December 21, 2006

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Re: Preliminary Review of the Brown *et al*. Manuscript "An Assessment of Risk from Particulate Released from Outdoor Wood Boilers"

Dear Phil:

At your request, Gradient has conducted a preliminary review of the draft Brown *et al*. manuscript "An Assessment of Risk from Particulate Released from Outdoor Wood Boilers." We understand that you obtained this manuscript from a trade association that requested and freely obtained the manuscript directly from Dr. Brown. It is also our understanding that this version of the manuscript has been accepted for publication in the journal *Human and Ecological Risk Assessment (HERA)*.

Based on our preliminary review, we have identified a number of major factual errors and flaws in methodology that undermine the validity of the risk assessment findings and the conclusions that the authors have drawn from their analysis. We have identified errors and methodological flaws along each step of the risk assessment process that in general contribute to the overestimation of health risks associated with outdoor wood boiler (OWB) emissions. Due to the magnitude of these errors in both fact and scientific judgment, we have strong reservations regarding the publication quality of this manuscript.

Among the most glaring errors in the manuscript involves the following statement at the end of the Conclusions section of the manuscript (p. 19): "In summary, cancer appears to be the sensitive endpoint with a 7-months-a-year, lifetime exposure of 6 $\mu g/m^3$: it yields over 1 in 1000 risk of cancer, presented above and as reported in Table 3." This statement is inconsistent with the data presented earlier in both the text of the paper as well as in Table 5, which both relate a lifetime cancer risk of 1 in 100,000 (10^{-5}) to a fine particle concentration of 6 $\mu g/m^3$ (note that the reference to Table 3 in the statement above is incorrect, as this table does not provide results of the cancer risk assessment). Actual cancer risks computed by Brown *et al*. are thus on the order of 100 times less than what is asserted in the Conclusions section of the paper. The fact that the above statement is clearly an erroneous misstatement is especially important given that it has been highlighted by both the press and NESCAUM as a key finding of the Brown *et al*. analysis. Although the "correct" estimate of 10^{-5} excess cancer risk associated with a 7-months-a-year, lifetime exposure of 6 $\mu g/m^3$ is itself an overestimate due to major methodological flaws and use of scientifically unsupported assumptions (as detailed further below), it falls within US EPA's CERCLA acceptable risk range of 10^{-4} to 10^{-6} (1 in 10,000 to 1 in 1,000,000) excess cancer risk.

Although a number of serious methodological flaws and errors in analysis are detailed below, we feel it necessary to call special attention to perhaps the most egregious of these flaws, namely the reliance on a total of 4.3 hours of ambient air measurement data as an estimate of chronic lifetime (*e.g.*, 30-year) exposure in the cancer risk assessment. As stated on page 5 of the manuscript, "The release by OWB of $PM_{2.5}$ as measured by Johnson (2006) will be the basis for this risk assessment." More specifically, Brown *et al*. extract the mean $PM_{2.5}$ measurement of 186 $\mu g/m^3$ and the 95%

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percentile of 665 μg/m^3 from Johnson (2006) to serve as exposure point concentrations (EPCs) in their cancer risk calculations. However, only buried in a footnote (footnote #3 on page 7) is it acknowledged that the entire Johnson (2006) measurement dataset comprised a total of 4.3 hours, and nowhere is it clearly acknowledged the extremely limited nature of the dataset collected in the Johnson (2006) pilot study- *i.e.*, measurements made over two days at just one residential property for one wood boiler with a nine-foot stack, at a range of distances from 50 to 150 feet from the stack. Importantly, Johnson (2006) refers in a number of places to his measurement study as a "pilot study," an "exploratory study," and a "screening level evaluation," stating on page 1166 that "this pilot study was not intended to quantify 24-h or longer-term average exposures (*e.g.*, a complete heating season)." In fact, the Johnson (2006) paper concludes with the following statement: "This screening level monitoring study recommends future research that would collect monitoring and exposure data of sufficient quality to support the evaluation of potential risks."

Furthermore, Brown *et al.* seem unaware of methodological issues raised by Johnson (2006), and many others in prior publications, regarding the well-known tendency of the DataRAM to overestimate ambient $PM_{2.5}$ concentrations. The DataRAM is a light-scattering monitor that, due to changes in light-scattering properties with the composition and size of ambient particles, provides an approximate estimate of $PM_{2.5}$ levels rather than an accurate measure of $PM_{2.5}$ concentrations. As acknowledged in Johnson (2006) but not in the Brown *et al.* manuscript, the U.S. Forest Service uses the DataRAM to provide general trends of ambient wildfire smoke $PM_{2.5}$ concentrations and recommends using a DataRAM correction factor of 0.37 to 0.48 based on tests showing that it reports high overestimations of $PM_{2.5}$ when sampling biomass combustion aerosol compared to gravimetric sampling. In other words, the U.S. Forest Service reduces DataRAM data by a factor of 2.1 to 2.7 to provide more accurate measures of ambient wildfire smoke $PM_{2.5}$ concentrations. Johnson (2006) chose not to correct his OWB field measurement data since the application of any correction factor would not have changed the qualitative nature of his study results, but he makes the observation "Nonetheless, a more conservative assessment of these data could interpret the $PM_{2.5}$ concentrations as indicators of general trends relating to monitoring distance from the OWB, boiler operating modes, and time after fuel loading." Based on the well-recognized high bias of the DataRAM, it is clear that data correction is a necessary step prior to any quantitative use of DataRAM data, such as in a risk assessment.

Other serious errors in fact and scientific interpretation include the following:

- On page 4 of the manuscript, Brown *et al.* give one of their assumptions as being that OWB's have 6.9 times the weight of PAHs compared to non-catalytic wood stoves. However, if you look at the citation they provide (NYS EPB, 2005, Table 2, p. 8), this document actually states that OWB's have 0.58 the amount of PAHs per unit particulate matter, compared to non-catalytic wood stoves, so there is another error here of a factor of 12. That is, since Brown *et al.* are doing their risk calculation on a per unit particulate matter basis, they have not done the PAH adjustment correctly.
- As shown in Table 2, Brown *et al*. multiply calculated excess lifetime cancer risks by an additional uncertainty factor of 10, with the table noting that this was done to account for "More particulate w/higher T, dispersion changes, different woods, OC increases." On page 17, however, Brown *et al.* refer to the fact that "the upper bound risk calculation is a factor of 10 higher based on the assumption that more semi-volatiles would adhere to particles in the lower temperature of the Northeast U.S. than in the Los Angeles area where Fine and his coworkers (2004) completed their analysis." Notwithstanding the confusing references to the origin of this

uncertainty factor, nowhere in the text is there any justification for the selection of a factor of 10 to account for these uncertainties. Furthermore, some of the rationale offered for such an uncertainty/conversion factor would not appear to support use of such a factor. Because carcinogenic risk is being calculated on a risk per unit particulate mass basis, it is not correct to include uncertainty factors for "more particulate" or "poor dispersion" or "increased OC mass," because the calculated risk will be applied to actual particulate mass concentrations of interest. That is, it is already assumed that risk will increase in proportion to particulate concentration, and it is inappropriate to "double count" within the unit risk, for the effect of increased particulate mass due to poor dispersion or increased quantity of organics. Unless an uncertainty factor is more fully justified (the authors are already using upper-bound values in many cases), this error makes the calculated risk value 10-fold too high.

- For non-cancer health effects, Brown *et al*. do not provide adequate discussion of the origin of the Unhealthy Air Day Concept (apparently derived from a prior Brown publication that appears to not have undergone peer review) and the scientific rationale underlying the "At Risk" (90 μg), "Moderate Risk" (120 μg), and "High Risk" (250 μg) 6-hour inhaled dose levels. Notably, Brown *et al*. assert that a 6-hour fine particle concentration of 30 $\mu g/m^3$ results in a "High Risk" of "serious health problems, hospitalization, and even death" (CONCLUSIONS, p.19, and last row of Table 5, p. 29). The National Ambient Air Quality Standard for fine particulate was for many years 65 $\mu g/m^3$ (24-hr average), and was just recently (Nov. 2006) revised down to 35 $\mu g/m^3$. The NAAQS is designed to be protective of public health "with an adequate margin of safety," and attributing "death" to an ambient level below the NAAQS is unduly alarmist.

- Throughout the manuscript, Brown *et al*. misrepresent the $PM_{2.5}$ literature, citing literature for ambient, all-source PM as providing evidence for the health effects of wood-smoke PM without acknowledging the large remaining uncertainties regarding the identity of potentially toxic PM constituents. As summarized in such documents as the US EPA 2004 *Air Quality Criteria for Particulate Matter*, it is the consensus of scientists and regulators that at this time we can neither pinpoint any specific components that are more toxic than others, nor eliminate any component(s) as being of negligible toxicity. In addition, Brown *et al*. inappropriately cite epidemiological studies that relied upon daily or even annual average PM data in their analyses as providing support for health effects induced by a "few hours" of exposure. Specific examples include:
    - the reference on page 4 to the Zanobetti *et al*. (2000) and Dockery *et al*. (1993) studies as supporting the statement that "there are studies that support that cardiopulmonary health effects are induced by a few hours of exposure."
    - the reference on page 11 to the Dockery *et al*. (1993) study as showing "2-hour and 24-hour lags between the increase in PM and the health outcome."
- Brown *et al*. provide an erroneous citation for Koenig *et al*. (1993) and mischaracterize the data and conclusions in this paper.
    - Brown *et al*. incorrectly give the title of this paper as "Pulmonary function changes in children associated with particulate matter air pollution in a wood burning community" when in fact, the actual title is "Pulmonary

function changes in children associated with fine particulate matter."

  - In a number of places (pages 10, 11), Brown *et al*. (1993) refer to this paper as providing evidence for the health effects of woodsmoke PM after exposures of 2 to 4 hours or less in duration to concentrations in the range of 12 to 29 $\mu g/m^3$. However, examination of the Koenig *et al*. paper does not support such a statement. Although Koenig *et al*. observe that residential wood burning was likely a major source of particulate matter measured in their study, the ambient PM measurements were not specific to woodsmoke and included contributions from other sources such as automotive and industrial sources. In addition, the Koenig *et al*. paper used a light-scattering instrument and provided data in light-scattering units (*e.g.*, light-scattering coefficients) rather than $\mu g/m^3$. Furthermore, light-scattering coefficients reported in the paper are for 12-hour and weekly-averaged data rather than the 2 to 4 hour periods asserted by Brown *et al*.

- On pages 4-5, Brown *et al*. incorrectly refer to the fact that US EPA's Air Quality Index (AQI) "does not consider specific cardiopulmonary risks." This is simply incorrect, as the AQI for $PM_{2.5}$ is based on the $PM_{2.5}$ NAAQS which were developed to be protective of adverse health effects, including cardiopulmonary risks specifically. Furthermore, health effect statements provided with AQI values specifically refer to possible aggravation of heart or lung disease in people with cardiopulmonary disease and older adults.

- Brown *et al*. provide incomplete details on their box model used to assess potential indoor exposures (page 12). In particular, no information is provided on some important model assumptions, including assumptions regarding the ambient particle infiltration factor and the indoor particle deposition rate.

As discussed above, we have identified the issues listed above following only a preliminary review of the manuscript. Thus, there may be additional errors and methodological flaws in the paper. Given the serious nature of the errors and methodological flaws that we have identified in our preliminary review, it is clear that the Brown *et al*. manuscript would benefit from additional careful re-review. Even beyond our opinion that reliance on such a limited dataset to characterize lifetime exposures raises such large uncertainties that undermine the entire cancer risk assessment and any conclusions that can be drawn from it, we have identified other errors and scientifically unsupported assumptions that serve to reduce the Brown *et al*. excess cancer lifetime risks associated with a 6 $\mu g/m^3$ woodsmoke $PM_{2.5}$ concentration by a factor of approximately 120, *i.e.*, for a 7-months-a-year exposure to 6 $\mu g/m^3$ each year for 30 years, the "corrected" Brown *et al*. methodology yields an actual calculated cancer risk of approximately 1 in 10,000,000 (one in ten million).

Please do not hesitate to contact me if you would like to discuss further.

Sincerely,

GRADIENT CORPORATION

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