ESTIMATED GROUND LEVEL CONCENTRATIONS OF POLLUTANTS FROM WASTE-TO-ENERGY FACILITIES

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Discussion by

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The authors stated in the abstract that they would describe the ground level concentration of "toxic material." I believe that they should have stated that they would try to determine if materials were emitted that would result in toxic concentrations. My description is a more positive approach and tends not to scare people.

In addition I would like to discuss several technical points:

1. The authors put a disclaimer ("a word of caution") in the conclusion regarding the validity of the EPA dispersion model. This disclaimer should have been put in the discussion section of the paper; therefore, one could have an understanding before he reads the paper.

2. A similar disclaimer should also have been used to describe the sensitivity and accuracy of the sampling methods for these trace materials. To my knowledge, the accuracy data for these testing methods has never been established.

3. I would like the authors to specify the stability classes that were used to present Table 5.

4. Table 5 only presents the maximum concentration. In what percentage of the total annual time does this occur and what is the annual average?

In conclusion, the authors have made a significant effort towards clarifying the myths about trace metal emissions. If we are going to utilize waste to energy plants in combating our waste disposal and energy problem, more similar work will have to be done for us to obtain public acceptance.

Discussion by

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The authors have made a valuable addition to the literature in their assessment of eight toxic pollutants as emitted from waste to energy facilities and their effect on ground level concentrations.

We would like to take this opportunity to express our opinions on various aspects of the paper.

1. The authors should consider where the heavy metals appear in the refuse and a quantitative analysis expressed for both the Braintree and St. Louis Plants. It would be interesting to compare the refuse input to both plants to better judge the PTMAX outputs and stack emissions.

2. The sampling Matrix for the St. Louis plant (Fig. 1) does not show the mercury (Hg) and how it was generated to appear in Table 3.
3. In the analysis of the mercury emission at the point source, was the pollutant considered as a solid or gas by the authors and did the authors consider the different transport characteristics for both solid and gaseous emissions?

4. The authors express the maximum ground level concentrations in g/m$^3$. Many national ambient air quality limits are expressed in terms of time (hours, days). It would be beneficial if the authors would present a method for converting the ground level concentrations to both forms for ease in comparison.

5. The authors mention that the EPA PTMAX dispersion program is not an absolute tool for ground level concentrations. Such matters as wind shear, complex terrain and meteorology must be reviewed to adapt the model to meet specific site needs. This point should be strongly emphasized and that different versions of atmospheric modeling are in existence and sometimes preferable.

6. The authors' conclusions might lead one into a false sense of security as relating toxic pollutants from waste to energy facilities to ground level concentrations.

   It would appear that extensive research is required into all types of refuse burning plants such as refractory lined furnaces with waste heat boilers, wet scrubbers and cooling tower and electrostatic precipitator installations.

   Toxic pollutants from waste to energy facilities must certainly be coordinated with the type and style of the facility.

   Before the public and regulatory agencies are lulled into a false sense of security an extended research into all toxic pollutant emissions must be performed.

   This paper is a first step in a long journey to the final conclusion.

Discussion by

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The authors have tackled a singularly difficult and complex topic and have made a “best efforts” assessment of the ground level concentrations of a limited list of pollutants emitted by two waste to energy facilities. I commend the authors for their valiant effort, but I hasten to also express a concern. Assessments of the nature discussed by Mr. Olexsey are unfortunately subject to the compounding of uncertainties. My research field has been in analytical chemistry and spectroscopy, consequently I may have a more sensitized appreciation of the uncertainties in quantitatively determining the eight emissions discussed by the speaker, to say nothing of the multitude of potentially toxic trace organic emissions. In this context it is timely to mention the controversial matter of alleged increase in emissions of the dioxins, particularly the notorious 2, 3, 7, 8 tetrachloro-p-dibenzo dioxin. No analytical chemist will argue that there are major uncertainties involved in collecting a truly valid sample, in extracting or isolating the pollutant in question from the sample prior to measurement, and finally in performing the quantitative determinations. When these uncertainties are then compounded further through the use of a computer based model of pollutant dispersion without verification by any ground level measurements, the estimated maximum pollutant concentrations at ground level may be subject to intolerable uncertainties. Fortunately the reduction ratios predicted by the dispersion model are of such magnitude that the eight pollutants considered do not appear to pose an environmental threat.

With reference to several details, the following are worth noting: (a) there seem to be some inconsistencies in the data presented in Table 3 and 5 which are reflected in inconsistent reduction of stack emission to ground level concentration ratios; (b) Table 3 shows that Pb emissions increased by a factor of 4.8 when 7 percent RDF was used; this factor translates to a Pb content in the RDF 70 times higher than in coal. Is this realistic? (c) Table 4 shows that stack gas temperatures and volume did not change significantly when RDF was added as a fuel. Why then the large change in Hg (particulate)/Hg (vapor) ratio? Less than 5 percent of the Hg is found on the particulate when only coal is burned, whereas 92 percent is reported on the particulates when only 7 percent RDF is burned. Do these data imply that Hg emissions shifted from vapor to particulates as the rather low percentages of RDF were burned, or could the results simply be a manifestation of analytical uncertainties in the data base?
Discussion by

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MODEL

The authors correctly point out that the model that they have used, PTMAX, has limited use in all but idealized conditions. It is noted that PTMAX is not applicable if aerodynamic downwash around buildings in the vicinity of the source can affect the plume emitted from the stack [1]. Corrections are also necessary for stack aerodynamic effects [2]. To assess the effect of limitations and modifications to the model results, the following information is required:

1. Wind speed
2. Stack gas exit speed (or stack diameter)
3. Effective height of associated and surrounding structures

The output of PTMAX is a function of atmospheric stability class and wind speed neither of which are noted in Table 1. Since these factors are required, the results shown in Table 5 lose much of their meaning without specifying wind speed and stability class.

The earlier effect of stack and building entrainment would be most applicable in the case of Braintree. Table 4 indicates that its stack is only 24.8 m (81 ft) high. Furthermore, if, as it seems to be printed, the Braintree total gas flow volume is only 9.9 m³/s, a 3 m (9.8 ft) stack diameter will result in an exit velocity of only 1.4 m/s (4.6 ft/sec) while a 2 m (6.6 ft) stack diameter will give an exit velocity of 3.15 m/s (10.3 ft/sec). Based on the critical ratio: \( \frac{V_s}{U} = 1.5 \)

\[ \text{where } V_s = \text{gas exit velocity} \]
\[ U = \text{wind speed} \]
the wind speed must be greater than 2.1 m/s and 4.7 m/s (4.7 and 10.5 mph respectively) to avoid stack entrainment.

CHLORIDES

Granting model inaccuracies and interpretive factors for evaluation of the model results, there is, in our opinion, a more serious consideration that must be addressed: the casual dismissal of chlorine emissions.

It has been noted most recently [3] that HCl emissions may undergo neutralizing reactions in the atmosphere by ammonium compounds. There is, however, competition for ammonia with sulfate [4, 5, 6, 12] which limits its availability for chloride. This leaves free HCl aerosol which will have the same corrosive action in the atmosphere as it will within the plant system. It has been noted [7, 8, 9, 10] that almost all chlorides in fuels, whether from incineration, coal burning or oil burning, are emitted in gaseous or aerosol HCl form. There is supporting evidence [4, 11, 12] that sea salt chlorides serve an important role in the generation of sulfate species, as well as \( \text{H}_2 \text{SO}_4 \), with subsequent release of HCl. These studies, however, fall short in that they do not account for the freed HCl.

Our own work with respect to acid fallout problems from oil-fired boilers has led us to hypothesize a catalytic-like role for HCl [13] with the deliquesce properties of chloride species as its key:

\[
\text{Non deliq. metallic oxide + HCl } \rightarrow \text{Metallic ion + H}_2 \text{S}_2 \text{O}_4 + \text{HCl + H}_2 \text{O} \rightarrow \text{Metallic sulfate + H}_2 \text{SO}_4 + \text{HCl + H}_2 \text{O} \rightarrow \text{Metallic sulfate + H}_2 \text{SO}_4 + \text{HCl} + \text{non deliq. particle + Metallic oxide} \rightarrow \]

A number of statements can be made relative to this hypothesis and its implications:

1. The creation of mixed acid solutions with significantly low vapor pressure will result in the effervescence of free hydrochloric and possible nitric acids.
2. Chlorides through their deliquescent property will generate acidic liquid filmed particulates and liquid aerosols in all size ranges that are conductive to the absorption of \( \text{SO}_2 \) with subsequent conversion of sulfites to sulfuric acid which may in due course lead to the effervescence of HCl.
3. Hydrochloric acid absorbed in sufficient abundance in a metallic solution can create a deliquescent droplet which can, upon absorbing atmospheric water vapor, provide sites conducive to later formation of strong low vapor pressure acids.
4. Hydrochloric acid impinging on the surface of dry particulate can begin to dissolve its substrate, convert to a deliquescent particle, absorb water vapor and provide an environment conducive to the formation of strong low vapor pressure acids.
5. The atmospheric recyclability role hypo-
the sizes for HCl give it characteristics of a catalyst for the formation of atmospheric sulfuric acid and sulfate compounds.

Since the recycling of HCl is an on-going process requiring moisture and excess SO2, it is not restricted by distance from a single known source. It may reach an equilibrium at any time and begin again when the equilibrium is upset. Thus, by the hypothesized role of recyclability, a relatively small amount of chloride or HCl would have disproportionate effects.

These effects can be manifested as sulfate artifact [11], as acidic aerosol of cloud condensation nuclei size [15], acid precipitation and settleable sized acid active particulates. The HCl effervescence concept is consistent with Eriksson [11], Yue and Mohnen [4] and Hitchcock, et al., [12] but does not rely on sea salt or biogenic sulfide. It can be an ongoing process in air masses of continental origin activated by SO2 oxides of nitrogen, whatever their source.

The roles of chlorides and HCl are not simple or fully documented. The hypotheses presented can be used, with consistency, to explain many "apparent" anomalies. Under any circumstances, chloride, HCl aerosol or chlorine emissions should not be considered innocuous with respect to humans, flora, fauna and material products in the ranges projected.

REFERENCES

AUTHORS' REPLY

The authors appreciate Mr. Licata's comments and the opportunity he has given us to expand on the material presented in our paper.

To Peter J. Waznys and Carmine J. Desio

Mr. Waznys and Mr. Desio bring out several key points. In response to the discussors' first point, Table 1 is presented. This table describes concentrations of materials in the input fuels as determined by SSMS. Table 1 also lists the average feed rates of fuels during the tests.

Mercury concentrations were obtained using a "standard" modified EPA method 5 train. The impingers containing potassium hydroxide (KOH) and potassium permanganate (Kmno4) were analyzed for mercury. Aliquots of each sample, as
TABLE 1 AVERAGE CONCENTRATION OF POLLUTANTS IN FEED MATERIAL (ppm)

<table>
<thead>
<tr>
<th>Pollutants</th>
<th>St. Louis</th>
<th>Braintree</th>
</tr>
</thead>
<tbody>
<tr>
<td>As</td>
<td>1.8</td>
<td>22</td>
</tr>
<tr>
<td>Be</td>
<td>0.9</td>
<td>0.26</td>
</tr>
<tr>
<td>Cd</td>
<td>0.12</td>
<td>28</td>
</tr>
<tr>
<td>Cl</td>
<td>583.3</td>
<td>2233</td>
</tr>
<tr>
<td>Cr</td>
<td>33.3</td>
<td>1993</td>
</tr>
<tr>
<td>Pb</td>
<td>2.3</td>
<td>2167</td>
</tr>
<tr>
<td>Hg</td>
<td>**</td>
<td>**</td>
</tr>
<tr>
<td>S</td>
<td>12,800</td>
<td>1520</td>
</tr>
</tbody>
</table>

Feed Rate (kg/hr) 40,692 10,727 4,500

Dispersion models are not a substitute for actual ambient measurements and, in all cases, should yield to promulgated standards.

To V. A. Fassel

From Table 1, it can be seen that input Pb concentrations for RDF can be very high. This is most likely due to the presence of lead in printing ink, batteries, etc. The fact that more Hg reports in to the particulate with RDF combustion at St. Louis is probably attributable to three factors:
1. Particulate emissions are higher with RDF than with coal, giving the Hg vapor more surface area to condense on in the stack.
2. Combustion gas temperatures are somewhat, although not extensively, lower with RDF than with coal. (There is much more air introduced with RDF than with coal.
3. A manifestation of analytical uncertainties in the data base.

To E. J. Kaplin

The discussor expands upon the difficulties in arriving at a representative model. His points are well taken. Also, the discussor's admonition that chloride emissions are not as benign a factor as an optimistic interpretation of the paper's results can lead one to infer is well stated.