 1-3 ppb for the 50th percentile national statistics during 2010-2012 shown in Table 2-7 (Page 2-37). A comparison between continuous SO_2 measurements and weekly averaged Clean Air Status and Trends Network (CASTNET) filter pack or other commercially integrated samplers should be provided to give some perspective on performance for low (<5 ppb) ambient SO_2 concentrations and their comparability for annual averages (Bennett et al., 1994; Bytnerowicz et al., 2002; Chow et al., 1993; Sickles et al., 1999; Sickles and Shadwick, 2002). As there are ~90 CASTNET sites that measure SO_2, it would be good to add the CASTNET monitors to the AQS monitors shown in Figure 2-11 (Page 2-30).

The number of sites with collocated standard monitor (1-hr) and trace monitor (5-min) should also be specified. Not much information was given regarding the relationship between collocated 1-hr (standard monitor) and 5-min (trace monitor) SO_2 concentrations. Only 6 out of the 42 sites in the six core-based statistical area (CBSA)/metropolitan areas have 5-min SO_2 measurements. As different AQS monitor IDs are given for standard versus trace monitors (Table 2-8, Pages 2-47 and 2-48), it is difficult to evaluate their comparability and collocated precision.

The number of SO_2 sites should be clarified. The introduction in Section 2.4.4 shows 400 monitors reporting 1-hr SO_2 concentrations to EPA's Air Quality System (AQS), which includes 195 monitors in the SLAMS (state and local air monitoring stations) network that report 5-min average SO_2 concentrations in 2012 (Lines 12-13, Page 2-29). These numbers differ from those shown in Table 2-6 (Page 2-36) with 337 monitors reporting 1-hr data and 309 reporting 5-min data for the period of 2010-2012.

Other SO_2 measurement methods (Section 2.4.3, Page 2-27) might include Fourier transform infrared spectroscopy (FTIR) (Horrocks et al., 2001) and microsensors (Do and Chen, 2007; Dye et al., 2014; Ohira and Toda, 2005; Roberts et al., 2014; Toda et al., 1998; U.S.EPA, 2013b). Less costly and more portable measurements for criteria pollutants are of growing interest for community involvement and exposure assessment (Dye et al., 2014; Snyder et al., 2013; Wang and Brauer, 2014). The method for treating values below LDL in annual average calculations needs to be specified (Kushner, 1976).

As there are a variety of solid-state options now available to replace the photomultiplier tube (PMT), the "PMT" (Line 28, Page 2-24) might be changed to "detector." Section 2.4.4.2 (Page 2-32) might mention the importance of using the proper material and seasoning the sampling probes (Mamane and Donagi, 1976; Wohlers et al., 1977).

<u>Section 2.5 Environmental Concentrations (2-34)</u>

Table 2-7 (Page 2-37) should specify how the mean (arithmetic average?) was calculated when many measurements were below LDLs. Does Figure 2-13 (Page 2-39), the map of the 99th percentile of daily 24-hour average SO_2 concentrations reported at AQS monitoring sites during 2010−2012, include both 5-min and 1-hr SO_2? Figures 2-12 and 2-13 (Page 2-38 and 2-39) might include a category of 75-140 ppb, since 140 ppb was the 1971 24-hr SO_2 NAAQS limit. Why are <13 ppb and >108 ppb used to limit the categories in Figure 2-13? It seems like rounder numbers such as 15 and 110 ppb would be more logical.

<u>Section 2.6 Atmospheric Modeling (Page 2-85)</u>

The purpose of applying dispersion modeling to estimate SO_2 concentrations needs to be clarified. Was the dispersion modeling to be used for informing attainment designation decisions (75 FR 35520) or for compliance with NAAQS? Will modeling efforts focus on: 1) location

where monitoring is not practical or sufficient; 2) areas with large point sources (e.g., areas >2,000 tons/yr or with major coal-fuel power plant with >16,000 tons/year, Hamel, 2015); or 3) areas where SO_2 is above the NAAQS? Examples of CMAQ or AERMOD derived 99th percentile 1-hour SO_2 concentrations compared to measured ambient SO_2 concentrations should be given to illustrate how well modeling results can be used to determine attainment.

Can a dispersion model represent the zones defined in Figures 2-14 to 2-19 (Pages 2-41 to 2-46)? A map denoting the comparison between modeled and measured ambient SO_2 concentrations would shed light on the representativeness of dispersion modeling to catch plume touchdown and the adequacy of the number of monitor sites within 15 km of major SO_2 sources.

A discussion of some of the AERMOD options and comparison with SO_2 and inert tracer data would be informative about how well the modeled distributions represent the measured distributions (Frost, 2014; Guerra et al., 2014; Hanna et al., 2001; Irwin, 2014; Isakov et al., 2007; Paine et al., 2015; Rehbein et al., 2014; Rood, 2014).

What is the extent to which the analyses of recent available 5-min SO_2 concentration data are informative in considering relationships between 5-min and 1-hr SO_2 concentrations?

<u>Section 2.5.2 Spatial Variability (Page 2-35)</u>

Much can be learned from the temporal and spatial analyses of past studies of SO_2 and $SO_4^{=}$ (e.g., Blanchard et al., 2013b, a; Hand et al., 2012; Hidy et al., 1978; Husain et al., 1998; Husain et al., 2004; Malm et al., 2002).

The definition of "near the source of origin" (Line 8, Page 2-31) should be clarified. This implies a micro- (<100 m) to middle-scale (100-500 m) zone of influence. Plume touchdowns from tall stacks may occur several kilometers downwind of the emission point. Is there a reference to the population weighted emissions index (PWEI) estimates described (Lines 12-15, Page 2-31)? If this combines population densities with emissions in the same CBSA, it may give a biased estimate of exposure, especially in the eastern U.S. where county areas are small. A better approach (and possibly the one used here) would combine modeled and measured SO_2 concentrations with population densities (e.g., Zou et al., 2009).

The explanation and purpose of Figures 2-14 through 2-19 (Pages 2-41 to 2-46) aren't very clear. What do the "buffer zones" represent? Define what the letters and circles mean. With a SO_2 reaction rate of a few percent per hour (Calvert and Stockwell, 1983), and wind speeds aloft that are often equal or exceed 15 km/hr at the exit of a tall stack, there is probably ample SO_2 left in the plume to touch down far beyond 15 km. Since SO_2 monitors within 15 km of large point sources are expected to experience the greatest emission impact, the source zones of influence within 5, 10, and 15 km circled in the selected six CBSA/metropolitan areas (Figures 2-14 to 2-

19) should be plotted based on major NEI facilities rather than centered on ambient SO_2 monitors. Maps of spatial variation of a 3-yr average of 1-hr SO_2 variations at the 99th percentile based on existing AQS monitors for the U.S. and for each selected CBSA/metropolitan area would provide inside on spatial variation of SO_2 concentrations.

One would not expect high correlations among monitors that detect plume touchdown as a high level at one location means the relatively narrow plume is not being measured at another location. This is largely confirmed in Figured 2-20 and 2-22 (Pages 2-50 and 2-52). Higher correlations would imply some accumulation of SO_2 within the mixed layer, which would be evident in a diurnal time series plot. Comparison of frequency distributions in Figures 2-21 and 2-23 (Pages 2-51 and 2-53) provide more realistic estimates of exposure likelihood. Figures 2-26 and 2-27 (Pages 2-59 and 2-60) show evidence of touchdown as surface layers couple to layers aloft a few hours after sunrise.

Note that Payson and Phoenix, AZ, represent two different airsheds separated by terrain and should be treated separately. The Payson measurements in Table 2-8, with 50th percentile SO_2 of 50 ppb that is one to two orders of magnitude lower than the 99th percentile (295 ppb) and the 1-hr maximum (1,501 ppb), serves as a good example for a case study. The distance between Site D and the Payson copper smelter should be given. More spatial/temporal analysis on wind direction/wind speed along with dispersion modeling that associates ambient SO_2 concentrations with the location of plume touchdown should be illustrated to differentiate maximum 5-min or 1-hr SO_2 concentrations from the 99th percentile concentration.

It is difficult to interpret the spatial concentrations exhibited in Figures 2-20 to 2-23 (Pages 2-50 to 2-53). Were the corresponding 24-hr average SO_2 concentrations between monitors used for Pearson correlations? One would expect a lack of correlation as plume touchdown at one site precludes the peak concentration at another. Diurnal variations of 5-min SO_2 concentrations can be used to demonstrate plume touchdown and downwind mixing.

<u>Section 2.5.3 Temporal Variability (Page 2-55)</u>

Ambient concentrations in Figure 2-24 (Page 2-55) should match the emissions trend in Figure 2-5 (Page 2-9) for the period of 1990-2013. If data is available, Figure 2-24 needs to be updated for SO_2 to include the longer period from 1980-2014/15 to demonstrate the trend of decreasing SO_2 over the past 35 years.

As emission reductions can be related to the decline of both SO_2 and $SO_4^{=}$ concentrations, a map showing sites with collocated SO_2 and $SO_4^{=}$ measurements (e.g., from IMPROVE and CSN sites) could be used to illustrate the temporal and spatial variations. Hand et al (2012) demonstrates the decline of SO_2 and $SO_4^{=}$ concentrations (in percent per year) in the U.S. for the period of 2001-2010. Diurnal variations of SO_2 and $SO_4^{=}$ with temperature can be used to illustrate atmospheric oxidation and/or local cold pools. Diel variabilities shown in Figures 2-26

and 2-27 (Pages 2-59 and 2-60) may be more descriptive if summer versus winter or warm versus cold seasons are shown separately. Earlier plume touchdown with higher ambient SO_2 concentration is expected during summer, as vertical mixing is enhanced under warmer temperatures.

Time series distribution in Figures 2-29 and 2-31 (Pages 2-71 and 2-73) is a better way to characterize exposure than correlations. The same plots should be generated for 99th percentile 1-hr SO_2 concentrations that correspond to 75 ppb to illustrate the frequency of exceeded 1-hr SO_2 NAAQS.

What emission was used in Figure 2-32 (Page 2-77) to estimate policy-relevant background (PRB) SO_2 using the MOZART model? What is the representativeness of the maximum PRB of 0.03 ppb SO_2 based on modeling with 2001 meteorological data? This level is one to two orders of magnitude lower than the LDL for SO_2 measurements.

How informative is the analysis of correlations between SO_2 and co-occurring pollutant concentrations for interpretation of epidemiologic studies?

Section 2.5.5 Copollutant- Correlations

Chapter 2 focuses on a single pollutant with only 5 pages designated to show Pearson correlations with other co-pollutants. It is not clear how many sites were included in Figures 2-35 to 2-38 (Pages 2-81 to 2-84) to address correlations between SO_2 and other co-occurring pollutants. Individual sites and date and/or time that show a correlation coefficient higher than 0.8 or 0.9 may warrant additional analysis. The possibility that SO_2 is more correlated with CO and average $PM_{2.5}$ (rather than $PM_{2.5}$ sulfur) needs to be explained.

References

Bennett, R.L., Stockburger, L., Barnes, H.M., (1994). Comparison of sulfur measurements from a regional fine particle network with concurrent ACID MODES network results. Atmospheric Environment 28, 409-419.

Blacker, J.H., Confer, R.G., Brief, R.S., (1973). Evaluation of the reference method for determination of sulfur dioxide in the atmosphere (Pararosaniline method). JAPCA 23, 525-527.

Blanchard, C.L., Hidy, G.M., Tanenbaum, S., Edgerton, E.S., Hartsell, B.E., (2013a). The Southeastern Aerosol Research and Characterization (SEARCH) study: Temporal trends in gas and PM concentrations and composition, 1999-2010. Journal of the Air & Waste Management Association 63, 247-259. http://www.tandfonline.com/doi/pdf/10.1080/10962247.2012.748523

Blanchard, C.L., Hidy, G.M., Tanenbaum, S., Edgerton, E.S., Hartsell, B.E., (2013b). The Southeastern Aerosol Research and Characterization (SEARCH) study: Spatial variations and chemical climatology, 1999-2010. Journal of the Air & Waste Management Association 63, 260-275. http://www.tandfonline.com/doi/pdf/10.1080/10962247.2012.749816

Bytnerowicz, A., Tausz, M., Alonso, R., Jones, D., Johnson, R., Grulke, N., (2002). Summer-time distribution of air pollutants in Sequoia National Park, California. Environ. Poll 118, 187-203.

Calvert, J.G., Stockwell, W.R., (1983). Acid generation in the troposphere by gas-phase chemistry. Environ. Sci. Technol 17, 428A-443A.

Chow, J.C., Watson, J.G., Bowen, J.L., Frazier, C.A., Gertler, A.W., Fung, K.K., Landis, D., Ashbaugh, L.L., (1993). A sampling system for reactive species in the western United States, in: Winegar, E.D., Keith, L.H. (Eds.), Sampling and Analysis of Airborne Pollutants. Lewis Publishers, Ann Arbor, MI, pp. 209-228.

Clarke, J.F., Edgerton, E.S., Martin, B.E., (1997). Dry deposition calculations for the Clean Air Status and Trends Network. Atmospheric Environment 31, 3667-3678.

Do, J.S., Chen, P., (2007). Amperometric sensor array for NOx, CO, O2 and SO2 detection. Sensors and Actuators B-Chemical 122, 165-173.

Dollard, G.J., Unsworth, M.H., Harve, M.J., (1983). Pollutant transfer in upland regions by occult precipitation. Nature 302, 241-243.

Dye, T.S., Gordon, B.J., Roberts, P.T., (2014). Air sensors: Quality data for the right application. EM, 22-27.

Ferek, R.J., Covert, P.A., Luke, W., (1997). Intercomparison of measurements of sulfur dioxide in ambient air by carbonate-impregnated filters and Teco pulsed-fluorescence analyzers. J. Geophys. Res 102, 16267-16272.

Frost, K.D., (2014). AERMOD performance evaluation for three coal-fired electrical generating units in Southwest Indiana. Journal of the Air & Waste Management Association 64, 280-290.

Guerra, S.A., Olsen, S.R., Anderson, J.J., (2014). Evaluation of the SO2 and NOX offset ratio method to account for secondary PM2.5 formation. Journal of the Air & Waste Management Association 64, 265-271.

Hand, J.L., Schichtel, B.A., Malm, W.C., Pitchford, M.L., (2012). Particulate sulfate ion concentration and SO2 emission trends in the United States from the early 1990s through 2010. Atmos. Chem. Phys 12, 10353-10365.

Hanna, S.R., Egan, B.A., Purdum, J., (2001). Evaluation of the ADMS, AERMOD, and ISC3 dispersion models with the OPTEX, Duke Forest, Kincaid, Indianapolis and Lovett field datasets. International Journal of Environment and Pollution 16, 301-314.

Hidy, G.M., Mueller, P.K., Tong, E.Y., (1978). Spatial and temporal distributions of airborne sulfate in parts of the United States. Atmospheric Environment 12, 735-752.

Horrocks, L.A., Oppenheimer, C., Burton, M.R., Duffell, H.J., Davies, N.M., Martin, N.A., Bell, W., (2001). Open-path Fourier transform infrared spectroscopy of SO_2: An empirical error budget analysis, with implications for volcano monitoring. J. Geophys. Res 106, 27647-27659.

Husain, L., Dutkiewicz, V.A., Das, M., (1998). Evidence for decrease in atmospheric sulfur burden in the eastern United States caused by reduction in SO_2 emissions. Geophys. Res. Lett 25, 967-970.

Husain, L., Parekh, P.P., Dutkiewicz, V.A., Khan, A.R., Yang, K., Swami, K., (2004). Long-term trends in atmospheric concentrations of sulfate, total sulfur, and trace elements in the northeastern United States. Journal of Geophysical Research-Atmospheres 109. 10.1029/2004jd004877.

Irwin, J.S., (2014). A suggested method for dispersion model evaluation. Journal of the Air & Waste Management Association 64, 255-264.

Isakov, V., Venkatram, A., Touma, J.S., Koracin, D., Otte, T.L., (2007). Evaluating the use of outputs from comprehensive meteorological models in air quality modeling applications. Atmospheric Environment 41, 1689-1705.

Janoe, S., Craft, Z., (2015). 1-hr SO2 NAAQS: Designations and implementation. EM 21, 4-8.

Kim, K.H., Kim, M.Y., (2001). Comparison of an open path differential optical absorption spectroscopy system and a conventional in situ monitoring system on the basis of long-term measurements of SO_2, NO_2, and O_3. Atmospheric Environment 35, 4059-4072.

Kok, G.L., Dasgupta, P.K., Adams, D.F., Appel, B.R., Farwell, S.O., Knapp, K.T., Pierson, W.R., Reiszner, K.D., Tanner, R.L., (1989). Method 704B: Determination of sulfur dioxide content of the atmosphere (Formaldehyde Absorber/ Pararosaniline Method), in: Lodge, J.P. (Ed.), Methods of Air Sampling and Analysis, 3 ed. Lewis Publishers, Chelsea, MI, pp. 499-502.

Kushner, E.J., (1976). On determining the statistical parameters for pollution concentration from a truncated data set. Atmospheric Environment 10, 975-979.

Leppanen, S., Anttila, P., Lattila, H., Makkonen, U., (2005). Long-term comparison of filter method and sensitive analyser in monitoring of sulphur dioxide. Atmospheric Environment 39, 2683-2693.

Lillis, D., Cruz, C.N., Collett, J.L., Jr., Richards, L.W., Pandis, S.N., (1999). Production and removal of aerosol in a polluted fog layer: model evaluation and fog effect on PM. Atmospheric Environment 33, 4797-4816.

Luke, W.T., (1997). Evaluation of a commercial pulsed fluorescence detector for the measurement of low-level SO_2 concentrations during the Gas- Phase Sulfur Intercomparison Experiment (Paper 96JD03347). J. Geophys. Res 102, 16255-16266.

Malm, W.C., Schichtel, B.A., Ames, R.B., Gebhart, K.A., (2002). A ten-year spatial and temporal trend of sulfate across the United States. J. Geophys. Res. Atmos 107, ACH 11-11-ACH 11-20. doi:10.1029/2002JD002107.

Mamane, Y., Donagi, A.E., (1976). The effects of sampling line on the measurement of sulfur dioxide: A case study. JAPCA 26, 991-992.

Medina, D.S., Liu, Y.D., Wang, L.M., Zhang, J.S., (2011). Detection of sulfur dioxide by cavity ring-down spectroscopy. Environ. Sci. Technol 45, 1926-1931.

Ohira, S.I., Toda, K., (2005). Micro gas analysis system for measurement of atmospheric hydrogen sulfide and sulfur dioxide. Lab on a Chip 5, 1374-1379.

Paine, R., Samani, O., Kaplan, M., Knipping, E., Kumar, N., (2015). Evaluation of low wind modeling approaches for two tall-stack databases. Journal of the Air & Waste Management Association 65, 1341-1353. 10.1080/10962247.2015.1085924.

Pandis, S.N., Seinfeld, J.H., (1989). Mathematical modeling of acid deposition due to radiation fog. J. Geophys. Res 94, 12911-12911.

Rehbein, P.J.G., Kennedy, M.G., Cotsman, D.J., Campeau, M.A., Greenfield, M.M., Annett, M.A., Lepage, M.F., (2014). Combined analysis of modeled and monitored SO2 concentrations at a complex smelting facility. Journal of the Air & Waste Management Association 64, 272-279.

Roberts, T.J., Saffell, J.R., Oppenheimer, C., Lurton, T., (2014). Electrochemical sensors applied to pollution monitoring: Measurement error and gas ratio bias - A volcano plume case study. Journal of Volcanology and Geothermal Research 281, 85-96.

Rood, A.S., (2014). Performance evaluation of AERMOD, CALPUFF, and legacy air dispersion models using the Winter Validation Tracer Study dataset. Atmospheric Environment 89, 707-720.

Seinfeld, J.H., Pandis, S.N., (1998). Atmospheric Chemistry and Physics: From Air Pollution to Climate Change. John Wiley & Sons, New York, NY.

Seinfeld, J.H., Pandis, S.N., (2006). Atmospheric Chemistry and Physics: From Air Pollution to Climate Change. John Wiley & Sons, New York, NY.

Shadwick, D.S., Sickles, J.E., II, (2004). Sample size for seasonal mean concentration, deposition velocity and deposition: A resampling study. Atmospheric Environment 38, 477-489.

Sickles, J.E., II, Shadwick, D.S., (2002). Biases in Clean Air Status and Trends Network filter pack results associated with sampling protocol. Atmospheric Environment 36, 4687-4698.

Sickles, J.E., II, Shadwick, D.S., (2007). Seasonal and regional air quality and atmospheric deposition in the eastern United States. J. Geophys. Res. Atmos 112.

Sickles, J.E., II, Hodson, L.L., Vorburger, L.M., (1999). Evaluation of the filter pack for long-duration sampling of ambient air. Atmospheric Environment 33, 2187-2202.

Snyder, E.G., Watkins, T.H., Solomon, P.A., Thoma, E.D., Williams, R.W., Hagler, G.S.W., Shelow, D., Hindin, D.A., Kilaru, V.J., Preuss, P.W., (2013). The changing paradigm of air pollution monitoring. Environ. Sci. Technol 47, 11369-11377.

Stelson, A.W., Bao, X., (1988). Water interference of pulsed fluorescence SO_2 monitor. JAPCA 38, 420-420. http://www.tandfonline.com/doi/pdf/10.1080/08940630.1988.10466393

Terraglio, F.P., Manganelli, R.M., (1962). Laboratory evaluation of sulfur dioxide methods and the influence of ozone oxides of nitrogen mixtures. Anal. Chem 34, 675-677.

Toda, K., Inoue, H., Sanemasa, I., (1998). Miniaturized detector of sulfur dioxide based on the flow conductometry of an absorbing solution. Bunseki Kagaku 47, 727-734.

Trieff, N.M., Wohlers, H.C., O'Malley, J.A., Newstein, H., (1968). Appraisal and modification of West-Gaeke method for sulfur dioxide determination. Journal of the Air Pollution Control Association 18, 329-331. http://www.tandfonline.com/doi/pdf/10.1080/00022470.1968.10469136

U.S.EPA, (2013a). 2011 National Emissions Inventory Data, Agency, U.S.E.P., Research Triangle Park, NC. http://www.epa.gov/ttn/chief/net/2011inventory.html

U.S.EPA, (2013b). DRAFT Roadmap for Next Generation Air Monitoring. http://www.epa.gov/research/airscience/docs/roadmap-20130308.pdf

Vadjic, V., Gentilizza, M., Cackovic, M., Eskinja, I., (1992). Comparison of mass concentrations of SO2 determined in air by two different methods. Environ. Mon. Assess 21, 19-26.

Wang, A., Brauer, M., (2014). Review of next generation air monitors for air pollution, Canada, E., Gatineau, QC. http://elk.library.ubc.ca/handle/2429/46628

West, P.W., Gaeke, G.C., (1956). Fixation of sulfur dioxide as disulfitomercurate (II) and subsequent colorimetric estimation. Anal. Chem.

Wohlers, H.C., Trieff, N.M., Newstein, H., Stevens, W., (1977). Sulfur dioxide adsorption on and desorption from Teflon, tygon, glass, stainless steel and aluminum tubings. Atmospheric Environment 1, 121-121.

Wright, R.S., Rickman, E.E., Jr., Green, A.H., Sickles, J.E., II, Decker, C.E., McElroy, F.F., (1989). Laboratory and field evaluations of extrasensitive NO_2 and SO_2 analyzers, Proceedings, 82nd Annual Meeting of the Air and Waste Management Association. Air & Waste Management Association, Pittsburgh, PA.

Zou, B., Wilson, J.G., Zhan, F.B., Zeng, Y.N., (2009). An emission-weighted proximity model for air pollution exposure assessment. Science of the Total Environment 407, 4939-4945.

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Comments on Chapter 2

Charge Question 2. Chapter 2 describes scientific information on sources, atmospheric chemistry, and measurements and modeling of ambient concentrations of gaseous sulfur oxides.

a. *To what extent is the information presented on sources, atmospheric chemistry, and measurements and modeling of ambient concentrations complete, and relevant to the review of the SO_2 NAAAQS?*

Major Points:

While EPA has done a good job of reviewing the concentrations and chemistry of the many gaseous species which can from SO_2, they have been incomplete in their review of the chemistry of SO_2, touching only on the formation of sulfate. They have not mentioned the formation of both inorganic and organic particulate S(IV) and organic S(VI) compounds which can form from the chemistry of SO_2 in plumes and in urban environments. The latter compounds include dimethyl sulfate, monomethyl sulfuric acid and bis-hydroxymethyl sulfone. The inorganic S(IV) compounds can be significant, especially in smelter, integrated steel mill and coal-fired power plant plumes. The alkyl sulfates and sulfone are often a significant fraction of urban particulate sulfur oxides. These oxysulfur compounds will have potential health effects quite different from sulfate and the S(IV) compounds will be analyzed as sulfate by most techniques presently used for analysis. The fraction of the aerosol S(IV) species which is probably most relevant to the ISA is the inorganic S(IV) compounds which have been shown to exacerbate SO_2 inhalation responses in animals (Alarie 1973, Amdur 1971). I had commented on the potential importance of these species in my April 29, 2014 comments to EPA in connection with the previous SO_2 review meeting. I would be happy to provide additional information as requested. I will make additional comments in this area in my response to parts b. and c. of this charge questions. I have also included and amplified on the points made in my 2014 comments in this area (modified to emphasize this ISA) in an Appendix at the end of my comments.

Other Points:

I believe that EPA has not been careful in the use of the terms SO_2 and SO_X. I would recommend the total document be named Sulfur Dioxide" and not "Sulfur Oxides" as they correctly point out this is the focus of the ISA. Likewise, the same change should be made to the heading of each of the chapters. While they point out there are many compounds present in the group "Sulfur Oxides", they point out the only relevant compound for the NAQQS review is SO_2. They also refer to SO_X as only gaseous compounds when the more common usage is for both gas and particulate species, including sulfate, e.g. definition of SO_X on page 2.2 line 7.

The relationships among the various sectors given in Figure 2.1, Figure 2-5, and Table 2-1 are not clear to me. Where are oil refineries, smelters and integrated steel mills included in these sectors, which appear to vary in name among the two figures and table? Having a Figure (such as Figure 2-3) for these three sources would be of interest to me. While the counties which exceed 75 ppb one hour SO_2 average are quite clear in Figures 2-2 and 2-3, they cannot be distinguished in Figure 2-4. Being able to so do would be useful.

The ratio of the red and green lines in Figure 2-5 looks to be about 4-5 which is quite different than suggested by Figure 2-1 where EGU = Coal exceeds all other sectors by a factor of nine..

EPA has been careless in the identification of free radicals in Section 2.3. I would recommend the use of the • symbol for clarity, e.g. OH•. For example, on line10, page 2-10, HSO_3 is called the bisulfate ion, however, what is meant is HSO_3•, the radical. Equation 2-1 would then read:

$$SO_2 + OH\bullet + M \rightarrow HSO_3\bullet + M$$

with similar changes throughout this section.

Page 2-21, line 13. Add that sulfuric acid is usually quickly neutralized by ammonia and give the reaction.

Page 2-12 line 16. “particle” should be "aqueous”.

Page 2-12 line 22. I would add the Pitts, Finlayson-Pitts book as it is better on chemistry than the ones listed.

Sections 2.3.2 and 2.3.3. We have written two papers which you might want to incorporate into this section (they are attached in the covering e-mail),

> Eatough D.J., Caka F.M. and Farber R.J. (1994) "The Conversion of SO_2 to Sulfate in the Atmosphere," *Israel J. Chemistry*, **34**, 301–314.

is a review of all plume studies on the conversion of SO_2 to sulfate conducted to the time of the article. It shows that the non-aqueous phase, clear sky conversion daytime conversion rate is constant when adjusted for T and relative humidity and is dominated by the hydroxyl radical chemistry (the second order chemistry not being important). It also show that the effect of forming an aerosol aqueous phase on the conversion is dramatic. The figure in this manuscript which summarizes these points follows:

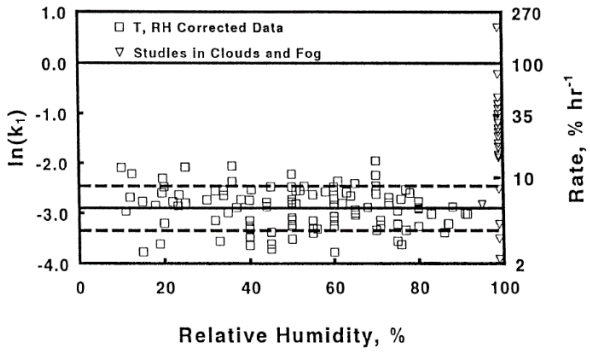


Fig. 3. Variation of $\ln(k_1)$ for the homogeneous conversion of $SO_2(g)$ to sulfate as a function of relative humidity. Solid line represents mean, dashed line, 1 standard deviation from mean. The k_1 values have been corrected to 50% RH using eq 12 and to 25 °C using eq 13. Also shown as the inverted triangles are uncorrected $\ln(k_1)$ values for the conversion of SO_2 to sulfate in fog and clouds.

Eatough D.J., Arthur R.J., Eatough N.L., Hill M.W., Mangelson N.F., Richter B.E., Hansen L.D. and Cooper J.A. (1984) "Rapid Conversion of SO_2(g) to Sulfate in a Fog Bank," *Environ. Sci. Technol.*, **18**, 855–859.

is a report on the formation of sulfate in an oil fired power plant plume and demonstrates both the rapid aqueous phase conversion of SO_2 to sulfate in a fog bank and the immediate shift to gas phase hydroxyl radical chemistry when the plume exits the fog bank. This paper also demonstrates the formation of bis-hydroxymethyl sulfone in the plume, but only in the absence of the fog bank. The Figure from the paper illustrating these points follows:

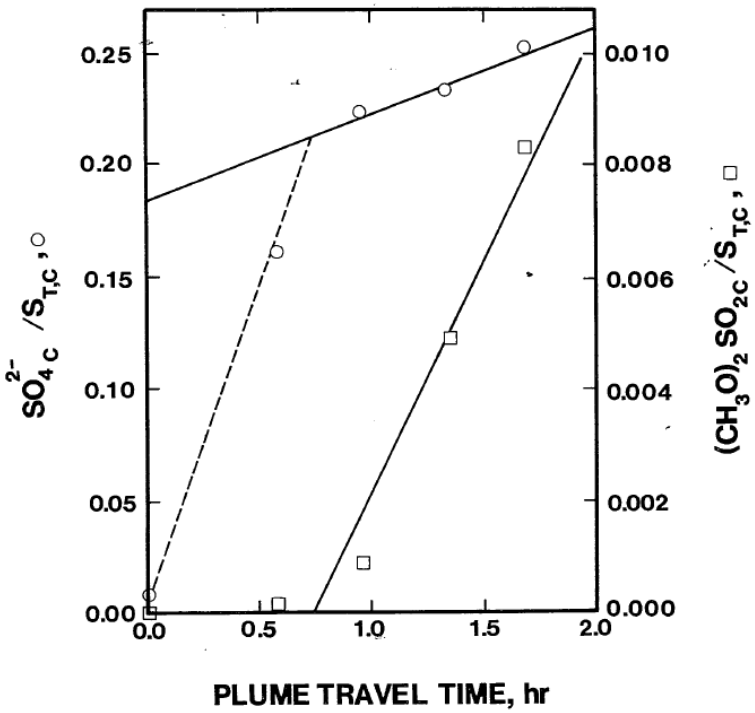


Figure 3. Variation of $SO_4^{2-}{}_C/S_{T,C}$ and $(CH_3O)_2SO_2/S_{T,C}$ vs. plume travel time for the aerosol samples collected Sept 26.

The material I have included in the Appendix further amplifies on the chemistry discussed in these two articles.

b. Please comment on the extent to which available information on the spatial and temporal trends of ambient SO_2 concentrations at various scales has been adequately and accurately described. In particular, what is the extent to which the analysis or recently available 5-min SO_2 concentration data are informative in considering relationships between 5-min and 1-hr SO_2 concentrations?

Major Points:

I found Section 2.5.2.2 on Urban Spatial Variability and Section 2.5.3 on Temporal Variability hard to correlate until I found they were working with a different data base in the two sections. Section 2.5.2.2 contains Table 2.8 which has 1-hour max sulfur dioxide concentration distribution for 42 monitors in six core-based statistical area/metropolitan focus areas. The nine highest monitors based on the 99 percentile data in this data set can be identified. Section 2.5.3 discusses the total national data set up to Section 2.5.3.3 and then discusses the 5 minute average data for the same 42 monitors as discussed in Section 2.5.2.2. However, the nine highest

monitors based on the 99 percentile data for the 5 minute data set are different for this data set and some of the 1 hour 99 percentile data are higher than the 5 minute 99 percentile data, which cannot be. Comparisons between the two are therefore suspect, e.g. Figure 2-28. The section on comparison of hourly average and 5 minute data would be more convincing if only hourly averaged data with complete 5 minute averaged data were used.

On the nature of sources responsible for high 99th percentile 1 hour SO_2 Data at sampling sites included in Figure 2-12:

I have correlated the data for the sampling sites given in Figure 2-12 with the possible presence of high concentrations of non-sulfate metal S(IV) aerosol species know to be present in plumes of metal smelters and integrated steel processing plants (the sampling site 99th percentile values is given om the following discussion where it was present in the data supplied by EPA). This comparison is only partially complete because not all sampling sites identified with a color other than blue were included in the data set provided to the committee by EPA.

There are only three currently operating smelters of importance in this regard in the U.S.

The Asarco Cu Smelter at Hayden, AZ (295). Figure 2.12 identifies this site as one of the 6 sites with 99th percentile daily 1-h max SO_2 concentration of 201-400 ppb. The summary proved by EPA correctly identifies the city as Hayden and gives the correct coordinates for the site. However, the CBSA name is incorrectly listed as Payson, AZ and the data are incorrectly included in the discussion of 2-19. (This error in Figure 2-19 and Table 2-8 needs to be corrected, it is not clear what the data for D in Figure 2-19 should be).

The Kennecott Cu Smelter in Magna, UT. The Salt Lake City area had no sites in where the 1-h data exceed the 99th percentile 75 ppb limit. However, prior to the installation of new acid plants and the construction of a tall stack, impacts in the Salt Lake Valley were frequent and significant. It has been suggested (see Appendix material to my comments) that metal S(IV) species played a role in the exacerbation of asthma as seen the early EPA CHESS study conducted when SO_2 concentrations in the Salt Lake Valley were high..

The Doe Run Herculaneum lead smelter in Missouri south of St. Louis on the Mississippi River. The previous ISA highlighted emissions from this facility. However, the source site in Herculaneum is given in Figure 2-17, but data from the site are not included in Figure 2-12. It can be anticipated that more local impact was still high (if monitored at Herculaneum during the time frame covered) and some impact from this smelter might have been seen in St. Louis. The smelter was discontinued in 2014 and will no longer be an SO_2 contributor to the area.

Integrated steel miles can also be expected to be a significant source of meatal S(IV) species. The following operating integrated steel mills can be expected to have influenced the 99th

percentile 1 hour SO_2 Data for sites given in Figure 2-12 which fell in the 75-200 concentration range:

Weirton Steel Corporation, Weirton, PA

- Steubenville OH (4 miles down the Ohio River)
- Follansbee WV (12 miles down the Ohio River)
- Weirton-Steubenville WV-OH (close to Weirton facility)

USS Edgar Thomson Plant, Pittsburgh (140): Liberty, Pittsburgh (5 miles from facility)

US Steel Corporation, AK Steel Holding and Great Lakes Steel Works, Detroit: Detroit

US Steel Gary Works and ArcelorMittal East Chicago Works, Indiana: Indiana

ArcelorMittal Cleveland Works, Cleveland (80): Cleveland

The last two sites are more generic because I could not identify the site from the list provided by EPA but a high site was clearly present in Figure 2-12.

These comparisons indicate high SO_2 concentrations are frequently associated with monitors near integrated steel mills.

Other Points:

Section 2.5.3.1 discusses long-term trends and the material at the top of page 2-56 implies the decrease is due to first the reduction in power plant emissions due to the Acid Rain Program and second regulations on sulfur content in diesel fuel. However, the diesel fuel content reduction has been much smaller than, for example, industrial fuel combustion emission from other than power plants (Figure 2-5). This material should reflect Figure 2-5 when giving credit.

Page 257, line 7. Should skewed right be skewed higher?

I found the discussion on seasonal trends to be rather conjectural. Will modeling support the causes for the trend listed? Have the effects of emission heights and meteorology been considered. I find it hard to accept the statements on highest concentrations being due to increased mixing heights.

Figure 2-11. It is quite difficult to distinguish the NCORE sites (as opposed to SLAMS sites) in this Figure. A change of symbol identification should be considered.

Table 2-6. It would be useful to refer the “Except during 2010” statement to the footnote for clarity. I also assume that the completeness criteria is the reason for including only 309 monitors

for the 5-min data and 337 monitors for the 1-h data in the subsequent large number of figures in this section. A dropout rate of near 25% seems rather large. Can you add a section on why the percent is so high and whether it is anticipated to improve in the future?

Throughout this portion of the document the Payson/Phoenix area the Gila County are both used to refer to the same geographical area. It would be helpful if only one term were used.

Section 2.5.4 discusses background concentrations using the 2001 data given in the 2008 ISA. There are a few points EPA should consider on this section.

Figure 2-32. It should be made clear in the figure caption that these are 2001 data. I assume the second "Background" figure refers to PRB as defined in the text. This might be made clear. The "Total" top figure is also based on 2001 data. However, as suggested by Figure 5-2, a figure based on current data would have a slightly different distribution and would average less than half the values given in Figure 2-32. If we assume background is about the same (do you have any data to indicate this is not about right?) then the "Percent Background Contribution" would average about twice the values given in the Figure. What would this imply for the percent due to background in the western states? Some thought and comment should be given to these considerations, particularly the statements at the end of the last paragraph on page 2-76.

Page 276 line 23. Is "absolute" that same as "total"?

Page 276, line26. The apparent impact of volcanic emissions on background extends quite east of the west coast.

Figure 2-33 is missing.

c. *How informative is the analysis of correlations between SO_2 and co-occurring pollutant concentrations for interpretation of epidemiological studies?*

I am not certain why EPA included both Max and 24-hour average co-occurring pollutant comparisons in Figure 2-35. I assume Max means the 1-hour maximum concentrations of the co-pollutant during the 24 hour period for which SO_2 daily average concentrations were obtained. It could also mean the co-pollutant concentration during the hour for which the highest SO_2 concentrations were seen. In either case, the Max comparison only makes sense if you expect the sources and emission diurnal patterns of SO_2 and the co-pollutant to be the same. EPA needs to better explain what Max means, and how you justify including the comparison, or just drop it.

A similar statement applies to Max in Figure 2-36. There I am even less sure of what Max refers to.

In addition, I am not sure what is meant by O3 Max, and why no average is included, even in Figure 2-36 which includes hourly SO2 data. A correlation here, of course would be quite surprising.

The same comment I have made on Figure 2-35 applies to the seasonal data in Figure 2-37.

Page 2-83, line 2-5. I have frequently used ozone hourly average ozone data in source apportionment analysis for both summer and winter studies and find the expectation of substantial measurement error in these data in the winter to not be justified.

Perhaps the wisest statement in this section is we do not expect copollutant correlation confounding in SO_2 focused epidemiological studies, but you better look for it anyway.

I have made no comments on Section 2.6.1 Dispersion Modeling, not because I do not think it important, but because I am not knowledgeable on the subject.

Comments on the Executive Summary

Sources and Human Exposure to Sulfur Dioxide

Page xxxvii, line 20. If EPA agrees with and adds material related to my discussion of the potential importance of metal S(IV) species, the confounding effects of particulate S(IV) compounds should be added to this sentence.

Page xxxvii, line 29. Likewise the non-photochemical formation of particulate S(IV) species including metal S(IV) species and bis-hydroxymethyl sulfone should be added here.

Page xxxvii, line 4. Likewise, smelters and integrated steel mills should be added to the anthropogenic sources.

Comments on Chapter 1 - Summary of the Integrated Science Assessment

1.4.1 Emission Sources and Distribution of Ambient Concentrations

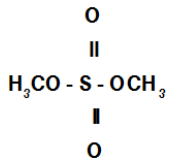
Page 1-7, line 13. The removal of SO_2 by reaction pathways other than the formation of sulfate, as summarized in the comments on Chapter 2, should be acknowledged and summarized.

Page 1-7, line 34. Smelters and integrated steel mills should be added to the anthropogenic sources.

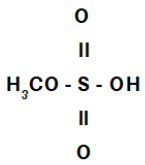
Appendix:

In this Appendix to my comments I summarize material on the conversion of SO_2 to compounds other than sulfate given in my previous April 29, 2014 comments. I also expand on the material given there to discuss the relative importance of the S(IV) species to sulfate in plumes from smelters, steel mills and power plants and comment on the chemistry which seems to be involved in the formation of these compounds in the atmosphere.

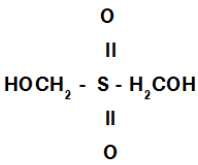
We have published a chapter which summarizes the information available through 1983 on Particulate Inorganic S(IV) and on Bis-Hydroxymethyl Sulfone, an organic S(IV) compound which are formed in emissions from SO_2 sources (Eatough 1983). Much of the information in the next two sections is found in that chapter. I have appended that chapter to my covering e-mail. The other two oxysulfer compounds we have identified, dimethyl sulfate and mono methyl sulfuric acid are S(VI) compounds and they are briefly discussed later..



Dimethyl Sulfate



Monomethyl Sulfuric Acid



Bis-Hydroxymethyl Sulfone

Particulate Inorganic S(IV).

The early laboratory studies of Amdur (1971) and Alarie (1973) indicated that exposure to both SO_2 and metal oxides present in smelter emissions resulted in an enhanced animal response and that the resulting aerosols were irritating. Postulating that this work might reflect the presence of stable metal sulfite species in the aerosols studied, and that the formation of such aerosol species, rather than aerosol sulfate, might explain the results of the EPA CHESS study in the Salt Lake City environment with substantial impact from Cu smelter emissions (EPA 1974), we conducted studies on S(IV) associated with ambient aerosols. This work demonstrated that stable transition metal ion - sulfite species existed in aerosols associated with smelter emissions (Smith 1976, Eatough 1979, Eatough 1980) and formed in aging smelter plumes (Eatough 1981a, Eatough 1982). Studies in several smelter and coal-fired power plant plumes indicated the inorganic sulfite species were present in both primary emission and were also formed in the plume (Eatough 1983). The inorganic S(IV) was most clearly associated with the sum of Fe and Cu in the aerosols (Eatough 1983, 1981c) and was inversely related to aerosol acidity (Eatough 1983). The inorganic S(IV) species were present at from 10 to 30 mol % of the sulfate species in these smelter plume associated aerosols. The sulfite species were less important in urban or coal-fired power plant plumes in the east being from 5 to 10 mol % of the sulfate (Eatough 1978) but

varied from 40 mol % in the fresh plume and present at concentrations comparable to sulfate in the aged plume of a coal-fired power plant in Utah (Eatough 1081a, 1983) Concentrations were generally low in oil-fired power plant plumes and varied from 8 mol % of sulfate in New York to 1 mol % of sulfate in Los Angeles (Eatough 1983). We also demonstrated that stable Fe(III) – S(IV) aerosols could be routinely generated in the laboratory (Hilton 1979).

In summary, the measurement, stability and formation of these inorganic S(IV) species in aerosols has been reviewed (Eatough 1983). These S(IV) species were present in emissions from all smelters studied, were also present in emissions from coal-fired power plants and additional material was formed during plume transport in both coal-fired power plant and smelter plumes. Based on the chemistry identified, we would expect to find these metal S(IV) compounds in emission from integrated steel mills. The amount of the S(IV) species, relative to sulfate average 0.1 mol S(IV)/mol sulfate in the coal-fired power plant plumes and 0.5 mol S(IV)/mole sulfate in aged smelter plumes. The formation of S(IV) in smelter plumes increased with decreasing acidity of the aerosol.

Bis-Hydroxymethyl Sulfone.

Several different methods of analysis of particulate samples collected from the plumes of coal-fired power plants or from areas heavily impacted by coal-fired boilers indicated that a S(IV) compound distinctly difference from inorganic S(IV) was present in the samples (Eatough 1978, Eatough 1981, Richter 1981). This compound was subsequently identified as bis-hydroxymethyl sulfone (Eatough 1984). The sulfone was usually present in emissions from coal- or oil-fired power plants at mol ratios of about 0.5 (range of 0.1 to 1.0) compared to sulfate (Eatough 1983). First order formation of the sulfone was observed in plumes from six different power plants at rates of from 0.4 to 3.0 % SO_2/hr. with the observed rate being inversely proportional to atmospheric water partial pressure (Eatough 1983). The sulfone was found in highest concentrations in the Los Angeles Basin in inland samples (mol fraction comparable to sulfate), but was not seen in coastal samples impacted by fog or clouds (Farber 1982). No information is available on the toxicology of bis-hydroxymethyl sulfone.

In the previous April 29, 2014 comments I made the following recommendation:

"A potentially enlightening exercise might be to examine if any relationship exists between the results of epidemiological studies and the source of SO_2 for a given epidemiological study. This suggestion is based on the early laboratory studies of Amdur (1971) and Alarie (1973) which indicated that exposure to both SO_2 and metal oxides present in smelter emissions resulted in an enhanced animal response and that the resulting aerosols were irritating. I suggest this because it might enlighten whether particulate S(IV) (e.g. absorbed SO_2) might be important in exacerbation of asthma. If these S(IV) containing aerosols identified in the above reviewed research account for the enhanced effect of SO_2 in the presence of transition metal containing aerosols in animal exposure studies, then this class of compounds may be important in the

interpretation of the morbidity effects associated with exposure to pollution from smelter or integrated steel mill sources. A careful review of pertinent epidemiological literature may inform this postulate.

The current set of counties which are nonattainment with respect to the current SO_2 NAAQS will probably not provide the needed information. A review of nonattainment counties with populations near or over 100,000 show that with two exceptions, the SO_2 exposures are dominated by emissions from coal-fired power plants, where aerosol S(IV) species may be less important. The two exceptions are Jefferson County, MO where about 30% of the SO_2 emissions currently are from the Herculaneum Lead Smelter, with the remainder being from coal-fired power plants, and the Steubenville, Weirton region in eastern Ohio and western Pennsylvania, where emission from the Weirton Steel are likewise, a minor portion of the SO_2 emissions in the immediate area, with coal fired power plants being more important. These two locations would only stand out from the other nonattainment areas if the morbidity influence of aerosol S(IV) species was much greater that that associated with SO_2 itself. In addition to being a nonattainment area, Jefferson County, MO was also highlighted in the September 2008 Integrated Science Assessment for Sulfur Oxides, but with no epidemiological discussion associated with this nonattainment area (part of the St. Louis MO MSA).

Probably a more fruitful set of data to evaluate the relative importance of aerosol S(IV) species associated with smelter emissions would involve past epidemiological studies from about two to three decades ago when smelter emission were much more significant, for example from the TX smelters in El Paseo (ASARCO Cu smelter, closed in 1999), and Corpus Christi (ASARCO Pb smelter, closed in 1985), Az smelters (ASARCO Cu smelter in Hayden, currently operating and Phelps Dodge Cu smelter in Douglas, closed in 1987), from the Kennecott Cu smelter in Magna, UT prior to construction of the tall stack, from the Tacoma WA smelter (American Smelting and Refining, a Cu smelter specializing in high As ore refining, closed in 1985), or the smelters in Montana (ASARCO Pb smelter in East Helena, closed in 2001, Anaconda Cu smelter in Anaconda, closed in 1981) and Idaho (Bunker Hill Pb smelter in Kellogg, closed in 1982). I know that several epidemiological studies were conducted at these locations, but I am not familiar with the results of these studies with respect to asthma exacerbation. I recommend that EPA look at this older data to see if an estimate of the relative potency of SO_2 and smelter associated aerosol S(IV) species can be determined. There will not be data on the concentrations of S(IV) in the aerosols emitted from these sources, so total particulate exposure or SO_2 would need to be used as a surrogate. The importance of elucidating the effect of these exposures is correctly alluded to in the ISA on Page 4-12, Line 11."

This recommendation was included in the final comments (albeit much more briefly) from the committee to EPA, but has not been acted upon are near as I can tell. In light of the importance of emissions from smelters (infrequent, but not nonexistent) and integrated steel mills, I believe the exercise would still be valuable in determining if aerosol inorganic S(IV) is a confounder to the SO_2 asthma response seen in epidemiological studies.

Organic Oxysulfur Compounds in the Atmosphere

This section was added to my preliminary comments to provide a written response to the question raised in the preliminary comments by Dr. Daniel Jacob. Compounds identified by BYU and discussed below include:

Alkyl Sulfates. We have previously identified monomethyl sulfuric acid and dimethyl sulfate in power plant plumes (Lee 1980, Eatough 1981b, Hansen 1987) and in the Los Angeles Basin (Eatough 1986, Hansen 1986). The alkyl sulfates have been shown to be present in emissions from both coal and oil-fired power plants (Eatough 1981b). In addition, formation of dimethyl sulfate during plume transport has been seen in the plumes of both an oil-fired and a coal-fired power plant (Hansen 1987). Dimethyl sulfate did not form in the plume of the oil-fired power plant studied while it resided in a fog bank, but formation was seen after the plume exited the fog bank. The rate of conversion of SO_2 to dimethyl sulfate was about 0.4 mole %/hr in the oil fired power plant plume and about 0.05 mole %/hr in the coal fired power plant plume. Particulate phase dimethyl sulfate dominated in these two studies. In the Los Angeles Basin studies (Eatough 1986) dimethyl sulfate was only seen in air masses not imbedded in a fog bank, i.e. generally in the inland area. Gas phase dimethyl sulfate was the dominate species in these studies, was present at highest concentrations in transported plumes in the Inland Empire and was seen to exceed 10 mole % of the total sulfur oxides present. It should be pointed out that at the time of these studies, substantial SO_2 emission from power plants were present in the Basin..

Dimethyl sulfate is a mutagen and suspected human carcinogen, so its presence may be important with respect to toxic species, but I am not aware of any data indicating that inhalation will exacerbate asthma.

.

Other Organic Oxysulfur Compounds. Aerosol phase methane sulfonic acid (Panter 1980) and gas phase ethylene sulfite (Jones 1974) have been identified in atmospheres impacted by emission from coal fired power plants, but only at concentrations much less than the above described species.

References

Alarie Y., Wakisaka I. Oka S. (1973) “Sensory Irritation by Sulfite Aerosols,” Environ. Physiol. Biochem., **3**, 182-184.

Amdur M.O. (1971) “Aerosols Formed by Oxidation of Sulfur Oxide.” Arch. Environ. Health, **23**, 459-468.

Eatough D.J., White V.F., Hansen L.D., Eatough N.L. and Cheney J.L. (1986) "Identification of Gas-Phase Dimethyl Sulfate and Monomethyl Hydrogen Sulfate in the Los Angeles Atmosphere," Environ. Sci. Technol., **20(9)**, 867-872.

Eatough D.J. and Hansen L.D. (1984) "Bis-Hydroxymethyl Sulfone: A Major Aerosol Product of Atmospheric Reactions of SO_2 (g)," Sci. Total Environ., **36**, 319-328.

Eatough D.J. and Hansen L.D. (1983) "Organic and Inorganic S(IV) Compounds in Airborne Particulate Matter," in Trace Atmospheric Constituents: Properties, Transformations and Fates, **12**, S.E. Schwartz, ed., John Wiley and Sons, New York, NY 221-268

Eatough D.J., Christensen J.J., Eatough N.L., Hill M.W., Major T.D., Mangelson N.F., Post M.E., Ryder J.F., Hansen L.D., Meisenheimer R.G. and Fisher J.W. (1982) "Sulfur Chemistry in a Copper Smelter Plume," Atmos. Environ., **16**, 1001-1015.

Eatough D.J., Richter B.E., Eatough N.L., and Hansen L.D. (1981a) "Sulfur Chemistry in Smelter and Power Plant Plumes in the Western U.S.," Atmos. Environ., **15**, 2241-2253.

Eatough D.J., Lee M.L., Later D.W., Richter B.E., Eatough N.L. and Hansen L.D. (1981b) "Dimethyl Sulfate in Particulate Matter from Coal- and Oil-Fired Power Plants," Environ. Sci. Tech., **15**, 1502-1506.

Eatough D.J. and Hansen L.D. (1981c) "S(IV) Chemistry in Smelter Produced Particulate Matter," Amer. J. Indus. Med., **1**, 435-448.

Eatough D.J., Eatough N.L., Hill M.W., Mangelson N.F., Ryder J., Hansen L.D., Meisenheimer R.G. and Fischer J.W. (1979) "The Chemical Composition of Smelter Flue Dusts," Atmos. Environ., **13**, 489-506.

Eatough D.J., Major T., Ryder J., Hill M., Mangelson N.F., Eatough N.L., Hansen L.D., Meisenheimer R.G. and Fischer J.W. (1978) "The Formation and Stability of Sulfite Species in Aerosols," Atmos. Environ., **12**, 263-271.

EPA (1974) “Health Consequences of Sulfur Oxides: A Report from CHESS, 1970-1971” EPA-650.1-74-004, May 1974

Farber R.J., Huang A.A., Bregman L.D., Mahoney R.L., Eatough D.J., Hansen L.D., Blumenthal D.L., Keifer W.S. and Allard D.W. (1982) "The Third Dimension in the Los Angeles Basin," Sci. Total Environ., **23**, 345-360.

Hansen L.D., Eatough D.J., Cheney J.L. and Eatough N.L. (1987) "The Formation of Dimethyl Sulfate in Power Plant Plumes," 80th Annual Meeting of the Air Pollution Control Association, Paper No. 87-96.3, New York, New York, 21-26 June.

Hansen L.D., White V.F. and Eatough D.J. (1986) "Determination of Gas-Phase Dimethyl Sulfate and Monomethyl Hydrogen Sulfate," Environ. Sci. Tech., **20(9)**, 872-878. **[362**

Hilton C.M., Christensen J.J., Eatough D.J. and Hansen L.D. (1979) "Fe(III)-S(IV) Aerosol Generation and Characterization," Atmos. Environ., **13**, 601-605.

Jones P.W. (1974) Proceedings of the 1974 APCA Meeting, Paper No. 74-25.

Lee M.L., Later D.W., Rollins D.K., Eatough D.J. and Hansen L.D. (1980) "Dimethyl and Monomethyl Sulfate: Presence in Coal Fly Ash and Airborne Particulate Matter," Science, **207**, 186-188.

Panter R. and Penzhorn R.D. (1980) “Alkyl Sulfonic Acids in the Atmosphere”, Atmos. Environ., **14**, 149-151.

Richter B.E. (1981) “S(IV) and Alkylating Agents in Airborne Particulate Matter,” Ph.D. dissertation, Brigham Young University, Provo, UT.

Smith T.J., Eatough D.J., Hansen L.D. and Mangelson N.F. (1976) "The Chemistry of Sulfur and Arsenic in Airborne Copper Smelter Particulates," Bull. Env. Contamination and Toxicology, **15**, 651-659.

Dr. Steven Hanna

The following paragraphs address the general charge questions and include specific comments regarding certain pages and lines in the report.

Note that my expertise is primarily in atmospheric transport and dispersion modeling and analysis of observed concentrations, and my comments focus on those areas. However, I do have some experience in other areas of the report, such as emissions categorization and distribution, background concentration determination, and exposure modeling, and provide some comments on those topics where appropriate.

Similarly, are there topics for which discussion should be shortened or removed? Does the Panel have opinions on how the document can be shortened without eliminating important and necessary content?

I notice that there are whole paragraphs that are identical in the Executive Summary and in Chapter 1 – Summary of the ISA. I checked only the topics that I am familiar with. For example, the Chapter 1 paragraphs in lines 25-34 on p 1-7, lines 3-34 on p 1-8, and lines 1-14 on p 1-9 are nearly identical to what is found in the Executive Summary. Clearly an attempt should be made to avoid such long examples of duplication. Perhaps the Executive Summary could be a briefer summary, written from scratch with fingers kept away from the "copy" and "paste" buttons.

Also, the material on dispersion modeling in Chapter 2 is inconsistent with that in Chapter 3. I like the write-up in Chapter 2 but have several issues with what is in Chapter 3. Obviously different people wrote those sections and they did not communicate enough with each other. The material on dispersion modeling in section 3 should be revised.

Another general comment is that the averaging times and sampling times and adjectives (peak, daily, etc) need always to be clearly defined. Often some key information is left out and the resulting statement or conclusion is ambiguous. Related to this, I repeat a comment that I made in our last workshop – the analysis of 5 min concentrations does not refer to a large number of papers over several decades on peak to mean concentrations, variation of peak and average concentrations with averaging time, and so on. See texts by Pasquill, Gifford, and others that I could provide. The current analysis is given as if the current ISA authors are the first to look at this subject.

It is said that the uncertainty is what is not explained by models. The meteorological (and other environmental) communities have a different view – namely that there is some inherent or natural uncertainty in the system that can <u>never</u> be explained by models. For example, this

inherent uncertainty in wind speed model estimates is about 1 m/s. For concentration predictions, the inherent uncertainty is at least 10 % even for the best of scenarios.

The Executive Summary is intended to provide a concise synopsis of the key findings and conclusions of the SOX ISA for a broad range of audiences. Please comment on the clarity with which the Executive Summary communicates the key information from the SOX ISA. Please provide recommendations on information that should be added or information that should be left for discussion in the subsequent chapters of the SOX ISA.

See my earlier response regarding the introductory comments above. I suppose that the reason why there are so many examples of exactly the same paragraphs in the Executive Summary as in Chapter 1 is that the Executive Summary was written last, and just before the report was due. Now there is time for a more careful preparation of the Executive Summary, with no copying and pasting.

Chapter 1 summarizes key information from the Preamble about the process for developing an ISA. Chapter 1 also presents the integrative summary and conclusions from the subsequent detailed chapters of the SOX ISA and characterizes available scientific information on policy-relevant issues.

a) *Please comment on the usefulness and effectiveness of the summary presentation. Please provide recommendations on approaches that may improve the communication of key findings to varied audiences and the synthesis of available information across subject areas. What information should be added or is more appropriate to leave for discussion in the subsequent detailed chapters?*

It seems odd to have a summary chapter following the Executive Summary. Is this required? If so, please try to use different wording, since there is much redundancy.

P 1-7, line 27 – Insert “1-hour” before “maximum”. This is just one example of the topic in my response in paragraph 3 on the introductory comments. Please be very clear about averaging times, etc.

Chapter 2 describes scientific information on sources, atmospheric chemistry, and measurement and modeling of ambient concentrations of gaseous sulfur oxides.

a) *To what extent is the information presented regarding sources, chemistry, and measurement and modeling of ambient concentrations accurate, complete, and relevant to the review of the SO2 NAAQS?*
b) *Please comment on the extent to which available information on the spatial and temporal trends of ambient SO2 concentrations at various scales has been adequately and accurately described. In particular, what is the extent to which the analyses of recently*

available 5-min SO2 concentration data are informative in considering relationships between 5-min and 1-hr SO2 concentrations?

c) *How informative is the analysis of correlations between SO2 and co-occurring pollutant concentrations for interpretation of epidemiologic studies?*

This chapter (2) is on topics most closely aligned with my area of expertise (analysis of observed concentrations and dispersion modeling).

The siting criteria for SO_2 monitors seem rather arbitrary and are not consistent with guidance for other types of instruments. For example, in line 11 of p 2-32 – I do not see why the monitor should be placed on the windward side of a building, and the text gives no scientific justification. On p 2-33 line 1, I do not see how a building can "scavenge" SO_2. Trees and other vegetation may be able to scavenge SO_2, but most building materials are rather inert.

The misuse of averaging times and maxima is seen is section 2.5.1. I think that the current report is often comparing apples to oranges. If the authors had reviewed the literature by Pasquill, Gifford, and others from 20-50 years ago, they would have seen that the 5 min max C should be compared to the one hour max C for a given <u>sampling time</u> (perhaps one day or one month, or one year, or ?). Note that the 5 min max C could occur during a different hour in the day than the 1 hr max C. Pasquill defines sampling time as the total period of data that is being analyzed, and averaging time as the time over which the C signal is being averaged. With the definitions by Pasquill and Gifford, max C is inversely proportional to averaging time (Ta) to the 1/5 power. Thus max C (5 min)/max C (1 hr) would be about 1.6.

I am further confused in Section 2.5.2 (spatial variability) where the quantities being studied are not clearly-defined. (see lines 4-16 on p 2-35). Also note Table 2-7 (and other tables), which need the entries to be completely defined.

The authors should review analyses of other environmental data, which usually show the long tailed distributions (with a small average and a few large maxima) as discussed, for example, at the bottom of p 2-36.

The section on urban mesoscale variability (2.5.2.2) should refer back to 40 year old studies such as the EPA St Louis RAMS study, which generated thick EPA reports and journal articles by what is now the ORD Air Modeling Group on the subject. Also, rather than first running the statistical software and then trying to interpret the results, it would be better to first list a few basic facts or hypotheses that are expected to be verified by the data. For example, it is expected that concentrations will be relatively high for monitors closer to large sources (e.g., see bottom of p 2-46 and lines 6-9 on p 2-49). This can also be shown by application of a one line Gaussian plume model.

The discussion of variability of 5 min max vs 1 hr max C's on p 2-54 can be easily explained by basic turbulence theory. Smaller averaging times will show more turbulent variations with smaller time scales (see Pasquill's book).

Seasonal trends discussion in Section 2.5.3.2 – Differences are also due to the effects of stack height (for large industrial sources) and plume buoyancy flux.

Section 2.5.3.3 – I cannot find a definition of deil and it is not listed in the acronyms.

Section 2.5.3.4 on 5 min data – At our last workshop I commented that the EPA should review the extensive literature over several decades on peak to mean concentration ratios. This has not been done and the analysis goes over a lot of old ground. Most importantly, it does not account for what we know about atmospheric turbulence spectra (variations in time and space and effective scales). As I mentioned earlier, there is a need to compare apples to apples (i.e., max at 5 min to max at 1 hr to max over 1 day) and clearly define the sampling period. Also we know that for summer daytime periods, the 5 min fluctuations can be large since that is the period of convective eddies in the boundary layer.

Section 2.5.4 on background – This is a difficult subject. I would like to see a discussion of how the variability in background is larger for observed concentrations than for modeled concentrations (e.g., by MOZART).

Section 2.6.1 – Dispersion Modeling – I found this section to be very good. I would like to see more discussion of Lagrangian puff models such as CALPUFF and SCICHEM, though, since they are very useful for the nearfield out to regional scales. Note that I do wonder where the statement comes from on p 2-86 lines 11-12 that "the terms "dispersion model" and Gaussian model" are associated with each other". I have never read this anywhere and it is certainly not true outside of the US EPA. Or even inside the EPA since CALPUFF and SCICHEM are available. And as CMAQ is run on smaller and smaller grids, it can resolve more types of sources.

Section 2.7 – Summary – Isn't H_2SO_4 a PM problem?

Chapter 3 describes scientific information on exposure to ambient SO2 and implications for epidemiologic studies.

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 SO2 NAAQS?*

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 SO2.*

My comments focus on Section 3.2.2 Modeling. As I said in my initial comments, the dispersion model discussion here and that in Chapter 2 are not consistent. This report should present a consistent view, and I greatly prefer the discussion in Chapter 2. Please revise Chapter 3 so that it reflects the material in Chapter 2. For example, include CALPUFF and SCICHEM (Lagrangian puff models), which have more extensive chemical mechanisms than AERMOD. Mention that OAQPS is currently testing a "plume-in-grid" version of CMAQ in which SCICHEM is used for the plumes.

The chapter makes a big point that CMAQ has a mean bias. I checked with my contacts in the CMAQ development group and they say that the mean bias for SO_2 was corrected years ago. Biases in the secondary pollutants (formed from SO_2) have also been removed, and this work is described in several papers and presentations. So apparently the Chapter 3 authors are using outdated information.

The chapter includes several statistical "models" that involve curves fit to concentration observations. I prefer a hybrid approach as I (and my coauthor Akula Venkatram) described in a presentation in the health effects session at the AMS Annual meeting in 2015.

Basic science relations should be used to guide the statistical curve fits. For example, on line 6 of p 3-7, it is said that concentration decreases with distance from the source, x, following a power law of $x^{-2/3}$. However basic dispersion principles would yield an $x^{-3/2}$ power for near-ground sources. The $x^{-3/2}$ relation has been verified with many field studies.

Regarding inverse distance weighting (discussed on p 3-10), there are numerous references over several decades supporting the use of the inverse square relation in the atmospheric boundary layer, and these should be listed.

Pp 3-10 – middle of 3-13 - Revise this overview of different kinds of dispersion models to be consistent with Chapter 2.

I have no comments on the other charge questions and chapters in the ISA.

Dr. Jack Harkema

Executive Summary and Chapter 1

General Comments

Currently, the summarized material (and format) in these introductory sections of the ISA appropriately highlights and summarizes the important information provided in the subsequent chapters. Overall the key scientific information, to *date*, that have dictated the authors' causality determinations has been appropriately highlighted. The authors have nicely used summary tables and chapter references to synthesize and streamline the study results that were key for their determinations of causality. There are, however, some important revisions that will have to be made in the individual later chapters that may need to be captured or revised in Chapter 1 and the Executive Summary. For example, see my comments/suggestions for Chapter 4 below.

In addition, the Executive Summary contains some technical jargon and phrasing that could be modified (eliminated or better defined) to be more readable for a wider sector of the public interested in this area.

Specific Comments

In the Executive Summary, *Sources and Human Exposure to Sulfur Dioxide* section is quite long and should be shortened by referring the more technical details to Chapter 1 or an appropriate later Chapter.

xliv, line 1. *The strongest evidence . . .* should be deleted and rewritten (e.g., *Strong scientific data indicates that . . . .*).

xlv, Iine 6. Does *additional studies* refer to more *recent studies* (since the last ISA)?

1-13, 1.5.2 Airway mucosal injury should be defined. Airway remodeling (epithelial mucous cell metaplasia/hyperplasia, intramural fibrosis) is another important pathological feature of asthma (COPD) and in the airways of laboratory animals repeatedly exposed to SO2.

1-14, 1.2, line 16-18. A clearer definition of *new asthma onset* is needed to set it apart from *asthma exacerbation*.

1-14, 1.6, line 31-33. For setting NAAQS to prevent health effects of sulfur dioxide, is it really appropriate to define short-term exposures as long as 1 month?

1-15, 1.6.1, line 6. Rewrite first sentence – see comment above xliv, line 1.

1-15, 1.6.1, line 16. Replace . . . *under increased ventilation conditions* . . . with . . .*when exercising.*

1-15, 1.6.1, lines 26-33. Not sure what is meant by *most severe asthmatics have not been tested* when *moderate-severe* asthmatics were reported to have larger changes in lung function. This needs clarification.

1-18, lines 1-2. The sentence *There is a lack . . . . across disciplines* is not clear and should be rewritten.

1-19, lines 4-6. Define *supportive evidence for health outcomes*. This is not clear in the context of the sentence and the paragraph.

Comments on Chapter 4 - Dosimetry and Mode of Action

In general, this chapter is very well written with appropriate dosimetry and mode of action information, and of suitable depth, for the known health effects of inhaled sulfur dioxide. A few specific comments are listed below.

Specific Comments

Since the nasal mucosa is the tissue receiving the greatest SO2 exposure (95-99% absorption in the nose), the SO2 metabolizing capabilities should be discussed in this chapter including a comparison with other tissues in the respiratory tract and elsewhere. For example, it has been reported that the activity of sulfite oxidase is comparable in the nose, trachea, and proximal and medium bronchi, but lower in lung parenchyma of laboratory dogs. As a start, see the paper by Maier et al. 1999 on xenobiotic-metabolizing enzymes in the canine respiratory tract for more specifics (1).

1. Maier KL, Wippermann U, Leuschel L, Josten M, Pflugmacher S, Schröder P, Sandermann H Jr, Takenaka S, Ziesenis A, Heyder J. Xenobiotic-metabolizing enzymes in the canine respiratory tract. Inhal Toxicol. 1999 Jan;11(1):19-35. PubMed PMID: 10380157.

In section 4.3.1 (*Activation of Neural Reflexes*) the authors should consider replacing the term *sensory irritation* with *nasal irritation* when comparing to *pulmonary (*better yet *bronchial) irritation*. Both are referring to SO2-induced sensory nerve activation but at different anatomical sites and different sensory nerves (trigeminal vs c fibers). In addition, are there any studies of airway TRP receptor activation with SO2 exposure? Recognition of these important airway (and

vasculature) receptors in response to irritating agents is important when developing a mode of action for an upper respiratory tract irritant.

In section 4.3.2. (Injury of Airway Mucosa). There is a noticeable lack of review of SO2-induced airway remodeling that should include mucous cell metaplasia of airway epithelium (and associate mucus hypersecretion) and intramural fibrosis. These are key hallmarks of COPD and chronic asthma in humans that are modeled in animals repeatedly exposed to SO2 alone – see 2006 paper by Wagner et al. (2). These pathological changes are recognized key events in the pathway of chronc airway diseases caused by inhaled SO2 in animals, which have been known for at least 30 years.

2. Wagner U, Staats P, Fehmann HC, Fischer A, Welte T, Groneberg DA. Analysis of airway secretions in a model of sulfur dioxide induced chronic obstructive pulmonary disease (COPD). J Occup Med Toxicol. 2006 Jun 7;1:12. PubMed PMID: 16759388; PubMed Central PMCID: PMC1559628.

On 4-21, the title of 4.3.4, *Transduction of Extrapulmonary Effects*, does not adequately convey the information in the text and a more appropriate title should be used (e.g., Induction of Systemic Effects).

Figure 4-1 does not reflect the text – Airway hyperresponsiveness is enhanced bronchoconstriction to a pharmaceutical agent, as noted on 4-25 lines 1-4. This needs to be better illustrated in this diagrammatic progression of key events.

In the *Mode of Action* section (and possibly elsewhere in this chapter) some mention of the potential role of airway innate immune responses (i.e., innate lymphoid cells group 2) in the induction (or enhancement) of type 2 immunity that is related to innate and adaptive allergy (even though this may not yet been examined with SO2). This is an important newly recognized pathway that at least should be mentioned in the text. See references below (3, 4, 5) that have found these cells to be important with inhaled irritants (e.g., O3). Restricting the discussion to only conventional Th1/Th2 pathways are no longer enough (see editorial by Koyasu, Int Immunol 28:1-2, 2015).

3. Kumagai K, Lewandowski R, Jackson-Humbles DN, Li N, Van Dyken SJ, Wagner JG, Harkema JR. Ozone-induced Nasal Type 2 Immunity in Mice is Dependent on Innate Lymphoid Cells. Am J Respir Cell Mol Biol. 2015 Nov 11. [Epub ahead of print] PubMed PMID: 26559808.

4. Yang Q, Ge MQ, Kokalari B, Redai IG, Wang X, Kemeny DM, Bhandoola A, Haczku A. Group 2 innate lymphoid cells mediate ozone-induced airway inflammation and hyperresponsiveness in mice. J Allergy Clin Immunol. 2015 Aug 15. pii: S0091-6749(15)00936-7. doi: 10.1016/j.jaci.2015.06.037. [Epub ahead of print]

PubMed PMID: 26282284.

5. Palm NW, Rosenstein RK, Medzhitov R. Allergic host defences. Nature. 2012 Apr 25;484(7395):465-72. doi: 10.1038/nature11047. PubMed PMID: 22538607; PubMed Central PMCID: PMC3596087.

6. Holtzman MJ, Byers DE, Alexander-Brett J, Wang X. The role of airway epithelial cells and innate immune cells in chronic respiratory disease. Nat Rev Immunol. 2014 Oct;14(10):686-98. doi: 10.1038/nri3739. Epub 2014 Sep 19. Review. PubMed PMID: 25234144.

7. Koyasu S. Introduction: Innate Lymphoid Cells Special Issue. Int Immunol. 2016 Jan;28(1):1-2. doi: 10.1093/intimm/dxv073. PubMed PMID: 26712897.

Dr. Farla Kaufman

Overall the document is a good compilation of the available information and the presentation is clear and well-written. However, the quality of the evaluation, integration, and synthesis of evidence varies across the document (see comments below). The causal framework also overall seems to be applied appropriately.

The evidence for respiratory effects of SO_2 pertaining to the lowest concentrations seems appropriately characterized in the ISA.

Comments on Chapter 5

In most sections of this chapter the previous and recent evidence is comprehensive and well presented.

Many summary sections provide good integration and evaluation of the body of evidence. However, there are other summary paragraphs for individual sections that report inconsistent findings and/or lack of multipollutant models without additional synthesis of the information. Further comparison of the studies on specific characteristics (e.g. study design, analytical approach, measurement error, confounding factors and the likelihood of bias), would help to elucidate strengths and limitations and could be used to provide a more detailed evaluative and integrative assessment of the evidence. This would aid in identifying studies of higher quality as well as contribute to helping understand the basis for inconsistent findings. In addition, these summaries and conclusions, which serve to identify the specific body of evidence forming the basis for determining the classification of causality could then be cited as the key references and key evidence. Currently the degree to which this evidence is provided in these tables varies across sections.

Although summarizing information in tabular format is very helpful, there seems to be room in certain sections for reformatting, combining and/or condensing the tables, and perhaps placing some of the tables in a supplementary materials section. It is recognized that this could be a trade-off in certain sections. However, combining tables that present study characteristics with those that present results, as done in Table 5-28, would be helpful in evaluating the evidence and in shortening the document. To the degree possible, consistency across tables in what information is presented and how it is presented could aid in the readability of the tables as well as the document.

Providing more detail about individual studies would be appropriate at times. A simple example is for the study by Rubinstein et al. (1990). The text states NO_2 induced greater airway responsiveness in only one subject; however, no information was included as to the total number

of subjects tested. Knowing that one subject constituted >10% of those tested would be important information.

As it is recognized that spatial variability is an important factor in exposure assessment for SO_2, it would be useful to include information on the distance to the monitor, whenever possible, in tables describing epidemiology studies.

Although there are only a few controlled studies on the effects of mixtures, there is evidence of interactions with other pollutants. Could this be given further consideration within the current framework.

Currently our U.S. population includes a significant percentage of children who are obese and a large percentage who are asthmatic, and many disadvantaged children live in conditions exposing them to allergens. For children in general, especially for those who are overweight or obese, increased exercise is the recognized goal. In this context, perhaps a summary of findings across all respiratory effects for potential "at risk" populations could be helpful given the evidence such as: 1) the, at times substantial, variability in responses to exposures; 2) greater decrements in lung function from exposure to mixtures; 3) the recognition that previous controlled studies of exercise in asthmatics were conducted in subjects who, by today's standards, would be classified as moderate, not moderate/severe; 4) greater sensitivity conferred in the context of allergic inflammation; and 4) greater vulnerability of children.

Pg 5-35 Table 5-8, also page 5-36 line 19 - Study by Spira-Cohen et al. (2011). Mean SO_2 and upper concentration level is NR (not reported). However, the Table references an email exchange between Dr. Spira-Cohen and Dr. Patel that suggests Dr. Spira-Cohen sent these data to U.S. EPA.

Pg 5-240 Section 5.4 Although the literature may be limited in the area of neurobehavioral effects, relevant studies should be included in this review of developmental toxicity (see studies listed below).

5-241 - line 30 - Geer et al (2012) also evaluated SO_2 adjusted for O_3.

Pg 5-242 Table 5-44 Information on reproductive and developmental studies should include more study details (e.g. study design, study population, statistical analysis, covariates, etc.).

Pg 5-247 Section 5.4.1.3 There are many studies that have examined SO_2 exposure during pregnancy and fetal growth. More of the older studies could be included in the section. Most studies report an increased risk of fetal growth restriction with SO_2 exposure. Thus, perhaps this endpoint warrants further discussion. Consideration of factors such as potential exposure misclassification, temporal and spatial variability, may help to weigh the study findings and evaluate inconsistency of findings across the body of literature.

5-250 line 16 Table 5S-13 Brauer et al. (2008), the mean SO_2 concentration for this study is listed as 5.3 ppb. However, the mean value is actually ~2.2 ppb, as in the publication SO_2 is 5.3 µg/m3. 2.2 ppb is close to the limit of detection for this study (2 ppb). Thus, exposure misclassification in this study might have decreased the likelihood of detecting an effect if one had been present.

5-250 Section on Birth Weight - It would seem that an expanded discussion of this outcome is warranted as there are a number of newer studies that should be viewed along with some more of the older studies (e.g. Lin et al. (2004).

5-251 line 26 As mentioned above, the summary sections should be expanded to include an integrative evaluations of these studies including the strengths and limitations. In addition, evaluating studies on the basis of important aspects such as the spatial and temporal variability of exposure is important in understanding how much weight to place on a particular study. Important factors such as a very low level of exposure, as noted above in the study by Brauer et al. (2008), or very low variability in SO_2 levels as in the study by Bell et al. (2007) provides a context for the study results.

5-252 Section on Birth Defects - There are two meta-analyses available but they were not included here, not sure why.

This section could also benefit from some evaluative summary such as to the characteristics of the studies in order to discern reasons for inconsistent findings, i.e. level of exposure, covariates considered, likelihood of exposure misclassification, classification of defects, etc.

Table 5-46 – "Under Key Evidence" it states that "associations not evaluated in co-pollutant models" . I am not clear which studies this is referring to since the three studies cited under "key references" (Liu et al., 2003; Le et al., 2012; and Sagiv et al. 2005) all evaluated co-pollutants, and found that associations with SO_2 remained significant. In addition, this was the only limitation mentioned in the table for this outcome; however, no mention of co-pollutant models was made in the summary for the corresponding section. The table should reflect the text for the corresponding section and reference the relevant studies.

In the table on 5-244 - Uncertainty around the timing of exposure across studies for the BW and LBW outcomes is noted in Table 5-46. However, for outcomes such as birth weight and low birth weight it is conceivable that different mechanisms could be influencing the outcome at varying timing of exposure, including early in pregnancy and late in pregnancy.

P5-256 as stated in the ISA -
"The state of California, under the auspices of Proposition 65, the California Safe 22 Drinking Water and Toxic Enforcement Act of 1986, has listed sulfur dioxide as a 23 chemical known to

cause reproductive toxicity based on evidence from laboratory animal 24 studies and epidemiologic studies. However, much of this evidence is from toxicological 25 studies with exposure to SO2 at 5,000 ppb or greater (beyond the scope of this ISA); 26 effects seen at the higher doses include male reproductive effects on sperm and fecundity, 27 as well as oxidative damage to the male reproductive organs, changes in birth weight or 28 litter size, delayed reflexes in early life, and aberrant behavior of pups after in utero 29 exposure. Epidemiologic evidence used for this listing is also evaluated under differing 30 criteria than are employed for the ISA."

Correction about the basis for the listing of SO_2 under Prop 65 -
The listing for SO_2 under the State of California's Proposition 65 is for developmental toxicity. The particular type of developmental toxicity noted by the Developmental and Reproductive Toxicant Identification Committee that formed the basis for this listing was intrauterine growth restriction. Only two toxicological studies were considered by the Committee for this outcome, and, as noted in the ISA, they were conducted at exposures greater than 5,000 ppb. Although studies relevant to male reproductive toxicity were considered, the DART IC committee did not find sufficient evidence of toxicity to list this endpoint under Proposition 65.

I am also assuming that the final document will have a Table of Contents that includes the various headings and sub-headings for the chapters and there will be separate lists for tables and figures.

Additional Studies of Reproductive Toxicity

Yao et al., Feb 2015
Chronic SO_2 inhalation above environmental standard impairs neuronal behavior and represses glutamate receptor gene expression and memory-related kinase activation via neuroinflammation in rats.

Yun et al., 2013
SO_2 inhalation causes synaptic injury in rat hippocampus via its derivatives in vivo.

Lin et al., 2014
Multilevel analysis of air pollution and early childhood neurobehavioral development.

Jung et al., 2013
Air pollution and newly diagnostic autism spectrum disorders: a population-based cohort study in Taiwan.

Vrijheid et al., 2011
Ambient air pollution and risk of congenital anomalies: a systematic review and meta-analysis.

Tong et al., Jan 2015
The association between air pollutants and morbidity for diabetes and liver diseases modified by sexes, ages, and seasons in Tianjin, China.

Dr. Richard Schlesinger

Comments on Chapter 4

Chapter 4 characterizes scientific evidence on the dosimetry and modes of action for SO_2. Dosimetry and modes of action are bridged by the absorption and reaction of SO_2 in the epithelial lining fluid to form SO_2-derived products (e.g., sulfite and/or S-sulfonates) that are widely distributed throughout the body.

a) *To what extent is the discussion of the chemistry of inhaled SO_2 and the processes of absorption, distribution, metabolism, and elimination accurate, complete, and relevant to the review of the SO_2 NAAQS?*

The discussion of inhaled SO2 and processes are complete and relevant to review of the NAAQS.

b) *Please comment on the discussion comparing endogenously generated and ingested sulfite with that derived from ambient inhalation.*

The discussion of endogenous and ingested sulfite with that derived from inhalation of SO2 could be a bit more concise.

c) *To what extent are the discussion and integration of the potential modes of action underlying the health effects of exposure to sulfur oxides presented accurately and in sufficient detail? Are there additional modes of action that should be included in order to fully characterize the underlying mechanisms of sulfur oxides?*

See General Comment re section 4.1 below. SO2 may result in effects due to generation of sulfuric acid, and that is not made clear in the document.

General Comments

<u>Section 4.1</u>

It is noted here that understanding the mechanisms of biological responses may provide biological plausibility for effects observed in the epidemiological studies, which are described in Chapter 5. However, it seems that it would be best to move Chapter 4 after Chapter 5, since the former actually introduces health effects in the context of mechanisms that are the discussed later in Chapter 5. This results in much redundancy. As a toxicologist, I would prefer to read about the

biological effects in Chapter 5 and then read potential mechanisms that would convince me of the biological plausibility, rather than the other way around.

An example of this issue is on page 4-15, where beginning on line 10 there is a discussion of health outcomes rather than mechanisms. Other examples are on page 4-16, beginning on line 12; page 4-17 lines 3-9 and beginning on line 29.

An alternative would be to limit the current Chapter 4 solely to dosimetry and then have a section at the end of Chapter 5 dedicated to mechanisms.

Section 4.2

The first paragraph of this section notes that few studies have been done since the last ACQD. However, there are detailed discussions of studies that were done prior to and incorporated into the prior ACQD. It would be best to summarize results of these earlier studies to make the chapter more concise.

Equation 4.1. The manner in which this is presented is a bit confusing.

Page 4.5, lines 4-7. Triggering of asthma may in fact reduce the amount of gas reaching the lower respiratory tract.

Page 4-7, lines 1-16. Line 4 notes that SO2 derived products are widely distributed throughout the body very early after exposure, yet line 16 notes that SO2 derived produces are only slowly absorbed into the blood, which would suggest that they are not widely distributed.

Page 4-12,

line 1. What is meant by the statement that SO2 is a reactive antioxidant gas?

Line 29. Penetration into which region of the respiratory tract?

Page 4-23, lines 29-32. If an endpoint is an effect measured in the clinic, how is this different from an outcome measured at the organism level?

Figure 4-1 Some of the evidence arrows are not consistent with discussion in the text as to what has been seen in animals vs humans.

Dr. Frank Speizer

The comments below are being submitted to meet the deadline for inclusion for discussion, but are preliminary and will be added to and revised both before and after the meeting on Jan 27-28, 2016. They are limited to the Preamble and Chapter 5, and do not attempt to answer the specific questions posed by Dr. Vandenberg in his memorandum of Dec 2, 2015. I assume these will be discussed in detail at the meeting.

Preamble to ISA

I would like to complement the Staff on the current state of this document. Although I have minor thoughts as to some of the wording, I have watched this document evolve of the last 15 years or so and believe it now is an impressive consensus of both structure and form that is critical for going forward for the whole Clear Air Act, CASAC and standard setting process.

Chapter 5

General Comment: The organizational structure of the Chapter follows a logical sequence. A vast amount of data and publication were reviewed effectively and summarized. Although as indicated below there are some inconsistencies in the way the tables are presented I suspect that most of these arise out of the lack of details presented in many of the paper. One comment that I suspect will not be appreciated is my concern that as an ISA document we have fallen back into the pattern that lead from a Criteria Document to an ISA, which is to consider almost all publications irrespective of quality on an equal "need to include" basis.

Table 5.1: Suggest some slight reformatting of the table. I recognize that the studies are presented alphabetically by first author, it turns out that only the studies of Koenig et al summarized on page 5.9 have subjects under age 18 (need to check need to check Jones and Magnussen 1990, and Linn, 1983a; need also age range for Tunnicliffe 2003). All the other studies are in adults. Suggest you break out by 12-<18 and 18 and above as I believe that "life course" will become a defining issue as we move forward with more recent studies and if we do not have enough will be part of the remaining uncertainty.

Page 5-17. These two paragraphs don't match up. In paragraph 1 the discussion focuses on the fact that you have "responders and non-responders". In paragraph 2 if I read it correctly the authors are rejecting the data because the sum of the studies discussed in paragraph 1 are not homoscedastic. By the nature of responses they shouldn't be and thus this seems the wrong reason for giving Goodman et al no further consideration.

Page 5-21. Although the data are summarized elsewhere, it would be useful at the end of this, and I assume other section, to offer a concluding sentence as what the interpretation of the previous work is.

Page 5-25. In the Dales et al, last column it is not clear how to interpret the effect of Diurnal change related to 12 hour averaging time., Which 12 hours?

Page 5-26. 5-27 In this study there are lots of tests reported with few being statistically significant. Do the authors (either of the paper or this document have a clear interpretation of whether the results are negative or positive? The text simply reports the findings.

Page 5-29-32. We are slipping into an old pattern of compulsive reviewing of the world literature, without selection: I see no reason for Table 5-7, as surely this is a table that appeared in the 2008 document and the results are described in paragraph above and last sentence in section on page 5-33.

Page 5-33, last paragraph. Too many decimal places are presented in OR and Confidence Intervals

Page 5-37 38. The discussion of the toxicological finding without presenting a table in contrast to the extensive review tables for the human studies seems striking, particularly because the tox finding sound impressive. Suggest add a table summarizing these studies discussed.

Page 5-39, Figure 5-2. Suggest reorder or divide into two separate figures to separate children only from adults and mixed. The details provided in Table 5-9 would also have to be reordered.

Page 5-42, para 1. Issue of ED visits potentially being less serious and caution on children less than 5. I suggest this be reconsidered. First the practice in EDs has been to watch and observe in the ED rather than admit does not mean that these are less serious cases. Particularly in children hospital admission are always traumatic and physicians have learned to manage very serious conditions, particularly asthma, on the ED floor. With regard to the <5 this is not an issue in the ED or hospital admission (as it might be on a death certificate) as clearly “wheeze” is wheeze when a physician sees a child. (this is discussed further on page 5-51 and might be included or modified here).

Page 5-43 48, Table 5-10. This table seems to belong in Chapter 3 rather than here as no biologic data are presented. In fact the text describing results from these sites uses summary statistics that really do not appear in table.

Pages 5-63-66. Another example of compulsive completeness 3 pages to say 2 studies are null! End of this section before starting section 5.2.1.3: I find no summary that highlights the important observations discussed on page 5-56 on the delayed hyperallergenic response to prior

SO2 exposure. I would have thought it should have been brought forward in summary of acute responses.

Page 5-71, end of line 5. Give reference for this statement. (assume it is Pencock et al).

Ditto: Could have left out lines 26-32.

Page 5-88 Summary of Respiratory Infections. I believe the interpretation is more conservative that it should be particularly as related to young children. Too many caveats are presented to minimize importance of findings. (Eg lack of multiple studies of same outcomes, effects of copollutants, and non discussion of lag structures, all issues that in the past had little impact of conclusions about major outcomes, and when subsequently considered have had modest modifying effects.

Page 5-105,part of Table 5-20. For the study of Steinvil, the effect estimates need more detail. I assume the FEV and FVC changes are in ml, however the numbers in the ratios of FEV/FVC make no sense since units are not specified.

Page 5-108, Table 5-21. Give units for effect in study. For Altuq study indicate what lung function was used.

Page 5-120 Table 5-24, for Zhao et al. Last entry on last column something missing for “Furry 3pet or pollen”

Page 5-122, line 1-3. The statement does not match up with the details presented in Table. Wheeze and nocturnal attacks of breathlessness are significant in the Table and are stated as no association in text.

Page 5-140 Conclusion. Mention should be made of the increased sensitivity to allergens as part of the conclusion.

Page 5-167-169 and Table 5-31. The discussion and table are incomplete in that what is left out is the evidence for Asthma exacerbation, particularly in children. I think another section after Asthma Development needs to be added as I believe the evidence for exacerbation is more than suggestive and actually causal. The summary of the evidence presented on page 5-169, lines 15-23 set up the individual with conditions that contribute to recurrent exacerbation, and thus are part of the causal chain for chronic effects.

Page 5-204 207, Table 5-39. In last column under Copollutants in each case where the SO2 association is mentioned as being robust or remaining positive it would be useful to add the word “significantly” if that is the case and if not add “not significantly”

Page 5-254, line 1-13. Reference is made to Table 5-44. It is true that the data are summarized there but because the respiratory effect are so different from the birth defects it would seem better to move this material to the respiratory section, or to make a separate sub-table so that it is not lost in the larger table of inconsistent and negative results.

Dr. James Ultman

Overall Comments

The EPA staff and its consultants should be commended for a well-written document that lays out in detail what is known about the inhalation toxicity of SO2 and its relation to the NAAQS The overall revisions that I suggest are:

1) Shorten the Executive Summary to eliminate unnecessary details that are covered in other chapters of the ISA.
2) Break chapter 5 into two separate chapters.

Executive Summary

General Comments

I believe that an executive summary should be to the point and contain "bottom line" information. As it stands, the Executive Summary restates to much of what is covered in chapter 1. For example, lines 11-25 on page xxxix concerning dispersion modeling in the Executive Summary are essentially the same as lines 8-24 on page 1-8 of chapter 1. I suggest that unnecessary detail be eliminated from the Executive Summary.

Specific Comments

Sources and Human Exposure:

- I could find no mention of background SO2 levels or their implication for standard setting.
- On page xxxix, it may be worthwhile to add a sentence regarding the relationship between a 5-minute maximum SO2 concentration and a 1-hr maximum SO2 concentration and its relevance to the use of a 1-hr standard.

Dosimetry and Mode of Action

- On page xli, is it possible to say something about the relative retention of sulfites in respiratory compared to systemic tissues and what this implies about the relative toxicity of inhaled SO2 to those tissues (see lines 22-26 on page 1-12 in chapter 1)?

Chapter 1

Perhaps, there should be a more direct discussion of uncertainties in the Conclusions section of this chapter.

Chapter 4

General Comments:

Response to charge questions: This is a well-written chapter that generally meets the objectives of a) being a complete and accurate description that provides a good context for chapter 5 on the health effects; b) contains a good discussion and comparison of inhaled, injested and endogenous pathways for SO2/sulfite; c) provides modes of action for short-term and long-term respiratory as well as for extra-pulmonary effects. Following are some suggestions for reorganization and clarification:

In section 4.2.1 on SO2 chemistry, the aqueous chemistry of SO2 is described (Eq. 4-1), but a discussion of SO2 destruction of disulfide bonds in amino acids and proteins (sulfitolysis) is postponed until section 4.2.3 on SO2 distribution. Auto-oxidation reactions of SO2 to generate free radicals is not mentioned until section 4.3 on mode of action. Enzymatic detoxification of sulfite by its oxidase is not described until section 4.2.4 on metabolism. Perhaps a complete discussion of these different reactions placed in section 4.2-1 would be better.

Section 4.2.2 on absorption begins with a comparison of the absorption distribution of SO2 and O3. This discussion is based on the assertion that the upper to lower distribution of the two gases is determined by their relative solubility. This is not entirely true since the relative diffusion coefficient and relative reaction rate also play a role. For example, for a mucous layer that is relatively thick (*i.e.* large Damkohler number), the SO2 uptake rate is proportional to the solubility times the square roots of the diffusion coefficient and pseudo first order reaction rate constant. I think that a conceptual model of respiratory gas transport is needed at the beginning of this section; this should include a discussion of how overall uptake and uptake distribution depend on inhaled dose (concentration×ventilation rate) and gas properties.

Also in section 4.2.2 on absorption, ventilation is given in units of m^3/kg-day and m^3/min. The document points out that "ventilation rates normalized to body mass should not be confused with median daily ventilation" but the possible significance of normalization by body mass is not discussed. Since inhaled dose is the product of ventilation and inhaled concentration, units of m^3/min are an indicator of the maximum total dose that the respiratory tract as a whole will receive. Assuming that body mass is an indicator of lung size, normalization of ventilation by body mass is a crude indicator of the average dose per unit area that the airway film receives. I think that a brief discussion of the two ways in expressing ventilation would be worthwhile.

In the summary paragraph on absorption on the bottom of page 4-4, there is no mention of the effect of exposure concentration on percent absorption. I think it is worth saying that there is some experimental evidence indicating that exposure concentration plays a very small role compared to ventilation and mode of breathing (nasal or oral).

Specific Comments

page	line/figure	Comment
4-2	5-8	To clarify Henry's Law and its relationship to solubility, I suggest replacing the sentence "Henry's Law shows…in the gas phase" with "The Henry's Law constant, defined as the ratio of partial pressure or concentration of SO2 in a gas phase to SO2 dissolved in a liquid phase, is an inverse measure of solubility."
4-3	7	Please add the phrase ", expressed as a percentage of amount inhaled," before the term "absorption" to ensure that the reader understands exactly what it meant.
4-4	5	The meaning of this sentence would be clearer if "also breath oronasally" was replaced with "also show an increased contribution of oral breathing"
4-11	29-34	Unnecessarily redundant with previous sections. I think that this portions of this paragraph could be moved to the beginning of section 4.2.1 on chemistry.
4-12	28-29	It's not clear to me that "higher concentrations lead to greater penetration." Is this statement based on experimental observation, a respiratory transport model or is it pure conjecture?
4-13	19	I don't understand what is specifically meant by "mirrors." It would help To be more specific.
4-22	20	Should "exogenous" be corrected to "endogenous?"

Chapter 5

As I read this chapter, I found it particularly difficult to see the "forest through the trees." I think that this arises because the extreme length of the chapter. I think it would benefit the ISA to divide the material in chapter 1 into two chapters. Perhaps, there could be one chapter on motality and morbidity effects of inhaled SO2 and a second chapter on extra-pulmonary effects of SO2 inhalation.