WASTE-TO-ENERGY: THE NEXT STEP IN THE HIERARCHY AFTER THE 3-Rs

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The authors are to be commended for analyzing the comparative air emissions for landfill and Waste-to-Energy (WTE) facilities for the disposal of municipal solid waste. My reading of the paper, however, produced a request and a few questions that, if spoken to, would strengthen and/or clarify the material presented.

I believe that the authors should provide literature citations (where the paper presents the data of others) or their estimation methodology and assumptions (where the numerical values in the paper were developed by the authors). Presentation of dramatic "numbers" without giving them the authority of a credible source greatly weakens the impact and utility of the material. Perhaps these tools can be provided in the Discussions volume.

I question the apparent assertion by the authors that the "greenhouse gas emissions" of WTE are much less than that of landfills. A good combustor will release almost all of the carbon in the waste as CO_2. In the landfill, the anaerobic conversion of organic material to CO_2 and methane does not proceed to the same extent. Perhaps, 50 to 60% carbon "release" would be an upper limit. One might argue that the greenhouse emissions from vehicle fuel burning associated with raw waste landfilling (haul and landfill equipment) will exceed that of residue landfilling, but I do not believe these factors entered into their calculations. Please clarify the basis of the assumptions that led to the result reported in the paper.

I have a final question about the relative magnitude of the "maximum year" and "average year" non-methane organic carbon (NMOC) emission estimates for landfills (although I do not question that they greatly exceed that of WTE!). My experience suggests that most landfills exhibit a "plateau-like" NMOC profile over their life with an induction period, a relatively flat gas/NMOC generation period, and then decay. The peaking suggested by the values in Fig. 7 appears to be related to the gas generation pattern described in the first-order kinetics or "Scholl Canyon" type of gas emission model. Although the mathematical treatment of the simplified Scholl Canyon model has been adopted by the federal EPA as a basis for "worst case" landfill gas emission evaluations, they (or other researchers) do not suggest that it is a realistic model. In particular, the first-order kinetic approach generates a "peak" generally not observed in the field. I suggest that this overstates the case such as to provide an unnecessary weakness to an adversary who is attacking the (legitimate) conclusion of the paper: landfills emit *substantially* greater quantities of NMOCs than do WTE plants.

AUTHORS' REPLY

With respect to the comment about expanding upon our references and methodology discussion, we refer interested readers to "Comparing Air Emissions from Landfills and WTE Plants," by Kay H. Jones, which appeared in *Solid Waste Technologies*, **8**, *.2*, March/April, 1994.

The question regarding greenhouse gas emissions appears to relate to CO_2 only. Our reported comparison is based on CO_2 equivalents which include methane. Methane is considered to be 25 times more effective in trapping infrared radiation than CO_2. (See Ref. 18 in the above cited paper.)

We do not disagree about the possible differences between observed landfill gas release rates and those estimated

by the Scholl Canyon model with respect to the maximum emissions rate estimate. However, when viewed from a regulatory review perspective, one is compelled to use the regulatory model that EPA used.

Since our major thrust is related to lifetime cancer risk estimates, the lifetime equivalent average gas release rate is the more important input to our analyses.