United States
Environmental Protection
Agency

Office of Research and
Development
Washington DC 20460

EPA/630/R-96/001
May 1996

EPA

Report on the U.S. EPA Technical Workshop on WTI Incinerator Risk Assessment Issues

RISK ASSESSMENT FORUM

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REPORT ON THE U.S. EPA TECHNICAL WORKSHOP ON WTI INCINERATOR RISK ASSESSMENT ISSUES

Risk Assessment Forum
U.S. Environmental Protection Agency
Washington, DC 20460

NOTICE

Mention of trade names or commercial products does not constitute endorsement or recommendation for use. Statements are the individual views of each workshop participant; the statements in this report do not represent analyses or positions of the Risk Assessment Forum or the U.S. Environmental Protection Agency (EPA).

This report was prepared by Eastern Research Group, Inc. (ERG), an EPA contractor (Contract No. 68-D5-0028), as a general record of discussions held during the Technical Workshop on WTI Incinerator Risk Assessment Issues. As requested by EPA, this report captures the main points and highlights of discussions held during the plenary sessions and includes brief summaries of the work group sessions. The report is not a complete record of all details discussed, nor does it embellish, interpret, or enlarge upon matters that were incomplete or unclear. In particular, the five work group summaries were prepared separately by the work group leaders (with or without help from group members) based on their groups' discussions. Thus, the recommendations of the groups might differ slightly. ERG did not attempt to harmonize all the recommendations.

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FOREWORD

This report presents information and materials from a peer review workshop organized by EPA's Risk Assessment Forum for Region 5 and the Office of Solid Waste and Emergency Response. The meeting was held in Washington, DC, at the Holiday Inn Georgetown on January 11, 1996. Due to severe weather conditions, the meeting was compressed from the planned two days to a single long day. The subject of the peer review was a draft document prepared by Region 5 assessing risk at an incinerator operated by Waste Technologies Industries (WTI) in East Liverpool, Ohio.

The peer review continued a process begun in 1993, when the Risk Assessment Forum held a workshop to review the project plan for the WTI incinerator risk assessment. In that workshop, 13 peer reviewers divided into work groups to discuss four major aspects of the project plan: combustion engineering, meteorology/air dispersion, exposure assessment, and toxicology. The workshop was attended by more than 100 observers. Workshop participants recommended that EPA expand the scope of the planned assessment to include more facility performance data, use additional computer models, include a screening level ecological risk assessment, and provide a comprehensive analysis of accident scenarios.

In 1994 and 1995, EPA conducted the WTI incinerator risk assessment now under review. To reflect the larger scope of work recommended by participants of the first workshop, EPA modified the peer review workshop format as follows: EPA expanded the scope of the air dispersion work group to cover deposition modeling and accident analysis, added a fifth work group on ecological risk assessment, and increased the number of peer reviewers from 13 to 19. Most of the 13 reviewers of the project plan were able to participate in the peer review of the risk assessment. In conducting the peer review, EPA sought comments on the technical accuracy, completeness, and scientific soundness of the WTI incinerator risk assessment. EPA will consider these comments in revising the assessment, which in turn will be used to set final permit conditions for the WTI facility.

This report summarizes the discussions that took place at the peer review workshop. The report opens with an overview of the workshop and a history of EPA's WTI incinerator risk assessment activities (section 1), then presents the chairperson's summary (section 2) and the five work group chairs' summaries (section 3). The body of the report ends with highlights of the peer reviewers' preliminary comments and of the comments offered by workshop observers (section 4). Appendices to the report present the workshop agenda, a list of the peer reviewers, their charge, their premeeting comments, and their work group assignments (appendices A-E) as well as a list of observers and printed materials distributed by observers (appendices F-G) and the written version of a comment that a citizen intended to offer at the workshop but was unable to do so due to severe, travel-hampering weather conditions (appendix H).

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SECTION ONE

OVERVIEW

GENERAL SUMMARY

The workshop provided a forum for the expert peer review panel to discuss the technical accuracy, completeness, and scientific soundness of the draft WTI incinerator risk assessment. The reviewers were in general agreement on the overall quality of the assessment and contributed useful suggestions for moving the process ahead to finalize the document.

Overall, comments on the draft WTI incinerator risk assessment were favorable. Indeed, throughout the workshop, as the expert peer reviewers discussed the assessment as a whole and specific parts of it, workshop participants repeatedly prefaced suggestions for improvement with praise for the overall thoroughness, quality, and integrity of the assessment. Noting that they had been quite critical of the draft project plan for the assessment, the peer reviewers stated that by contrast they were very impressed with the thoroughness, organization, and clarity of the draft assessment—and with the seriousness and faithfulness with which EPA had followed the comments and recommendations of the project plan peer reviewers. Their most substantive comments pertained to three topics (accident scenarios, cumulative risk, ecological risk) that were not covered in the initial project plan for the assessment and thus had not benefitted from previous review. The peer reviewers described most of their other comments as questions of clarification or as other minor issues not likely to affect the overall results of the assessment.

Members of the Combustion Engineering Work Group, for example, praised EPA's work on WTI stack emissions, noting that the Agency's efforts to determine the composition of the waste feed could be labeled heroic. Due to the basic soundness of the analysis, they focused on attempting to trace the progression of information on chemicals of concern from the waste feed through emissions estimation (see Combustion Engineering Work Group summary and diagram in Section 3). The main question generated by this exercise sought information on why EPA had not prorated measured products of incomplete combustion (PICs) to compensate for unmeasured PICs, as had been recommended by the peer reviewers of the project plan. The peer reviewers recommended that EPA clarify the explanation in the assessment that doing so would cause chronic effects to be overestimated and acute effects to be underestimated or prorate the measured PIC values as suggested.

Members of the Air Dispersion/Deposition Modeling and Accident Analysis Work Group also praised EPA's work, asserting that the assessment does a good job estimating the dispersion of emissions from the WTI facility, at least during routine operations. They suggested that EPA work with the CALPUFF model or other models to characterize dispersion of ordinary emissions under stagnant air conditions, which could magnify air quality problems, and to better characterize accidental emissions. They also suggested that EPA consider whether additional release scenarios (e.g., release following pressurization of wastes in containers) might be important and that EPA explain mitigation measures more clearly. Noting that they had been unable to trace the process themselves, they also recommended that EPA clarify how it had obtained the entries presented in the following summary tables in Volume VII:

- Table VIII-1: Probability/Severity Matrix—Typical Meteorological Conditions.
- Table VIII-2: Probability/Severity Matrix—Conservative Meteorological Conditions.
- Table VIII-3: Probability/Severity Matrix—Calm/Inversion Meteorological Conditions.

EPA's analysis of accident scenarios engendered lively discussions not just in the Air Dispersion/Deposition Modeling and Accident Analysis Work Group, but throughout the workshop. Although some peer reviewers said that the analysis seems reasonable, other peer reviewers and two workshop observers (see section 4) contended that it is incomplete and excessively qualitative. In addition to suggesting that EPA use the CALPUFF model to improve its estimation of accidental emissions, peer reviewers suggested that EPA more clearly address the location of the nearby elementary school and consider using a compound other than acetone for future modeling purposes because acetone has been delisted. Several peer reviewers and workshop observers also expressed reservations about the use of immediately detrimental to life and health (IDLH) values; because these values pertain to healthy adult workers exposed to contaminants for up to 30 minutes (during which time they will have donned protective gear or otherwise protected themselves from further exposure), the use of IDLH values might underestimate the risks that accidents pose to a residential population. Members of the Toxicity Work Group suggested using level of concern (LOC) values instead; they referred EPA to George Alexeeff's premeeting comments on this topic for further information.

Members of the Exposure Assessment Work Group commented that the exposure assessment in the WTI incinerator risk assessment is among the most comprehensive they have seen and that EPA did a good job addressing the recommendations of the project plan peer reviewers. They noted that EPA had not addressed house dust as had been recommended, but they contended that a rigorous quantitative analysis is not needed because house dust is unlikely to be an important exposure pathway. They suggested discussing house dust in conjunction with soil exposure pathways among children. They also recommended that EPA conduct further work to determine cumulative exposures (i.e., to the combination of WTI emissions and background contaminant levels) and, on a more long-term basis, to develop more refined methods and models.

Like the issue of possible accidents, the issue of cumulative risk arose several times during the workshop. The general consensus was that EPA should address this issue further to determine whether WTI-related exposures have the potential to increase total exposures to unacceptable levels. Members of the Toxicology Work Group suggested that simply examining how WTI-related exposures compare to background exposures might be sufficient to accomplish this goal.

Members of the Toxicology Work Group also offered a number of specific comments and suggestions related to the human health risk assessment. For example, they suggested that EPA use California EPA's slope factor to model lead toxicity, add a table of noncancer endpoints (while noting in the text that cancer endpoints are more sensitive), discuss the contribution of exposure to metals in breast milk to total metal exposures, assess exposures in the subpopulation of individuals who both work in and live near the WTI facility, provide a more quantitative analysis of uncertainty, and include a discussion of uncertainties related to data gaps.

Finally, peer reviewers discussed EPA's screening level ecological risk assessment (SERA), the third of the three topics in the assessment that peer reviewers felt needed substantive work. At the most basic level, peer reviewers were unclear about the goals and purpose of the SERA. Members of the Ecological Risk Assessment Work Group, for example, commented that EPA apparently included a permit limit scenario because it would be needed to design permit limits for metal emissions. This led the peer reviewers to wonder whether EPA conducted the SERA to support the setting of sufficiently conservative permit limits rather than to generate information that regulators and the public could use to understand the ecological risks associated with the WTI facility. The peer reviewers recommended that EPA clarify the goals of the SERA and conduct further work if needed to accomplish these goals. They also suggested that EPA address the issue of cumulative ecological risk and include provisions for monitoring in any facility permits (although distinguishing between background and WTI contributions might be difficult).

Members of the Ecological Risk Assessment Work Group also noted that the problem of data gaps is even greater in ecotoxicology than in human health toxicology. They recommended that EPA discuss the implications of data gaps more systematically in future versions of the assessment. They also wondered whether data from recent WTI plant operations might permit validation of the test burn data used for the SERA. Acknowledging that all ecological risk assessments suffer from a lack of established data, tools, and procedures, the peer reviewers noted that research is needed to provide the infrastructure necessary for improved ecological risk assessments in the future.

At the conclusion of the workshop, the peer reviewers attempted to sum up by asking two questions:

- **If fully implemented, would any of the recommendations of the work groups change the results of the assessment?** The peer reviewers concluded that recommendations in three areas—accident scenarios, cumulative risk, and ecological risk—have the potential to change some of the results of the assessment. Except for those three areas, the assessment is adequate in its current form.

- **Should the risk assessment present information on conditions that increase the risks associated with operating the facility and recommend mitigation measures?** This question related to the line between risk assessment and risk management. The peer reviewers concluded that the WTI incinerator risk assessment should provide information about the contribution of various conditions to the predicted risks (information that will help risk managers make informed decisions), but it should not recommend mitigation measures (because those are risk management decisions based on social and policy factors as well as scientific/technical factors).

For example, the assessment could state that certain weather conditions substantially increase the risks associated with operating the WTI facility, but it should not make recommendations as to operations of the facility during such weather conditions.

HISTORY OF EPA RISK ASSESSMENT ACTIVITIES FOR THE WTI INCINERATOR

The WTI incinerator, the subject of the risk assessment reviewed at the workshop, is located in East Liverpool, Ohio, across the Ohio River from West Virginia and about a mile and a half west of the Pennsylvania border. A permit to store and treat hazardous waste regulated under Subtitle C of the Resource Conservation and Recovery Act (RCRA) was issued to the WTI facility on June 24, 1983. Because the original permit was appealed, however, it did not become effective until January 25, 1985. On November 30, 1990, WTI began constructing the incinerator.

Due to intense interest in the WTI facility in East Liverpool, EPA's Region 5 initiated a risk assessment in 1991 before authorizing interim operations. Because site-specific information was unavailable, the risk assessment was conducted using regional meteorological data and stack emissions data from other incineration facilities. In accordance with EPA's Office of Solid Waste guidelines, only the direct inhalation exposure pathway was assessed. This initial risk assessment, conducted by a contractor and referred to as Phase I risk assessment activities, was completed and made available to the public in July 1992. The risk assessment indicated that predicted inhalation exposure levels were below the level of concern. Subsequently, EPA's Office of Health and Environmental Assessment conducted an additional screening level analysis of potential cancer risks from dioxin stack emissions, generating preliminary risk estimates for four exposure scenarios, each of which included indirect exposures through the food chain.

Since these initial, Phase I risk assessments were completed, a full year of onsite meteorological data as well as WTI incinerator-specific waste composition and emissions data from trial burns and performance tests have been collected. With input from scientists in a number of EPA offices, Region 5 prepared a project plan for a Phase II risk assessment, which was to include a multipathway assessment using the new data. The project plan was peer reviewed in December 1993 under the auspices of EPA's Risk Assessment Forum.. In conducting the Phase II risk assessment in 1994 and 1995, EPA conscientiously tried to follow the recommendations of the peer review panel to include facility performance data, use a variety of computer models, include a SERA, and provide a comprehensive analysis of accident scenarios. Accordingly, the assessment consists of three separate analyses: a human health risk assessment, a SERA, and an accident analysis.

Since the completion of Phase II risk assessment activities in 1995, EPA's Risk Assessment Forum has been organizing a peer review of the draft WTI incinerator risk assessment to obtain comments on its technical accuracy, completeness, and scientific soundness—and to obtain comments on whether and how well the Agency succeeded in implementing the recommendations of the project plan peer review panel. EPA will consider these comments in revising the assessment, which will then be used to set final permit conditions for the WTI facility.

SECTION TWO

CHAIRPERSON'S SUMMARY OF THE WORKSHOP

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GENERAL SUMMARY

On January 11, 1996, EPA held the second of two external peer reviews of documents related to an assessment of risks associated with the WTI incinerator in East Liverpool, Ohio. The purpose of this workshop was to bring together a team of scientific experts to comment on the draft WTI incinerator risk assessment. The assessment was based on a project plan developed in 1993 and subjected to peer review at a similar workshop held in December 1993. Most members of this 1996 peer review (Elmar Altwicker, James Butler, Walter Dabberdt, Mary Davis, Barry Dellinger, George Fries, Thomas Gasiewicz, Halstead Harrison, Pim Kosalwat, Thomas McKone, Randy Seeker) also participated in the 1993 review.

The peer review of the draft WTI incinerator risk assessment was carried out in stages. A few months before the workshop, reviewers received the full assessment report and were assigned to one of five work groups, each focusing on a specific technical aspect of the assessment. In December 1995, each reviewer submitted written premeeting comments to his/her work group chair and to the workshop chair. In January 1996, these comments were circulated to all members of the peer review panel. At the January 11 workshop, the peer reviewers met in plenary and work group sessions to discuss the draft assessment and prepare this summary report. In addition to the peer reviewers, some 30 observers also participated in this public meeting. They included EPA staff and consultants, citizens from East Liverpool and other communities, representatives of the press, employees of WTI, and representatives of other industries.

The U.S. National Research Council (1982, 1994) divides the practice of risk analysis into two substantially different processes: risk assessment and risk management. The goal of risk assessment is to estimate a risk; that is, to produce a specific risk value and explain the precision of this estimate. The goal of risk management is to establish the significance of the estimated risk, compare the costs of reducing this risk to the benefits gained, compare the estimated risk to the societal benefits derived from incurring the risk, and implement any political and institutional processes needed to reduce the risk. As a risk assessment document, the main goal of the draft WTI incinerator risk assessment is to give the public and decision-makers adequate information about the nature and likelihood of any health detriment associated with the WTI facility. Prescriptions for technological, social, legal, or political control actions are risk management decisions and are not explicitly discussed in the draft assessment. Neither the draft risk assessment nor this workshop considered risk management issues.

The draft WTI incinerator risk assessment is a large, comprehensive document consisting of several volumes:

Volume I:	Executive Summary
Volume II:	Introduction
Volume III:	Characterization of the Nature and Magnitude of Emissions
Volume IV:	Atmospheric Dispersion and Deposition Modeling
Volume V:	Human Health Risk Assessment Evaluation of Potential Risks from Multipathway Exposure to Emissions
Volume VI:	Screening Ecological Risk Assessment
Volume VII:	Accident Analysis: Selection and Assessment of Potential Release Scenarios

To cover the extensive volume of material in the draft assessment, the peer review panel divided into five work groups focusing on the following areas:

- Combustion engineering (focusing on emissions)
- Air dispersion/deposition modeling and accident analysis
- Exposure assessment
- Toxicology
- Ecological risk assessment

The peer reviewers also met in plenary sessions to discuss cross-cutting issues, such as accident analysis results. During the first plenary session, the peer reviewers addressed the following general issues identified in the premeeting comments:

- The scientific quality of the draft WTI incinerator risk assessment is considerably better than that of the 1993 risk assessment plan.
- In preparing the assessment, EPA addressed the major recommendations of the 1993 peer review panel.
- Some important uncertainties (the confidence with which accident scenarios and impacts can be specified, the influence of data gaps on emission and health impact estimates, the quantification of noncancer impacts of dioxin-like compounds) need to be addressed prior to closure.
- Emissions of WTI chemicals of concern from other proximate industrial sources and even from local residential combustion sources should be factored into the assessment to facilitate the calculation of better cumulative dose and impact estimates and the development of validation studies.
- The goals and conclusions of the SERA are vague.
- In the accident analysis, the accident scenarios are incomplete and their contribution to information on possible health detriment is inadequately addressed; also, accident-related risks are not fully quantified.

After the first plenary session, the peer review panel broke into work group sessions to discuss specific areas of the assessment and prepare the work group summary reports included in section 3 of this workshop report. The reviewers agreed that the work group reports should:

- Focus on scientific issues, not issues of policy.
- Collect and summarize the opinions of the experts (i.e., consensus was not necessary).
- Make recommendations that EPA can use to finalize the assessment.
- Identify the most important uncertainties and information gaps in the assessment—those likely to alter the assessment's conclusions on the likelihood of health detriment.
- Consolidate the reviewers' comments, present these comments as concise problem statements, and identify the likely impact of these problems on the results of the assessment.

After the work groups completed their tasks, the peer review panel met again in a plenary session for presentation of reports from the work group leaders and discussion of general recommendations. For the most part, the work group meetings supported and better defined the issues identified in the reviewers' premeeting comments, as summarized above. The issue of accidents, in particular, sparked extensive discussion (see below). Some peer reviewers recommended that a WTI site visit be part of any additional peer review of WTI-related EPA documents.

As noted by several peer reviewers, the draft WTI incinerator risk assessment is one of the most extensive and comprehensive risk assessments ever compiled for a stationary combustion source. The assessment goes to great lengths to address regulatory requirements and EPA guidance. Although addressing such issues is necessary, the often neglected goal of the risk assessment process is to address the questions of how precisely we can estimate a source's "true" impact on public health and how well we can address and answer the concerns of affected communities. Certainly, these are challenges that remain for future risk assessments.

SUMMARY OF RECOMMENDATIONS ON ACCIDENT ISSUES

The peer reviewers had several general and specific concerns related to characterizing the occurrence and impacts of accidents. Because accident issues cut across all aspects of the risk characterization, a separate work group was not assigned to this topic. As a result, comments regarding accidents appear throughout the work group summaries (see section 3). These recommendations are briefly summarized below:

- The accident analysis does not address all potentially important accident scenarios. For example, pressurized jet releases from the incinerator containment might occur and result in aerosol formation due to mixing of chemicals or heating by fire. Although the accident analysis in the draft assessment provides a useful beginning for assessing nonroutine emissions and accidents, it is not well developed and it lacks the precision and depth needed for reliable estimates of impact.

- The predicted effectiveness (or failure) of mitigation measures needs to be more clearly addressed.

- The accident analysis does not adequately communicate the expected value of accident impacts, nor does it adequately explain the reliability of the estimates given. Whereas the assessment conveys cancer risk estimates in terms of the likelihood of detriment (i.e., less than one chance in a million per lifetime), it conveys accident severity and consequence information in vague terms (e.g., "likely" or "unlikely" events, "moderate" to "catastrophic" consequences). These estimates should be quantified more rigorously.

- The accident scenarios do not characterize in any quantitative fashion the sequence of events that might result in an accident or the likelihood of these events. The absence of this information hampers use of the accident analysis as a guide for planning to reduce the incidence and consequences of accidents in an efficient and cost-effective manner.

- The IDLH values used in the accident analysis are designed to provide short-term protection to healthy workers and do not account for the greater variation in sensitivity likely to exist in a non-occupational population that includes children. The peer reviewers recommend that some other measure of accident health impacts be considered. It was noted that the American Industrial Hygiene Association's Emergency Response Planning Guidelines (ERPG) levels would probably have been more appropriate than IDLH values for characterizing the severity of accident consequences.

- The accident analysis would be strengthened by an examination of the safety record of other hazardous waste facilities such as the Biebesheim facility in Germany, which is similar to WTI and has apparently reported two release incidents.

- The accident analysis focuses on the acute impacts of the accident scenarios, but does not address how chemical exposures during an accident could impact an individual's lifetime exposures to chemicals from the WTI facility.

- The atmospheric dispersion analysis used in the accident analysis should explicitly report the chemical concentration ranges expected to occur at the East Elementary School under the various accident scenarios.

- The chemical release model for accidental fires should be changed to include the same chemicals and relative emission rate estimation procedures used for stack emissions. In addition, an improved method for calculating the total emissions rate from the fire should be developed.

- The dispersion modeling performed for the accident scenarios should be re-examined in light of the peer reviewers' recommendation that calm/stagnant conditions be reanalyzed with a more appropriate data set in the CALPUFF model.

- The model used to estimate the rate of chemical evaporation from spills is not appropriate for calm conditions. More appropriate models are discussed in the report of the work group on atmospheric dispersion.

- Because this facility is located on a flood plain, the risk assessment should include the likelihood that a flood of sufficient magnitude to inundate the facility will occur and that hazardous materials would be released during such a flood.

- Because acetone has now been deleted from the list of toxic chemicals used for emergency planning, the peer reviewers recommend that it not be used as a sentinel chemical for the accident analysis.

The peer reviewers also offered one important long-term recommendation: that more sophisticated accidental fire and chemical release models be developed. It is unlikely that such models can be incorporated in the WTI assessment, but they would be useful for future incinerator risk assessments.

REFERENCES

National Research Council. 1982. Risk and decision-making: Perspectives and research. Washington, DC: National Academy Press.

National Research Council. 1994. Science and judgment in risk assessment. Washington, DC: National Academy Press.

SECTION THREE

WORK GROUP SUMMARIES

COMBUSTION ENGINEERING

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The WTI risk assessment document represents a highly professional and dedicated effort by EPA and its contractors. In 1993, the combustion engineering panel offered detailed recommendations for improving the draft risk assessment. EPA made an exceptional effort to follow the spirit of the recommendations and, in some instances, the Agency's efforts can be termed heroic. The Combustion Engineering Work Group is confident that the WTI risk assessment document (at least the part we reviewed in detail) is fair and scientifically unbiased.

EPA used a very detailed procedure to estimate possible stack emissions and did a good job identifying and discussing uncertainties in the procedure. To our knowledge, this is the first time a risk assessment document has included this much detail. As a result, it substantially improves our understanding of how to conduct these estimates. In general, the procedure described in the draft risk assessment is a good model for developing emissions estimates for future risk assessments. The level of detail provided in the draft risk assessment does point to some weaknesses, however.

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*Dr. Seeker reviewed the WTI incinerator risk assessment and provided premeeting comments, but was unable to attend the workshop.

The Combustion Engineering Work Group discussed six general types of emissions or emissions issues:

- Emissions from accidental fires or other hot releases
- Gas-solid partitioning of emissions
- PIC emissions
- Dioxins/furans (PCDD/F) emissions
- Metals emissions
- Fugitive emissions

The first issue, the accidental fire release scenario, was not assigned to the Combustion Engineering Work Group. Nevertheless, we reviewed this topic in some detail. As discussed below, the work group believes that the procedure used to estimate emissions from an accidental fire is largely inadequate. This is not a criticism of EPA's regulatory staff, since appropriate models are not currently available. In fact, EPA's work clearly illuminates the deficiencies of existing models for the first time.

Regarding gas-solid partitioning, the work group believes that the procedure used is probably very inaccurate, but sufficiently conservative to ensure that the risk results present a reasonable worst case. Regarding the last four emissions issues, the work group believes that a few concerns remain, but that nothing short of a complete paradigm shift *(vide infra)* will significantly affect the results of the overall risk assessment.

NEAR-TERM RECOMMENDATIONS

Some of the Combustion Engineering Work Group's recommendations should be readily implementable and should improve the results of this risk assessment. For the near term, we recommend that EPA:

- Change the chemical release model for the accidental fire scenario to include the same chemicals and relative emission rate estimation procedures used for the stack emissions. An improved method for calculating the total emission rate from the fire should be developed. Perhaps a range of overall destruction efficiencies (e.g., 90 to 99.99 percent) could be evaluated.

- Obtain actual particle size distribution data for stack emissions to improve the risk assessment's estimates of gas-particle partitioning of PCDD/F and metals (and other PICs). If it can be demonstrated that the currently used assumptions are the most conservative, the requirement for additional stack sampling can be omitted.

- Adjust the estimate of normal PIC emission rates to reflect emissions during abnormal operations (i.e., based on the percentage of operation time during which emission violations or automatic waste feed cutoffs occur).

- Obtain actual facility-specific sulfur dioxide (SO_2) removal efficiency data over a wide concentration range so that the SO_2 surrogate will better model the behavior of selenium (Se).

- Abandon the assumption that fugitive emissions particles are the same as coal dust in favor of using actual fugitive emissions particle characterization for the WTI facility.

LONG-TERM RECOMMENDATIONS

Some of the Combustion Engineering Work Group's recommendations should be implemented in future risk assessments. Over the long term, we recommend that EPA:

- Develop far more sophisticated accidental fire and chemical release models. Improved dispersion models that better reflect low-level releases and complex terrain might also be necessary. Existing models are inappropriate for portraying releases from a hazardous waste incineration facility.

- Generate experimental data on the gas-solid partitioning of PCDD/F and other toxic air pollutants on various types of particles. Existing modeling approaches are very inaccurate.

- Consider using carcinogenicity assay screening of incinerator effluents to assess overall risk. This approach might be necessary to address the unaccounted-for fraction of incinerator emissions.

DETAILED COMMENTS

Accidental Fire Model

Because the WTI incinerator is located in a valley near an elementary school and residential properties, one of the most sensitive release scenarios may be an accidental fire. Whereas a simple spill could result in a moderate release (i.e., due to evaporation), an accidental fire could produce a catastrophic release. In fact, such a fire involving hazardous waste could be the most insidious of all combustion releases. In contrast to combustion in the incinerator, where it is controlled and results in extraordinarily efficient destruction of waste, combustion in an accidental fire would be very uncontrolled. A raging fire with relatively high temperatures and good waste-air mixing might produce reasonably good combustion, significant plume rise, and thorough dispersion of toxic gases. Because chlorine and other halogens are flame inhibitors, however, a fire consuming material containing chlorinated hydrocarbons would likely burn much more slowly—potentially resulting in less complete combustion, vaporization of solvents at the periphery of the fire, and less plume rise and dispersion. This is not to say that models and experiments have shown this scenario to be true or untrue. The issue is that we have not adequately addressed these important safety questions.

The model used to determine the physical characteristics of a pool fire resulting from a chemical spill is very simplistic. More detailed fire models appear to be available from the chemical industry, but these models have not been adapted for simulating a hazardous waste fire. The chemical release model, too, is totally inadequate. Thus, additional model development is needed. Adapting the general procedures used to estimate stack emissions might yield a dramatic improvement in fire emissions estimates.

The existing fire model uses empirical equations to estimate burn rate, flame temperature, flame height, and vertical velocity. It does not account for variations in fuel mixture or the burn properties of different fuels. The complexity of the fire model is consistent with the chemical decomposition model for the formation of phosgene and hydrochloric acid, but it is inadequate for use as a comprehensive release model. A fire model is needed that describes the structure of the flame and plume in terms of a profile of flame temperature, vertical velocity, and oxygen and fuel concentrations within the flame. Several research groups have developed models of this type for simulating liquid and solid fuel fires; their models could be adapted for use in characterizing a hazardous waste fire.

The chemical release model includes emissions of phosgene and hydrochloric acid based on 30- to 40-year-old experimental studies on the burning of various chlorinated hydrocarbons. The model does not address the individual components of the waste, their potential for vaporization without burning, or the likelihood that they will form toxic byproducts other than phosgene and hydrochloric acid. Accident models often consider only acutely toxic chemical exposures. Because only limited data exist on the acute toxicity of hazardous waste combustion byproducts, very few chemicals can be explicitly included in a fire emissions model. A prorating approach, such as that used in the stack emissions model, might be a viable alternative.

Also, the notion that fire contributes insignificantly to exposure to chronically toxic chemicals has not been proven. In contrast, given the magnitude of emissions in a catastrophic event, the contribution of fire might indeed be significant. Adapting the procedure used to estimate annual stack emissions to estimate toxic chemical emissions might yield improved estimates of these emissions. To develop quantitative estimates, however, an improved fire model will be needed to better estimate combustion conditions in the fire.

Better dispersion models are also needed to address the effects of local terrain and variable plume rise. The Combustion Engineering Work Group is concerned that a slow-burning, cool fire will produce large low-level releases whose plume rise remains close to the ground. Under stable atmospheric conditions, the dispersion might be controlled by drainage flow that follows the contours of the terrain. Although ISC-COMPDEP and CALPUFF are excellent state-of-the-art dispersion models, they do not model the influence of terrain on the local air flow. Local terrain might significantly affect short-range dispersion of the release.

Gas-Solid Partitioning

Although phase distribution of PCDD/F is important from the standpoint of risk assessment, little seems to have been added since the initial, Phase I risk assessment. The discussion on partitioning appears to be unchanged. The statement (Volume III, page III-11) that "substances in the stack gas will generally be present in either the vapor phase or in the particle phase" seems to ignore the possibility that many compounds, among them PCDD/F, could be partitioned in both phases in the stack gas. Although the material on partitioning appears in Volume III, it is applied only in Volume V, Table IV-5. The numbers in that table appear to derive from an assumption of $T = T_{ambient}$ and initial concentrations in the gas phase. As noted in the work group's December 1993 review of the risk assessment work plan, Biddleman (1988) cites $DS_f/R = 6.79$ as an average and gives three references; he does not say that it can be "satisfactorily estimated." In fact, McKay et al. (1982) state that it is an average empirical value and "may be substantially in error for certain compounds." Thus, this analysis has two problems:

- It does not clearly show how the results in Table IV-5 (Volume V) were obtained. What was the source of the vapor pressures of the different PCDD/F congeners in the subcooled liquid state?
- It seems to ignore chemisorption and potential differences in stack particle properties that determine partitioning prior to emission from the stack. Although we are not aware of any description of hazardous waste incinerator ash particles in terms of their PCDD/F-partitioning properties, laboratory results with municipal solid waste incinerator fly ash suggest that an idealized physical adsorption/desorption based on a Langmuir adsorption isotherm is not tenable. Given the particle emission rates cited (0.07 g/s) and volumetric flow rates, and assuming fly ash surface areas of less than 10 m^2/g, the surface area available in the stack gas particulate matter would appear to be substantially greater than 10^{-4} cm^2/cm^3 (Volume III, page II-12). WTI stack particles should be characterized by surface area and size distribution to permit meaningful in-stack partition calculations. A more critical review of partitioning must be conducted.

Our understanding is that the risk assessment considers uptake of gases by plants and animals (not terrain uptake of pollutants adsorbed on particulates) to be the primary source of risk from WTI operations. EPA appears to have used a conservative gas-solid partitioning factor. Considering the importance of partitioning, however, the Combustion Engineering Work Group believes it advisable to generate experimental gas-solid partitioning data for at least PCDD/F on various types of particles.

Stack Emissions of PICs

The procedure used to estimate normal stack emissions was quite complex (see figure 1). In essence, it involved:

- Compiling a list of chemicals to be fed into the incinerator.
- Applying an average destruction and removal efficiency (DRE) value to each chemical to obtain an emission rate.
- Identifying a target list of PICs for stack testing.
- Including emission rates for detected and nondetected chemicals.
- Combining these results to produce a single list of estimated emissions.
- Prorating the estimated emission rates of known chemicals to account for the roughly 60 percent of the emissions that are unknown.

Although improving this procedure might be possible, the Combustion Engineering Work Group considers the procedure satisfactory. Chemicals on the target analyte list that were not detected in stack tests were included at one-half their detection limit or at their detection limit for the central tendency and worst case emission scenarios, respectively. The prorating of known emissions to account for unknown emissions is tantamount to assuming that all emissions are equally toxic. In the absence of better data, this method is as good as any; modifying the procedure is unlikely to have a significant effect on the overall assessment.

The work group is still concerned, however, about the nature of the 60 percent of organic emissions that remain uncharacterized (see figure 2). This means that 60 percent of the total *mass* of organics are uncharacterized. Although as much as 90 percent of this 60 percent might be light hydrocarbons such as methane and ethane, more than 99 percent of the *number* of organics are probably uncharacterized. The large number of uncharacterized emissions increases the likelihood that one of the organics is a "supercarcinogen." The lack of full characterization is exacerbated by the present practice of not testing for chemicals for which no approved EPA method exists, even though commonly used laboratory methods can reliably detect many of the chemicals. Even so, using the most comprehensive battery of analytical techniques would likely characterize only a few percent more of the organic emissions. At present, the best way to assess

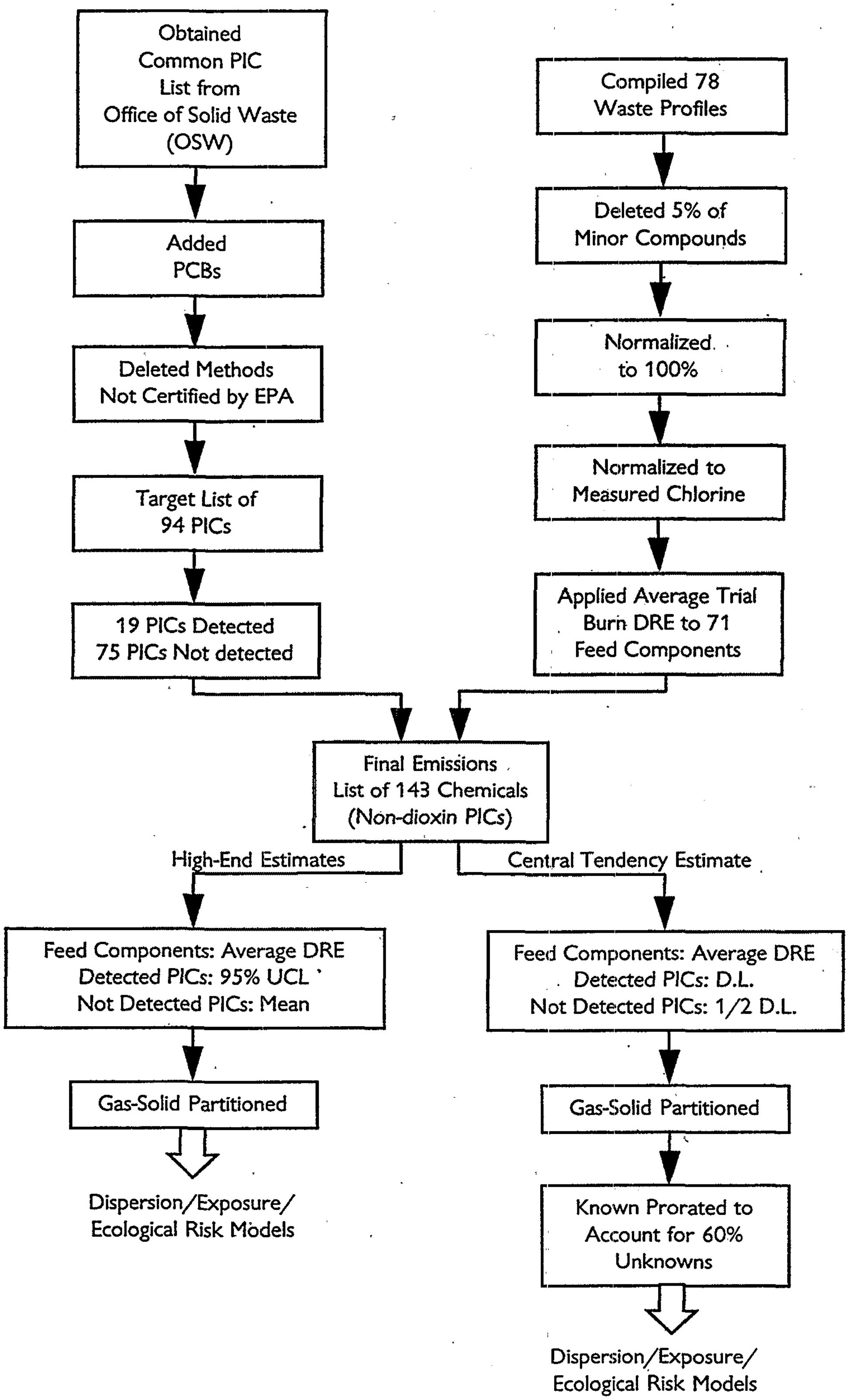

Figure 1. Stack emissions estimation procedure.

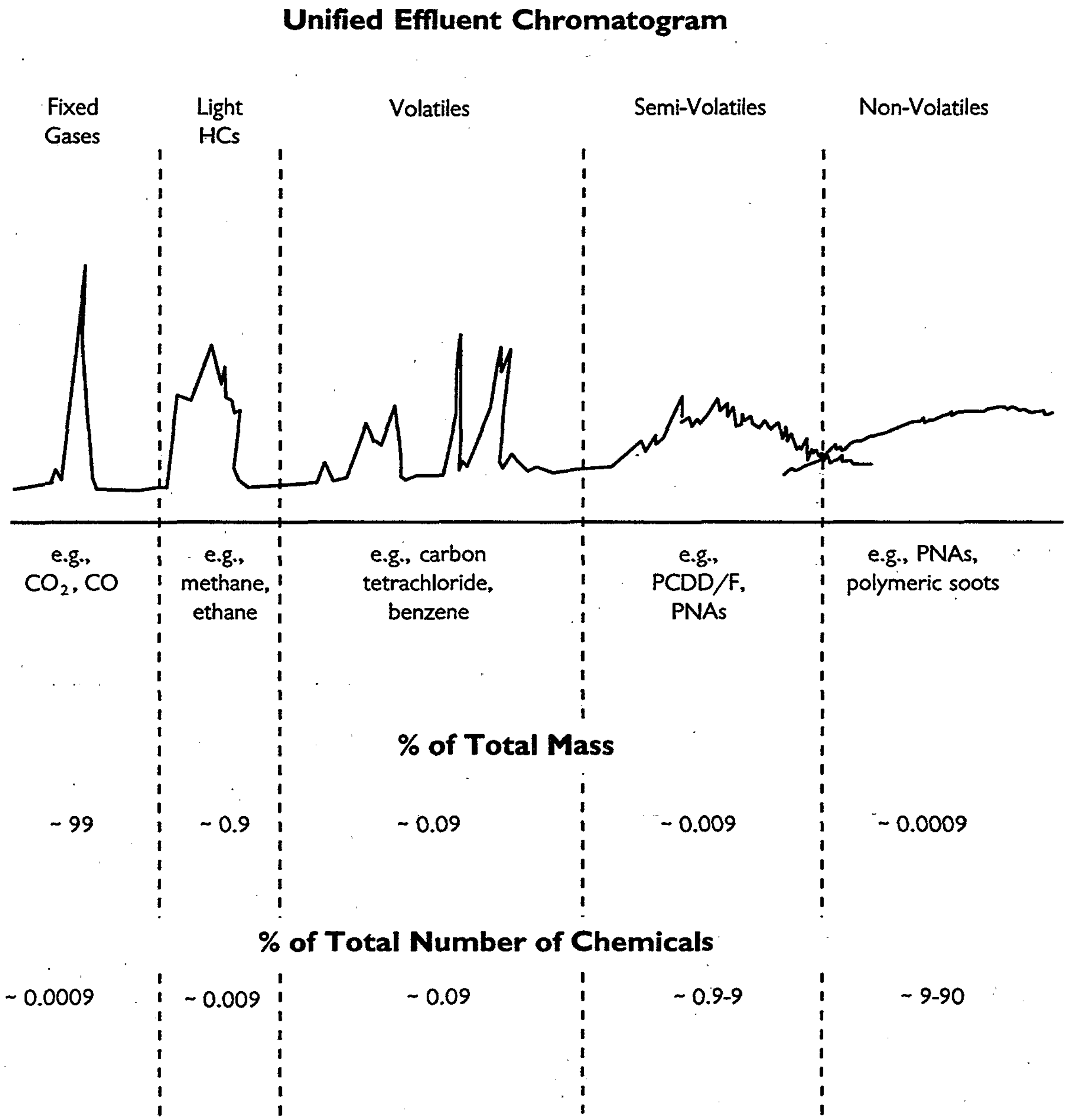

Figure 2. Chemical positioning of organic wastes in an incinerator.

the toxicity of incinerator emissions is to conduct carcinogenicity testing. Simple Ames test screening of mutagenicity is inappropriate because chlorinated hydrocarbons do not respond to the test. A somewhat more complex mouse papilloma screening test could be reasonably performed instead. This represents a shift in the risk assessment paradigm, but it is difficult to envision a better method for including the uncharacterized fraction of emissions in the risk assessment.

In estimating actual kiln emissions, a factor for process upsets should be included as a final step in the estimation process. The percentage of operating time under upset conditions can be estimated from continuous monitoring data, records of automatic waste feed cutoffs, and records of emissions violations. EPA does not seem to have included this factor in the present risk assessment, except when discussing worst case assumptions. The Combustion Engineering Work Group believes that data from a joint National Institute of Occupational Safety and Health (NIOSH)/EPA study on process upset emissions, although not comprehensive, can be used to adjust normal emissions rates to reflect process upsets.

PCDD/F Emissions

The PCDD/F data (Volume I, Table m-l) are interesting. The 1994 results, including the December 1994 performance test results made available at this January 1996 workshop, are presented slightly differently in Tables A through D. Of particular interest is a comparison between the February 1994 performance test (PT) and trial burn (TB). Compared to the TB, the PT generated a higher total PCDD/F value, but a lower toxic equivalent (TEQ) value; in addition, this PT generated the smallest value for the ratio $TEQ\bar{x}/(PCDD/F)\bar{x}$, 0.0058 (see Table B). Unfortunately, carbon injection locations, carbon injection rates, and adjustments in electrostatic precipitator (ESP) operating parameters were withheld. This information would have aided in the interpretation of these results.

Table C lists the particle concentrations (gr/dsef) measured during the PTs and TB. Calculating the ratio of the average toxic equivalent, $(TEQ)\bar{x}$, to the average particle concentration, $(PC)\bar{x}$, produces an interesting trend. Although these are all ECIS-runs, this ratio declines by a factor of five between 2/94 and 12/94 (see Table D). Because all performance

1994 Performance Test (PT) and Trial Burn (TB) Results for Chlorine, PCDD/F, and TEQ

Table A **Mean $(\bar{x})$, standard deviation (s) and variability (v)[1] for $(PCDD/F)_T$ and (TEQ)**

	Total, $(PCDD/F)_T$			(TEQ)		
Type and Date	$\bar{x}$	σ	ν	$\bar{x}$	σ	ν
PT, 2/94	5.34	1.85	0.35	0.031	0.009	0.277
TB, 2/94	4.60	1.06	0.23	0.067	0.012	0.123
PT, 4/94	3.74	0.83	0.22	0.035	0.002	0.053
PT, 8/94	1.34	0.38	0.28	0.017	0.003	0.202
PT, 12/94	1.58	0.71	0.45	0.022	0.014	0.623

1 $v = \frac{\sigma}{\bar{x}}$

Table B **Ratio of TEQ $\bar{x}$ to (PCDD/F) $\bar{x}$**

Type and Date	
PT, 2/94	0.0058
TB, 2/94	0.0146
PT, 4/94	0.0094
PT, 8/94	0.0127
PT, 12/94	0.0140
PT, 8/931	0.01801

1Run 3, apparent outlier, deleted

Table C **Stack Particle Concentrations (PC), gr/dscf**

	$\bar{x}$	σ	ν
PT, 2/94	0.0013	0.0007	0.5376
TB, 2/94	0.0016	0.0001	0.0000
PT, 4/94	0.0035	0.0013	0.3731
PT, 8/94	0.0018	0.0012	0.6466
PT, 12/94	0.0046	0.0022	0.4876

Table D **Ratio of TEQ $\bar{x}$/(PC) $\bar{x}$, $ng/m^3/gr/dscf$**

PT, 2/94	23.8
TB, 2/94	41.9
PT, 4/94	10.0
PT, 8/94	9.4
PT, 12/94	4.8

parameters have not been made available, this change cannot necessarily be attributed solely to changes in ECIS-injection quantity and location. Nevertheless, this change suggests that lumping together PCDD/F data from all 26 runs will not help us understand the results, nor is calculating a 95-percent upper confidence limit very meaningful. The data raise additional questions as well: Do the average and high values (Volume m, Table m-2) represent results for repeat analyses (if they were done)? Some of the values differ by less than 30 percent. Which of the 17 congeners were not detected? The conclusion that TEQ is at best a weak function of the chlorine content of the fuel seems reasonable. In addition, using the August 1993 test data represents a conservative approach.

Metals Emissions

Although the modeling results seem impressive, some of the assumptions are unclear. The Combustion Engineering Work Group infers that particle size has not been measured since the March 1993 TB and that EPA's analysis assumes:

- A large particle mode for ash particles.
- Condensation of vaporized metals forms a second (0.5-mm) mode.
- No submicron mode (a mode smaller than 1 μm) is present initially (on a mass basis).

Most ash size distributions appear to be monomodal (on a mass basis), but that does not preclude the presence of a large number of submicron particles that could play a role in nucleation/condensation of metal vapors. Thus, the apparent assumption that no submicron particles initially exist should be justified. If all metals that vaporize subsequently condense to form 0.5-mm particles, their density should be very different from the (typical) ash particles.

SO_2 was used to model removal of Se by air pollution control devices (APCDs) because of its similar chemistry. Although use of SO_2 might be justifiable, the analysis was based on data involving large SO_2 concentrations; this would make mass transport (which is normally rate-limiting in APCDs) less important for SO_2 than for Se. As a result, additional data may be needed to estimate Se emissions at the WTI facility. One possibility would be to operate the incinerator in a low-SO_2 mode, monitor removal efficiency as a function of SO_2 concentration, and extrapolate to expected Se concentrations. Alternatively, actual Se data could be collected; Selsun Blue could be a source of Se for such a test burn.

Despite some modeling similarities between chromium and aluminum, the former would seem to be a poor surrogate for the latter, since their chemistries differ.

In the past, some have argued that historical data were unavailable or insufficient to assist in the estimation of emissions. Although the Biebesheim facility in Germany is not exactly like the WTI facility, data from this facility have been cited in previous examinations of PCDD/F emissions and carbon injection and could be used for a metals emissions comparison as well. Tillman (1994) and references cited therein should be a good starting point.

Fugitive Emissions

Under the Fugitive Ash Emissions heading, the draft WTI risk assessment mentions that a monthly fly ash sample (1994) was analyzed for 80 organics and that none was found. The Combustion Engineering Work Group assumes that the fly ash sample must be some sort of composite sample. We wonder how the sample was obtained, how it was stored, and what the detection limits of the analytical method(s) were.

The work group also questions using a coal ash emission factor of 0.107 lb/ton and multiplying it by 10. This does not seem justifiable. Could the estimate not be refined based on the composition of WTI facility fly ash (some fraction of which consists of large carbon particles, which presumably are much less dense than ash particles)? We believe that sufficient information might be available to generate a WTI-specific emission factor, especially since the physical and chemical composition of the fugitive emissions is known. (The work group is not aware of specific facility data or other methods that could be used to refine the coal ash emission factor other than through consideration of the physical and chemical composition of the fugitive emissions.)

AIR DISPERSION/DEPOSITION MODELING AND ACCIDENT ANALYSIS

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CHARGE TO THE WORK GROUP

The following is a list of issues in the charge to the full review panel that specifically pertain to the review conducted by the Air Dispersion/Deposition Modeling and Accident Analysis Work Group.

General issues:

- Organization of the document

* Dr. Meroney reviewed the WTI incinerator risk assessment and provided premeeting comments, but was unable to attend the workshop.

- Scope of the Executive Summary
- Consideration of the 1993 review panel's recommendations
- Major data or methodological gaps
- Recommendations for long-term study

Dispersion and deposition modeling:

- Does the draft risk assessment adequately summarize work performed in response to the recommendations of the 1993 review panel?
- Comment on the adequacy of the sensitivity tests relating dispersion and deposition to geophysical variables.
- Is the ISC-COMPDEP model sufficiently conservative?
- Comment on the adequacy of the CALPUFF (and INPUFF) analyses for assessment of calms and strong inversions.
- Comment on the use of the SLAB model.
- Comment on the overall adequacy of the model sensitivity tests.

Accident analysis:

- Comment on the appropriateness of the SLAB and ISC-COMPDEP models for estimating atmospheric concentrations resulting from fires and the mixing of incompatible wastes.
- Assess the adequacy of conclusions pertaining to severity of consequences and probability of occurrence.
- Have the magnitudes and "directions" of the effects been properly and adequately characterized?
- Comment on the appropriateness of using IDLH values for characterizing the severity of consequences.
- Comment on the adequacy of assumptions of uncertainty pertaining to accident severity and emission rates, atmospheric-concentration averaging times, and meteorological conditions.

PREMEETING COMMENTS

Each of the six work group members submitted comments on the voluminous materials in the draft WTI risk assessment (see Appendix D). Although in general the comments do not follow the outline suggested in the charge to the reviewers, they do address the essential elements of the questions and issues raised by the EPA Risk Assessment Forum. Together with the major recommendations offered at the January 1996 workshop (as summarized below), they constitute the full body of this work group's comments and recommendations.

WORKSHOP COMMENTS AND RECOMMENDATIONS

The work group focused its review on two areas: atmospheric dispersion and deposition, and accident analysis. Note that WTI plans to operate a second incineration unit at the East Liverpool site; impacts associated with the second unit were not considered in the risk assessment document nor discussed by the work group.

Atmospheric Dispersion and Deposition

The work group concluded that overall EPA developed a thorough prediction and assessment of routine releases for the WTI facility. Recommendations from the 1993 work group on atmospheric dispersion and deposition were taken seriously and a genuine effort was made to address the work group's concerns.

One area for which the work group at the January 1996 meeting suggested additional work should be performed concerned the treatment of calm/stagnation conditions. Specifically, the CALPUFF model analysis was limited to "simple terrain" receptors and a greatly simplified meteorological data set due to data limitations. The work group recommends [ST]* performing an analysis that utilizes CALPUFF with a "synthesized" calm/stagnation event. The event would have to be synthesized based on reasonable assumptions regarding the duration and spatial

* ST indicates the short-term nature of the recommendation; LT indicates the recommendation is of a long-term nature.

distribution of winds. Possibly such an event could be portrayed using a subset of the multiyear meteorological data base available from the nearby power plant tower in conjunction with the data periods concurrently available from that tower and the onsite WTI tower. The CALMET meteorological processor or other wind flow models could be used for this analysis. The meteorological data base comprising surface and multilevel tower observations that was compiled for the risk assessment (together with high-resolution terrain data) would provide suitable inputs to various diagnostic wind flow models. The gridded output fields from the diagnostic model should then be used to provide the high-resolution meteorological data required as input to CALPUFF (and other time- and space-variant dispersion models). Maximum chemical concentrations predicted for the event could then be compared to concentrations predicted by the ISC-COMPDEP modeling to better understand the impact of calm/stagnation conditions on predicted concentrations.

In summary, the work group believes that the CALPUFF model was not used in a manner consistent with its potential for providing enhanced realism of dispersion simulation. The use of a zero-dimensional wind field (i.e., wind constant in time, height, and horizontal dimension) precluded any measure of enhanced realism with CALPUFF and does not provide a basis for a meaningful assessment (qualitative or quantitative) of the ISC-COMPDEP modeling results. Our recommendation [ST] is to use a realistic four-dimensional wind field over a reasonable period of time to assess concentrations under adverse dispersion conditions, and then to compare these results with ISC-COMPDEP. As presently constituted, the CALPUFF analysis adds little to the overall risk assessment.

Also, the work group recommends [ST] that extended dispersion modeling performed for the accident scenario should be re-examined in light of the CALPUFF calm/stagnation analysis. Accident scenario concentrations should be recomputed based on the occurrence of an accident during the meteorological event to assess whether ambient concentrations during such an event are significantly exacerbated by accident concentrations and vice versa. Accident impacts generally occur over a much shorter timeframe than impacts from typical air quality events associated with routine plant operations. Also, they occur over much shorter distances than impacts from stack releases. Thus, calculating accident concentrations based solely on the stagnation event may not be appropriate.

The following comments address the adequacy of the ISC-COMPDEP and SLAB models for application in accident analyses. The risk assessment authors clearly state the limitations of ISC-COMPDEP in Appendix VII-4, Volume VII: "....ISC-COMPDEP does not simulate instantaneous or transient releases" (page 5), and only "fire scenarios are modeled using the ISC-COMPDEP model" (page 7). Thus, the remaining question on the use of ISC-COMPDEP is whether it is appropriate for fire scenarios. The answer is "perhaps," but a more appropriate model for fire situations would have been CALPUFF in conjunction with a meteorological/wind field model such as CALMET or an equivalent model. Regarding the use of SLAB, the work group could not reach a firm recommendation because the risk assessment document does not provide a meaningful description of the model's physics. There are hints to SLAB's features in the discussion on "Modeling Parameters," and based on these it would appear that SLAB is acceptable in terms of its ability to treat transient emission conditions. Whether it can treat transient wind conditions or regions of horizontal or vertical shear in the wind field is unclear. It would have been helpful if the work group had been provided copies of the two cited references to SLAB: Ermak (1990) and U.S. EPA (1993); see page 7. Also unclear is the appropriateness of the very short duration (less than 30 minutes) of some of the accident scenarios in the context of worst case scenarios; longer scenarios should have been considered.

The use of spatially (x, y, and z) invariant wind fields in the dispersion modeling performed both with ISC-COMPDEP and CALPUFF/INPUFF is a cause for concern. There are two potentially significant consequences of this simplistic approach: (1) worst case conditions for routine operations may not be adequately described due to the effects of recirculation conditions occurring during multiple-day events, and (2) the direction of worst case impacts relative to the stack (for routine operations) or a ground-level source (for accidents) will likely not be described properly. This could lead to under- or overestimates of human health or ecological consequences. The calm/stagnation event analysis should help address this concern.

Existing air quality data collected in the vicinity of the WTI facility should be examined [LT] to provide some basis for comparing the relative increase in ambient chemical concentrations due to incinerator operation. Data from existing local, state, or federal ambient stations should be examined to perform this assessment. The document would be strengthened by adding consideration of the existing air quality conditions in the valley. Does the valley at present meet EPA standards for "criteria pollutants"? How often, if at all, have exceedances occurred for SO_2, PM_{10}, O_3, etc? What are the trends? WTI does not exist in isolation, and the permitting process has to start at the margin, not from zero.

The WTI risk assessment also would be strengthened [ST] by providing an accounting of the safety record at a similar hazardous waste incinerator such as the Biebesheim facility in Germany. Some concern was expressed that the risk assessment's assumption of "one emergency incident involving hazardous waste release for every 25 or 30 years of operation" may be inconsistent with the two hazardous release incidents already reported at the WTI site (on December 1993 and October 1994) and the "frequent occurrence of kiln overpressures."

Finally, the discussion of the potential health effects associated with inhalation of NO_x, SO_x, and particulate matter does not provide adequate consideration of the impacts of the additional load on respiratory function, particularly in asthmatic and elderly individuals. The discussion should be expanded [ST] to include consideration of how the increased emissions will affect respiratory function. This could be done by comparing the estimated increments to published reports on the effects of pollution episodes on respiratory function (Dockery et al., 1984; this is the Steubenville TEAM study). The treatment of noncancer effects through the use of the Hazard Index does not provide adequate discussion of noncancer health effects. The atmospheric work group recommends [ST] that the present discussion in the risk assessment be expanded to include an analysis of the likely range of risks associated with noncancer health effects.

Accident Analysis

The work group found that potentially important release scenarios have not been considered. Specifically, investigations should be performed to determine whether pressurized jet releases from containment can occur (with aerosol formation due either to the mixing/reaction of chemicals or as a result of heating by fire). The risk assessment/accident analysis should seek to identify scenarios in which liquid can be driven through an orifice in a vessel or pipework at high pressure. Such scenarios might occur when there is high pressure storage (if there is any at the site) or they might occur if vessels are pressurized by some external agent, such as fire. The issue to be decided is whether there are any circumstances at the site where aerosolization could occur, allowing a large fraction of the release to remain airborne as fine liquid droplets, thus increasing the effective magnitude of the source term several-fold over what it would be if only the vapor component is taken into account. This issue was originally raised by the panel because there was some concern that the range of such scenarios had not been fully considered in the WTI risk assessment.

The model used to estimate the rate of evaporation is not appropriate for calm conditions. Evaporation models are available for extremely low wind speeds. One of these should be used (e.g., Rife, 1981), and the results used to support the present estimates or to replace them in an updated accident analysis [ST].

Also, the predicted effectiveness (or lack thereof) of mitigation measures needs to be more clearly addressed, including the influence of time-varying rates of release [LT] and explicit or implicit assumptions about the toxicity [ST] of the hazardous chemicals involved (such as Haber's law).

The work group also discussed the justification of the 20,000-gallon cap on accident emissions as cited in EPA's *Proposed* Rule on Accidental Release Prevention Requirements: Risk Management Programs Under Clean Air Section 112(r)(7). The proposed definition of a worst case release is "the largest quantity of a regulated substance resulting from a vessel or process piping failure." While the largest vessels have capacities of 20,000 gallons each, it is not apparent that a risk assessment should have the same limitation as a risk management plan. Considering the nature of this facility and its history, a risk assessment based on a truly worst case accident scenario should be considered [ST]; this might involve more than a single storage vessel.

Finally, it is not clear how the final summary tables of risk (Tables VIII-1, -2, and -3) were developed from the information presented elsewhere in Volume VII. The authors should provide a reproducible trail of analysis and clearly justify any conservative or nonconservative aspects of their assumptions [ST].

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EXPOSURE ASSESSMENT

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These comments pertain primarily to sections of Volume V of the draft WTI risk assessment document concerned with the characterization of human exposures attributable to contaminants in the gas and particle phases of the atmosphere. The Exposure Assessment Work Group offered three types of comments on this material:

- General issues.
- Comments on the specific issues raised in the charge to the Exposure Assessment Work Group.
- Comments on the accident analysis section of the risk assessment.

All these should be addressed in the near term. The work group also offered suggestions for improving future assessments; these are long-term recommendations.

NEAR-TERM RECOMMENDATIONS—GENERAL ISSUES

The exposure assessment is a large and comprehensive document. EPA expended a great deal of effort to assemble data, construct models, run simulations, and evaluate data. The resulting draft risk assessment addresses most of the recommendations of the 1993 project plan peer reviewers. The document contains information required for an informed debate on health issues, but the information is frequently buried and difficult to track.

The Exposure Assessment Work Group addressed four general issues related to this assessment and future activities:

- **The surrogate selection process, which utilized the quantity-carcinogenic potency-bioaccumulation (QCB) scores of compounds potentially present in emissions.** The work group recommends that the Executive Summary include an expression of the likelihood that an important compound was omitted in the surrogate compound selection process. This would involve a few simple reality checks that can be described qualitatively in a single paragraph.

- **The small amount of information on existing exposures to incinerator-type contaminants in the area.** This omission is consistent with EPA risk assessment guidance, but it raises questions about cumulative or total exposures to a given contaminant in the area. Although methodologies for evaluating cumulative exposures have not been developed fully, the document should address this issue qualitatively. Such a discussion would facilitate evaluation of environmental equity issues as well as more effective communication of relative risks.

- **Total risk from the facility.** Total risk encompasses exposure to continuing emissions from routine operations, episodic exposures to fugitive emissions, and exposures resulting from accidents. The derivation of risk numbers for these three sources should remain separate, but the combined risk should be discussed together in the risk characterization.

NEAR-TERM RECOMMENDATIONS—SPECIFIC ISSUES

Exposure Descriptors

EPA's *Exposure Assessment Guidelines* document identifies descriptors that should be used to characterize exposure, including central tendency estimates (representing the center of the exposure distribution) and high-end estimates (representing individuals above the 90th percentile in the exposure distribution). For the draft WTI risk assessment, average and maximum environmental concentrations were modeled for each medium of concern. Similarly, typical and 90th-percentile values were obtained for most of the exposure factors. If the central tendency exposure estimates were calculated using average values for both media concentrations and exposure factors, and high-end exposure estimates were calculated using 90th-percentile values for both media concentrations and exposure factors, the exposure descriptors were properly used to characterize exposures. Median values rather than mean values generally were used to estimate the central tendency values. This is appropriate because environmental concentrations are often skewed to the high end. The document does not make clear whether 90th-percentile values were used for all inputs in the high-end exposure estimations.

Estimation Approach

The draft risk assessment explains the general approach used to estimate central tendency and high-end exposure values. This approach took into account three factors: concentrations in environmental media, intake rates, and durations and/or frequencies of exposure. These three factors appear to have been combined properly to characterize central tendency and high-end exposures. Many procedures utilize models described in EPA guidance documents. As noted above, however, in some cases the draft risk assessment does not clearly specify when mean and 90th-percentile values were used. Adding a summary table that identifies the specific factors used for each exposure descriptor would be useful.

Exposure Sources and Pathways

Exposure assessments should identify all important exposure sources and pathways. The draft WTI assessment examines a fairly wide range of potential exposure scenarios; the selection of these scenarios and the conclusions concerning the importance of the various pathways are consistent with the current state of knowledge. Adequate justification now exists for omitting ground-water and surface water pathways, as recommended during the 1993 peer review of the project plan for this risk assessment. The approach and algorithms used to calculate exposure doses listed in Volume V, Section VII, of the assessment are in harmony with approaches suggested in the draft EPA Dioxin Reassessment—as well as with approaches used by other agencies, such as the California Environmental Protection Agency.

The 1993 peer review of the project plan for this assessment recommended that EPA include a discussion of exposure via household dust. The assessment does not consider this pathway, nor does it explain why this is not a significant route of exposure for sensitive subgroups (e.g., infants and children). Dermal and ingestion pathways for outdoor soil do not necessarily represent how these contacts occur inside houses. House dust likely originates from three sources:

- Airborne particles that move from outside air to indoor air.
- Surface soil and dust tracked into buildings.
- Sources related to occupant activities, material degradation, and household products.

The assessment should include a brief qualitative discussion of exposure via ingestion of house dust as part of its discussion of soil ingestion.

Estimation of Concentrations and Exposure

The assessment identifies the key assumptions for estimating chemical concentrations and exposures. The magnitude and direction of effect generally are correct, except that the assumption that fate and transport modeling accurately reflects reality is uncertain and does not necessarily result in a "likely overestimate." Often, model parameters are derived from a small number of observations of only a few animal or plant species. The direction of effect is really unknown until these models are better validated with monitoring data collected for that purpose.

EPA could make progress toward verifying its surrogate selection process by comparing the QCB scores of chemicals listed in Volume V, Table IV-1, with the relative contribution of each of these chemicals to the total estimated risk in the actual risk assessment. The assumptions that fate and transport models are accurate should be broken down further (i.e., by including more components in Volume V, Table VI-20). At a minimum, the biotransfer, diffusion, and advection (i.e., deposition) of the fate and transport models should be separated out and listed as separate assumption categories.

Conservative Assumptions

This assessment is based on conservative assumptions, and the cumulative impact of this conservativeness probably results in an overstatement of risk. Including uncertainty analyses for two representative compounds was a useful way of addressing the overall uncertainties—and identifying input parameters that have the greatest effect on the final risk estimate. The discussion on pages VI-14 to VI-15 of Volume V is particularly useful. The evaluation of the fate and transport models includes both model and parameter uncertainties.

One item that remains unclear in the Executive Summary relates to the ratio of high-end to low-end exposure estimates attributable to uncertainty. The former reflects heterogeneity, whereas the latter reflects uncertainty. This provides more confidence about the *relative* values of high-end and central tendency exposures than it does about their *absolute* values. The risk assessment addresses uncertainty by biasing both the high-end and central tendency values toward the upper end of their likely range.

NEAR-TERM RECOMMENDATIONS—ACCIDENT ANALYSIS

The Exposure Assessment Work Group has the following comments on the accident analysis:

- The facility is located on a flood plane. EPA should evaluate the likelihood (1) of a flood large enough to inundate the facility, and (2) that such a flood would cause hazardous materials to be released to the environment.

- The key assumptions made in the identification of accident scenarios and ranking of accident events appear to be reasonable with respect to magnitude and direction of effects. The report does not adequately express or communicate the expected value of harm associated with accidents, however. Rather, the report expresses severity and consequence information using ambiguous phrases, such as "likely" and "unlikely" events, and "moderate" to "catastrophic" consequences. A rough calculation, assuming proper interpretation of the tables, suggests a 1 in 1,000 chance per year of an accident that involves approximately 10 fatalities. Does this mean that, over 10 years, there is a 0.1 or 10 percent (10x10/1,000) likelihood of one fatality in the community from accidents? This is a very large risk compared to the 1 in 1,000,000 limit typically used for cancer risk. Some clarification would be useful.

- The American Industrial Hygiene Association's Emergency Response Planning Guideline (ERPG) levels would probably have been more appropriate than IDLH values for characterizing the severity of accident consequences because IDLH values are designed for healthy workers rather than the general population. LOC values would be an acceptable alternative to ERPG values because LOC values are more stringent than IDLH values for assessing the acute effects of short-term exposures. The Toxicology Work Group also discussed this issue; see that group's summary (below) for more on the use of ERPG versus LOC values instead of IDLH values.

LONG-TERM RECOMMENDATIONS

Although the work group does not have specific recommendations for future research, we note that no provisions are made for validation and periodic checks to determine the reliability of the assessment. If feasible, these activities should be conducted. The work group also recommends that models and parameters be updated as new information is developed. This is particularly important for physical-chemical parameters (e.g., log K_{ow}), which drive many fate and transport models.

TOXICOLOGY

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HAZARD IDENTIFICATION, DOSE RESPONSE, AND RISK CHARACTERIZATION

The Toxicology Work Group noted that, overall, the draft WTI incinerator human health risk assessment is thorough and comprehensive. Highlights of the work group's discussions are summarized below. These comments, which supplement the reviewers' premeeting comments (see appendix D), are divided into near-term recommendations (priority issues and minor clarifications) and long-term recommendations (for future risk assessments/method development).

Near-Term Recommendations—Priority Issues

Cumulative Risk

Exposures from WTI emissions occur against a background of existing emissions. To evaluate the potential impact of the WTI facility on human health, it is necessary to consider the facility's emissions against existing exposures. Therefore, the Toxicology Work Group recommends that EPA quantitatively or qualitatively evaluate releases from other facilities in the area or other data on existing emissions. In addition to the emissions data base, the Total Exposure Assessment Methodology Study data from Steubenville might be useful.

*Dr. Alexeeff was unable to travel to the workshop, but participated in the Toxicology Work Group via teleconference.

Uncertainty

The Toxicology Work Group considers discussions of uncertainty to be an integral part of risk assessments. The treatment of uncertainty in the WTI incinerator risk assessment is more thorough than is typical. Nonetheless, the work group recommends adding further discussion of the uncertainties associated with:

- Data gaps, since not all of the chemicals in the stack emissions have been identified, their toxicities characterized, and/or a valid reference dose (RfD), reference concentration (RfC), or slope factor determined.
- Extrapolating the slope factor and/or hazard index across different routes of exposure.

Lead Cancer Risk

The draft risk assessment does not treat lead as a carcinogen because EPA does not have a slope factor for lead. On an interim basis, the Toxicology Work Group recommends using the slope factor that California EPA has developed to provide a sense of the magnitude of the cancer risk from lead. To address the uncertainty associated with the lack of a U.S. EPA slope factor, the work group suggests comparing California EPA's slope factor with those developed by other agencies. The work group recommends that the discussion of lead's noncancer (neurobehavioral) effects be retained.

Noncancer Endpoints for Dioxin-Like Chemicals

The draft risk assessment does not estimate noncancer risks associated with dioxin-like chemicals because EPA has not yet determined which is the most sensitive toxic effect, nor has the Agency developed an RfD for that effect. The draft risk assessment addresses noncancer risk by comparing the releases estimated to occur from the WTI facility to estimated background exposures. Some dioxin effects (reproductive/developmental, immune) occur in experimental animals at exposure levels lower than those producing cancer, creating concern that these effects will occur more frequently than cancer. Thus, the risk assessment should explicitly discuss differences in how EPA estimates noncancer and cancer risks (e.g., use of hazard index versus slope factor, assumption of no threshold or a threshold, other assumptions) to put the cancer risk estimate into perspective. In particular, the risk assessment should point out that the cancer risk estimate might be more conservative than a noncancer estimate would be (if one were calculated) because it is derived using a slope factor, which assumes no threshold.

Complex Mixture Toxicology: Additivity Versus Synergy Versus Antagonism

To address the fact that WTI emissions represent a complex mixture of toxicants, the draft risk assessment assumes that toxic effects are additive. The Toxicology Work Group recommends that EPA explain the rationale for this assumption more fully, particularly in light of recent studies. Jonker et al., for example, compared the acute (24-hour) toxicity of a combination of four nephrotoxicants in rats compared with that of the individual compounds. Another study (*Food Chem Toxicol* 31:45-52, 1993) suggests that noncancer effects are not additive when exposures occur at or below the no effect level.

Near-Term Recommendations—Minor Clarifications

In reviewing the draft plan for the WTI incinerator risk assessment, the Toxicology Work Group suggested that EPA include health effects data from similar facilities. These data apparently are not available. To clarify this situation, the work group recommends that EPA include in the final risk assessment a list of the data sources examined and what was found in each.

The risk associated with polycyclic aromatic hydrocarbons (PAHs) is based on seven PAHs and expressed as their potency relative to benzo[a]pyrene. The other PAHs are not included. As indicated in Dr. Alexeeff's premeeting comments, beginning on page 8, this introduces uncertainty into the risk estimate. The Toxicology Work Group recommends that EPA briefly explain the rationale for and impact of this risk estimation procedure. In addition, members of the Combustion Engineering Work Group expressed concern that only benzo[a]pyrene was detected.

The Toxicology Work Group also recommends:

- Including a list of noncancer endpoints for the chemicals addressed in the risk assessment, probably in the form of a table.
- In the characterization of releases, clarifying which chemicals were estimated to be released and which were actually detected.
- Clarifying why the risk assessment does not address metals (especially methylmercury) in breast milk.
- Using better terms or acronyms for maximum concentration and area average exposures.
- Expanding the discussion of endocrine disrupters to clearly indicate which emitted (estimated or measured) chemicals are endocrine disrupters and what effects might have endocrine disruption as a mechanism of action.

Long-Term Recommendations—Future Risk Assessments/Method Development

Validation of Dispersion and Uptake Models

The usefulness of a model depends on the ability of the model to accurately predict the fate of the study chemicals. To validate and improve the models used in the draft risk assessment, the Toxicology Work Group recommends that EPA conduct followup monitoring of chemicals of concern in the air, soil, vegetation, and locally produced food. By providing information about the appropriateness of the models under specific conditions, such monitoring might also facilitate model selection in future risk assessments.

The Subpopulation of Workers Who Reside in the Area

The draft risk assessment does not consider the total (occupational and environmental) exposures of individuals who both work in and live near the WTI facility. The Toxicology Work Group recognizes that occupational and environmental exposures fall under the purview of different agencies. Nevertheless, both agencies are charged with protecting human health. The work group recommends that the two agencies jointly develop policies and procedures to integrate the assessment of occupational and environmental exposures to ensure that future assessments adequately consider and protect the health of worker residents.

Subpopulations and Exposure Assumptions

The draft risk assessment uses Superfund defaults for exposure parameters such as residence time and duration of exposure. The Toxicology Work Group recommends evaluating the appropriateness of these assumptions given conditions at the WTI site. For instance, do individuals living in the area have the same mobility as the nation as a whole? Does that mobility reflect true population movement into and out of the affected area, or is the apparent mobility a result of relocations within the area? This information is crucial for judging the validity of the assumption of a 9-year exposure period. It would also shed light on the validity of the assumption that exposure via breast milk can be treated separately (rather than one of several exposure routes applicable to the same individual over his or her lifetime).

The cancer risks for the different subpopulations appear to be spread over different times. Indicating the length of time used for each subpopulation would be helpful. Ideally, the assessment would present cancer risks associated with different exposures to the younger population (-0.75 to 1, -0.75 to 6 or 9, -0.75 to 30) and would analyze exposures from all pathways (including breast milk).

ACCIDENT ANALYSIS

In reviewing the draft plan for the WTI incinerator risk assessment, the Toxicology Work Group expressed great concern that nonroutine and fugitive emissions might be an important exposure source and recommended that they be a major part of the risk assessment. The accident analysis represents a valiant start, but this topic is not well developed and many tools needed to perform a thorough risk assessment of potential accidents are lacking. Many of the work group's concerns with the accident analysis arise from EPA's use of methods not designed for analyzing chemical risks.

Near-Term Recommendations—Priority Issues

Chemicals Selected for Evaluation

Acetone has been deleted from the list of toxic chemicals used for emergency planning because severe toxic levels are not expected to occur (see Dr. Alexeeff's premeeting comments, page 16). The Toxicology Work Group recommends that EPA select a different chemical.

Use of IDLH Values for Characterizing Severity of Consequences

The Toxicology Work Group discussed the use of IDLH values at length. While these values have the advantage of being a comprehensive set of values designed for accident analysis, they are based on assumptions that are not appropriate to the WTI facility. Specifically, they are designed to provide short-term protection to healthy workers trained in emergency procedures and who would be exposed to IDLH levels for a limited time. They do not account for the higher respiratory rates of children, nor do they account for pre-existing conditions (e.g., childhood asthma) that render some of the population more sensitive.

The Toxicity Work Group considered several possible control levels for the accident analysis, including IDLH, ERPG, EEGL, and SPEGL. All of them have problems or limitations, and none is ideal. The work group noted that, for some chemicals, the LOC is similar to ERPG, EEGL, and SPEGL values; however, LOC values exist for *more* chemicals. The overall consensus was that IDLH values are inappropriate for the risk analysis of the WTI site and that uncertainty analysis using the LOC would be a better assessment of the risks. For individual chemicals, ERPG-2, EEGL, or SPEGL might be more appropriate. LOCs are available for more compounds, which is an advantage, and are often similar to ERPG-2, EEGL, and SPEGL values.

Long-Term Sequelae of an Accident

The accident analysis focuses on the acute effects of the accident scenarios. It does not address how chemical exposures during an accident might impact an individual's lifetime exposure to chemicals from the WTI plant. The Toxicology Work Group is concerned that the magnitude of chemical exposure from accidents or other upset conditions might be far greater than exposures from normal operations.

Subpopulation at Risk

The Toxicology Work Group recommends that EPA perform an appropriate modeling and dispersion analysis to determine the worst case concentrations of chemicals at the adjacent East Elementary School. A large number of children attend this school; they might be more sensitive than the adult worker population, and they might be exposed for more than 30 minutes, the maximum exposure period assumed in the IDLH values. EPA should evaluate both short-term acute effects and long-term sequelae.

Near-Term Recommendations—Minor Clarifications

For each accident scenario, the Toxicology Work Group recommends explicitly addressing the East Elementary School in the discussion of exposure area.

Long-Term Recommendations—Future Risk Assessments/Method Development

Severity Categories

Federal Emergency Management Agency (FEMA) categories appear to be structured for widespread disasters (earthquakes, floods, hurricanes), with a large difference between minor and major events. In the case of the WTI incinerator, however, even a minor event would pose a significant challenge to the local community. Thus, the Toxicology Work Group recommends developing a classification system that:

- Includes an intermediate category.
- Characterizes impact according to magnitude of severity and likelihood of occurrence within a specified period of time.
- Uses easily understood terms.

ECOLOGICAL RISK ASSESSMENT

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Members of the Ecological Risk Assessment Work Group agree that EPA conducted the SERA in a technically competent manner that conforms with the state-of-practice for SERAs. Work group members provided premeeting comments covering all aspects of the SERA, and the work group encourages EPA to consider those comments when revising the document. The following points address areas where work group members can recommend specific actions to improve the document.

NEAR-TERM RECOMMENDATIONS

Goals and Purpose of the SERA

The principal problem with the SERA is that its goals and purpose are not clear. As a result, the implications of the results of the assessment and the appropriateness of possible recommendations are unclear. This general problem has the following consequences:

- The purpose of screening assessments is to narrow the scope of subsequent assessments by eliminating chemicals, scenarios, routes of exposure, or receptors that are clearly not hazardous. When, as in this case, certain chemicals in certain scenarios are retained by the screen, the assessment should suggest what additional data collection or analysis will be performed to resolve those issues.

That was not done, at least in part because the assessors were uncertain about the need to resolve the issues.

- The work group's attempts to recommend additional activities to resolve the potential risks identified by the SERA elicited a clarification that called into question the need to resolve these risks: the scenarios related to unresolved hazards were included to help define RCRA permit limits, not to produce actual estimates of risk. If this is true, EPA should clearly state this in the SERA. Otherwise, EPA should consider how to resolve the hazards.

- The lack of an accident analysis in the SERA concerned the work group because such an analysis might contribute to a determination of the acceptability of the incinerator. We would guess that atmospheric releases during an accident could exceed routine emissions and could result in greater exposures because the deposition could occur during a relatively brief period. An accident might also result in significant land and water pollution. Direct terrestrial and aqueous contamination was not considered for routine operations, however, so it was not clear whether such exposures were out of scope.

  The work group's concern about the lack of accident scenarios met with the following clarification: because RCRA permits do not permit accidents, the goal of helping to define permit limits does not require consideration of the ecological risks of accidents. If this is EPA's position, the SERA should clearly state this because a serious accident could release more contaminants than decades of routine operation. EPA should also clarify why this logic was applied to the SERA but not to the human health risk assessment.

  In summary, EPA should consider whether an accident analysis is needed for the SERA and present the results of that consideration. If EPA has a good reason for omitting an accident analysis, this should be presented. Possible reasons might include:

  — EPA performed an analysis that indicated that ecological risks due to accidents could not exceed those from routine emissions.

  — The human health consequences of an accident are so large that they clearly overwhelm ecological considerations.

  — EPA intends to use accident analyses only to devise contingency plans (e.g., evacuation plans), not to help determine the acceptability of the incinerator. Because EPA does not devise ecological contingency plans, accident analyses serve no purpose in a SERA.

  — EPA made a policy decision to not consider accidents relative to ecological endpoints.

  If EPA instead decides to conduct an accident analysis for the SERA, the work group suggests that EPA resolve questions such as what endpoints to include, what results will be useful for risk management, and whether transportation accidents should be considered during the problem formulation stage; these are not strictly technical questions. The work group knows of no precedent or guidance for this accident analysis, but we believe the greatest difficulty lies in the source terms and dispersion rather than the ecological assessment. That is, if accident scenarios can be defined and the associated release and transport modeled, the analysis should not be inherently more difficult than that for routine emissions. If persistent chemicals are released in an accident, chronic as well as acute effects should be considered.

- The permit limit scenario, which seems to drive the SERA, does not appear in the human health risk assessment. The work group wonders if this reflects a judgment on the part of EPA that only health risks will contribute to the decision concerning acceptability of the facility—and that ecological risk considerations will be considered only to refine permit limits.

- The permit-setting goal of the SERA also might explain why the assessment fails to include nonatmospheric emissions (e.g., disposal of ash, scrubber sludge, wastewater). The work group recommends that EPA consider whether the SERA's goals are fulfilled in the absence of consideration of these emissions.

- The work group recommends that EPA staff and contractors responsible for performing the SERA meet with the Region 5 and State of Ohio risk managers for the WTI incinerator. The meeting should follow a Comprehensive Environmental Response, Compensation, and Liability Act (CERCLA) data quality objective (DQO) process to establish the purpose of the SERA and the types of results needed.

  The meeting should address whether the SERA is intended to reveal the risks of the plant to the environment (i.e., analogous to the health risk assessment), reveal the consequences of alternative permit limits, determine whether the risk of severe effects is sufficient to shut down the plant, and so on. Depending on the decision, the SERA might include a best estimate case, focus on critical risks, or address accidents or other emissions.

- Given the relatively large effort devoted to the SERA and the relatively high profile review, a nonmanagement purpose of the SERA might be to develop methods for this type of assessment and to serve as a model for future assessments. The work group's recommendations are based on the assumption that this SERA is not intended to serve as a model assessment, so only issues likely to change the SERA's conclusions are important for this review. If EPA reconsiders the purpose of the SERA and decides to make it a model assessment, the work group recommends that the Agency carefully consider *all* premeeting comments.

Other Issues

The Ecological Risk Assessment Work Group recommends that EPA:

- Edit the SERA to tighten it up and make it more accessible to stakeholders (if the document will be used as a communications piece).

- Indicate in the uncertainty analysis which uncertainties are most important to the conclusions. For example, EPA should state explicitly that the metals found to be hazardous are the same as those for which removal efficiencies were unknown—and that the removal efficiencies of these metals were assumed to be 100 percent.

- Clarify the description of the emission scenarios to indicate their intended interpretation, the reasons for inconsistencies in the degree of conservatism used, and the reasons for inconsistencies compared to the human health risk assessment.

- Briefly address whether, how, and why startup, shutdown, and other nonstandard operating conditions were or were not addressed in the SERA. This is an important issue in the minds of many people who have followed the WTI incinerator debate.

LONG-TERM RECOMMENDATIONS

SERA Methods

The Ecological Risk Assessment Work Group feels strongly that the SERA illustrates the need to develop data sets, models, and other methods for screening assessments. SERAs should be relatively quick and inexpensive so that time and effort can be devoted to definitive assessments that provide realistic estimates of exposure and effects and that resolve risk management issues. In the absence of the types of data sets and default methods available to human health risk assessors, high-effort SERAs will be required to reinvent the wheel. Thus, efforts to develop tools and data for SERAs would not only be efficient use of resources, but would increase the quality of SERAs by providing tools and data that are consistent, reliable, and peer reviewed.

Background Contamination

The Ecological Risk Assessment Work Group recommends addressing the issue of background contamination in the SERA. Background contamination might be important for two reasons:

- The WTI incinerator is located in an industrialized area. The incremental risk posed by the incinerator might be small or significant, depending on how it compares to the magnitude of the background risk.
- Given the large uncertainties in the SERA, environmental monitoring would be desirable; however, background contamination might preclude monitoring of ambient media and biota to detect the influence of the incinerator. Thus, EPA should evaluate the practicality of environmental monitoring in this context.

Ecotoxicological Data Gaps

The Ecological Risk Assessment Work Group is concerned about the implications of the many ecotoxicological data gaps. The work group recommends that EPA attempt to analyze the implications of the data gaps on the reliability of the SERA. EPA could address the lack of data for specific endpoint taxa by considering the sensitivities of uncharacterized taxa relative to those of taxa for which data are available. EPA could address the lack of data for specific chemicals by determining whether the estimated risks are credible given each chemical's maximum credible toxicity.

Other Issues

The Ecological Risk Assessment Work Group recommends that EPA:

- Consider the work group's premeeting comments on selection of ecological chemicals of concern (ECOCs) if the Agency reconsiders the purpose of the SERA or decides to make it a model assessment. Although the work group believes EPA's method of ECOC selection yielded acceptable results for this SERA, the method is questionable.

emissions to model fugitive emissions, the use of sulfur dioxide as a surrogate for selenium, the particle size assumptions used for the metals analysis, and the gas-solid partitioning assumptions used in the PCDD/F analysis. They also wondered which PICs were nondetects and why EPA had not prorated measured PICs to compensate for unmeasured PICs. On the whole, however, the peer reviewers responded favorably to this part of the risk assessment, praising several aspects of EPA's work (e.g., the Agency's efforts to determine the composition of the waste feed and its discussion of uncertainty in PIC estimation).

Similarly, the peer reviewers praised EPA's efforts to follow the recommendations of the project plan peer reviewers on air dispersion/deposition modeling. Some voiced concern that the ISC-COMPDEP model might be so conservative as to mask sensitive effects, commented that the discussion does not clearly explain how and to what extent EPA used the INPUFF and CALPUFF models, and suggested that EPA run ensemble types of sensitivity tests to obtain a range of stochastic distributions. Over the long term, they suggested, EPA should develop improved methodologies for analyzing potential dense gas emissions and dispersion and more sophisticated tools for modeling deposition (especially wet deposition).

In their preliminary comments on the exposure assessment, the peer reviewers again observed that EPA had addressed nearly all of the recommendations of the project plan peer reviewers. Describing the exposure assessment as comprehensive, the peer reviewers stated that EPA had appropriately addressed all important descriptors and pathways except for house dust. The peer reviewers suggested that EPA consider using actual operating experience to validate its exposure predictions and to determine how WTI-related exposures contribute to total exposures in the area.

On the topic of toxicology, too, the peer reviewers noted that EPA had generally followed the recommendations of the project plan peer reviewers. They also commented that EPA's selection of surrogate chemicals seems reasonable and that the special consideration given to some chemicals is appropriate, although they suggested that EPA provide more on PAHs and lead and enumerate specific noncancer endpoints. The peer reviewers also suggested that EPA consider risks to the subpopulation of individuals who both work in and live near the WTI facility, that EPA clarify how it assessed the noncancer risks associated with chemicals without an RfC or RfD, and that EPA more clearly identify gaps in the data.

OBSERVERS' COMMENTS

During the workshop, observers were given two opportunities to offer questions and comments. First, at the discretion of each work group chair, observers were invited to participate in work group discussions. Second, they were invited to make statements to the full workshop during the afternoon plenary session. Observers raised issues concerning the technical and scientific aspects of the draft risk assessment as well as the political and personal aspects of the WTI incinerator. Highlights of observer comments include:

- If possible, it would be useful for peer reviewers to visit the WTI incinerator to obtain a clearer understanding of the layout/impacts; in so doing, peer reviewers should interview local residents to balance industry information with the knowledge and experiences of the public.

- The emergency plan to be used in case of an accident at the WTI facility will not prevent exposure of children to toxic gases.

- During previous incidents, WTI has not acted uniformly responsibly (e.g., WTI failed to notify public health officials when a gas main was ruptured). Several incidents (e.g., mistaken placement of caustic waste into the WTI facility's pollution control system) have engendered fear in local residents and, in some cases, have prompted evacuations.

- Residents impacted by the WTI facility are people, not just numbers. Use of the term "moderate risk" to describe an accident involving 10 fatalities is insensitive to impacted residents. WTI should not be allowed to operate if EPA is not extremely confident that there will be no life-threatening accident or other accident that will impact the children attending the nearby school.

- The accident analysis is the weakest part of the risk assessment. It fails to:

    — Include information from other relevant facilities (e.g., the Biebesheim facility in Germany, other American hazardous waste incinerators, and chemical plants).

    — Provide true worst case scenarios (e.g., a situation in which a liquid vaporizes and the condensate aerosol ignites to cause a gasoline bomb-type explosion) and other relevant scenarios (e.g., a major fire in the bunker where solid hazardous waste is dumped and commingled prior to burning; accidental burning of radioactive waste).

    — Consider the unique location of the WTI facility, which might make offsite damage and injuries more likely than at other facilities.

    — Use sufficiently protective values (IDLH values are inappropriate).

APPENDIX A

WORKSHOP AGENDA

United States
Environmental Protection Agency
Risk Assessment Forum

Technical Workshop on WTI Incinerator Risk Issues

Holiday Inn - Georgetown
Washington, DC
January 11-12, 1996

Final Agenda

Workshop Chair: Dr. Thomas McKone, University of California

THURSDAY, JANUARY 11

7:30AM	**REGISTRATION/CHECK-IN**
	OPENING PLENARY SESSION
8:30AM	Welcome *Dr. William Wood, U.S. Environmental Protection Agency (EPA), Risk Assessment Forum (RAF)*
8:45AM	History of EPA Risk Assessment Activities for the WTI Incinerator *Dr. Harriet Croke, EPA Region 5*
9:00AM	Charge to the Reviewers *Dr. Thomas McKone*
	PREMEETING COMMENTS
9:15AM	Combustion Engineering *Dr. Barry Dellinger, University of Dayton*
9:30AM	Air Dispersion/Deposition Modeling and Accident Analysis *Dr. Walter Dabberdt, National Center for Atmospheric Research*
9:45AM	Exposure Assessment *Dr. George Fries, U.S. Department of Agriculture*
10:00AM	Toxicology *Dr. Mary Davis, West Virginia University*
10:15AM	Ecological Risk Assessment *Dr. Glenn Suter, Oak Ridge National Laboratory*
10:30AM	B R E A K

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THURSDAY, JANUARY 11 (Continued)

11:00AM	**CONCURRENT WORKGROUP BREAKOUT SESSIONS**
12:00PM	L U N C H
1:00PM	**CONCURRENT WORKGROUP BREAKOUT SESSIONS**
3:30PM	B R E A K
	PLENARY SESSION
3:30PM	Observer Comments
4:00PM	Workgroup Status Reports and Discussion (Workgroup Leaders)
6:30PM	A D J O U R N

APPENDIX B

REVIEWER LIST

United States
Environmental Protection Agency
Risk Assessment Forum

Technical Workshop on WTI Incinerator Risk Issues

Holiday Inn - Georgetown
Washington, DC
January 11-12, 1996

Final Reviewer List

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APPENDIX C

CHARGE TO REVIEWERS

CHARGE TO REVIEWERS
FOR THE WTI DRAFT FINAL RISK ASSESSMENT

The draft final WTI risk assessment is divided into several volumes covering the scientific disciplines of toxicology, environmental fate and transport, combustion engineering, atmospheric modeling, exposure assessment, ecological risk assessment, and accident analysis. As a reviewer of the WTI draft final risk assessment, you should use your best technical knowledge and professional judgment to comment on the technical accuracy, completeness and scientific soundness of the assessment. Each reviewer is asked to focus on several specific issues in his or her area of expertise with comments on other areas invited but optional. Your comments will be considered in finalizing the risk assessment.

For the peer review workshop reviewers will be organized into 5 work groups: Combustion Engineering, Air Dispersion and Deposition Modeling and Accident Analysis, Toxicology, Exposure Assessment, and Ecological Risk Assessment. All reviewers should be familiar with the Executive Summary (Volume I) and the Facility Background (Volume II) sections of the draft risk assessment. In addition, each work group should focus on specific Volumes as specified below:

Workgroup	Risk Assessment Volumes	
Combustion Engineering	Volume III - Facility Emissions	
Air Dispersion and Deposition Modeling and Accident Analysis	Volume IV - Atmospheric Dispersion and Deposition Modeling	Volume VII - Accident Analysis
Toxicology	Volume V - Human Health Risk Assessment	Volume VII - Accident Analysis
Exposure Assessment	Volume V - Human Health Risk Assessment	Volume VII - Accident Analysis
Ecological Risk Assessment	Volume VI - Screening Ecological Risk Assessment	

While reviewing these sections of the document, please address the following general issues.

1. Comment on the organization of the risk assessment document. Does the layout follow a logical format? Is the presentation of information in the document clear, concise and easy to follow?

2. Does the executive summary accurately reflect the data and methodologies used and the conclusions derived in the risk assessment?

3. Were the major recommendations of the 1993 peer review workshop for the risk assessment plan addressed?

4. As with any risk assessment, there are always additional data and method development efforts that could be undertaken to reduce the level of uncertainty. However, are there any major data or methodological gaps that would preclude the use of this risk assessment for decision making? If so, How should they be addressed?

5. What long-term research would you recommend that could improve risk assessments of this type in the future?

In addition, the following workgroup specific issues should be addressed.

<u>Emissions Characterization</u>

Emissions characterization includes identification of substances of concern and the development of emission rates for these contaminants. Emission rates were developed through a combination of site specific stack test data and models. Please comment on the following issues with respect to this aspect of the draft risk assessment.

1. To characterize the nature of the emissions, waste stream profiles were developed and entered into a database. Several refinements and adjustments (e.g., the Subtraction Correction Factor for chlorinated compounds) were applied to the profiles before substances of concern were identified. Please comment on whether or not these adjustments are appropriate. What is the anticipated effect on the risk assessment?

2. Comment on the selected chemicals of concern. Have important chemicals been missed due to the selection technique?

3. Comment on the approaches used to estimate stack emission rates (e.g., use of the 95% UCL of the arithmetic mean or the maximum detected value, whichever is <u>smaller</u>, for high end emission rates). Are the approaches appropriate? Are their effects on the risk assessment adequately characterized? Comment on the adjustment made to PCDD/PCDF emission rates to account for brominated dioxin-like compounds. Also, comment on the approach to characterizing emission rates from fugitive sources (e.g., use of the TANKS 2 model for the Carbon Adsorption Bed).

4. Comment on the identified sources of fugitive emissions. Was the approach used to select these sources appropriate? Have important sources been

missed? Have emissions from process upsets been given appropriate consideration?

5. There have been a number of controlled burns at the WTI facility. Please comment on the adequacy of these data in estimating potential exposure. Please comment on the assumptions made from the tests in regard to composition of wastes received at WTI and emissions when the plant operates in the future.

6. Comment on the use of emission factors from coal burning to estimate the emission rate of fly ash from WTI. Are the factors used to adjust the coal emission rate appropriate? Are the uncertainties introduced from this approach adequately characterized?

7. Overall, is the identification of the key assumptions used in characterizing the nature and magnitude of emissions thorough? Are the magnitude and direction of effect of these assumptions on the overall risk assessment accurately characterized? Is the uncertainty and variability inherent in this analysis adequately discussed? Does the sensitivity analysis cover the major parameters expected to have an effect on the risk assessment?

Dispersion and Deposition Modeling

To develop this risk assessment, computer models have been used with site specific data on emission rates and meteorological conditions to simulate the air concentrations and deposition rates for contaminants potentially emitted from the WTI facility. The models used include the Industrial Source Complex - Complex Terrain Deposition (ISC-COMPDEP), the CALPUFF, and the INPUFF models. In your review, please address the following issues.

1. Since the 1993 peer review of the risk assessment plan, a number of efforts have been completed to reduce the uncertainty associated with the air dispersion and deposition modeling. These efforts include the collection of site-specific data for emission rates and meteorological conditions. Also, a wind tunnel study was conducted to evaluate the effects of the complex terrain surrounding the WTI facility. Does the risk assessment document adequately summarize these activities? Is the link between these data collection efforts, the air dispersion models, and the risk assessment clearly established?

2. The results of 12 sets of sensitivity tests indicate that geophysical variables (e.g., terrain) are more likely to affect dispersion and deposition than emission variables (e.g., stack temperature). Were these sensitivity analyses adequate? Comment on the conclusions reached. To further examine the effect of geophysical variables, wind tunnel testing was conducted to model the terrain induced flow effects expected near WTI. It was concluded that changes in peak concentrations attributed to these effects are relatively minor and that the

ISC-COMPDEP model is sufficiently conservative. Comment on this conclusion. Have these analyses helped to characterize and/or reduce the uncertainty in the air dispersion modeling associated with the complex terrain surrounding WTI.

3. The ISC-COMPDEP model does not allow for non-steady state conditions such as calm winds and strong temperature inversions. Therefore, CALPUFF was used to estimate air dispersion and deposition under these conditions. However, CALPUFF gave similar peak, 24 hour, and annual average concentrations as ISC-COMPDEP. Comment on the adequacy of this analysis. Comment on the conclusions reached. Has this analysis helped to characterize and/or reduce the uncertainty in the air dispersion modeling associated with non-steady state meteorological conditions?

4. Atmospheric dispersion modeling was used to estimate air concentrations of hazardous chemicals for the accident analysis. The SLAB model was used for vapor releases from spills and the mixing of incompatible wastes. ISC-COMPDEP was used for releases associated with fires. Comment on the selection of the models and inputs. Are they appropriate selections?

5. Overall, have adequate sensitivity tests been conducted to demonstrate the magnitude of variation in concentrations and deposition estimates with model inputs?

Human Health Risks

Human Health Risk Assessment includes hazard identification, dose-response evaluation, exposure assessment, and risk characterization. To develop the risk assessment, potentially exposed populations have been identified and the magnitude, frequency, and duration of their exposure quantified. This information was then integrated with the hazard identification and dose response evaluation for the risk characterization. For this risk assessment, both carcinogenic and non-carcinogenic health effects have been evaluated. In your review, please comment on the following issues.

Exposure

1. EPA's Exposure Assessment Guidelines identify certain exposure descriptors that should be used to characterize exposure estimates. The Guidelines define high end exposure estimates as those representing individuals above the 90th percentile on the exposure distribution but not higher than the individual in the population who has the highest exposure. Bounding exposure estimates are those that are higher than the exposure incurred by the person in the population with the highest exposure. Central tendency exposure estimates are defined as the best representation of the center of the exposure distribution (e.g., arithmetic mean for normal distributions). Comment on whether or not

the WTI exposure assessment properly characterizes each of the exposure estimates in terms of these descriptors.

2. The factors that go into estimating a central tendency or high end exposure, once the population has been defined, include the environmental media concentration, the intake rate, and the duration and/or frequency of exposure. Comment on whether or not the WTI exposure assessment does an adequate job of describing the logical procedure of combining these factors to develop central tendency, high end, and/or bounding estimates of exposure for each of the exposed subpopulations.

3. An important factor in an exposure assessment is identifying all of the important exposure sources. Please comment on the adequacy of the WTI assessment in identifying the important sources and pathways of exposure.

4. Have the key assumptions for estimation of chemical concentration and for estimation of exposure been identified? Are the magnitude and direction of effect correct for the assumptions that have been identified?

5. Supposedly, conservative assumptions have been applied in this assessment to account for uncertainty. Are the conservative assumptions appropriately factored into the ultimate characterization of what descriptor best applies to each exposure estimate? Please comment on whether the uncertainties were confronted in an adequate manner. If they were not, please state what should be done differently.

Hazard Identification/Dose Response and Risk Characterization

1. To select surrogate compounds for quantitative risk assessment, a two step process was used in which chemicals were ranked on the basis of emission rate, toxicity (both cancer and non-cancer), and bioaccumulation potential. Please comment on this selection process. Are the ranking factors appropriate? Could important compounds have been omitted from the analysis based on the ranking procedure?

2. For the majority of the chemicals of concern, traditional approaches to dose response evaluation were employed (e.g., use of a slope factor for cancer and use of a RfD/RfC for non-cancer). However for certain chemicals or groups of compounds a different methodology was used. Specifically, dioxins, furans, PAHs, lead, mercury, nickel, chromium, acid gases, and particulate matter were given special consideration. Please comment on the methodology used for these compounds. Was it appropriate? Have the uncertainties associated with the methodology been adequately characterized? Comment on the assumptions used due to a lack of chemical specific data.

3. Please comment on the selection of the overall population and the various subpopulations at risk. Were site specific data, such as the informal home gardening survey, properly utilized to identify these subpopulations?

4. It is stated in the risk assessment that average risk estimates are based on average emission rates, average air dispersion/deposition within a subarea, and typical exposure factors. Further, maximum risks are based on average emission rates, typical exposure factors, and the maximum air concentration within a subarea. Please comment on this use of the terms average and maximum risks. Are these descriptive terms appropriate given the parameters used to derive each?

5. Comment on whether or not the non-cancer risks of chemicals of concern have been adequately addressed by the risk assessment? For example, has an adequate discussion of endocrine disrupters been provided which either characterizes their risks or clearly explains why their risks cannot be characterized? Further, have non-cancer chronic toxicities of dioxins and furans been adequately addressed in the risk assessment?

6. Please comment on whether or not the uncertainties associated with the additivity and/or synergy of risks from pollutants emitted together from the WTI facility are adequately discussed in the risk assessment.

7. Have the key assumptions for estimation of dose and risk been identified? Are the magnitude and direction of effect correct for the assumptions that have been identified? Please comment on whether the uncertainties were confronted in an adequate manner. If they were not, please state what should be done differently.

8. Please comment on the overall adequacy of the risk characterization. Does the risk characterization include a statement of confidence in the risk assessment including a discussion of the major uncertainties. Are the hazard identification, dose-response assessment, and exposure assessment clearly presented? Have sufficient risk descriptors which include important subgroups been presented and discussed?

Screening Ecological Risk Assessment

As with the human health risk assessment, the ecological risk assessment pulls together elements of exposure analysis and dose-response evaluations to develop a risk characterization. For the Screening level Ecological Risk Assessment (SERA), Ecological Chemicals of Concern (ECOC) and indicator species have been identified to provide conservative estimates of risk. Please address the following issues in your review.

1. Are there any components of the SERA which you feel undermine the scientific validity of the assessment? If so, what are they and can you provide suggestions to strengthen the identified components?

2. Is the organization of the document clear and does it present the material in a clear and concise manner consistent with the *Framework for Ecological Risk Assessment* (EPA, 1992)?

3. Uncertainties are discussed in numerous sections of the SERA and compose Section VIII of the SERA. In each case, do these discussions cover all relevant and important aspects of the uncertainties which you think should be addressed in the SERA?

4. In your opinion, what is the weakest and what is the strongest aspect of the SERA? Can you make any suggestions on how the weakest parts can be strengthened by the Agency?

5. In Section II, are the stressors, ecological effects, and both the assessment and measurement endpoints adequately characterized? Are the five emission scenarios adequate to characterize the exposures for the WTI facility? Are there other emission scenarios which you think should be included in the SERA?

6. In Section III, is the site characterization adequate to support the SERA? Why or why not?

7. In Section IV, is the tiered process used to identify the ecological chemicals of concern (ECOC) from the initial list of potential chemicals considered scientifically defensible? Does application of this tiered approach support the statement made in the SERA "by focusing on the potential risk from the selected ECOCs, the SERA provides a thorough screening-level evaluation for the WTI facility?"

8. In Sections V and VI, are the exposure and ecological effects adequately characterized? Are the most appropriate estimation techniques available used? Are the assumptions clearly stated?

9. In Section VIII, are there any major elements missing from the risk characterization which you think need to be included or which would strengthen the risk characterization? Does the risk characterization support the summary and conclusions presented in Section IX?

10. In Section IX, given the assumptions made and the processes used to select and evaluate chemicals, receptors, and exposure pathways, do you think the SERA adequately met its objective of not inadvertently underestimating risk?

Accident Analysis

The Accident Analysis for the WTI Incinerator involves evaluating the probability of an emergency incident occurring which results in the release of hazardous waste. The consequences of this release are also evaluated using exposure and human health effects information. Unlike the human health risk assessment which has a primary goal of quantifying risks, the accident analysis typically provides information that can be used to reduce the likelihood, extent and impact of possible accidents. *Please comment on the following issues in your review of this aspect of the risk assessment.*

1. The WTI accident assessment selected five scenarios for quantitative evaluation that were considered to be of primary concern. The scenarios are an on-site spill, an on-site fire, an on-site mixing of incompatible waste, an off-site spill, and an off-site spill and fire. Please comment on the selection of these scenarios. Were any significant scenarios missed?

2. Specific chemicals were selected to evaluate each scenario. Please comment on the selections. Would other chemicals have been more appropriate?

3. Chemical specific release rates are calculated for each scenario. Please comment on the procedures used to estimate the release rates. Was an appropriate approach used?

4. Atmospheric dispersion modeling was used to estimate air concentrations of hazardous chemicals. Specifically, the SLAB model was used for vapor releases from spills and the mixing of incompatible wastes. ISC-COMPDEP was used for releases associated with fires. Comment on the selection of the models and inputs. Are they appropriate selections? Should other models or inputs been used?

5. Please comment on the assessment's conclusions on the severity of consequences and probability of occurrence. Has the report correctly categorized the severity of the consequences of the different accident scenarios? Has the assessment adequately justified the reported probability of occurrence of each of the accident events?

6. Key assumptions were made in the identification of accident scenarios and the description of the conservative and typical events. Included were a description of the magnitude of the effect of the assumptions and direction of the effect. *Please comment on the assumptions. Are they justified? Are the descriptions* of the magnitude and directions of the effects correct? Has the accident assessment adequately confronted the uncertainties involved in doing this type of analysis? If not, what else should be done?

7. Comment on the appropriateness of using IDLH values for characterizing the severity of consequences in the accident analysis. Comment on the appropriateness of using 10 X LOC for chemicals for which IDLH values have not been established.

8. In the accident analysis, IDLH (or 10 X LOC) values were used to determine the downwind distances over which adverse human health effects might occur. To evaluate the uncertainty introduced by using the IDLH, a sensitivity analysis was conducted where these distances were recalculated using the LOC (a more stringent health criteria). Other sources of uncertainty that are identified in the accident analysis include concentration averaging times, chemical concentrations, emission rates, and meteorological conditions. For most of these parameters it is stated that conservative assumptions were used to avoid underestimating risks. Have the uncertainties inherent in the accident analysis been adequately characterized? For those parameters where sensitivity analyses were not conducted, is the conclusion that conservative assumptions have avoided underestimation valid?

APPENDIX D

PREMEETING COMMENTS

Combustion Engineering

This assessment appears to be a considerable improvement over the last one and the authors are to be complimented for addressing most issues with thoroughness. My comments focus on certain specific aspects of the emission portion of the document (Volumes I - III). The Executive Summary could be a bit more results-oriented.

Facility Description

The facility description is not as complete as it could be, though Figures II-5 and II-6 (Vol. II) give a good over view. The latter is a pre-enhanced carbon injection system (ECIS) -schematic and while the location(s) of carbon (C)-injection is confidential, quantities injected appear to be equally so. Since (some) C-injection occurs upstream of a electrostatic precipitator (ESP) one would expect some effect on performance due to a change in dust resistivity. For the 3-field ESP no rapping cycle information under ECIS-conditions is provided. From Figures II-6 the ESP-efficiency works out to 99.63%. Is it different under ECIS-conditions? It is stated that the removal efficiencies for PCDD/F are assumed to be directly dependent on the concentration of activated carbon, but there is no way to verify this contention (cf. below).

Chlorinated Dioxin and Furan (PCDD/F) Emissions

The PCDD/F-data (Vol. I, Table III-1) are of considerable interest. The 1994 results are reviewed slightly differently in the enclosed tables (A-D). Of interest is a comparison between PT (performance test) and TB (trial burn) for 2/94; the former gives higher total (PCDD/F), but lower (TEQ); in addition, this PT gave the smallest value for the ratio $(TEQ)_{\bar{x}}/(PCDD/F)_{\bar{x}}$, 0.0058 (Table B). However, the ratio $(TEQ)_{\bar{x}}/(Cl)_{\bar{x}}$ was the lowest for this TB, though the chlorine feed rate there was the highest (Table C). Unfortunately, all 2,3,7,8-TCDD-emission rates are reported (presumably due to analytical limitations) as "less than" (Table D). Though this means that these emissions rates are very low, this result does not lend itself to interpretation in terms of chlorine feed variability and other possible (though not reported) differences between these tests. Thus, I am not quite sure how this data set (Table D) was used to generate an average emission rate of 1.08×10^{-11} and a high-end emission rate of 2.16 x 10^{-11} g/s for this compound (Vol. III, III-16, Table III-2). Once the 12/94 results are available, a more sophisticated statistical analysis should be attempted. It is unfortunate that C-injection locations, C-injection rates and adjustments in ESP-operating parameters were withheld. Such information would aid in the interpretation of these results. It is not clear, therefore, that lumping all 26 post ECIS-PCDD/F-results to calculate a 95% UCL is all that meaningful. Another questions is: are the average and high values (Vol. III, Table III-2) within the analytical results for repeat analyses (if taken)? Some of the differences are less than 30%. Which of the 17 congeners were not detected? The conclusion, that (TEQ) is at best a weak function of the Cl-content of the fuel seems reasonable. And using the 8/93 test data represents a conservative approach.

Although phase distribution of PCDD/F is important from a risk assessment point of view, little seems to have been added between this and the original assessment. The discussion on partitioning appears unchanged from the previous one. The statement (Vol. III, III-11) that "substances in the stack gas will generally be present in either the vapor phase or in the particle phase" seems to ignore the possibility that many compounds, among them PCDD/F, could be partitioned in both phases in the stack gas. Although the material on partitioning appears in Vol. III, it is applied only in Vol. V, Table IV-5. The numbers in that table appear to derive from an assumption of $T = T_{ambient}$ and initial concentrations in the gas phase. As I noted in my earlier comments (December 1993), Biddleman (1988) cites $\Delta S_f/R = 6.79$ as an average and gives three references; he does not say that it can be "satisfactorily estimated". In fact, McKay, et al. (1982) state that it is an average empirical value and "may be substantially in error for certain compounds". Given the particle emission rates cited (0.07 g/s), volumetric flow rates, and assuming fly ash surface areas of less than 10 m^2/g, the surface area available in the stack gas particulate matter would appear to be substantially greater than 10^{-4} cm^2/cm^3 (Vol. III, III-12). The WTI-stack particles should be characterized by surface area and size distribution to enable meaningful in-stack partition calculations. It is likely that the nonideality of the ash particle surfaces needs to be described in terms of a

Freundlich isotherm. To make this approach useful for risk assessment purposes, a more critical review has to be conducted.

Waste Profile Data/PICs

Why were waste profile sheets from only the first year operation used to generate waste feed data? It would seem to make sense to compare the estimated emissions to current sampling/analytical data. Is there a error/reliability estimate for the generator range of chlorine for use in the correction factor (CF) -equation

$$CF = \frac{\text{mol/y Cl}|_{\text{anal.}}}{\text{mol/y Cl}|_{\text{generator range}}}$$

How are some of the constituent ranges (0-30, 5-25, etc.) justified? Using these in the data base refinement (Vol. III, Appendix III-1, II-4) assumes that the upper value is the highest value possible. How certain is one that 0-30% means that the actual content of a particular compound within that range (Vol. III, Appendix III-1, II-9)? Is "caution of the part of the shipper" the best criterion?

With respect to other PICs and organic residues (Table III-3, III-8) was chloranil considered as a possible PIC; it is an expected oxidation product of pentachloro- and other chlorophenols?

It is stated that a quick analysis (to obtain a finger print) is normally performed on incoming wastes. Can normally be related to some frequency with which this is done?

On page II-5 (Vol. III, Appendix III-1) pumpable and nonpumpable wastes are defined. The waste profile data sheets (ibid., p. 2, attachment 1) ask (11.D): is this waste pumpable? Is there another judgment made later when wastes "... are aggregated ...?" I attach some importance to the actual sequence of events here. Both types of waste contain substantial quantities of such compounds as toluene and MEK, but the firing methods differ.

What is the exact interpretation of Table IV-1 (Appendix III-1)? Presumably monochlorobenzene (POHC) equals chlorobenzene (PIC). Only ranges are given. Can we associate these compounds more directly with the PCDD/F-measurements made during the same trial burn (3/93)? Key assumptions for chapter IV (Table IV-3), such as "factor accurately reflects sample loss (2)" and "factor accurately reflects instrument response (3)" appear to be inadequately justified in the text. The statement that "these data show that there is no large removal effect of a carbon injection system on the bulk of combustion THC-emissions" seems premature.

Continuous Monitoring

Several species, i.e., CO, SO_2, NO_x, etc. are monitored continuously. However, there is no mention of a) % data capture and b) number of excursions, if any, associated with kiln mishaps, etc.

Fugitive Emissions

Under fugitive Ash Emissions, it is mentioned that a monthly fly ash sample (1994) was analyzed for 80 organics and that none were found. This must be some sort of composite sample. How was it obtained? How was it stored? What were the detection limits of the analytical method(s) used?

The use of a coal ash emission factor of 0.107 lb/ton and multiplying it by 10 seems tenuous. Could the estimate not be refined in terms of the WTI-fly ash composition (some fraction of large carbon particles present, which presumably are of much lower density than ash particles)? It would seem that there might be enough information to generate this emission factor for this facility.

Most of the other approaches described appear reasonable.

Metals

Though the modeling results appear impressive, some of the assumptions are not clear. Apparently, there have been no particle size specific measurements since the 3/93 trial burn. So what were initial number distributions used in the modeling? If all metals that vaporize subsequently condense to form 0.5 μm particle, their density should be very different from the (typical) ash particles. For Figure III-6 (p. III-42) is the ordinate scale correct? And the basis for the (assumed) value of the saturation ratio? In spite of some modeling similarities between chromium and aluminum, the former would seem to be a poor surrogate for the latter; their chemistries differ.

Elsewhere on the discussion of metal emissions it has been argued that historical data were insufficient or unknown to assist in the estimation of emissions. The HIM facility in Biebesheim, Germany, which has been cited previously when looking at PCDD/F-emissions and C-injection, could be used for metals emissions comparison too, though it is not exactly like the WTI-facility. Tillman (1994) and references cited therein should be a good starting point. Why were waste feed data not available?

Other

I counted eight kiln outages for full or partial rebricking between 5/27/93 - 1/08/95, with no apparent systematic intervals. Is some thought being given to do this (rebricking) on a scheduled basis? These and other process upsets can lead to variable emissions. Some consideration has been given to them. This is a good starting point, but more refinement is advisable, for example, with respect to actual duration of emissions after waste cutoff.

1994 Performance Test (PT) and Trial Burn (TB) Results

for Chlorine, PCDD/F, and TEQ

Table A Mean ($\bar{x}$), standard deviation (σ) and variability (v)[1] for $(PCDD/F)_T$ and (TEQ)

Type and Date	Total, $(PCDD/F)_T$ $\bar{x}$	σ	v	(TEQ) $\bar{x}$	σ	v
PT, 2/94	5.34	1.85	0.35	0.031	0.009	0.277
TB, 2/94	4.60	1.06	0.23	0.067	0.012	0.123
PT, 4/94	3.74	0.83	0.22	0.035	0.002	0.053
PT, 8/94	1.34	0.38	0.28	0.017	0.003	0.202

[1] $v = \frac{\sigma}{\bar{x}}$

Table B Ratio of $(TEQ)_{\bar{x}}$ to $(PCDD/F)_{\bar{x}}$

Type and Date	
PT, 2/94	0.0058
TB, 2/94	0.0146
PT, 4/94	0.0094
PT, 8/94	0.0127
PT, 8/93[1]	0.0180[1]

[1] Run 3, apparent outlier, deleted

Table C Chlorine feed rate and ratio of $(TEQ)_{\bar{x}}$ to mean chlorine feed rate, $(Cl)_{\bar{x}}$

	Chlorine feed rate			$(TEQ)_{\bar{x}}/(Cl)_{\bar{x}}$, x 10^4
	$\bar{x}$	σ	v	
PT, 2/94	1979	342	0.173	0.16
TB, 2/94	3151	131	0.042	0.10
PT, 4/94	2039	359	0.180	0.17
PT, 8/94	798	529	0.660	0.21

Table D Reported 2,3,7,8-TCDD ranges of emission rates from 1994 TB and PT's, g/s

TB, 2/94	$< 2.28 - 3.98 \times 10^{-11}$	(4)
PT, 2/94	$< 2.20 - 6.72 \times 10^{-11}$	(5)
PT, 4/94	$< 3.37 - 6.27 \times 10^{-11}$	(6)
PT, 8/94	$< 2.16 - 5.22 \times 10^{-11}$	(7)

Review of Risk Assessment for the WTI Hazardous Waste Incinerator
Barry Dellinger, Ph. D.
Leader, Combustion Engineering Peer Review Panel

Public concern and objections to siting and operating a hazardous waste incineration facility can be placed in three categories: 1) "nuisance" value, 2) concern over stack emissions of toxic combustion by-products, and 3) concerns over accidental catastrophic releases of toxic materials. The *nuisance* value includes concern over "quality of life" limits such as declining property value, degradation of aesthetic value of the landscape, increased heavy vehicle traffic, etc. There appears to be little that can be done from a scientific or policy perspective that can ever resolve the nuisance issues other than many years of operation without incident. As a society, we have not reached the point of acceptance; in fact, concern is on the increase.

However, science can address the other two areas of concern, which is the purpose of this risk assessment. Two years ago the overall review committee was quite critical of the preliminary risk assessment. The combustion engineering panel, which I chaired, was at least as critical of the emissions assessment and accidental fire assessment portions of the draft document. We furnished a number of recommendations and suggestions that needed to be implemented in order to significantly improve the risk assessment.

On the basis of a general reading of the entire document, the US-EPA should be congratulated on what initially appears to be a very thoughtful, detailed, and extraordinarily well documented report. This is a thoroughly professional effort in which every possible effort has been made to be devoid of bias and emotion. I have reviewed the combustion emissions section of the report very closely and find it to be well organized, with the critical issues and assumptions well presented. At worst, the combustion section represents a summary of what we have learned about incinerator emissions in the last 15 years of research; and at best, provides a highly advanced method for assessing incinerator emissions.

I was also impressed by the faithfulness of the EPA in following the recommendations of the review committee. In many cases their efforts were even heroic. Some very difficult tasks were suggested by the combustion engineering panel that were accomplished and in some cases approved upon by the preparers of the report. Any criticism that I may have concerning the assessment of stack emissions is essentially nit-picking and I expect them to have very little impact on the risk assessment results.

Assessment of Stack Emissions Estimation Procedure

As expected, there is little adverse risk from the emissions of toxic metals even when conservative assumptions are made. The existing data base on metals is well defined, and modeling is effectively used to fill data gaps when appropriate. While kinetics may play a significant role in determining the speciation of metals in some cases, the thermodynamic modeling approach appears to be sufficient to minimize most concerns.

The organic emissions estimation procedure is generally quite rigorous and closely follows the recommendations of the panel. It is somewhat simplified from the more idealistic suggestions of combustion panel but, nevertheless, is the most complete and professionally executed assessment that I have seen. I have a few minor criticisms and questions of concern.

The correction factors applied to THC to calculate total organics is not well documented. I am aware that EPA gathered some data from a field test a few years ago. Surprisingly it is still not well recognized that total hydrocarbons (THC), as measured by a conventional analyzer, is not anywhere near the total organic content of the stack effluent. Analyzers really only measure C1 through C5 chemicals whereas the greatest <u>number</u> of chemicals emitted from an incinerator are polycyclic aromatic hydrocarbons (PAH) with more the five carbon atoms in their structure.

Although this point is recognized in the report, the total organic to THC correction factor is critical to calculating the unaccounted-for fraction of the stack emissions. How complete and reliable is EPA's data base for calculating this critical factor? How does this basis for calculating the uncharacterized fraction compare to calculation on a mass balance basis? It would be most useful if this information could be furnished to the committee at the meeting.

My second criticism is also somewhat of a question. The report states on page IV-8 that prorating of the emissions rate of the characterized emissions to account for the uncharacterized fraction was considered but not implemented because it would overestimate the carcinogenic impact and ignore the toxic (i.e., non-carcinogenic) impact. I do not understand why both can't be included. Furthermore, it is not clear why not including the uncharacterized fraction is better than trying to account for it with some associated error. I would like for this decision to be better explained to the committee.

My third concern is over the number of chemicals reported as analyzed-for but not detected in the stack during trial or test burns. Examination of table III-3 reveals that almost no PAHs were detected. I have never seen a combustion emissions test when there was not a large number of PAHs present. Was the detection limit for PAHs very high? Could the carbon injection system have eliminated PAHs entirely? If this is the case, then it is very impressive. I would like for this to be explained further with possibly more detail on the stack tests made available to the committee.

As a final point on the subject of stack emissions, the report makes a good point that it is the nature, as opposed to quantity, of the uncharacterized fraction of emissions that creates the most uncertainty in risk. How can we ever eliminate this concern? If just one of the uncharacterized chemicals has the toxicity of 2,3,7,8 TCDD, then the calculated risk would probably increase by orders of magnitude. As I see it, this is the <u>only</u> real issue about stack emissions. This is certainly not to say that the event is likely, but I am not sure that I can say it is unlikely. Can we arrive at a scientific basis for assessing this uncertainty?

Assessment of Accidental Fire Modeling Procedure
I also reviewed the sections relevant to accidental fires at the facility. At the last review, the combustion engineering panel recommended that this issue needed to be addressed in much greater detail, although the review of this section is now the responsibility of another panel.

I am disappointed in this portion of the report. The fire model itself is very crude. In addition, it only includes emissions of HCl and phosgene in the risk assessment. Considering the depth of assessment of the stack emissions, how can this data not be included in the fire emissions assessment? Instead, the approach relies on empirical data generated in 1952. It does not include the mix of chemical likely to occur at the facility or the type of by-products likely to be emitted.

I suspect that the fault lies less in the authors of this report than in the availability of appropriate fire models, especially those that include thermal decomposition properties of hazardous chemicals. Having studied hazardous waste incineration for the past 15 years, my greatest personal concern about living next to a facility would be exposure from such a fire. In spite of the peril that could be present from just one accident, I know of no effort to develop appropriate fire models. This is a serious deficiency in the scientific infra-structure that does not allow us to address a serious issue. We have also had the operating permit of one waste destruction facility in Ohio revoked based on inadequate fire modeling. I would feel deficient as an environmental scientist if this were allowed to happen again without the proper caution.

I suspect that the authors of the report have done the best they could with fire modeling using the available models. This risk assessment is not a research project, and they should not be expected to develop a new model as part of a risk assessment. However, the available tools must be improved before adequate risk assessments can be performed for this facility or any other facility.

Dr. Randy Seeker
Energy and Environmental Research Corporation
January 2, 1996

Preliminary Comments on Risk Assessments fro the WTI Hazardous Waste Incinerator Facility

Incinerator Stack Emissions

The following are preliminary comments focussed on the emission rates and chemical speciation used in the risk assessments. In general, I would conclude that the concerns that I have identified are (probably) not significant to the overall results of the risk assessment with the possible exception of selenium behavior as discussed below. Nonetheless there remains some uncertainties and issues that should be addressed prior to closure of the study.

Much of the stack emissions rates used in the risk assessments are based upon actual emissions. The WTI facility is one of the most extensively studied combustion facilities in the world. Nonetheless there are still some gaps in the emissions data that could not be (or have not yet been) obtained from direct emissions but rather were estimated by analogy to emissions of other species. There are two guiding principles that must be followed when using emissions data from one species to estimate the emissions of another: (1) use species with similar physical and chemical behavior and (2) use species with similar concentrations. While the former principle was generally followed, the latter was ignored and may lead to some additional uncertainties in the emissions rates that may or may not be significant to the overall risk assessment.

Metal Emission Rates

The most important example of this issue is the assumed behavior of selenium. No direct emissions measurements data are currently available from WTI on selenium. Selenium is correctly identified as a volatile metal that will volatilize at the kiln temperatures. The thermodynamics of selenium indicates that it is even more volatile than mercury under most conditions (mercury is more volatile when chlorine is present). Selenium was assumed to be captured in the scrubber with the same efficiency as SO_2 as measured in the trial burn. There was little support given for this assumption and I would questions its validity. While there are data in the literature that indicates selenium is chemically similar to sulfur and that it can be scrubbed with high efficiency in scrubbers, the use of SO_2 systems removal efficiency data from the trial burn is challenged due to the very different concentration levels of interest. Scrubbers are generally mass tranfer limited devices that work better at high concentrations than at low concentrations due to gas phase concentration driving the mass transfer into the liquid phase. The SO_2 concentration in the trial burn was over 1700 times higher than the expected average selenium; hence just based upon the concentration levels alone the selenium emission estimate is expected to be under estimated. The authors need to further justify the use of the trial burn sulfur data as an indicator for selenium and to address the impacts of concentration on the emissions of selenium. It is not clear without further analysis whether the underestimation is significant to the overall conclusions of the risk assessment. An examination of the selenium issue is recommended at least with a sensitivity analysis to determine if the risk assessment results are sensitive to larger selenium emissions levels. If this sensitivity analysis indicates that the results are sensitive to estimated selenium emissions then further emissions testing may be warranted. It is noteworthy that the SERA components of the study have indicated the importance of selenium at the permit standards.

The same issues are present but at much less degree with the approach used to estimate other metal emissions rates for which emissions data were not available (i.e., aluminum, barium, copper, nickel, silver, thallium and zinc). The grouping of metals used in the study used is somewhat inconsistent with other groupings proposed by the European Union and the EPA Office of Solid Waste regulatory development office. These other parties group metals as follows:

Volatile - Hg and Se
Semi volatile - Pb, Cd and Tl
Low volatile - Be, Ba, As, Ag, Ni, Cr, and Sb

The study as discussed in Appendix III-1 of chapter III, did a good job of examining of the behavior of other metals relative to one another and grouping metals with similar behaviors. Clearly aluminum would be low volatility as assumed in the WTI study. The volatility of copper is significantly impacted by chlorine and expected to exhibit both semi volatile and low volatile behavior depending on the level of chlorine present. The other metals are

Air Dispersion/Deposition Modeling and Accident Analysis

W.F. Dabberdt

Preliminary Comments on Atmospheric Dispersion Aspects of WTI Risk Assessment

- The current risk assessment is improved through the use of improved site-specific meteorological and emissions data.
- What percentile concentration is considered "high-end" and how does this differ from the central tendency value (is this value the median or the modal value)?
- Does the conclusion that "it is not anticipated that any individual in [the entire] population would develop cancer as a result of exposure to routine ... emissions" refer to central tendency or high-end exposure?
- Why are the permitted stack emissions limits so much larger (10**4) than the "expected" emission rates?
- What is the impact or effect, if any, of the current risk assessment on the permitability of the proposed second rotary kiln?
- How do the unused December 1994 incinerator performance test data compare with the post-July 1993 data actually used in the assessment?
- What is the significance, if any, of the OctaCDD estimated emission rate values being nearly an order of magnitude larger than the largest PCDD/PCDF values actually measured and "used in the WTI RA" -- as given in Table III-1?
- The percentage of calm conditions -- 22% -- actually determined to occur at the WTI site is a large value, and has significant implications for the type of dispersion model used in the RA. It is surprising that there is a large degree of consistency between the CALPUFF and ISC-COMPDEP values of peak one-hour, 24-hour, and annual average concentrations.
- The agreement between CALPUFF and ISC-COMPDEP values applies to stack-level emissions; were the two models compared for accident

analyses?

- Do the CALPUFF and ISC-COMPDEP models predict concentration-maxima at the same source-receptor distance and orientation, or are the peak values the same independent of receptor location?
- Did the authors consider wind shear as another source of uncertainty in the dispersion modeling; the report states the significance of valley channeling of the wind flow, but this observation is not discussed in the context of the model(s) performance.
- Why are on-site impacts not considered for the accident analyses?
- Why is it assumed that "most plausible accident scenarios" would affect only relatively small areas?
- It is not reasonable to assume that the most conservative, yet plausible, accident scenarios are among those that have occurred in the past 18 years at existing incineration facilities. While this is one approach, another should be based on a failure analysis of on-site storage and incineration facilities as well as failure of transport facilities. An historical analysis would not have projected many notable accidents, such as the Chernobyl, the metam spill in India, and the oleum spill in the San Francisco Bay Area.
- What is the waste storage volume at the facility, and how does it compare to the 20,000-gal maximum spill scenario? The larger value would represent a more conservative case.
- The ISC-COMPDEP model does not seem as appropriate to fire scenarios as does the CALPUFF model.
- The calm/inversion meteorological accident-scenario is a good one.
- "The probability of occurrence is ranked ... on the probability of the accident event, the probability of the meteorological conditions, and an estimated waste composition ..." Does this mean that the overall

probability of occurrence is the joint probability? If not, how is the probability actually estimated?

- The summary of the accident analyses is written in a way that appears predisposed toward demonstrating negligible risk.
- Why would "more extreme events, with potentially greater off-site consequence..." not "affect the overall conclusions of the Accident Analysis?"
- Why do all accidents where the severity of consequence is "catastrophic" have accompanying probabilities of occurrence that are "very unlikely?"

BACKGROUND

A risk assessment has been performed for the Waste Technologies Industries (WTI) Hazardous Waste Incinerator Facility located in East Liverpool, Ohio. The draft risk assessment is documented in seven volumes that were provided for review by letter dated November 14, 1995:

Volume I:	Executive Summary
Volume II:	Introduction
Volume III:	Facility Emissions
Volume IV:	Atmospheric Dispersion and Deposition Modeling of Emissions
Volume V:	Human Health Risk Assessment
Volume VI:	Screening Ecological Risk Assessment
Volume VII:	Accident Analysis

No electronic files were provided for this review.

I have performed a review of the WTI risk assessment based on the charge articulated to peer reviewers. This charge was expressed as a series of issues and questions, first of a general nature and then specific to the field of expertise of each reviewer, based on the reviewer's workgroup assignment. As a member of the Air Dispersion Modeling and Accident Analysis workgroup, I have focussed on the specific issues and questions for that group. My review consisted of a careful reading of Volumes I and II, a critical review of Volumes IV and VII, and a cursory review of the other volumes. My comments are given below following a re-statement of each of the specific issues.

GENERAL COMMENTS

General Issue # 1: ***Comment on the organization of the risk assessment document. Does the layout follow a logical format? Is the presentation of information in the document clear, concise and easy to follow?***

I was generally favorably impressed by the overall layout of the document. I found that it was relatively easy to find the answers to questions that came to mind while reading one part of the document, by looking in the Table of Contents of other volumes. I do have some comments on re-organizing some of the presentation (particularly Volume IV, Section IV) and other comments on strengthening the presentation that are presented below.

***General Issue # 2:* Does the executive summary accurately reflect the data and methodologies used and the conclusions derived in the risk assessment?**

The executive summary provides a reasonably accurate picture of the results of the dispersion and deposition modeling that are described in detail in Volume IV (some of my comments on this Volume, as reflected in more detail below, may be appropriate for summarizing in the executive summary), and also insofar as the numbers in Tables IV-3 and IV-4 appear to be accurate. Table IV-3 should be modified to indicate that maximum concentrations are annual averages (as opposed to short-term maxima such as 24-hr values). Also, Table IV-4 does not need to indicate that emission rates are in $g/m^2/s$ since no area sources were modeled. In the Human Health Risk Assessment (HHRA) section, Table V-4 provides a concise summary of the results of the analysis but I felt that, since this table represents the "bottom line" for the whole effort, some additional information should be presented in between Tables V-3 and V-4: for example, a breakdown of risk components from each of the exposure pathways for the worst-case pollutant. The interested reader would benefit from some more details.

***General Issue # 3:* Were the major recommendations of the 1993 peer review workshop for the risk assessment plan addressed?**

The major recommendations of the 1993 peer review workshop are listed in Volume II, page IV-2, and also in Volume IV, pages I-2 and I-3 (I did not review the workshop report itself). I believe that the documents that I reviewed addressed the major recommendations and reflected a genuine, dedicated effort to provide additional information for the overall project. Some specific comments that I have on aspects of the overall analysis, that should be considered prior to finalizing the risk assessment, are discussed below.

<u>General Issue # 4:</u> As with any risk assessment, there are always additional data and method development efforts that could be undertaken to reduce the level of uncertainty. However, are there any major data or methodological gaps that would preclude the use of this risk assessment for decision making? If so How should they be addressed?

I do have a number of comments that refer primarily to organization and presentation and a few things that may have to be looked at in some depth, however, pending a satisfactory resolution of these issues I do not believe that there are any major gaps that would preclude the use of this risk assessment for decision-making.

<u>General Issue # 5:</u> What long-term research would you recommend that could improve risk assessments of this type in the future?

I believe that research in the following areas would help improve risk assessments of this type in the future:

Given the critical nature of the deposition pathway, additional research on deposition approaches would be quite beneficial - particularly in terms of developing and testing wet deposition algorithms that more accurately reflect the physics of this removal process.

Non-steady state models have not been subject to rigorous testing, particularly in complex terrain areas. Additional testing and possibly development of CALPUFF or other models would help improve risk assessments of this type, particularly if the research is given the additional direction of improving modeling for accidental releases of short duration.

DISPERSION AND DEPOSITION MODELING COMMENTS

<u>Dispersion and Deposition Modeling Issue #1:</u> Since the 1993 peer review of the risk assessment plan, a number of efforts have been completed to reduce the uncertainty associated with the air dispersion and deposition modeling. These efforts include the collection of site-specific data for emission rates and meteorological conditions. Also, a wind tunnel study was conducted to evaluate the effects of the complex terrain surrounding the WTI facility. Does the risk assessment document adequately summarize these activities? Is the link between these data collection efforts, the air dispersion models, and the risk assessment clearly established?

The two questions posed here will be dealt with in reverse order. I have reviewed the meteorological data and wind tunnel issues in some detail, and in general I felt that the documentation of these efforts was reasonably thorough (although see the comments in response to issue no. 2 below, related to the presentation of sensitivity results). The link between these efforts and the overall risk assessment is established through the prediction of ambient concentration and deposition values. These concentration and deposition values are absolutely indispensable to the estimation of environmental concentrations and exposures which are the core of the risk assessment. The link between the additional data collection and the prediction of ambient concentration and deposition values, therefore, is the key to establishing the link that is referred to in the question. In that sense the link between additional data collection and the risk assessment has been clearly established.

Although emissions are also an important ingredient in the exposure assessment, I did not review the collection of the site-specific rates presented in Volume III carefully since I believe that others are doing so. The point in the risk assessment where concentration/deposition estimates and emission rates are combined and then utilized either for direct exposure pathways (inhalation) or indirect pathways (e.g. consumption of contaminated food) is found in Volume V (the HHRA), especially Chapters VI (environmental concentrations), VII (exposure doses) and VIII (risk characterization). Although I did not review Volume V in detail, the development of the risk characterization was laid out fairly clearly. One thing that would have been valuable in terms of a review but that I was not able to do because of time constraints would have been to trace a single chemical through the entire process from emissions estimation to concentration calculation, dose assessment, and risk characterization.

The question as to whether the data collection efforts were adequately summarized will be dealt with in detail for the meteorological data and the wind tunnel study (but not the site-specific emissions characterizations).

Meteorological data

The site-specific meteorological data collection effort included measurements taken at two on-site towers (10 and 30 meters high), and incorporated measurements taken at a 500-ft (152 meter) tower located approximately eight miles away at the Beaver Valley Power Station.

I have some reservations about the representativeness of the lower levels of the BVPSMT data to the area in the immediate vicinity of the WTI. My conclusion, from a modeler's perspective, after looking at Figures III-5 and III-6 (reproductions of topographic maps of the area surrounding WTI and BVPSMT) is that lower levels of both towers are likely to show different types of influences: the BVPSMT is located in a broader area of the valley, not too distant from a significant bend and widening of the river. Furthermore, comparing Figures III-3 (WTI 30-meter wind rose), III-8 (BVPSMT 10-meter wind rose), and III-10 (BVPSMT 45.7-meter wind rose), it does not appear to me as though the WTI 30-meter level "fits in" between the BVPSMT 10-meter and 45.7-meter levels.

Having said this, however, I believe that the use of the BVPSMT wind data in the WTI analysis does accomplish an important objective: namely, that since the 152-meter level allows for more cross-valley flow than the WTI 30-meter data, it probably adds a degree of realism and a degree of conservatism to the analysis - especially since the base complex terrain model (COMPLEX-I) is a model that is itself widely regarded as extremely conservative. The closest terrain with which the plume from the WTI stack would interact (or be predicted to interact based on COMPLEX-I) is of course cross-valley with respect to WTI, and utilizing meteorological data strictly from the on-site 30 meter tower would understate the frequency with which the plume is transported in this direction (the plume from the stack is buoyant and therefore rises considerably higher than the 45.7-meter physical stack height - see the discussion below).

I think that the summary of the meteorological data collection and usage should have a different focus than on making a case for representativeness of the BVPSMT, especially the lower levels; the focus should be on the use of the BVPSMT wind data to introduce a degree of realism and conservatism to the analysis. The degree to which the BVPSMT data is appropriate is also a function of what parameter is being used - temperature profiles are probably more widely applicable to different parts of the valley than speed and direction profiles.

The following are some additional comments on the summary of the meteorological data and its use in concentration and deposition modeling.

- The discussion in Section III.C was a little confusing in terms of what time period of was covered by the meteorological data used in the modeling. The WTI 30-meter data were analyzed for the time period April 1992 to March 1993. BVPSMT data were analyzed for the time periods 1986-1990 and 1992. Evidently the 4/92 to 3/93 time period was used; were BVPSMT data available (hopefully) for the same time period? Section III.C.4 should clearly state what time period was actually used. Given that the data set used in the modeling was a hybrid consisting of data from two locations, some further analysis of the actual profiles used (e.g., scatter plots of speed or direction values from one level to the next, stability-dependent wind roses) would I think be very informative. Since the WTI stack is 45.7 meters high, the actual use of the meteorological profile in the modeling means that effectively the 30-meter data collected at the WTI site was not used at all for the stack modeling.

- A specific analysis of plume height should be included to better illustrate the elevations typical of plume transport (particularly stable atmospheric conditions). A table of plume heights is presented below for different meteorological conditions (stable conditions assumed, plume heights as calculated by COMPLEX-I, wind speed shown assumed to be at stack top). Since measured temperature gradients are used in the analysis, the plume elevation analysis should reflect observed values (by using average values or possibly ranges).

Potential Temperature Gradient, °C/100 m	*Wind Speed @ stack top (m/s)*	*Plume Rise (meters)*	*Plume Elevation (ft msl)*
1.0	1.0	115.5	1,225
1.0	2.5	85.2	1,126
1.0	5.0	67.5	1,067
1.0	8.0	57.8	1,036
2.0	1.0	91.7	1,147
2.0	2.5	67.6	1,068
2.0	5.0	53.6	1,022
2.0	8.0	45.9	997
3.5	1.0	76.1	1,096
3.5	2.5	56.1	1,030
3.5	5.0	44.5	992
Note: default PTG for E stability is 2.0 °C/100 m; for F stability, 3.5 °C/100m			

The plume elevations as illustrated in this table reveal that under most conditions the plume from the WTI stack is out of the immediate influence of nearby valley walls (approx. 1000 ft msl) and in a "transition" zone between in-valley and out-of valley flow. This provides more support, I believe, for looking to other sources of "out-of-valley" wind data, such as the BVPSMT.

- Using measured temperature gradients for plume height calculations is a feature of many advanced models (including CTDMPLUS and the new AERMOD model). The use of measured gradients with simpler models such as COMPLEX-I (which is incorporated into ISC-COMPDEP) introduces some degree of realism but diminishes the confidence with which the claim can be made that the model is conservative for stable case, complex terrain impacts. Some comparison should be made between measured gradients and the defaults built into regulatory models to have some means of judging the effect of using the measured values. If any "minimum" values for stable conditions were used in the processing, the values should be identified.

<u>Wind tunnel study</u>

The wind tunnel modeling study presented in Volume IV, Appendix IV-6, did not fully resolve all of the complex technical issues associated with the potential for terrain-induced downwash at the WTI facility. The authors acknowledge this, but state that nonetheless the broad picture is understood well enough to utilize the wind tunnel concentration results in a risk assessment of the WTI site. All three reviewers of the wind tunnel study (R. Hosker, M. Schatzmann, and R. Britter) generally concur that adequate experimental methods were employed in the study and that the conclusions are sound, but also contend that some areas of uncertainty remain. The primary areas of uncertainty (based on my reading of the comments, and highly paraphrased) are first, the issue of <u>combined</u> effects of terrain and building downwash; second, the fact that the study did not address convective or nocturnal, stable cases; and finally, the issue of marginally separating flow in the upwind terrain. Although I believe that none of these issues would invalidate the way in which wind tunnel results were analyzed and utilized, the summary of the wind tunnel work that is contained in Section IV.B.6 would benefit from a discussion of these issues and why they do not invalidate the stated conclusions.

The "bottom line" in the analysis of terrain downwash is that the inherently conservative nature of the ISC-COMPDEP modeling produces peak concentrations for similar meteorological conditions that are much higher than peak concentrations measured in the wind tunnel, and therefore the model does not need to be changed to specifically account for the terrain downwash phenomenon. Although I fully agree with this conclusion (as a practical although perhaps not scientifically satisfying approach), I think that the discussion in Section IV.B.6 would leave fewer questions unanswered if the following issues were addressed (as a minor note, the reference to Figures IV-7 through IV-9 in this section should be changed to Figures IV-11 through IV-13):

- State that in the ISC-COMPDEP modeling conducted for comparison to wind tunnel results, neutral atmospheric conditions (i.e. stability D) were utilized for ISC-COMPDEP, if this is the case (if not, explain why).

- Provide some reference to maximum hourly concentrations over all conditions, which would further help understand the context of the concentrations being discussed in the overall picture.

- Since in a risk assessment the spatial distribution of concentration patterns, as well as long-term averages, are generally more relevant than the value of hourly maximum concentrations, provide some simple means of demonstrating that model-predicted spatial patterns are not greatly different than tunnel measured spatial patterns for relevant meteorological conditions; also, provide a qualitative discussions of the frequency of the conditions depicted (specific to speed and direction; reference can be made to wind rose patterns presented elsewhere).

- Provide information related to the maximum concentrations separated by model algorithm (i.e., the ISC part and the COMPLEX-I part of the model).

- Provide a discussion (only a very brief one is necessary) that addresses the points made by the reviewers of the wind tunnel study. These can be as simple as: stable and convective conditions were addressed by ISC-COMPDEP; building downwash was simulated by ISC-COMPDEP and, since the terrain-distorted wind field produces compensating effects (lower wind speeds, descending mean streamlines), these effects are not likely to exacerbate building downwash impacts; and the recirculating region was acknowledged by the authors of the study but deemed to be insignificant in the broad picture of wind tunnel results - especially since the wind tunnel results were not used directly, but compared to model results and found to be not critical.

Most of these points only need to be addressed with respect to the actual stack height at the facility (i.e. 45.7 meters) and not the other stack heights that were examined in the tunnel.

Dispersion and Deposition Modeling Issue # 2: ***The results of 12 sets of sensitivity tests indicate that geophysical variables (e.g., terrain) are more likely to affect dispersion and deposition than emission variables (e.g., stack temperature). Were these sensitivity analyses adequate? Comment on the conclusions reached. To further examine the effect of geophysical variables, wind tunnel testing was conducted to model the terrain induced flow effects expected near WTI. It was concluded that changes in peak concentrations attributed to these effects are relatively minor and that the ISC-COMPDEP model is sufficiently conservative. Comment on this conclusion. Have these analyses helped to characterize and/or reduce the uncertainty in the air dispersion modeling associated with the complex terrain surrounding WTI.***

To answer these questions, I reviewed Section IV which includes a section on sensitivity simulations (Section IV.B) and a section on uncertainty (IV.D). I am not sure where the reference to 12 sets of sensitivity tests comes from. 13 sets of results are presented in Table IV-2, but three of them are base case runs and there are really only four types of tests listed in the table.

My overall comment on the sensitivity analyses presented in Section IV is that they provided useful and valuable information for characterizing the uncertainty of modeling results. I would recommend adding only one additional test, namely, the effect on concentration and deposition values of using default potential temperature gradients instead of measured values from BVPSMT (see the discussion above related to temperature profiles). I would also recommend performing the sensitivity tests that were run with a previous version of ISC-COMPDEP with the most recent version (see the discussion below). I also believe that Section IV could be strengthened by some re-organization of the section. I found it difficult to sort out base case results (i.e., what values were actually used in the exposure assessment?), sensitivity runs with the newer vs. older versions of ISC-COMPDEP, and sensitivity analyses that involved models or approaches other than ISC-COMPDEP. My recommendations are as follows:

- Confine the discussion of base case results to one section and one table, and treat all of the sensitivity analyses separately. Table IV-1 presents base case results and the results of several sensitivity runs that are first mentioned in Section IV.A but not discussed in detail until Section IV.B. It should be made clear that the values presented for the base case are those that were carried forward for use in the exposure assessment ad risk characterization sections of the risk assessment, after consideration of sensitivity runs and incorporating any modifications performed as a result of the sensitivity runs. Since the concentration and deposition values contained in Table IV-1 for the base case provide a critical link in the risk assessment, I recommend that the area-specific concentration and deposition values presented in Tables VII-14 through VII-17 of Volume V be presented in Volume IV, in Table IV-1. Since the values in Tables VII-14, etc. were actually used in calculating risk, presenting those values in Volume IV would more clearly establish the link between the modeling and the risk assessment.

- Present the results of the sensitivity runs in one section and one table. This would include the values currently in Table IV-1, but it should also include the results of sensitivity analyses selected from Table IV-2 and re-run with the most current version of ISC-COMPDEP. I believe that the four sets of analyses presented in Table IV-2 (i.e., "mass < 0.4 μm at 0.03 μm", "vapor modeled as 0.03 μm particle", "no depletion", and "receptor-specific land use") are worth repeating with the latest ISC-COMPDEP to eliminate any question that the sensitivity is affected by different model versions. Table IV-2 could be presented as-is for some historical perspective, but the "insights" provided would be more valuable if re-run with the latest version of the model - especially since the location of the maximum impacts changes significantly between versions of ISC-COMPDEP.

- Present the calm/fumigation and terrain downwash discussions in separate sub-sections.

- The section on uncertainty could remain pretty much as-is.

Other comments related to the questions posed and to sensitivity:

- I fully support the performance of the wind tunnel study as a means of examining the possible influence of terrain. The results were somewhat surprising, due apparently to the shielding effect of the terrain allowing for lower wind speeds at stack top and increased plume rise, but the conclusion that the model should not be modified is well supported (my specific comments on the terrain downwash study and the presentation of results are found in the previous section). Terrain downwash, it should be noted, is totally unrelated to stable-case plume impacts on elevated terrain that are associated with the highest concentration predictions.

- One of the sensitivity tests - for receptor-specific land use types - appears to give results that would suggest that this approach be used in the "base case" modeling.

<u>Dispersion and Deposition Modeling Issue # 3:</u> The ISC-COMPDEP model does not allow for non-steady state conditions such as calm winds and strong

temperature inversions. Therefore, CALPUFF was used to estimate air dispersion and deposition under these conditions. However, CALPUFF gave similar peak, 24-hour, and annual average concentrations as ISC-COMPDEP. Comment on the adequacy of this analysis. Comment on the conclusions reached. Has this analysis helped to characterize and/or reduce the uncertainty in the air dispersion modeling associated with non-steady state meteorological conditions?

The issue of calm winds (associated with temperature inversions) and fumigation events is a valid one to consider in the WTI setting. The CALPUFF model, with its puff and "slug" sampling functions that allow for near-source assessments, is quite appropriate for performing the analysis. The usefulness of the analysis performed here, however, is severely limited based on the following considerations:

- Limiting the analysis to receptors less than stack top: it would seem to me that interaction with terrain could also be a concern under calm conditions. CALPUFF has the capability to handle complex terrain, and I am not sure why complex terrain receptors were not modeled for this sensitivity analysis.

- It is not surprising that CALPUFF produced similar values to the ISC part of ISC-COMPDEP. Minimum wind speeds were apparently set equal to 1.0 meter/second, and meteorological conditions representative of one hour were used in the analysis. Since a plume can travel 3600 meters in one hour at 1 m/s, the features of CALPUFF that make it useful for assessing low wind-speed conditions are not fully realized. Since the BVPSMT data is available in 15-minute increments and the wind speed threshold 0.27 m/s, it may be more valuable to use BVPSMT data in this assessment.

I recognize that the desire was to present a limited analysis to examine these phenomena, but I think that the analysis was so limited as to not provide significant additional information. One reason that further modeling was not performed with CALPUFF was that data limitations prevented the full benefits of CALPUFF to be realized. An analysis that could provide some of the additional insights possible with CALPUFF would be to create a "synthesized" stagnation event, possibly based on examining the period of record of the BVPSMT measurements. The event could span several days and the necessary vertical and horizontal data synthesized to represent expected wind fields (or rather lack of winds) in such an episode. The maximum hourly and 24-hr concentrations predicted for this event could be compared to concentrations from ISC-COMPDEP, and provide a more meaningful insight into concentration predictions for stagnation conditions. One thing that could be evaluated as an alternative or as a supplement to this analysis is whether the conservativeness of the terrain interaction used in the COMPLEX-I part of ISC-COMPDEP is sufficient to "cover" these phenomena, similar to the argument made for the issue of terrain downwash.

***Dispersion and Deposition Modeling Issue # 4:* Atmospheric dispersion modeling was used to estimate air concentrations of hazardous chemicals for the accident analysis. The SLAB model was used for vapor releases from spills and the mixing of incompatible wastes. ISC-COMPDEP was used for releases associated with fires. Comment on the selection of the models and inputs. Are they appropriate selections?**

(Please see my comments in the accident analysis section)

***Dispersion and Deposition Modeling Issue # 5:* Overall, have adequate sensitivity tests been conducted to demonstrate the magnitude of variation in concentrations and deposition estimates with model inputs?**

Overall, I believe that adequate sensitivity tests have been conducted. My previous comments contain recommendations for a small number of additional tests and different ways of discussing and presenting results that can be considered for strengthening the presentation of the results of the dispersion and deposition modeling.

COMMENTS ON THE ACCIDENT ANALYSIS

Please note that my comments on the accident analysis are based on less actual experience with these issues than with the issues related to modeling. For the most part, my review consisted of a careful reading of Volume VII and a response to the questions posed based on whether the information presented was logical and credible.

Accident Analysis Issue # 1: ***The WTI accident assessment selected five scenarios for quantitative evaluation that were considered to be of primary concern. The scenarios are an on-site spill, an on-site fire, an on-site mixing of incompatible waste, an off-site spill, and an off-site spill and fire. Please comment on the selection of these scenarios. Were any significant scenarios missed?***

The selection process described in Volume VII, Section II appears to be quite thorough and convincing in the logic of selecting both conservative and typical versions of each scenario. Considering the type of facility and delivery modes that exist for the WTI incinerator, I do not believe that any significant scenarios were missed.

Accident Analysis Issue # 2: ***Specific chemicals were selected to evaluate each scenario. Please comment on the selections. Would other chemicals have been more appropriate?***

The selection of chemicals for quantitative analysis based on the five identified scenarios appears to have followed an appropriate screening and selection process, as described in Section III of Volume VII, supplemented by the rankings documented in Appendix VII-2.

Accident Analysis Issue # 3: ***Chemical specific release rates are calculated for each scenario. Please comment on the procedures used to estimate the release rates. Was an appropriate approach used?***

The procedures used to calculate release rates, summarized in Chapter IV and explained in detail in Appendix III-3, appear to be reasonable. My only specific comment in terms of the parameters used to calculate the release rate for spills is that an average ambient temperature of 68°F will understate emissions on hot summer days (this is acknowledged in the write-up); a higher temperature may be more appropriate for estimating worst-case emissions.

Accident Analysis Issue # 4: ***Atmospheric dispersion modeling was used to estimate air concentrations of hazardous chemicals. Specifically, the SLAB model was used for vapor releases from spills and the mixing of incompatible wastes. ISC-COMPDEP was used for releases associated with fires. Comment on***

the selection of the models and inputs. Are they appropriate selections? Should other models or inputs have been used?

The SLAB and ISC-COMPDEP are appropriate models for the scenarios analyzed. Modeling procedures and results are summarized in Chapter V and discussed in detail in Appendix VII-4. Although it is not possible to provide a detailed review of the inputs for all runs for both models, my review of selected inputs revealed that for the most part the models and inputs were configured appropriately. The following comments are a result of this abbreviated review (note that the first comment could change the results of the on-site fire analysis):

- For the on-site fire analysis using "conservative" screening meteorology (i.e. the 54 conditions used widely for screening analyses) the wind direction was set to 270 degrees - i.e. wind from the west that will transport a plume to the east. Receptors were set up in a line with a y-coordinate of 0.0 and an x-coordinate ranging from 100 meters to 50 km. For a source located at (0.0, 0.0) this set-up will identify plume centerline concentrations at the stated downwind distances. The fire "source", however, was located at (186, 105) which means that the fire plume completely misses the first few receptors (see Appendix VII-4, Attachment 4.A). I made an independent model run with which I reproduced (approximately) the results for the on-site fire in Att. 4.A, and then re-ran the model using coordinates (0.0, 0.0) for the fire source. The maximum concentrations nearly doubled, with the maximum occurring at 200 meters from the source. I believe that locating the source at (0.0, 0.0) is the right approach, and that the modeling should be modified to reflect this approach. I do not believe that this change will affect the bottom line, since ISC-COMPDEP with real meteorology predicted results that are in the range of what would be predicted with the correct source location.

- This is a minor point, but one that can cause confusion - the model run titles in Attachment 4.A and 4.B do not reflect the on-site and off-site fire scenarios as they should (the 4.A title refers to a storage tank rupture, and 4.B refers to a truck accident).

- The building dimensions used in 4.A (conservative meteorology) used a minimum building width, at least according to the dimensions identified in 4.B. This is probably all right, since the height is still less than this width, but a few words on why the minimum was selected should appear in the write-up.

- In Table V-5, phosgene concentrations at 100 meters are shown as 3 ppm. This can't be right and should be corrected.

- Presentation of concentrations is given in different units in different places - g/m^3 and ppm. The different units make it difficult to cross-check values from one table to the next; either both units should be presented, or one set of units should be used consistently.

- As with the analysis of stack impacts, a table showing plume heights for the fire scenarios would be helpful in terms of assessing whether the source characterizations appear realistic.

The expanded CALPUFF analysis is performed (as discussed above),

<u>Accident Analysis Issue # 5:</u> Please comment on the assessment's conclusions on the severity of consequences and probability of occurrence. Has the report correctly categorized the severity of the consequences of the different accident scenarios? Has the assessment adequately justified the reported probability of occurrence of each of the accident events?

On balance, the severity of consequences and the probability of occurrence for the scenarios analyzed that are presented in Chapter VI appear to be well supported (however, see my comment on Issue 7 below). Severity of consequences may have to be re-visited based on the comments regarding source placement (see the previous comment).

<u>Accident Analysis Issue # 6:</u> ***Key assumptions were made in the identification of accident scenarios and the description of the conservative and typical events. Included were a description of the magnitude of the effect of the assumptions and direction of the effect. Please comment on the assumptions. Are they justified? Are the descriptions of the magnitude and directions of the effects correct? Has the accident assessment adequately confronted the uncertainties involved in doing this type of analysis? If not, what else should be done?***

I believe that the key assumptions discussion and tables presented in each chapter for the most part identify the magnitudes and direction of the effects correctly. Time constraints prevent a detailed discussion of each instance where I would have given a slightly different estimate for the magnitude of effects, although time at the workshop should be devoted to this issue.

<u>Accident Analysis Issue # 7:</u> ***Comment on the appropriateness of using IDLH values for characterizing the severity of consequences in the accident analysis. Comment on the appropriateness of using 10 X LOC for chemicals for which IDLH values have not been established.***

I believe that it is appropriate to consider some level of impact lower than the IDLH to further qualify the severity of consequences. An event that produces an impact less than both the LOC and IDLH is in my mind considerably different than an event that produces an impact nine times the LOC but less than the IDLH. For the present analysis, the only chemicals for which there is a large difference between the two are HCl and phosgene (see Table VII-5 - factor of 5 for HCl, factor of 10 for phosgene). The additional information presented in Chapter VII related to distances to the LOC should, in my opinion, be presented directly in the analysis of severity of consequences. Since the information is available and is presented in the report, its consideration directly in the determination of severity of consequences (as long as the significance of the IDLH and LOC thresholds is clearly spelled out) would seem to me to make sense.

<u>Accident Analysis Issue # 8:</u> ***In the accident analysis, IDLH (or 10 X LOC) values***

were used to determine the downwind distances over which adverse human health effects might occur. To evaluate the uncertainty introduced by using the IDLH, a sensitivity analysis was conducted where these distances were recalculated using the LOC (a more stringent health criteria). Other sources of uncertainty that are identified in the accident analysis include concentration averaging times, chemical concentrations, emission rates, and meteorological conditions. For most of these parameters it is stated that conservative assumptions were sued to avoid underestimating risks. Have the uncertainties inherent in the accident analysis been adequately characterized? For those parameters where sensitivity analyses were not conducted, is the conclusion that conservative assumptions have avoided underestimation valid?

I believe that the accident analysis has been conducted with appropriately conservative assumptions and inputs (I feel more confident about this conclusion as it relates to the modeling than to the emissions estimates, with which I have less experience). As stated in the previous comment, I believe that the LOC values should be considered directly in the severity of consequences presentation.

ADDITIONAL COMMENTS (NOT SPECIFICALLY SOLICITED)

- In Volume V (HHRA), the presentation of deposition values does not appear to be consistent with values presented in Volume IV (modeling). To illustrate: Volume IV, max concentration and deposition values are reported as 0.9111 µg/m^3-g/s and 0.3052 g/m^2/yr-g/s (surface area distribution); Volume V, Table VII-14, E1 subarea, identifies a concentration of 0.91 µg/m^3-g/s (consistent with Volume IV) and a depostion value of 0.025 (wet) + 0.0052 (dry) = 0.0302 g/m^2/yr-g/s. The deposition value appears to be about one-tenth of the value reported in Volume IV. Unless I am missing something, one or the other is right but they both can't be right.

- In Volume III (emissions characterization) emissions from the ash unloading operation are captured and vented to a baghouse. The only emissions quantified are those emitted from the baghouse itself. This assumes that 100% of the emissions are captured. If this is the case, then it should be stated (what is stated is that there is a "fraction escaping capture"). If some emissions are anticipated to escape capture, then those emissions should be quantified and modeled or, if they are inconsequential, a statement should be made that they are.

- In Volume V (HHRA), Table VIII-3 compares facility impacts of "critera" pollutants to the National Ambient Air Quality Standards (NAAQS). The comparisons would be more meaningful if each pollutant's "significant impact level" was also identified on this table, since a true comparison to the NAAQS should include total concentrations (i.e., due to all sources and assumed background).

- In Volume IV (modeling) on page IV-9 the reference to Figure IV-1 should be changed to Figure IV-5, and subsequent references to figures should be incremented by 1.

A Review of:

RISK ASSESSMENT FOR THE WASTE TECHNOLOGIES INDUSTRIES [WTI]
HAZARDOUS WASTE INCINERATOR FACILITY [EAST LIVERPOOL, OHIO]

EPA Region V
with A.T. Kearney, Inc. Chicago IL
Draft, Dec. 1995

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December 20, 1995

Phase I of an EPA review process on potential health and environmental effects that may be associated with a high temperature toxic waste incinerator presently operating in eastern Ohio [Dec. 1993] included recommendations that Phase II should consider potential effects of accidents and of plume downwash, that non-steady state pollutant dispersion models should be exercised with improved meteorological data, and that attention should be paid to wet and dry removal processes.

The present Phase II draft diligently touches all these bases. The draft's layout is logical and clear. It is not concise, nor should it be. With mild reservations, I judge the executive summary of volumes I-IV reasonably represents the data, procedures, and conclusions described in the bulk of this draft. There are methodological gaps and presentational shortcomings that diminish the usefulness of this risk assessment for wise decision making:

1. WTI does not live alone, and the permitting process has to start at the margin, not from zero. Emissions from local housing and valley industries should be accounted also, especially from wood smoke, that asphalt plant, and the refinery. What is the history of air-quality in this valley? By what increment is WTI expected to degrade this quality?

2. The draft assessment well describes exercises with the The COMPDEP dispersion model for annual averages [Vol IV, IV-28]. Cursory mention is made of exercises to estimate largest 1-hr averages with the CALPUFF and INPUFF models [Vol I, IV-4]. These appear to be summarized only in a single two-line table [Table IV-3, Vol IV, IV-26], without supporting information on the assumptions that produced those numbers.

Most of the COMPDEP modeling appears .. sensibly .. to have been conducted with standard 1 gm/s sources. Figures IV-1 through IV-4, and most of the figures IV-4-1 through IV-4-45 appear to be presented as relative concentrations and depositions, but it is not clear to me whether all of these figures are so: their captions should be more explicit. At any event, converting relative numbers for each tracer requires absolute emission rates, which are separately tabulated for many tracers in tables III-1 through III-5. It would be useful if this were made clearer to the reader by bringing these disparate pieces together.

I suggest expanding the tables in the executive summary to include additional columns showing both the highest predicted (annually averaged) concentrations and depositions for each tracer .. mostly from COMPDEP .. and highest 1-hr concentrations .. mostly, I presume, from CALPUFF.

I remark as an aside that in a valley 2 km wide, under a 100 meter impermeable inversion with 0.5 meter winds along the valley axis, a 1 gm/s source would be associated with a steady-state concentration of 10 microgm/m^3. This not implausible case is about 100 times higher than the 0.1 microgm/m^3 isopleth of figure IV-4-2, which reports the annually averaged relative concentrations attributable by COMPDEP to the WTI stack [with the same 1 gm/s source intensity] in an ellipse of about 5 x 15 km.

3. I judge the estimate of "one emergency incident involving hazardous waste release ... for every 25 or 30 years of operation" [vol I, VII-2] to be inconsistent with the two hazardous release incidents already reported at the WTI site on December 1993 and October 1994 [Vol III, V-12], and the "frequent occurrence of kiln overpressures" [Vol III, V-13].

This is "Challenger Optimism".

4. The summary asserts that among metals "the risks are highest for thallium, selenium, and nickel" [Vol I, I-8]. Table III-4 [Vol I, III-14] lists mercury emissions of 0.0014 g/s [44 kg/yr], higher on a weight basis than either selenium or thallium. All these metals are seriously toxic.

5. The emissions of nitrogen oxides are estimated at 2.4 g/s [Table III-5, Vol I, III-5]. This converts to a non-trivial increment of about 16 ppb(v) NO2 in a well-mixed plume 1 km wide by 100 meters high, at a wind velocity of 1 m/s. Other NOx sources are expected in the valley from cars, a refinery, and an asphalt plant. Conditions occur when O3 production at rural sites is NOx limited at NO2 levels below 20 ppb, with O3 at or above Federal primary standard of 120 ppb [Cardelino and Chameides 1990; Chameides et al, 1992]. It is likely that the NOx increment from WTI will contribute with other sources towards occasional exceedances of the Federal O3 standard.

6. A critique of health effects to be expected from WTI is beyond my central competence. It is my understanding, however, that both chronic and acute respiratory effects of particulate inhalation on human health have been demonstrated at low thresholds. [Pierson and Koenig, 1992; Koenig et al, 1994; Larson and Koenig, 1994] I judge the draft discussion of non-cancer effects [Vol V, VIII] to be both inadequate and excessively compressed into an obscuring "Hazard Index". What is closer to what we really want to know is the expected increment of asthmatic distress, especially in children and the elderly.

This is what hit Donora.

7. In my judgment, the ambient-air concentration estimates through CALPUFF and ISC-COMPDEP are optimistic under the rare .. but not implausible case .. of a strong, Donora-like inversion, with stagnant winds.

In my judgment, it is unreasonable to try to control WTI emissions down to this really worst case: instead, provisions should be made to shut down all industrial sources, and private woodsmoke emissions, when the air is severely stagnant for extended periods. Are adequate, continuous, and calibrated air-quality monitors, an alerting system, conservative protocols, and an authoritative regulatory machinery in place to do this?

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APPENDIX
Review of Phase II WTI Risk Assessment

Halstead Harrison
Dec. 22, 1995

In sections 2 and 7 of my review of the draft Phase II WTI risk assessment report I express doubts that air-quality simulations are sufficiently conservative for short term averages and with stagnant winds. It is always unattractive to state unsupported doubts in the face of what has clearly been considerable effort by competent people. To support my judgment I have therefore undertaken a supplementary simulation of pollutant dispersion in a confined river valley.

For this task I have used WPUFF, a Lagrangian-puff dispersion model that is similar in broad outline to CALPUFF, though the two models differ in assumptions about wind algorithms and diffusivities.

Lacking a convenient data base for the topographic relief in the Ohio river valley, I have adopted a generic valley [actually a section of the Columbia River, a bit east of Deshutes], illustrated in figure 1. The horizontal dimensions of the modeled domain are 10 x 15 km. The valley is about 2-4 km wide, which is comparable to the site near Liverpool, and the highest point of the adjacent rim is about 500 meters above the valley floor. This is higher than at Liverpool, but the difference is not relevant as with stable air at low wind speeds the puffs may not climb above a few tens of meters. The simulations were bounded by an inversion lid at 150 meters.

I have simulated three successive days with an assumed wind-speed distribution illustrated in figure 2. The mean speed was 0.66 m/s, the mode was 0.50 m/s, and in no period was the wind speed less than 0.10 m/s. This distribution resembles stagnation conditions that are observed in Seattle several days each year.

"Steering wind" directions were assumed as quasi-random, clustered about the valley axis with successive 12-hr periods predominantly up- and down-valley. Trajectories are biased parallel to the valley walls by a mass-conserving algorithm. Figure 3 illustrates the vertical and horizontal diffusivities, which were parameterized as proportional to wind speeds and time-of-day. These curves derive from angular variances of observed wind-directions and from vertical temperature gradients measured in the Puyallup river basin in "class D" stabilities. In severe winter stagnations [classes E and F] the afternoon maxima of figure 3 are largely suppressed.

Figures 4 and 5 illustrate surface concentrations averaged over the final 24 hours of 3-day simulations, with the same winds. Each was for a 1 gm/s reference source at the base of the northern wall of figure 1, 7.5 km from the western edge [just hidden in that figure by the perspective overlap from the southern valley wall.] The simulations emitted puffs at one minute intervals. The model advects the puffs about the valley, with diffusive dilution. At every time step surface concentrations were averaged into 250 x 250 meter grids. At the last step the resulting concentration fields were smoothed through a binomial filter with a spatial coherance length of 1 km.

The two simulations differ only in their source's heights above the local surface. In figure 4 this height is 80 meters [46 m stack + plume rise]; in figure 5 it is 10 meters, to approximate fugitive emissions.

Note in figure 4 that the highest 24-hr surface concentration associated with the 1 gm/s reference stack source is 3 micrograms/m^3 [3047 ng/m^3], which is higher by a factor of 6-15 than the annual averages reported by COMPDEP for the WTI stack [figure IV-1, vol IV, IV-28], but comparable to the 4.48 micrograms/m^3 listed as a 24-hr average by CALPUFF in Table IV-3 [vol IV, V-26]. I am not certain how to interpret this last number, however, as the draft assessment document does not identify it as stack or fugitive, or, indeed, whether it is an absolute number, or relative to a 1 gm/s emission source. [I take it as likely to be "stack" and "relative".]

Note in figure 5 that WPUFF estimates the highest 24-hr surface concentrations associated with a 1 g/s fugitive emission source to be 70 micrograms/m^3, a factor of 230 higher that with the same emissions from the stack.

The present simulations do NOT approximate a really severe stagnation, as might perhaps occur once per decade. The wind-speed distribution of figure 2 is typical of several events per year in Seattle weather. The stack simulations of figure 4 are likely low owing to the neglect of coherent vertical mixing processes from downwash, from eddies produced by the complex terrain, and from valley oscillations or "rolls". The simulations of figure 5 are likely low owing to overestimates of afternoon ventilations during very stagnant air, in figure 3. Both simulations are certainly low owing to spatial averaging necessary to reduce the "shot" noise associated with finite puff numbers, some of residue of which can be seen to remain in figure 4. Resolving these reservations is beyond the resources of this present brief effort. As a guess .. and emphasizing that it is only a guess .. an additional factor of 3-5 might be applied to both figures to estimate highest 24-hr averages from 1 gm/s sources, to be expected once a decade.

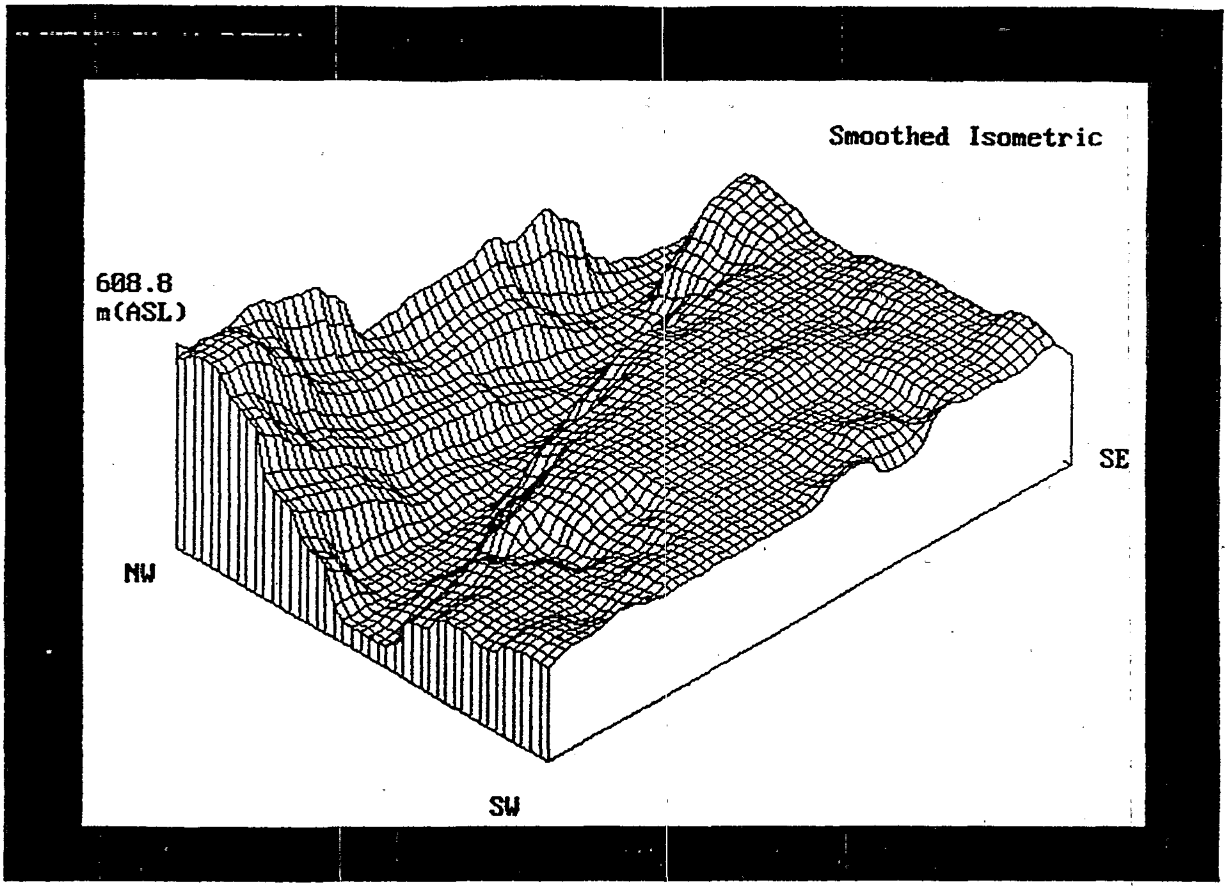

Figure 1

A generic river valley.
Horizontal dimensions are 10 x 15 km.
The vertical scale is exaggerated.

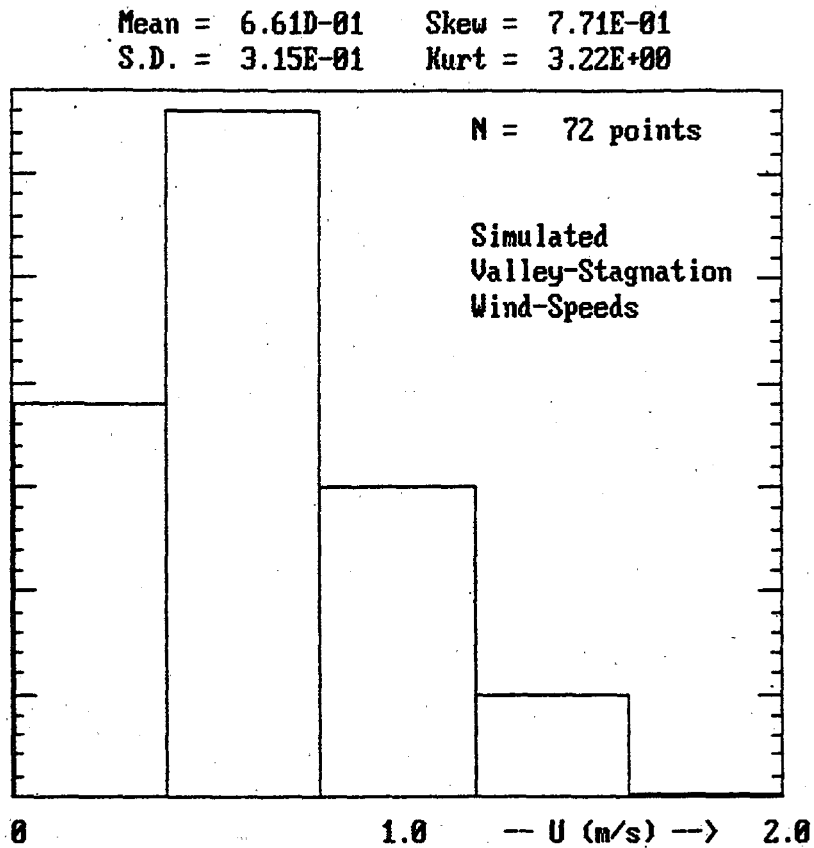

Figure 2

Histogram of wind speeds for a 3-day stagnation, expected once or more per year.

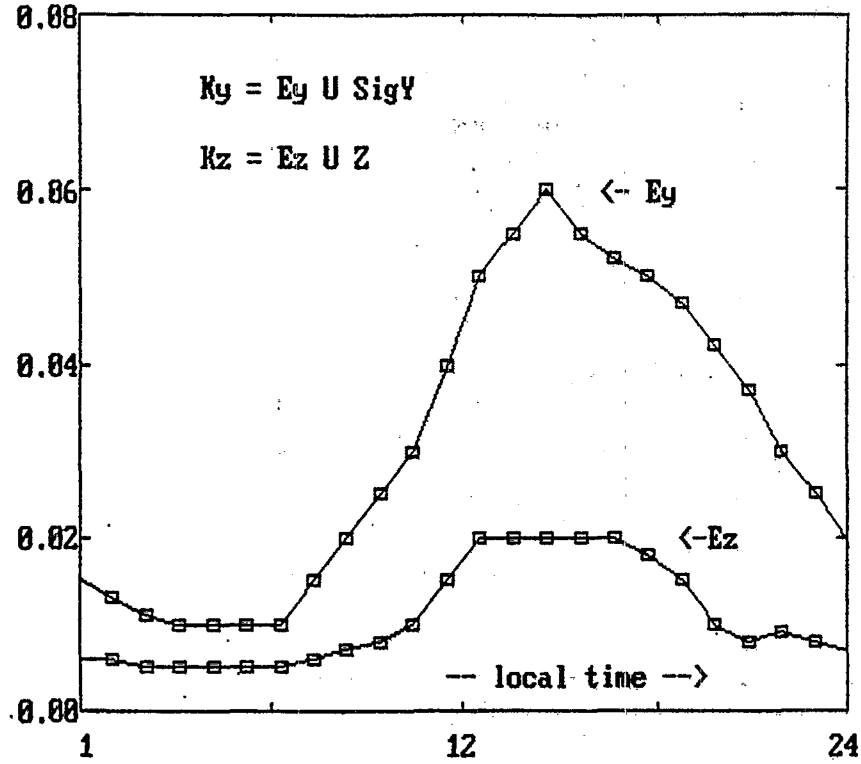

Figure 3

Horizontal [Ey] and vertical [Ez] entrainment coefficiencts, measured with "Class D" stability in the Puyallup river valley.

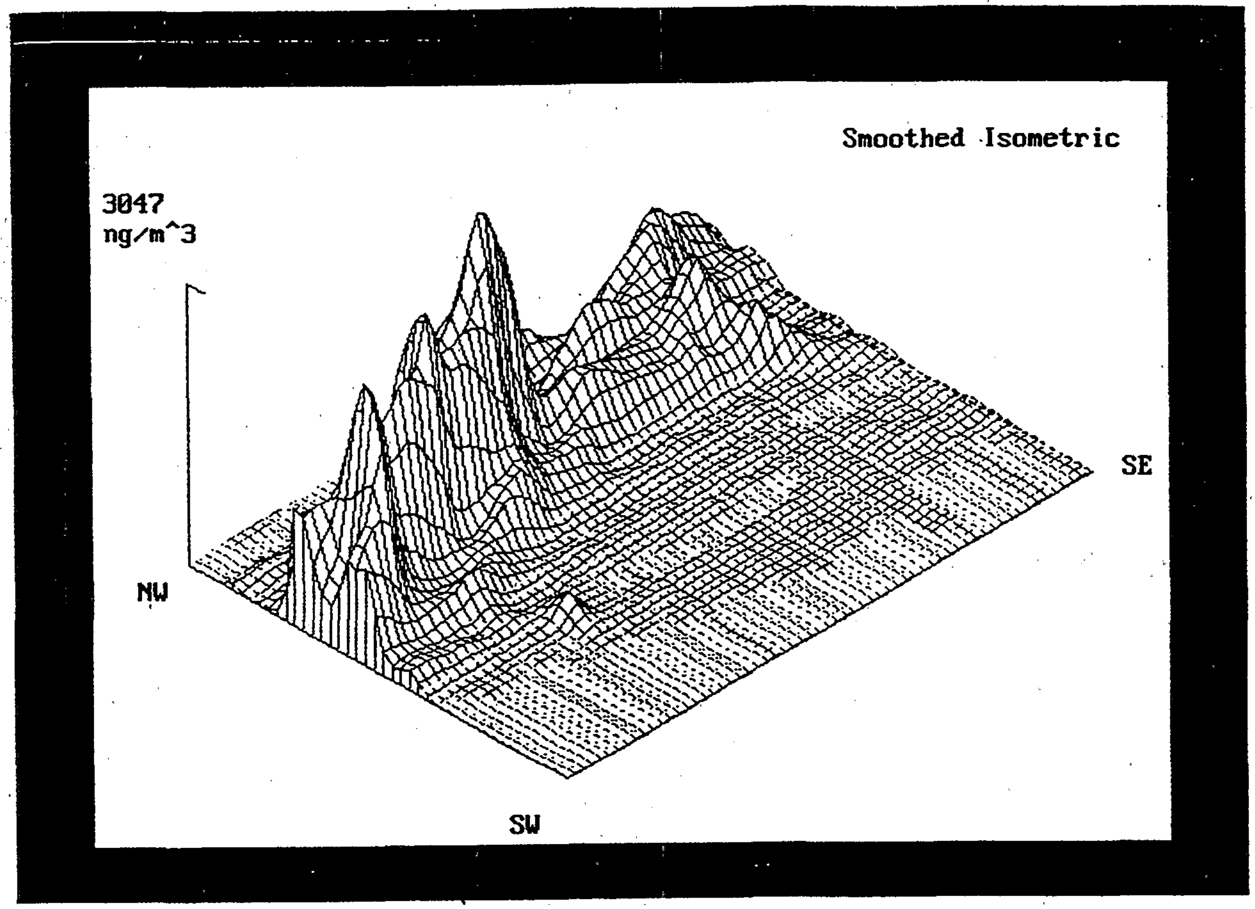

Figure 4

Isometric plot of 24-hour averaged tracer distributions associated with a 1 gm/s source 80 meters above the local terrain. The contours have been smoothed through a binomial filter with 1 km coherance. Residual sampling noise is still apparent.

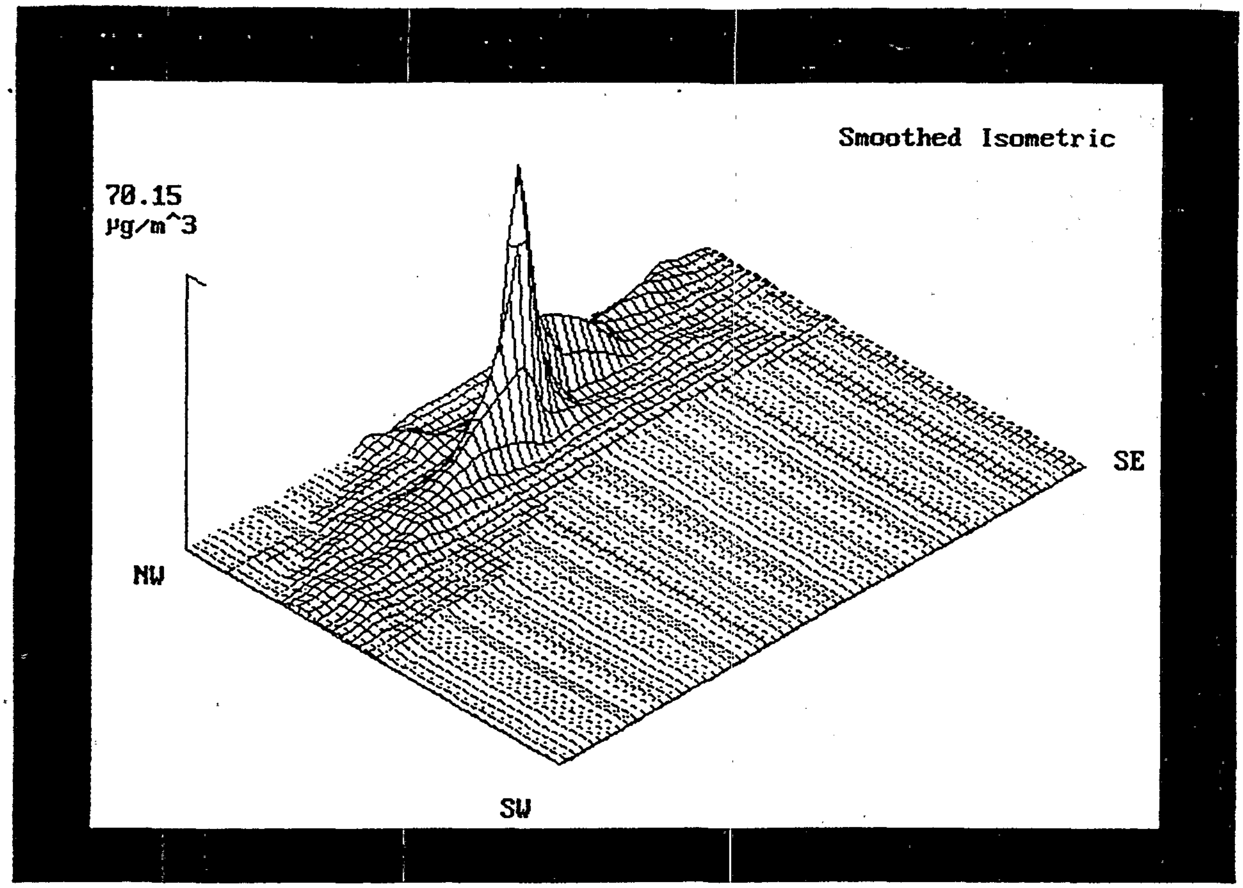

Figure 5

Isometric plot of 24-hour averaged tracer distributions associated with a 1 gm/s source 10 meters above the local terrain. The contours have been smoothed through a binomial filter with 1 km coherance.

Pre-Meeting Comments on the Risk Assessment for the Waste Technologies Incorporated Hazardous Waste Incinerator Located in East Liverpool, Ohio

Prepared by: Jerry Havens
Distinguished Professor of Chemical Engineering
University of Arkansas

This multifaceted study appears to have been considered carefully and there is evidence that the advice of the scientific community was sought, received, and acted upon. With an important exception (discussed below), I found this risk assessment to be realistic and comprehensive, and I found it to to be fairly presented. In my opinion, it is worth the considerable cost and effort expended, and I believe it deserves to be received by all parties concerned as a balanced attempt to realistically assess the risks associated with the operation of the hazardous waste incineration facility.

Following my summary critique of the sections which I feel most qualified to address, I offer suggestions for further consideration.

The Risk Assessment is composed of seven volumes:

Volume 1: Executive Summary
Volume 2. Introduction
Volume 3. Characterization of the Nature and Magnitude of Emissions
Volume 4. Atmospheric Dispersion and Deposition Modeling of Emissions
Volume 5. Human Health Risk Assessment (HHRA): Evaluation of Potential Risks from Multipathway Exposure to Emissions
Volume 6. Screening Ecological Risk Assessment (SERA)
Volume 7. Accident Analysis: Selection and Assessment of Potential Release Scenarios

There was not sufficient time to consider carefully all of the material provided. My review focused on the methods used to identify the potential for releases of hazardous materials, both routine and accidental, and on the methodology for estimating the potential consequences offsite using atmospheric dispersion models.

After reading the Summary and the Introduction, I read carefully Volumes 3, 4, and 7. I considered Volumes 5 and 6 only in the light of those sections' dependence on the results of Volumes 3 and 4.

My comments are divided into two categories:

1. Estimation of risks associated with routine emissions.
2. Estimation of risks associated with accidental releases.

<u>Risk Assessment for Routine Emissions</u>

Identification and quantitative estimation of the materials present in routine stack and fugitive emissions is a very difficult task, requiring realistic, accurate forecasts of the schedules for receiving, processing, temporary storage, and incineration of several hundred potentially hazardous chemicals. In addition to the hazards associated with the potential for release of individual chemicals, requirements for segregation of the materials to preclude reactive conditions (which could cause, or increase the severity of a release) considerably complicate the realization of safe operation. Nevertheless, I believe the simplification adopted to identify surrogate hazardous chemicals, based on forecasts of the incinerator facility's receipts, is reasonable, and I doubt that the information upon which the forecasts are based would justify a more specific approach.

Following the identification of surrogate chemicals, the potential rates of release (routine emissions) are specified partly by reference to the expected operating characteristics of the release control technology which is applied (which appears to be state of the art) and partly by reference to measurements conducted at the site. This procedure appears reasonable, and I cannot suggest improved alternatives.

Given the specification of the materials, amounts, and physical states (solid, liquid, and vapor) of the (routine) releases, the estimation of the potential offsite consequences is based on the use of atmospheric dispersion models. I found this part of the overall risk assessment to be thorough and comprehensive.

The modeling of routine emissions is based primarily on the ISC-COMPDEP model developed by EPA. A technical description of the ISC-COMPDEP model is provided in sufficient detail to allow assessment by an independent reviewer. The ISC-COMPDEP model is supplemented by modeling exercises with the CALPUFF and INPUFF models to provide for address of low wind/calm conditions which are specified to occur (on average) about 22% of the time. It appears that the ISC-COMPDEP model is the best modeling procedure available for the modeling of the routine releases from the WTI facility. The CALPUFF and INPUFF models strengthen the process by providing for estimates of the dispersion of transient emissions and emissions in calm conditions. It is important that the CALPUFF and INPUFF model applications indicate that the maximum distance indicated for offsite consequences (from routine releases) occurs under other-than-calm conditions. The modeling methodology provides for consideration of the following aspects which are important for realistically modeling the atmospheric dispersion of the effluent from the WTI facility:

1. Terrain features, including the effects of stack height less than and greater than the surrounding terrain elevations.
2. Use of on-site and/or near-site meteorological data, including precipitation data for wet deposition calculations and turbulence measurements for dispersion estimation.
3. Provision for evaluating the short-term concentration increases resulting from process upset conditions.
4. Provision for evaluation of the effects of calm wind conditions and fumigation on short- and long-term concentrations in and beyond the valley.
5. Provision for evaluation of the impacts of fugitive emissions.

The physical modeling study provided by EPA's Fluid Modeling Facility (FMF) effectively addresses the issue of terrain downwash (which is not explicitly accounted for in the ISC-COMPDEP model). The comparisons between the FMF wind tunnel predictions and the ISC-COMPDEP model predictions greatly strengthen the credibility and reliability of the process.

Overall, I found the methodology used to identify and assess the offsite risks due to routine emissions to be thorough and reasonably complete.

Risk Assessment for Accidental Releases

In parallel with the methods employed for estimating the consequences of routine releases, a three-step process was used to define the scenarios representative of the (accidental release) risks, including, specifically, "worst case" scenarios:

1. Identification and selection of accident scenarios.
2. Specification of chemical-specific (accident) emission rates.
3. Atmospheric dispersion modeling.

First, my comments on the latter steps. I did not have time to verify the estimates of (accident) emission rates and (fire) heat effects, which were stated to have been made using EPA methods as well as the Automated Resource for Chemical Hazard Incident Evaluation (ARCHIE) model developed by FEMA. However, I have no reason to believe that the correct use of these models would not be adequate to provide reasonable estimates of the evaporation rates of spilled liquids as well as the radiative heat effects from pool fires.

Based on the surface meteorological data set developed for use in the dispersion modeling for routine emissions, three meteorological conditions were selected for use in the accident analyses:

1. A "typical" meteorological condition was determined to be a neutral atmosphere with average wind speed (3.2 m/s). The non-fire scenarios were modeled for the "typical" meteorology with the SLAB model and the fire-related scenarios were modeled with the ISC-COMPDEP model.

2. A "conservative" meteorological condition was determined by evaluating 54 combinations of atmospheric stability and wind speed to determine which combination resulted in the maximum downwind ground-level concentrations off-site. The 54 combinations were used to determine the conservative meteorological conditions and the resulting concentrations using both the SLAB model (for non-fire scenarios) and the ISC-COMPDEP model (for fire scenarios).
3. The "Calm/Inversion" condition assumes that emissions accumulate in the air immediately above the source for one hour during calm conditions and a stable atmospheric lapse rate and are then transported downwind with a wind speed of 1 m/s. The limited mixing in the surface layer imposed by the temperature inversion is represented by a mixing height of 100 m in the SLAB model, and the worst-case meteorology is represented by the combination of 1 m/s wind speed and a stable atmosphere with Monin-Obukov length of 8.3 m (said to be roughly equivalent to Pasquill-Gifford atmospheric stability E or F).

Again, I did not have time to verify any of the mathematical model predictions, either with the models used in the risk assessment or with alternative models. However, I am familiar with the SLAB model, and I consider it appropriate for the use specified here. The SLAB model has been validated against several benchmark data sets, and in my opinion can satisfactorily account for the effects of cloud density on dispersion. *(This requirement is probably important only for the accidental non-fire releases.)* I am not aware of any validation exercises which test the applicability of the ISC-COMPDEP model to the fire scenarios, but the provisions for positive buoyancy which the ISC-COMPDEP model includes are probably as good as any available. In my opinion, the fire-product dispersion modeling with the ISC-COMPDEP model is suitable, particularly in view of the strong suggestion that the dispersion downwind of the fire scenarios considered here do not result in the maximum distances required for consideration.

In my opinion the methods used to estimate the consequences of the (selected) accident scenarios are appropriate, and I have no reason to doubt that the results provide estimates which are sufficiently accurate and realistic for the use made in this risk assessment.

Regarding the identification and selection of accident scenarios, the assessment identifies (five) scenarios considered to be of primary concern:

1. On-site spill of 90% methanol/10% formaldehyde (worst-case waste), or 90% toluene/10% acetone (typical waste) with formaldehyde or toluene, respectively, released to the atmosphere.
2. On-site pool fire involving 15% tetrachloroethene/85% toluene, with hydrogen chloride and phosgene released to the atmosphere.
3. On-site mixing of incompatible wastes, consisting of 15% tetrachloroethene/85% methanol mixed with waste consisting of 70% nitric acid, with hydrogen chloride released to the atmosphere.
4. Off-site tanker truck spill of waste consisting of 90% methanol/10% formaldehyde (worst-case waste) or 90% toluene/10% acetone (typical waste), with formaldehyde and acetone, respectively, released to the atmosphere.
5. Off-site pool fire involving 85% toluene/15% tetrachloroethene with hydrogen chloride and phosgene released to the atmosphere.

I have no basic disagreement with the methods used to quantify the the consequences of these accident scenarios, including the proposed use of IDLH and LOC for characterizing the severity of consequences.

However, I am concerned that potentially important accident scenarios have not been given sufficient attention. Specifically, I noted that the organic waste tank farm consists of several tanks (I believe six are indicated) with individual volumes of approximately 20000 gallons. The tanks are enclosed in a building that has four vents to the atmosphere. I found no specification of the (planned or actual) contents of these tanks, nor of the potential for interconnection (planned or otherwise) between tanks. Although there is an indication that the tanks are diked and that fire protection is provided, there is no clear specification of the separation or segregation (by diking) in the tank farm. I did not find a description of the provisions (managerial or technical) made to preclude accidental mixing of reactive chemicals in the tank farm or in the collection system which feeds effluents from the tank farm area to the carbon absorption bed.

It is repeatedly assumed in the specification of the five accident scenarios of greatest concern that explosion and/or BLEVE incidents are sufficiently unlikely that they are not included. It may be true that sufficient measures have been taken to prevent (for example) fire involvement of multiple tanks in the organic waste tank farm in the event of a pool fire in the enclosure, but in my opinion the assessment does not satisfactorily consider this question. There is no information given to allow specification of the contents of the large tanks in the organic waste tank farm. Without further information, given the indicated individual tank size of 20000 gallons, and the siting of as many as six tanks in a building which is assumed not to be designed to contain either explosion overpressures or gases which might be released in the event of emergency pressure relief operations, it is not justified to dismiss these potentially catastrophic scenarios as being "sufficiently" unlikely.

Having opened this can of worms, I would expect that the questions I have raised were considered in the design and in the plans for operation of the facility, and that information may be available which would alleviate the concerns which I have stated. If that is the case, the Risk Assessment should address those issues carefully.

Overall, I am most concerned that in the effort to deal quantitatively with the technical questions of emission estimation and atmospheric dispersion, important questions regarding the provision of good engineering design and operation principles to minimize the probability (and potential consequences) of catastrophic events have not been given sufficient attention.

REVIEW OF THE RISK ASSESSMENT FOR THE WASTE TECHNOLOGIES INDUSTRIES (WTI) HAZARDOUS WASTE INCINERATOR FACILITY (EAST LIVERPOOL, OHIO)

by

Geoffrey D. Kaiser
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November 1995

GENERAL QUESTIONS

1. **Comment on the organization of the risk assessment document. Does the layout follow a logical format? Is the presentation of information in the document clear, concise and easy to follow?**

The organization of the risk assessment is logical. The presentation is generally clear, but the document can hardly be said to be concise. The presentation is generally easy to follow except for Volume VII, the Risk Assessment, which often does not clearly explain assumptions and in which it is often difficult to see how results were obtained. See below for my extensive comments on Volume VII.

2. **Does the executive summary accurately reflect the data and methodologies used and the conclusions derived in the risk assessment?**

The Executive Summary is clearly written and adequately summarizes those portions of the report that I have reviewed (chiefly Volumes II, IV and VII). However, on pages VII-6 and VII-7 of the Executive Summary (and Table VI-1 of Volume VII) the authors attribute an accident severity ranking to FEMA which does not seem to actually be in one-to-one correspondence with the FEMA scheme:

Severity Ranking	From Page VII-7 of the Executive Summary	Actual FEMA Definitions
Minor	No exceedance of IDLH value in inhabited off-site areas; and negligible potential for off-site fatalities or serious injuries due to heat effects from a fire.	Low potential for serious human injuries; no potential for human fatalities; and no need for a formal evacuation, although the public may be cleared from the immediate area of the spill or discharge.
Moderate	Exceedance of IDLH values in inhabited areas over distances of 100 meters or less; injuries due to heat effects limited to a distance of 1,000 meters into inhabited areas.	Up to 100 potential human injuries requiring medical treatment or observation; up to 10 potential human fatalities; or evacuation of up to 2,000 people.
Major	Exceedance of IDLH values in inhabited areas over distances between 100 meters and 1,000 meters; injuries due to heat effects limited to a distance of 1,000 meters into inhabited areas.	Up to several hundred potential human injuries requiring medical treatment; up to 100 potential human fatalities; or evacuation of up to 20,000 people.
Catastrophic	Exceedance of IDLH values in inhabited off-site areas over distances greater than 1,000 meters; injuries due to heat effects extend to distances greater than 1,000 meters into inhabited areas.	More than 300 potential human injuries requiring formal medical treatment; more than 100 potential human fatalities; or evacuation of more than 20,000 people.

In Volume VII, the authors do not show why they believe that there is a one-to-one correspondence between the two sets of definitions. In my answer to Question 5 under Volume VII (see below) I explain why the authors, by adopting the definitions in Column 2 of the above table, seem to have introduced considerable conservatisms for which I cannot find an explanation. Furthermore, in the summary Tables VII-2 and-3 in the Executive Summary (Tables VIII-1,-2 and -3 in Volume VII), the authors appear also to have introduced conservatisms into the <u>frequency</u> assignments (see also my answer to Question 5 under Volume VII). Consequently, the summary of results on the bottom of p. VII-8 continuing on p. VII-9 of the Executive Summary overstates the risks arising from the accident analysis.

3. Were the major recommendations of the 1993 peer review workshop for the risk assessment plan addressed?

The major recommendations of the 1993 peer review workshop in the areas which I have reviewed are as follows:

- *In developing an appropriate meteorological data set for the air dispersion modeling, it was suggested that site-specific meteorological observations be combined with Beaver Valley Power Station data collected at multiple elevations.*

- *Wet deposition estimates were recommended to be refined using local precipitation data.*

- *Fumigation conditions and terrain induced downwash were identified as having the potential to cause locally elevated concentrations. Further, evaluation of such conditions by modeling or by conducting wind tunnel studies was suggested.*

- *Sensitivity and uncertainty analyses were recommended to estimate the uncertainty of the model's concentration and deposition outputs.*

An impressive amount of work has been done to address these recommendations.

4. As with any risk assessment, there are always additional data and method development efforts that could be undertaken to reduce the level of uncertainty. However, are there any major data or methodological gaps that would preclude the use of the risk assessment for decision making? If so, how should they be addressed?

I do not think that additional short term (i.e. realistically accomplished within a year or two) data or method development efforts would be cost beneficial in the short term. The analyses presented in the areas that I have reviewed are consistent with the state-of-the-art and further work would not lead to further insights. In other words, further data or methods development efforts would not give decision makers additional insights. The one exception is Volume VII, which could with profit be rewritten to enhance the clarity of explanation and either to remove or justify currently unexplained conservatisms (see below).

5. What long-term research would you recommend that could improve risk assessments of this type in the future?

Specifically for risk assessments of the type reported in Volume VII, long term research devoted to determining the levels of airborne concentration that will cause injury or fatality as a function of exposure time would be very helpful, especially if it could be expressed in the probit format. Such data are currently available for very few toxic substances..

ATMOSPHERIC DISPERSION AND DEPOSITION MODELING

1. **Since the 1993 peer review of the risk assessment plan, a number of efforts have been completed to reduce the uncertainty associated with the air dispersion and deposition modeling. These efforts include the collection of site-specific data for emission rates and meteorological conditions. Also, a wind tunnel study was conducted to evaluate the effects of the complex terrain surrounding the WTI facility. Does the risk assessment document summarize these activities? Is the link between these data collection effects, the air dispersion models and the risk assessment clearly established?**

Yes. This part of the work has been well done and is well explained.

2. **The results of 12 sets of sensitivity tests indicate that geophysical variables (e.g. terrain) are more likely to affect dispersion and deposition than emission variables (e.g. stack temperature). Were these sensitivity analyses adequate? Comment on the conclusions reached. To further examine the effect of physical variables, wind tunnel testing was conducted to model the terrain induced flow effects near WTI. It was concluded that changes in peak concentrations attributed to these effects are relatively minor and that the ISC-COMPDEP model is sufficiently conservative. Comment on this conclusion. Have these analyses helped to characterize and reduce the uncertainty in the air dispersion modeling associated with the complex terrain surrounding WTI?**

a) Sufficient sensitivity studies have been performed.

b) The wind tunnel modeling was conducted thoroughly and with exemplary professionalism. The conclusions arising from the wind tunnel work are credible.

c) The analyses have helped to reduce the uncertainty in the air dispersion modeling associated with the complex terrain surrounding WTI.

3. The ISC-COMPDEP model does not allow for non-steady state conditions such as calm winds and strong temperature inversions. Therefore, CALPUFF was used to estimate air dispersion and deposition under these conditions. However, CALPUFF gave similar peak, 24 hour and annual average concentrations as ISC-COMPDEP. Comment on the adequacy of this analysis. Comment on the conclusions reached. Has this analysis helped to characterize and/or reduce the uncertainty in the air dispersion modeling associated with non-steady state meteorological conditions?

The CAL-PUFF analysis is credible and helps make conclusions drawn from air dispersion modeling in non-steady state conditions more robust. It has helped to characterize the uncertainties in the modeling.

4. Atmospheric dispersion modeling was used to estimate air concentrations of hazardous chemicals for the accident analysis. The SLAB model was used for vapor releases from spills and the mixing of incompatible wastes. ISC-COMPDEP was used for releases associated with fires. Comment on the selection of the models and inputs. Are they appropriate selections?

See the answer below to identical question 4 under Volume VII

5. Overall, have adequate sensitivity tests been conducted to demonstrate the magnitude of variation in concentrations and deposition estimates with model inputs?

Yes.

ACCIDENT ANALYSIS - VOLUME VII

A. GENERAL COMMENTS

- There appear to be unjustified (or, at least, unexplained) conservatisms introduced into both the frequency and the magnitude of the consequences. See the answer to Question 5 below for an explanation of this observation. This means that the "probability/severity" matrices Tables VIII-1,2 and-3 consistently overestimate the risk and that the conclusions in Chapter 8 overstate the risk.

- Throughout volume VII, the word "probability" is used when the word "frequency" is what is meant. Probability is dimensionless, whereas frequency has dimensions of events per unit time. The authors should review wherever "probability" is used and replace it by "frequency" in almost every case.

- The use of "averaging time" is sometimes confusing because it is used in two different ways. In some places, it is used as the time that is placed in the equation for the increase in plume width as a function of time. In other places, it is used as the length of time for which an individual is exposed to the passing plume. Paragraph 11 on Pages 26 and 27 of Appendix VII-4 is an example where the use of the terminology is particularly confusing. Perhaps different phrases could be used such as "dispersion averaging time" and "exposure averaging time." The authors should review the entire volume to make sure that this distinction is always clearly observed.

B. ANSWERS TO SPECIFIC QUESTIONS

1. The WTI accident assessment selected five scenarios for quantitative evaluation that were considered to be of primary concern. The scenarios are an on-site spill, an on-site fire, an on-site mixing of incompatible waste, an off-site spill, and an off-site spill and fire. Please comment on the selection of these scenarios. Were any significant scenarios missed?

The chosen scenarios provide an adequate foundation for the semi-quantitative risk evaluation in Volume VII. I do not think any significant scenarios were missed.

2. Specific chemicals were selected to evaluate each scenario. Please comment on the selections. Would other chemicals have been more appropriate?

I doubt that selecting other chemicals would have led to more insights. The use of formaldehyde as a worst-case chemical and acetone as a typical chemical is appropriate for the spill scenarios, HCl and phosgene for the fire scenarios and HCl for the inadvertent mixing scenarios.

3. Chemical specific release rates are calculates for each scenario. Please comment on the procedures used to estimate the release rates. Was an appropriate approach used?

a) The procedure used to calculate the evaporation rates of spillages of formaldehyde and acetone in the conservative weather condition and the calm/inversion weather condition is not appropriate, since the same evaporation rates are used as for the typical weather condition - that is, Equation (12) on p. 6 of Appendix VII-3 is used with the same windspeed for all three conditions, namely 3.2 m/s. This evaporation rate is too high for the conservative condition (windspeed 1.5 m/s) and is completely inappropriate for the calm/inversion condition where the windspeed is essentially zero.

There are models for evaporation in calm conditions. One such gives the evaporation rate Q as follows:

$$Q = 292(1 + 0.51\, Re^{1/2} Sc^{1/3}) \ln(1/(1-P_v))(M_L/T) d\lambda/2 \text{ g/min}$$

where Re is the Reynolds number using a windspeed of 0.03 m/s
Sc is the Schmidt number
P_v is the vapor pressure of the liquid (atmospheres)
M_L is the molecular weight of the liquid
T is the ambient temperature (K)
d is the diffusivity of the air (cm^2/g) and
λ is the diameter of the spill (m).

The windspeed of 0.03 m/s used to calculate the Reynolds number here seems to be consistent with the spreading speed of 0.03 m/s used to represent the growth of the cloud in calm conditions on p. 19 of Appendix VII-4.

Overall, the authors should reconsider their calculations of evaporation rates in conservative and calm/inversion conditions. The actual release rate used seems too high and therefore the estimated downwind distances in these conditions are likely too high.

b) For the calm/inversion condition, the authors assume that, over a period of one hour, the vapors evaporating from a pool occupy a volume that is 108m x 108m x 100m. This will not be true for evaporating vapors that are heavier-than-air. These vapors will slump and form a cloud that may be only a few centimeters in depth (I have seen videos of experiments at e.g. Porton Down where this happened). When the wind picks up, it will entrain vapor through a process of quasi-evaporation. I am not sure that we know how to calculate this, but downwind concentrations will probably not be very high.

4. Atmospheric dispersion modeling was used to estimate air concentrations of hazardous chemicals for the accident analysis. Specifically, the SLAB model was used for vapor releases from spills and the mixing of incompatible wastes. ISC-COMPDEP was used for releases associated with fires. Comment on the selection of the models and inputs. Are they appropriate selections? Should other models or inputs have been used?

a) The use of SLAB for the spill evaporation scenario in calm/inversion conditions is inappropriate. If the authors truly believe that, after one hour, the evaporating material occupies a volume that is 108 x 108 x 100 m (P.19 of Appendix VII-4), which then begins to move downwind once the wind picks up, then a more appropriate model would be an inversion-lid-limited Gaussian model with a volume source. The SLAB model as used in the WTI risk assessment almost certainly overestimates the airborne concentrations.

b) In all of the SLAB runs for this project, the input parameter TAV is set equal to 1,800 seconds = 30 minutes, which represents the exposure averaging time for which the IDLH is defined. The input parameter TSD (the dispersion averaging time) is set equal to the duration of release which, near the source, is also the duration of cloud passage. When TSD is less than TAV, the authors reduce the calculated peak centerline concentration by the ratio r = (TSD/TAV). Thus, if TSD is 600 seconds, as it is in the mitigated runs of SLAB such as Run No. 2, then r = 1/3. This implicitly assumes that the IDLH is not a constant concentration, but rather a constant dose, an expression of Haber's law. The authors should explicitly recognize that this is a major assumption about the toxicity of the vapor and should review the evidence that it is valid for formaldehyde, acetone, hydrogen chloride and phosgene. It is an assumption which has a large effect on the predicted IDLH propagation distances for the mitigated cases and for any other case where TSD is considerably less than TAV.

I haven't had time to check the following out, but it seems to me that, when TSD exceeds TAV, the authors do not use a similar averaging algorithm- that is, for exposure times in excess of 30 minutes, toxic vapors are treated as if the IDLH is a constant concentration and not a constant dose. Is this correct? If true, this certainly represents an inconsistent approach to the IDLH, which apparently obeys Haber's law for exposure times $<$ 30 minutes but not for exposure times $>$ 30 minutes.

c) It is appropriate to apply SLAB to evaporating pools in a steady wind and to dense vapor jets such as that which may evolve from inadvertent mixing scenarios.

d) The applications of ISC-COMPDEP seem to be appropriate.

5. Please comment on the assessment's conclusions on the severity of consequences and the probability of occurrence. Has the report correctly categorized the severity of the consequences of the different accident scenarios? Has the assessment adequately justified the reported probability of occurrence of each of the accident events?

a) Regarding frequencies of occurrence, there seem to be a number of inconsistencies between various parts of the text. Some examples follow:

At the end of Chapter I (page 1-11) it is stated that the probability (it should be frequency) of occurrence of an event having minor consequences would be classified as reasonably likely —. However, in Table VIII-1 there is an event of minor consequence with a likely frequency of occurrence. On Page VIII-1 it is stated that events with minor consequences are classified as likely to occur. However, in Table VIII-1, there are seven events with minor consequences, but only one of them is likely, the rest being only reasonably likely or unlikely. On Tables VIII-2 and VIII-3, there are several events with minor consequences, none of them with a frequency exceeding reasonably likely.

On pp VI-10,11, it is stated that spills of approximately 100 gallons are expected to occur at least once every ten years and spills of 5,000 gallons are expected to occur between once every ten years and once every hundred years. [The sentence in question reads " -- the available information summarized in Table 2 of Appendix VII-1 would suggest that spills of approximately 100 gallons might be considered at most reasonably likely (i.e. expected to occur at least once every ten years on average) while spills of 5,000 gallons might be considered at most reasonably likely (i.e. expected to occur between once every ten years and once every hundred years on average)". This sentence needs some attention because it is internally inconsistent, see in particular the use of "reasonably likely".] However, on page I-11 the frequency of occurrence of all accidental spills is estimated to be once in every 25-30 years, so it is not clear where the once in ten years on pp VI-10,11 comes from. Starting with the once in 25 -30 years, and using the estimate that only 5% of spills are of 5,000 gallons or more leads to a frequency of once in 500 - 600 years for a 5,000 gallon spill, much lower than in the sentence quoted above.

There is a clear need for the authors to review their written and tabular summaries of the risk assessment results and to make sure that there is consistency throughout Volume VII and to make it easy for the reader to understand how estimates of "likely", "reasonably likely" etc. were derived.

b) I tried to derive some of the results in table VI-10. e.g the predicted frequency of a 5,000 gallon spill of formaldehyde in typical weather conditions, which I took to be the product of the following factors:

- Frequency of occurrence of any spill: 1/25 = 0.04/yr
- Probability that the spill is 5,000 gallons: 5% = 0.05
- probability that the spill is formaldehyde: 0.5% = 0.005 (see p.VI-11)
- Probability that the weather is "typical": 0.57 (see p. V-2)
- Product of all of the above: 5.7 x 10^{-6}/yr or less than once every 100,000

years, more than two orders of magnitude below the once in a thousand year threshold for the unlikely category, yet in Table VI-10, this event is categorized as unlikely. Even if formaldehyde is taken to be a part of as many as 10% of all spills (I think this is what the authors actually mean, but it is not clear on page VI-11; I have assumed 10% in all the following discussion) the above product is still about once in 10,000 years, well below the "likely" threshold. Similarly, if we assume that 90% of all spills contain acetone, the same reasoning as above would put the frequency of a large acetone spill in the range of once in ten thousand to once in a thousand years, i.e. barely approaching the lower threshold for the unlikely category, yet it is assessed to be reasonably likely (more than once in a hundred years) in Table VI-10.

If we proceed to investigate calm/inversion conditions, which occur 1.7% of the time per page VI-11 and replace 0.57 in the above product by 0.017, the frequency of occurrence of a 5,000 gallon acetone spill is now 0.017/.57 = 1/33 of what it is in typical weather conditions - that is, instead of somewhere in the range once in ten thousand years to once in a thousand years it now becomes once in 330,000 years to once in 33,000 years. However you look at it, this is very unlikely, whereas the frequency given in Table VI-10 is unlikely, i.e. more than once in a thousand years.

c) Turning to the estimate of the frequency of onsite fires, we have the following calculation:

- frequency of a large spill: 1/500/yr = 0.002/yr
- probability that spill will ignite: 1/25 = 0.04 (see p. VI-17)
- probability of typical weather: 0.57
- product of the above: 4.56×10^{-5} = once in 20,000 years, i.e. very unlikely, yet in Table VI-10, the frequency of occurrence of a large fire in typical weather conditions is characterized as unlikely, i.e.

more than once in 1,000 years. Since the ranking of the on-site mixing of incompatible wastes has been assumed to be the same as that for fires (see p. VI-12), the frequency of occurrence of these mixing events also seems to be overestimated.

d) Turning to offsite events, the frequency of occurrence of a fire involving a 100 gallon spill is [1/60 (chance of such a spill per year, p. VI-12)] x [1/20 (largest of the probabilities of ignition given on p. VI-14)] x [0.57 (probability of typical weather)] = 4.75 x 10^{-4}/yr = ~ once in 2,000 years, i.e. very unlikely. However, this event is characterized as unlikely in Table VI-11, i.e. occurring more than once in a thousand years.

e) The overall conclusion is that many of the frequencies in Tables VI-10 and VI-11 are overestimated; in some cases by more than an order of magnitude. These tables need to be carefully revisited.

f) Characterization of the <u>severity</u> of the consequences is discussed in Chapter VI of Volume VII. At the bottom of p. VI-1 and the top of p. VI-2 we are told that approximately 1,000 - 1,500 (isn't this figure more accurately known?) people live within a rough semicircle of radius 1,100 m. Assuming that a contaminant plume has a radius of 5-10° enclosing an area with concentrations above the IDLH, then between 1/36 and 1/18 of these people would be affected, or ~27 - ~ 83. Within 175 m of the site, there are 25 - 50 people (this is another figure that ought to be better known), so that between 1 and 3 people might be affected.

On Page I-8, a four tier system for classifying human accidents is presented, attributed to FEMA. **Minor** consequences or those for which there is a low potential for serious human injuries (among other criteria); **moderate** consequences are those for which there are up to 100 potential injuries requiring medical treatment; **major** consequences are those for which there up to several hundred injuries requiring medical treatment and observation and, finally, in a catastrophic accident there are more than 300 human injuries requiring medical treatment. Assuming that the IDLH indicates the need for medical treatment, then, based on the first paragraph of f) above, if the IDLH extends for 1,100 m from WTI, the consequences are **moderate** (up to 100 injuries). If the IDLH extends up to 175 m, with the potential for only up to about three injuries, then the severity would appear to be on the boundary between **moderate and minor**.

In contrast, on Table VI-1, an accident with the IDLH extending out to 100 m is defined as having moderate consequences and an accident with the IDLH extending out to 1,000 m is defined as major. This ranking appears to overstate the actual severity of the consequences, at least when compared with the FEMA rankings. The authors should either return to the FEMA rankings or explain why additional conservatisms appear to have been added.

Similarly, consider Scenario 4B, the off-site spill of 100 gallons of formaldehyde waste. The downwind distances to the IDLH are given in Table VI-8. In conservative meteorology, the IDLH extends downwind to 630 m. According to our discussion above, this ought to be characterized as moderate, yet in Table VIII-2 this scenario is listed in the catastrophic column. [Even according to Table VI-1, it ought to be no more than major]. Similarly, in calm/inversion conditions, this scenario propagates to 1,080 m according to Table VI-8. As we have seen, propagation to 1,000 m leads to less than 100 predicted injuries, so an extra 80 m is not going to lead to enough extra casualties to warrant a catastrophic ranking (> 300 injuries). I suspect that this scenario remains in the moderate category, certainly no more than major.

One can make similar comments about other scenarios. For example, scenario 3A does not cause the IDLH to propagate more than ~ 500 m downwind in any weather condition (see Table VI-7), so that it should not have more than moderate consequences, yet on Tables VIII-2 and VIII-3 it has major consequences. I haven't investigated other scenarios in detail, but it appears that there is a tendency to overstate the consequences and each scenario should be reviewed to make sure that its consequences have been correctly characterized.

g) In summary, items a)- f) above seem to indicate that there has been a considerably tendency to overstate both frequency of occurrence and magnitude of the consequences in the matrices in Tables VIII-1,2,3. Overall, Figures VIII-1,2,3 overstate the risks that have been predicted by the accident analysis. My own conclusions (for which I am prepared to provide details - see the attached handwritten worksheets) are that (1) there are no scenarios for which comprehensive planning and preparedness are essentially mandatory: (2) almost all of the scenarios lie in the region where comprehensive planning may be unwarranted and unnecessary and (3) the only scenarios that lie in the region where comprehensive planning is optional are 1D, 3D and 4D (all large formaldehyde spills) in conservative or calm/inversion conditions. I estimate that these all have frequencies of less than 10^{-4}/yr - i.e. more than an order of magnitude less than the frequency threshold that divides unlikely from very unlikely. This additional order of magnitude should influence the decision as to whether planning is undertaken.

6. Key assumptions were made in the identification of accident scenarios and the description of the conservative and typical events. Included were a description of the magnitude of the effect of the assumptions and direction of the effect. Please comment on the assumptions. Are they justified? Are the descriptions of the magnitude and the directions of the effects correct? Has the accident assessment adequately confronted the uncertainties involved in doing this kind of analysis? If not, what else should be done?

I am not sure what part of Volume VII is being referred to here, at least as far as the "magnitude of the effect of the assumptions and the direction of the effect" is concerned. The choice of accident scenarios and the description of conservative events seems reasonable.

7. **Comment on the appropriateness of using IDLH values for characterizing the severity of the consequences in the accident analysis. Comment on the appropriateness of using 10 X LOC for chemicals for which IDLH values have not been established.**

a) Although there are known problems with the consistency of the definition and the peer review of IDLH's, I do not know of any source of more consistent data. It is appropriate to use an endpoint that is meant to be a threshold for injury or incapacitation that would prevent the individual from taking countermeasures (i.e. taking significantly lower endpoints would likely overestimate the risk). For a qualitative/semi-quantitative risk assessment of the type reported in Chapter VII, I do not think it is possible to find a toxic endpoint that is obviously better. The ERPG would in principle be better, but it has not yet been derived for acetone or formaldehyde.

b) IDLH values exist for all of the four chemicals used in the risk assessment - formaldehyde, acetone, HCl and phosgene. Therefore, the question about 10 x LOC is moot.

8. **In the accident analysis, IDLH (or 10 X LOC) values were used to determine the downwind distances over which adverse health effects might occur. To evaluate the uncertainty introduced by using the IDLH, a sensitivity analysis was conducted where these distances were recalculated using the LOC (a more stringent health criterion). Other sources of uncertainty that are identified in the accident analysis include concentration averaging times, chemical concentrations, emission rates and meteorological conditions. For most of these parameters, it is**

stated that conservative assumptions were used to avoid underestimating risks. Have the uncertainties inherent in the accident analysis been adequately characterized? For those parameters where sensitivity analyses were not conducted, is the conclusion that conservative assumptions have avoided underestimation valid?

As can be seen from my answer to Question 5 above, I think that, by the time the authors come to the final presentation of the results in Tables VIII-1,2 and-3, they have unnecessarily overstated conservatisms. Therefore, conservatisms have indeed avoided underestimation, but by too much! The authors are aware of and discuss the major areas of uncertainty.

C. ADDITIONAL COMMENTS ON VOLUME VII

1. Effects of Mitigation (Chapter VII.C.1)

The discussion of sensitivity studies in Chapter VIIC (pp VII-5 - VII-7) is not helpful because it does not explain why the results turn out to be the way they are. For example, on p. VII-8, it is stated that "for the mixing of incompatible waste scenario involving 200 gallons of waste, active mitigation within 10 minutes does not significantly reduce the maximum distance to the IDLH." The reader has to correlate this observation with the SLAB inputs in Table 4 on p. 32 of Appendix VII-4, runs no. 55 - 60. Only then does it become clear that the mitigated and unmitigated cases have the same duration of release, namely 10 minutes, so that mitigation in 10 minutes is not a meaningful action and of course there is no reduction in the maximum distance to the IDLH. Similarly, mitigation in one hour has no meaning for a scenario that is over in 10 minutes.

Looking at Attachment 3 of Appendix VII-4 (actual data from SLAB output files), the distance to the IDLH in typical weather conditions for a 5,000 gallon spill of formaldehyde is 640 m (unmitigated) and 730 m (mitigated) (RUN01.OUT and RUN02.OUT respectively). However, on Table VII-1 on P. VII-12, both of these numbers are presented as 640 m. No explanation is given. In fact, this result should set the warning bells ringing because it doesn't make any physical sense that mitigation could increase the distances within which people might be injured. Several mathematical factors combine to give this result: a) the average release rate over 10 minutes (1.99 kg/s) is greater than that averaged over 3120 seconds (1.04 kg/s), see Table 2 on p. 30 of Appendix VII-4: b) from the information on the RUN1.OUT and the RUN2.OUT printouts in Attachment 3 of Appendix VII-4, the average vapor phase mole fraction of formaldehyde is 0.742 for the first ten minutes of evaporation and 0.478 for an evaporation time of 3120 seconds; c) the peak centerline concentration at 10 minutes is $(3120/600)^{0.2}$ = 1.39 times higher than that at 3120 seconds. All these factors together make the 10 minute peak centerline concentration effectively 1.39 x (1.99/1.04) x (.742/.478) = 4.13 times higher than the 3120 second peak centerline concentration. However, SLAB averages the 10 minute release over 30 minutes for comparison with the IDLH, so introducing a further factor of (1/3), which reduces 4.13 to 1.38, still greater than unity, which is mathematically why the 10 minute mitigated case propagates further than the 3120 second unmitigated case. The authors need to devote more thought to the further explanation of this effect.

2. Annual Average Off-Site Air Concentrations (Appendix VII-4 p. 25)

There is a paragraph about annual average off-site air concentrations in the middle of page 25. It is not clear why it has been included. It has no relevance to a risk assessment of accidental releases.

3. Appendix VII-4, p.27

The paragraph in the middle of the page that begins "If the release is instantaneous or very short ---" is not clearly written. It is not clear to what scenario it applies. It needs to be rewritten in order to make its point clear (that, because a puff elongates along the wind as it travels, exposure times may in fact equal or exceed thirty minutes when the cloud has travelled (say) 10 km and the predicted distance to the LOC may not exceed that predicted using the continuous, finite duration release model?).

4. Page II-15

The second of the three bullets at the bottom of this page is not always correct - "chemical mixing with the water, or sinking below the surface of the water, would reduce emission rates relative to a spill onto the road.". If the chemical reacts with water, evaporation rates driven by the heat of reaction can be very high.

Table 1

Revised Tables VIII-1, VIII-2 and VIII-3 Frequency/Severity Matrix				
Frequency of Occurrence (f)	Severity of Consequence			
	Minor	Moderate	Major	Catastrophic
Common: f ≥ 1/yr	-	-	-	-
Likely: 1>f≥0.1/yr	-	-	-	-
Reasonably likely 0.1> f ≥ 0.01/yr	-	-	-	-
Unlikely 0.01 > f ≥ 0.001/yr	(1A), (1C),(4A) [4A], [4C] [1A]	-	-	-
Very Unlikely (1) 0.001 > f ≥ 10^{-4}/yr	(3A), (2A), [1C] {1A}, {4A}	(1B) [4C], [5A]	(1D), (4D) [1B] (4B)	[4B]
Very Unlikely (2) 10^{-4} > f ≥ 10^{-5}/yr	(2B), [2A] [5A], {1C}	(5B), {4C}	(3B) [5B], {1B} [3A]	[1D], [4D] {4B}
Very Unlikely (3) f < 10^{-5}/yr	[2B]		{3A}	[3B], {1D} {3B}, {4D}

Key: ( ) Typical weather conditions, Table VIII-1
[ ] Conservative conditions, Table VIII-2
{ } Calm/inversion conditions, Table VIII-3
← Represents change in estimated magnitude of consequences as described in the accompanying text. Other scenario severities might need to be changed. All of values of f in the above table have been re-evaluated.
(←) Consequence severity could be in either category

Assumptions in Table 1

Frequency of on-site spill - 0.04/yr
Probability that spill is 100 gallons - 0.35
Probability that spill is 5,000 gallons - 0.05
Probability that spill is acetone - 0.9
Probability that spill is formaldehyde - 0.1
Probability that weather conditions are typical - 0.57
Probability that weather conditions are conservative - 0.1
Probability that weather conditions are calm/inversion - 0.017
Probability of ignition given a spill - 0.04

Frequency of small off-site spill - 0.017/yr
Frequency of large off-site spill - 0.0034/yr
Probability of ignition given a spill - 0.05

typical
f, on-site spill small spill conservative

Examples
acetone calm/inv

Scenario 1A: Frequency = 0.04 x 0.35 x 0.9 x (0.57) or [0.1] or {0.017}
= (0.007) or [0.0013] or {2.14×10^{-4}}

unlikely unlikely very unlikely

ignition
f, large off-site spill

Scenario 5B: Frequency = 0.0034 x 0.05 x (0.57) or [0.1] or {0.017}
= (9.69×10^{-5}) or [1.7×10^{-5}]

Very unlikely Very unlikely

Robert N. Meroney

PREMEETING COMMENTS ON WTI INCINERATOR RISK ISSUES

<u>General Comments</u>:

1. The organization of the risk assessment document is logical, the text is clear, concise and easy to follow.

2. The executive summary reflects the data and methodologies used and the conclusions derived.

3. The 1993 peer review workshop recommendations (as listed on pages IV-2 and IV-3) related to Ch IV: Atmospheric Dispersion and Deposition Modeling were addressed.

4. Uncertainty Issues:
   - <u>Quantifying Uncertainties:</u> There is far more to uncertainty analysis than just listing assumptions and admitting models are approximate. Sensitivity tests are important, but they also do not guarantee consideration of the cumulative effects of various uncertainties. The use of "conservative" assumptions throughout would initially appear to accomplish the goal of protecting public health, but it may also encourage undue expense to protect the public from non-existent hazard levels! Ranges of confidence and quantitative statements of error about "realistic" calculations of exposure would appear to be a more appropriate way to protect health and environment economically. At least then one knows quantitatively what factor of safety is being imposed. Finally, the final risks involved should be phrased in terms of common risks that the community already endures or accepts, so that the lay person can also appreciate the situation.

   - <u>Spurious Correlations</u>: In many cases correlations are used to consolidate data or identify similarities which can artificially reduce variance through "virtual" or "spurious" correlations. It is not uncommon for such spurious correlation to be as high as 50 to 80%! The proportion of reduced variance due to virtual correlation can be estimated for any parameter scheme, and some indication of the fraction of virtual correlation present is a valuable measure of the value of any statement of correlation.

5. Long-term research which would improve risk assessments of this type include:

- Statistical analysis and evaluation of common correlations to evaluate level of spurious correlation present.
- Wind-tunnel evaluation of fluctuating and instantaneous peak and root-mean-square (rms) concentrations present during building and terrain induced fumigation. Instantaneous concentrations will govern probabilities for flammability and the level of odors. RMS levels imply mixing potentials.
- Examination of the influence of terrain irregularities and slope on the transport and dispersion of dense-gas emissions using fluid modeling. These emissions will tend to drain and channel, yet no existing numerical models properly account for these effects.
- Evaluate the proportion of unsteady gas clouds which are likely to infiltrate into buildings during an emergency event. Currently most models presume concentration levels which exist outside buildings will be the same in the occupied spaces within the building.
- The Huber-Snyder and Schulman-Scire downwash models seem to be limited to distances downwind exceeding $x/H_b > 3$, yet many situations exist where short stacks, fume hoods and flush vents emit gases which re-impinge or re-entrain on to the original building or the near cavity wake region. A model or protocol should be developed to deal with these situations including average and instantaneous concentration levels.
- The SLAB model used in the WTI calculations does not include the influence of sloping terrain, gullies, or gorges on the transport and dispersion of heavy gas plumes. Lee and Meroney (1988) have demonstrated that depth-integrated models can predict dispersion in such situations. A series of fluid model experiments complimented by further depth-integrated calcualtions should be carried out to determine the worst-case situations for heavy plume dispersion over irregular terrain.

Robert N. Meroney

Workshop Specific Issues:

ATMOSPHERIC DISPERSION AND DEPOSITION MODELING: CHAPTER IV

1. Section II, C, 8, pp. II-8 to II-10 and D, II-10 to II-13. Downwash Effects:

   - The Huber-Snyder and Schulman-Scire models are limited to $x/H_b > 3$. What are the possibilities of plumes producing maximum concentrations in the near cavity region. (If not for the WTI case, then in other circumstances.)

2. Section IV, B, 6, pp. IV-16 to IV-18. Sensitivity tests: Terrain Downwash Simulations

   - The report of the wind-tunnel tests by the U.S. EPA Fluid Modeling Facility (Appendix IV-6) together with the associated Peer Review statements (Appendix IV-7) provide convincing documentation that the effects of building and plume downwash have been properly considered during the WTI analysis.

   - A major reason for the wind-tunnel study was to document the presence of terrain fumigation, flow separation, reattachment and associated recirculation regions. As noted in Appendix IV-6, page 4, a terraced model was constructed from ½ inch plywood. Normally such steps are considered appropriate since they help simulate the local roughness effects. Unfortunately, the use of a terraced model may incorrectly predict the location and strength of recirculation regions. Meroney (1978, 1979) found that when correct simulation of near surface winds and associated recirculation regions are required it is appropriate to simulate the terrain with a smoothly contoured model with simulated vegetation added.

     Meroney, R. N., Bowen, A. J., Lindley, D., and Pearse, J., WIND CHARACTERISTICS OVER COMPLEX TERRAIN: LABORATORY SIMULATION AND FIELD MEASUREMENT AT RAKAIA GORGE, NEW ZEALAND, FINAL REPORT, PART II, Department of Energy Contract No. EY-76-S-06-2438, A001, 219 pp., Report RLO/2438-77/2, 219 pp., May 1978

Meroney, R. N., FIELD VERIFICATION AND LABORATORY SIMULATION OF AIRFLOW PATTERNS IN COMPLEX TERRAIN, Proceedings of Fourth Symposium on Turbulence, Diffusion, and Air Pollution, January 15-18, 1979, Reno, Nevada, pp. 592-595

- Only neutral stratification situations were considered during the wind-tunnel model program under the argument that only "moderate and high-wind conditions" need be simulated to evaluate terrain effects. It is true, however, that stratification effects can be present under moderate and high-wind conditions. In particular if an elevated inversion exists, then the local terrain may cause strong down-slope winds which can enhance or degrade recirculation situations. Indeed in a valley situation at night local radiation inversions may further enhance stagnation and decouple the valley flows from the stronger winds above (see page IV-21, Section D.b, line 7). Meroney and Grainger(1992, 1993) discuss the influence of stratification on winds over depressions.

  Meroney, R.N., Grainger, Clyde, DISPERSION IN AN OPEN-CUT COAL MINE IN STABLY STRATIFIED FLOW., *Jrnl. of Boundary Layer Meteorology*, Vol. 63, 1993, pp. 117-140.

  Meroney, R.N., Grainger, Clyde, NIGHTTIME FLOW AND DISPERSION OVER LARGE BASINS OR MINING PITS. Symposium on Measurement and Modeling of Environmental Flows. 1992 ASME Winter Annual Meeting, Anaheim, CA, November 8-13, 1992. FED-Vol. 143/HTD-Vol. 232, pp. 209-215,

3. Charge Question 1: The risk assessment document does summarize the additional tasks taken to address the 1993 peer review concerns. The links between the data collection, the air dispersion models, and the risk assessment seem adequate.

4. Charge Question 2: The considerations of the effects produced by terrain downwash have been adequately addressed through the use of fluid modeling and the subsequent comparison of the numerical model and wind-tunnel results. The comparisons suggest the ISC-COMPDEP model is conservative. This would appear to be the case for neutrally stratified conditions; however, the possibility that stratification or raised inversions might enhance downwash without associated increases in terrain induced turbulence has not been considered.

5. Charge Question 3: Comparisons of the results from the steady state ISC-COMPDEP model and the non-steady CALPUFF model do suggest that receptor concentrations will not be significantly increased due to nonstationarity.

6. Charge Question 4: Given that many hazardous chemicals likely to be released in a spill or accident have either heavy molecular weight or may produce a cold gas cloud as a result of decompression or evaporation it is commendable that the SLAB model was used to evaluate hazards. Unfortunately, the version of the depth-integrated model used presumes absolutely flat terrain with no slope or channeling due to terrain irregularities. The presence of such terrain perturbations changes the consequences of such releases significantly; hence, high concentrations may be carried to significantly greater distances downwind in such situations. A complete hazard analysis for on- or off-site releases should examine worst case scenarios of terrain induced transport. Terrain effects have been predicted by depth-integrated models, but no systematic evaluation of the enhancement of transport has been completed (Lee, 1988; Lee and Meroney, 1988).

Lee, J.T. and Meroney, R.N., NUMERICAL MODELING OF DENSE GAS CLOUD DISPERSION OVER IRREGULAR TERRAIN, Proceedings of Eighth Symposium on Turbulence and Diffusion, American Meteorological Society, San Diego, California, 25-29 April, 1988, Paper 4.9, pp. 392-395

Lee, J.T. (R. N. Meroney, advisor), A NUMERICAL INVESTIGATION ON DENSE PLUME DISPERSION OVER COMPLEX TERRAIN, M.Sc. Thesis, August 1988, 116 pp.

7. Charge Question 5: The sensitivity tests completed appear to be appropriate and inclusive. The tests respond to the 1993 Peer Review panel concerns, and the conclusions drawn from the tests are logical and appropriate.

Exposure Assessment

James P. Butler

Comments on the Risk Assessment for the Waste Technologies Industries (WTI) Hazardous Waste Incinerator Facility (East Liverpool, Ohio)

Based on my review of *Volume V: Human Health Risk Assessment: Evaluation of Potential Risks from Multipathway Exposure to Emissions* and *Volume VII: Accident Analysis: Selection and Assessment of Potential Release Scenarios*, I can offer the following comments on technical aspects of the WTI Risk Assessment. My comments are organized into two sections. The first part addresses issues raised in the Charge to Reviewers, and the second part contains specific comments and recommendations that were not considered in the charge.

PART I

General Issues

1. This is one of the most comprehensive risk assessments that has been conducted for a waste disposal facility. Considering the amount of data and complexity of the analysis, the risk assessment is presented in a clear and logical manner. In particular, there is a reasonable balance between the extensive exposure-related data in the appendices and summary tables in the texts (with cross-referencing). It is also helpful having the key assumptions clearly identified and tabulated in the discussion of uncertainties.

2. The executive summary accurately conveys the general approach, results, and conclusions of the risk assessment.

3. Most of the major recommendations of the 1993 peer review workshop pertaining to exposure assessment have been addressed to some extent in the document. However, the issue of background exposures to mercury has not been evaluated in the human health risk assessment (discussed below in Part II).

4. The human health risk assessment is generally concerned with the potential for

incremental risks due to emissions from the WTI facility. While this approach is consistent with EPA risk assessment guidance, it does not account for cumulative or total exposures to a given contaminant. Because East Liverpool, Ohio has been characterized as a depressed, industrial city, its low-income population may already be exposed to a variety of environmental hazards. While the methodology for evaluating multiple and cumulative exposures and risks has not been fully developed, the document should at least qualitatively address this issue for contaminants commonly found in industrial areas. To use the results of this analysis for risk-based decision making, it will be necessary to consider environmental equity issues related to health effects.

5. One of the most significant long-term research efforts would be to continue, and expand, the ongoing baseline biomonitoring program that is being conducted in the vicinity of the WTI incinerator. There is a need to collect additional environmental monitoring data for key input parameters in order to validate the exposure modeling approach. It would also be valuable to conduct follow-up surveys to confirm earlier findings related to key exposure parameters (garden usage, fish consumption, etc.). Another useful study would be to collect tap water samples to confirm the assumption that the water treatment plant is removing any contamination resulting from the incinerator's emissions. One of the real strengths of this risk assessment is the inclusion of more site-specific data than is typically available. Building on that database would be beneficial for future risk assessments of similar facilities.

Human Health Risks - Exposure

1. Average and maximum environmental concentrations were modeled for each medium of concern. Similarly, typical and 90th percentile values were obtained for most of the exposure factors. If the central tendency exposure estimates were calculated using average values for both media concentrations and exposure factors, and high-end exposure estimates were calculated using at least 90th percentile values for both media concentrations and exposure factors, then the exposure descriptors were properly used to characterize exposures. However, it is not clear in the document which set of values

were used as inputs for the exposure dose equations.

2. The general approach to developing estimates of exposure is adequately explained in the exposure assessment. However, the specific procedure used to develop central tendency, high end, and/or bounding estimates is not clear (as mentioned above). Perhaps it would be helpful to have a summary table that identifies the combinations of factors that make up each exposure descriptor.

3. The most important exposure pathways have been identified and evaluated in the risk assessment. As recommended in the 1993 peer review workshop, there now is adequate justification for omitting the groundwater and surface water pathways.

4. The key assumptions needed for estimating environmental concentrations and exposures have been identified. The magnitude and direction of effect are correct for the assumptions, with one exception. The assumption that fate and transport modeling accurately reflects reality is highly uncertain and does not necessarily result in a "likely overestimate." The direction of effect is really unknown until these models are better validated with monitoring data collected for that purpose.

5. The uncertainty analysis for two representative compounds was probably the most useful way to confront the overall uncertainties and to identify input parameters that have the greatest effect on the final risk estimate.

Accident Analysis

1. Two other scenarios should also be considered. Because there is a history of on-site worker injuries and fatalities at other commercial hazardous waste incinerators, a plausible accident scenario involving workers at the WTI facility should be included. Given the incinerator's location in a floodplain, it would also make sense to evaluate the impacts of a flooding accident.

6. The key assumptions made in the identification of accident scenarios and ranking of accident events appear to be reasonable with respect to magnitude and direction of effects. In general, conservative assumptions were made to compensate for the uncertainties resulting from a lack of information needed to fully characterize the exposure and dose.

7. The American Industrial Hygiene Association's Emergency Response Planning Guideline (ERPG) levels would probably have been more appropriate than IDLH values for characterizing the severity of accident consequences. At a minimum, for compounds with both values, ERPG levels should be compared to IDLH levels to make it clear which are the more stringent criteria for assessing the acute effects of short-term exposures.

PART II

Background Exposures

The general approach to estimating exposures and doses involves assessing incremental intakes of chemicals emitted from the facility. For some contaminants, it is essential to factor in estimates of existing body burdens and intakes from other sources. This has been done in evaluating potential health effects of lead using the IEUBK model. However, background intake of methylmercury through the consumption of non-local fish and seafood should also be evaluated in the exposure modeling. While not the case for subsistence fishermen, consumption of commercial seafood and fish is the primary source of methylmercury intake for much of the U.S. population. To decide whether or not an incremental exposure is acceptable, one needs to know the current intake levels and existing body burden of mercury for the fish-eating population. It has been calculated that a significant fraction of women of childbearing age already have an unacceptably high level of methylmercury in their diets based on estimates of seafood consumption and Hg levels in the U.S. catch [Stern, A. H., 1994, "Re-evaluation of the Reference Dose for Methylmercury and Assessment of Current Exposure Levels," *Risk Analysis* 13, 355-364]. If this is true, any increase in incremental exposure to methylmercury could present a health risk, at least for specific sensitive populations.

Biomonitoring Results

It was mentioned in the document that site-specific monitoring data indicates that lead and mercury levels in local vegetable gardens has not been increasing, but are present at higher levels than observed in other Ohio communities. These results should be included and discussed in the risk assessment.

Breast Milk Concentrations

Although consumption of mother's milk was found to a significant pathway of exposure to organics for breast-feeding infants, inorganic compounds were omitted from this pathway analysis. The point that metals are not expected to accumulate in breast milk, however, should not be used to rule out mercury which can be converted to the lipophilic organic form, methylmercury.

Contingency Plans

The accident analysis section of the risk assessment should include a description of measures taken to prevent on-site accidents, and a summary of emergency response plans that are in place in the event an accident does occur.

George F. Fries

General

1. The document is organized in a logical format. I did have a little difficulty originally in tracking the emission and deposition rates in Section IV with the exposure analysis in Section V. More cross-references between the sections would have been helpful for those who wish track residue concentrations from emissions to humans but these cross-references are not absolutely necessary.

2. The Executive Summary is a good reflection of the full document. There are no discrepancies between the conclusions in the summary and in the main document, and I did not find significant omissions. The Executive Summary would read more smoothly if Section A (Overview) and Section B (Introduction and Overview of Results) were rewritten to highlight the conclusions. Much of the material in the Introduction subsections could be omitted because it appears too detailed and its presence detracts from the readability of these overviews. This material is covered later in the Volume I.

3. The recommendations of the exposure subgroup were addressed in this assessment. Volumes VI and VII addressing ecological risks and accidents were clearly developed in response to the 1993 recommendations. Concerns raised by the other subgroups appear to have been addressed, but I am not familiar enough with the topics to determine of the responses are adequate.

4. This risk assessment is the most comprehensive site-specific assessment that has come to my attention. I have not identified significant gaps in methodology or data in the exposure area, where I have the greatest experience and background. The gaps that may exist are small and the deficiencies that may exist will be covered be the general conservatism of the assessment. This assessment is adequate for making decisions.

5. I have no specific suggestions for long term research. Within the exposure area, almost all of the parameters can be refined and improved. For example, the beef bioconcentration factors are based on one animal, and the air-to-plant factors are based on only a few plant species. Physical parameters, such as log K_{ow}, also show inconsistencies that reflect difference among laboratories in techniques. Many models depend on these parameters and more reliable values would be useful. A recent trend has been to use Monte Carlo analyses to obtain probability distributions of the potential exposure. Better estimates of both the means and the variation of the more sensitive parameters would be useful in carrying out these analyses in future exposure assessments.

__Emissions Characterization__

No Comments

__Dispersion and Disposition Modeling__

No Comments

__Human Health Risks__

Exposure

1. The high-end exposure appears to be estimated only for the subsistence farmer group in the E1 area. This group and area were identified as those most likely to have the highest exposure. The mean exposures of all other groups and areas were lower. It is likely that the relative variations of other groups will be similar. Thus, the characterization for all groups can be inferred even if the data is not provided. One might question if a normal distribution is the most appropriate assumption. Often, residue concentrations in a large sample of environmentally exposed individuals will follow a log normal distribution.

2. The assessment does a good job of describing the central tendencies for the various groups and the logical basis for combining the factors. The end exposure has on been estimated for one group, which has the highest predicted exposure. This failure has no serious consequences for the reasons noted above. The exposure assessment follows standard procedures in combining the environmental media concentrations, intake rates, and duration and frequency of exposures. Many of these procedures follow models that have been accepted as policy and the estimates follow from emission rates that follow from stack tests of this incinerator. A sensitivity analysis is appropriate for developing the bounding estimates. Overall the approach is appropriate given to current state of the art.

3. The WTI assessment has examined a fairly wide range of potential exposure scenarios. The selection of scenarios, and the conclusions concerning the importance of the various routes is consistent with the current state of the knowledge. The only significant future refinement would possibly involve a re-evaluation of the relative importance of the various animal food chain pathways. I cannot now envision any important routes that were not considered, nor do I feel that the nonfood routes will assume greater importance.

4. The key assumptions for estimation of chemical concentrations and exposures have been identified. Generally, the magnitude and direction of effects appear correct for the assumptions. There are some areas of uncertainty with regard to the concentration along various points of the chain from emissions to exposure for specific chemicals such as dioxins, but he appropriate parameters have been used based on current knowledge. Often these parameters depend on a small number of observations of only a few animals or plant species that can lead to uncertainties.

5. I agree that conservative assumptions have been applied in this assessment. The accumulative effect of these conservative assumptions is a very conservative assessment. This high degree of conservatism has not been reflected in the risk characterization section of the exposure document. I would not, however, consider this a serious problem because to the extent that this document is used for policy decisions, the conservatism of the document will be health protective.

Hazard Identification/Dose Response and Risk Characterization

I will not comment on this section other than to note the some of the specific questions to overlap with questions in the Exposure Subsection. My responses in that subsection may be referred to.

<u>Screening Ecological Risk Assessment</u>

I have no particular comments except to note that in my comments for the previous workshop, I suggested that it difficult to visualize a scenario in which the ecological concerns would outweigh the human health. My quick review of the SERA indicates that the conclusions are consistent with my suggestion.

<u>Accident Analysis</u>

I am not particularly familiar with the subject of accident analysis. My limited comments reflect intuition rather than specific knowledge.

1. The five scenarios appear to be logical. The classes are broad enough so that almost every imaginal accident is represented.

2. The specific chemicals selected are appropriate given the waste that the facility is permitted or likely to incinerate.

George F. Fries

3. No Comment.

4. No comment, except to note that these are accepted methods to deal with chemical release and dispersion.

5. Probably logical. The conclusions appear to be based on established methodology.

6. The key assumptions are reasonably stated.

7. IDLH values are a logical way to deal with one-time accidental exposures. It appears to be a standard procedure that LOCs are about one tenth the IDLH, and therefore, ten times LOC would be appropriate when there is no established IDLH value.

8. I agree that generally conservative assumptions were made.

Comments on the WTI Draft Final Risk Assessment for the Technical Workshop on WTI Incinerator Risk Issues

Thomas E. McKone
Exposure Assessment Workgroup

GENERAL ISSUES

This is a large and comprehensive document. The EPA staff and their consultants have clearly expended an enormous amount of effort in assembling large amounts of data and constructing models, running simulations, evaluating the quality of the data and the reliability and uncertainty in the estimates of exposure and risk. However, in reading the summary for this document, one gets a sense that this effort was expended mainly to comply with regulatory requirements and guidelines. Even though such compliance is necessary, if this is the only motivation for the time and energy invested in this document then it will be hard to argue that the document provides much in the way of public service. I believe that one important goal of the risk assessment is to address the impact of a process or facility on public health and to address the concerns of the affected community. In reading the risk assessment, it becomes clear to me that there is sufficient information provided to inform any intelligent debate on community health issues. However, much of the information relating to these issues is buried in text, equations, tables and appendices. Thus, it would be useful to include in the executive summary and in the introduction to the document a summary table of the local community concerns and a brief description of how these concerns are addressed in the risk assessment. Failure to express the community concerns in a risk assessment leaves one with the impression that the concerns of the regulators are all that matter.

The risk assessment is still overly focused on the conservative estimate (with respect to uncertainty) of exposure and risk at the middle range and high-end of the heterogeneity scale. This approach still fails to give some sense of the likely or plausible range of outcomes. For example, when you role a six-side die the expected outcome is 3.5, but the outcome realization is anywhere from 1 to 6. This issue is particularly relevant in the section on accidents.

It appears to me that the risk assessment fails to merge the stack and fugitive emissions and accident exposures into a single measure for expected harm within the affected community.

In my review of the risk assessment, it appears to me that much of the public health risk (in terms of likelihood of harm) is associated with accidents and not with routine stack and fugitive emissions. This does not come across in the executive summary and the introduction.

Throughout the document the justification for the models and data used are EPA guidance documents. In all cases where I was able to check the citation, I found that the model was indeed consistent with the guidance in the EPA documents. This type of cross-referencing is a useful approach for consistency and quality control. However, it is not a substitute for verification and validation. In the longer term there is a need to apply a criteria of validation against data. Since many of the cited documents are not final or still under external peer-review or under EPA-SAB review, citation of these documents should not be interpreted as consistent with reliability or veracity. Defining reliability and accuracy will require matching the results against measured data.

Organization of the Risk Assessment Document

If this document is to continue to inform the debate and planning processes within the affected community there needs to be some stated plan within the risk assessment document for how the conditions assumed for the risk assessment will be monitored and audited. There should be some discussion of continuing efforts to assess the stack and fugitive emissions source terms and conditions that give rise to accidents. There should be guidelines for how the exposure media concentrations and estimated doses in the high-end groups could be monitored to verify that the predictions of the risk assessment are within the estimated confidence bounds.

There has been a very visible and consistent effort to include an uncertainty assessment in ever aspect of the report. Nevertheless, the uncertainty analysis still often seems to be an add-in put at the end of each section and at the end of each chapter instead of being a more integral part of the assessment. One rather simple change that could improve on this problem is to move the chapter on uncertainty analysis in the Health Risk Assessment, Chapter IX in Volume V, to be presented before the chapter on risk characterization. In this way the material on risk characterization could more easily address the results of the uncertainty analysis.

Adequacy of the Executive Summary

The Executive Summary should include some consideration of the combined effect of both routine (stack and fugitive) emissions and the releases associated with accidents on public health. It would be useful to summarize the likely risk of cancer associated with both stack and fugitive emissions and explicitly define the relative contributions to risk. It would also be useful to compare the probability of health detriment from stack and fugitive emissions to those from accidents.

The Executive Summary makes note that the 1983 National Research Council (NRC) report on risk assessment served as a guide for the framework of the WTI risk assessment. There are two other NRC reports that provide guidance for the exposure assessment component of the risk assessment and should be noted here:

> National Research Council 1991, *Frontiers in Assessing Human Exposure to Environmental Toxicants*, (National Academy Press, Washington, D.C.)
>
> National Research Council 1991, Human *Exposure Assessment for Airborne Pollutants: Advances and Opportunities*, (National Academy Press, Washington, D.C.)

These reports should also be cited on page II-2 of Volume V.

Outside of Volume VII, I could not find a definition of the acronym IDLH used on page I-11 of Volume 1.

Are Major Recommendations of the 1993 Peer-Review Workshop Addressed?

In order to address this question, I went back to our 1993 recommendations and compiled them into tables. I focus here only on issues that were raised by the Exposure Workgroup in 1993. With regard to exposure assessment, the 1993 Peer-Review Group developed recommendations on the WTI Risk Assessment Plan and divided these recommendations into two categories--priority issues and issues of lesser significance that needed to be fixed or addressed as part of the EPA research plan. The priority issues are listed in Table 1. In the column to the right I have noted to what extent I believe these issues are addressed in the actual risk assessment.

Table 2, which is listed in the section below that provides my comments on the health risk assessment, covers our comments that dealt with issues of lesser significance that we felt should be addressed or fixed. Since most of these are related to the overall health risk assessment. They are included in that section.

Table 1 Priority Recommendations

Recommendation from 1993 Workshop	Was it addressed?
Existing and future exposures to other sources in the area are not addressed. This issue becomes relevant if there are future efforts to monitor exposure in this region. The risk assessment can not be used to estimate total exposure for this population but only the exposures attributable to WTI. Also, this plan does not address the regional impacts, that is beyond 50 miles, of this facility in combination with other combustion sources.	Not very well
Uncertainties need to be confronted quantitatively. The treatment of uncertainty and the issues of uncertainty and variability should be integrated as much as possible into all aspects of the report and not just included as an addendum to the section on risk characterization.	Mostly
Use of site specific data is to be commended and encouraged.	Yes
Compounds selected as surrogates for the risk analysis on the basis of quantity, toxicity, and K_{ow} should not necessarily be used to carry out validation studies unless it can be demonstrated that these are indeed also persistent compounds—as is implied by the exclusion of a persistence factor from the selection criteria	Not very well
Evaluating exposures to short-term releases—accidents, fires, equipment malfunctions, fugitive emissions is necessary.	Yes
Where possible, information on feed stocks and ash residuals should be used together with a mass balance to verify emissions estimations	Mostly

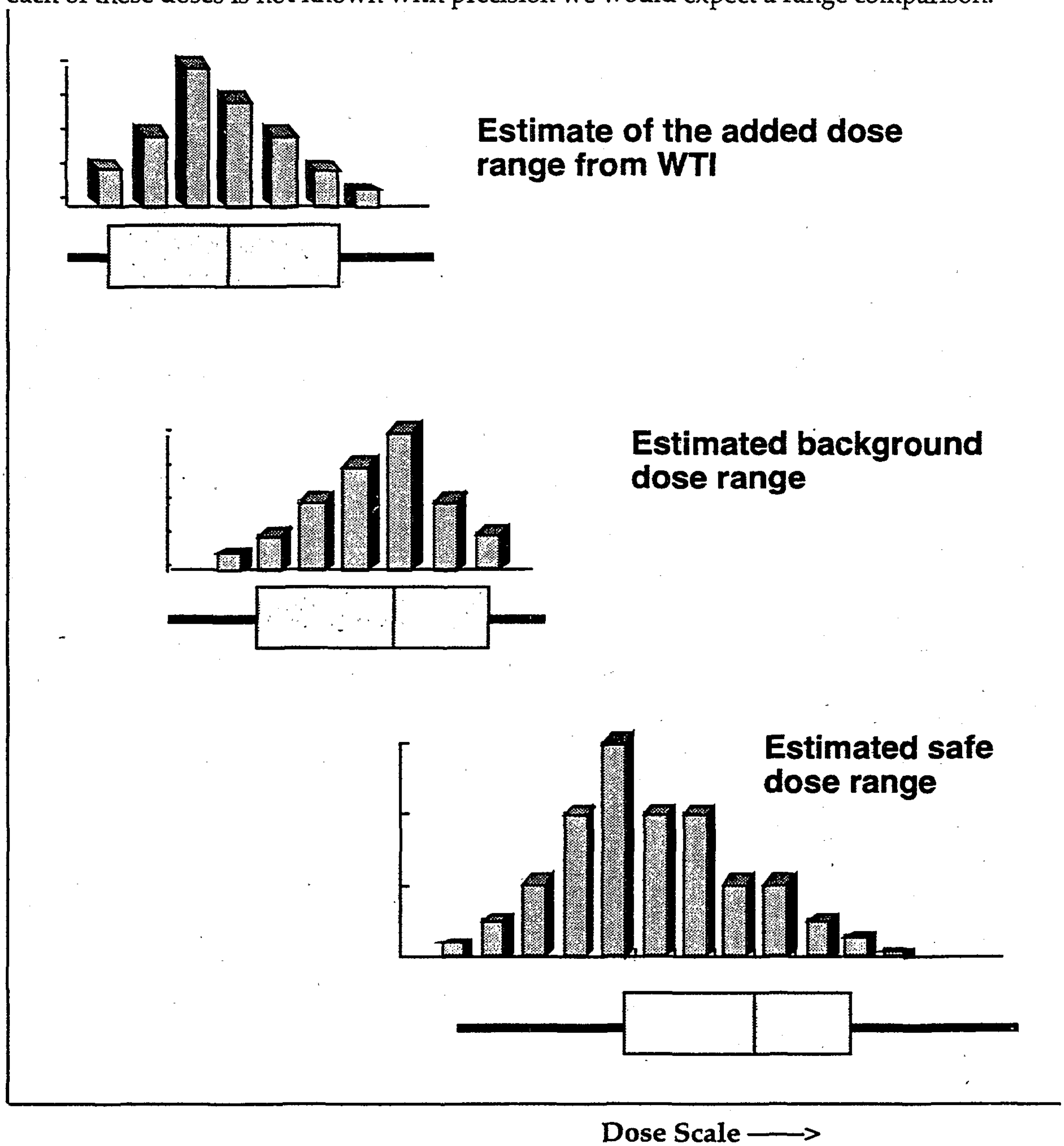
The atmospheric dispersion modeling section reflects an exemplary effort to address the concerns raised by the peer review panel.

Major Data and Methodological Gaps

As far as I can determine the document still provides very little information on existing and future exposures to incinerator-type contaminants from other sources in the area. With the newly released EPA dioxin report and with existing data bases on levels of PAHs and metals in the environment, there is ample opportunity to carry out this sort of assessment. This issue is particularly relevant if there are future efforts to monitor exposure in this region. Until the total exposure assessment for the population is included, the WTI risk assessment can not be used to inform public health assessment in this community. In Volume V it is stated that total TEQ exposure due to operation of the WTI incinerator is compared to the expected background TEQ exposure dose for individuals living in the vicinity of WTI, but I could not find this comparison.

Long-Term Research Recommendations

The one long-term issue that I believe needs to be addressed is research and development of better methods for organizing and presenting information in a risk assessment. I think the risk assessment would have been much easier to interpret if risk for each contaminant or contaminant category (i.e. CDD/CDF TEQ, PAH, particles, acids, metals etc.) is presented so that we could assess the estimated level of dose and compare it to background and to what toxicologists would consider a safe dose. Since each of these doses is not known with precision we would expect a range comparison.



HUMAN HEALTH RISK CHARACTERIZATION

As was done above, I went back to the 1993 Peer Review Report to compile a list of our recommendations regarding the exposure component of the WTI Risk Assessment Plan. Table 2 which is listed below covers our comments that dealt with issues of lesser significance that we felt should be addressed or fixed. In the column to the right I have noted to what extent I believe these issues are addressed in the draft risk assessment.

Table 2

Recommendation from 1993 Workshop	Was it addressed?
The models proposed in the plan are not suited to speciation of inorganic metals. Mercury is a particular problem.	Somewhat
The process of handling collected ash from the kiln should be included in the estimation of fugitive emissions.	Apparently
A number of groups that might be considered as high-end or sensitive exposure groups were identified. These are, (1) children who live near the facility and attend school near the facility, (2) elderly people (who apparently make up a larger than expected fraction of the East Liverpool community), (3) those who are already highly exposed to metals (i.e., lead) and dioxin-like compounds from other sources, (4) hunters of deer and waterfowl (5) very active people with higher breathing and food consumption rates, and (6) individuals who both work at the WTI facility and live near it.	Mostly
There needs to be a survey of gardening patterns within the primary plume area—that is within East Liverpool itself and up and down the river valley where contaminants might be blown much of the time.	Yes
The Biddleman model is used to determine the relative amount of chemical in gas phase versus particles in air. There are empirical parameters used in this model that have been fit using a small set of chemical data.	Yes
No discussion of exposure via household dust is included in the plan.	No
The transfer of chemicals from air to soil is by deposition, which does not explicitly include diffusion.	Somewhat
The COMDEP model does not count snow as a form of precipitation for purposes of estimating deposition of contaminants from air to soil.	No
The exposure concentrations included in this risk assessment should be reconciled with those that have been compiled in several states.	No
High-end exposure duration for breast feeding is 270 days is too low to be a high-end value.	Yes
Food consumption rates compiled for the risk assessment are based on survey data that is at least fifteen years old and should be made site specific.	Yes

Table 2 continued

Recommendation from 1993 Workshop	Was it addressed?
How much uncertainty is added to the assessment by using data from the similar incinerators and/or from the trial burn as the primary basis for estimating the source of emissions.	Mostly
There are a number of partition and biotransfer factors that are included in the model and many of these are likely to be quite uncertain. The impacts of the uncertainty in these inputs should be addressed by sensitivity and uncertainty analyses.	Yes
The results of this risk assessment will be more credible, if the variance in the input values is clearly stated and the impact of these variances on the final estimates of risk is assessed. At a minimum, this can be done by listing the estimation error or the experimental variance associated with the parameters when these values or their estimation equations are listed in tables.	Yes
A sensitivity analysis should be used to assess how model predictions are impacted by model reliability and data precision. The goal of a sensitivity analysis is to rank the input parameters on the basis of their contribution to variance in the output.	Yes
Variance propagation methods (including but not necessarily Monte-Carlo methods) should be used to carefully map how the overall precision of risk estimates is tied to the variability and uncertainty associated with the models, inputs, and scenarios.	Yes

Are the High-End and Central Tendency Exposures Properly Characterized?

The exposure media concentrations, intake rate and duration/frequency of exposure appear to be properly combined in order to characterize high-end and central tendency exposures.

Sources and Pathways of Exposure

The 1993 Peer-Review Report recommended that discussion of exposure via household dust be included in the risk assessment. The risk assessment does not consider this pathway or explain why this is not a significant route of exposure for sensitive sub groups such as infants and children. In my view, dermal and ingestion pathways for outdoor soil do not necessarily represent how these contacts occur inside houses. House dust likely originates from three sources, (1) airborne particles that penetrate from outside air to indoor air; (2) surface soil and dust tracked into buildings on shoes or clothes, by pets, or other vectors; and (3) a variety of sources related to occupant activities, material degradation, and household products.

Throughout the report, the population-averaged potential dose (for ingestion or inhalation routes) or absorbed dose (for dermal contact) is expressed as an average daily dose rate (ADD), in mg/kg-d either during a lifetime (LADD) or in some cases during the exposure duration. The general form of the expression used is as follows

$$\mathrm{ADD} = \left[\frac{C_i}{C_k}\right] \times \left[\frac{IU_i}{BW}\right] \times \frac{EF \times ED}{AT} \times C_k$$

In this expression $[C_i/C_k]$ is the intermedia-transfer factor, which expresses the ratio of contaminant concentration in the *exposure* medium *i* (i.e., personal air, tap water, milk, soil, etc.) to the concentration in an environmental medium *k* (ambient-air gases or particles, surface soil, root-zone soil, surface water, and ground water) and $[IU_i/BW]$ is the intake or uptake factor per unit body weight associated with the exposure medium *i*. For exposure through the inhalation or ingestion route, $[IU_i/BW]$ is I_i the intake rate per unit body weight of the exposure medium such as m^3(air)/kg-d, L(milk)/kg-d, or kg(soil)/kg-d. For exposure through the dermal route, $[IU_i/BW]$ is replaced by UF_i, the uptake factor per unit body weight and per unit initial concentration in the applied medium (L(water)/kg-d or kg(soil)/kg-d). EF is the exposure frequency for the exposed population, in days per year; ED is the exposure duration for the exposed population, in years; AT is the averaging time for the exposed population, in days; and C_k is the contaminant concentration in environmental medium *k*.

From my experience working with the California Environmental Protection Agency (Cal-EPA), I can report that this approach and the algorithms for calculating exposure dose listed in Section VII of Volume V are very much in harmony with the approach used by the Cal-EPA.

Have Critical Assumptions Been Identified?

In order to provide some verification for the surrogate selection process, the quantity-carcinogenic potency- bioaccumulation QCB scores of chemicals listed in Table IV-1 of Volume V should be compared to the relative contributions of each of these chemicals to total estimated risk in the actual risk assessment.

In Table VI-2 on page VI-20 of Volume V, there is a need to break out the assumption that fate and transport models are accurate into more components. At a minimum the biotransfer, diffusion, and advection (i.e. deposition) of the fate and transport models should be separated out and listed as separate assumption categories.

Were Uncertainties Confronted in an Adequate Manner?

On pages VI-14 to VI-15 of Volume V, the discussion here is particularly useful. The authors should be commended for their efforts to address both model and parameter uncertainties in their evaluation of the fate and transport models.

One item that is not made clear in the executive summary is that the ratio of high-end to central-tendency exposure (or risk) has a value much lower than ratio of the high to low end of the range of exposure estimates attributable to uncertainty. The former ratio reflects heterogeneity, whereas the latter reflects uncertainty. This means that we have more confidence about the relative values of high-end versus central tendency exposure than we do about the absolute value of the exposure. The risk assessment addresses uncertainty by biasing both the high-end and central tendency values toward the upper end of their likely range.

ACCIDENT ANALYSIS

Listed in Table 3 are the 1993 recommendations of the Peer-Review Exposure Workgroup regarding accidents, along with my assessment as to whether the recommendation has been addressed in the risk assessment.

Table 3

Recommendation from 1993 Workshop	Was it addressed?
There are three types of events that should be included in this category—upset conditions, fugitive emissions, and accidents. These events could result in larger annual releases than the routine emissions to which the majority of the report is devoted.	Yes
In the case of releases from accidents it will be necessary to make use of a combination of fault-tree studies, local transportation accidents data, reviews of operating experience, and reviews of past experience to determine both the frequency of accidents and the chemical source terms associated with these accidents.	Yes

Comment on the Conclusions Regarding Severity of Consequences and Probability of Occurrence

The report does not adequately express or communicate the expected value of harm for accidents. The accident severity and consequence information is coded into phases that are hard to interpret--such as "likely", "unlikely", etc. events and "moderate" (1 fatality?!) to "catastrophic" consequences. Based on my applications of a little fuzzy logic and what I read from the tables, I calculate roughly one in a thousand chance per year of an accident that could kill something on the order of 10 people. Does this mean that over 10 years of operation, we have a 0.1 or 10% (10x10/1000) likelihood of at least one fatality in the community as a result of accidents. If so this is a very large risk relative to the one in a million chance of cancer per individual.

Has the Accident Assessment Adequately Confronted Uncertainties?
Most of the uncertainties are dealt with in a qualitative manner this leads to concerns about the reliability of the estimates

Toxicology

The document review has been organized in response to questions posed to the "Charge to Reviewers." Only questions for which I have formulated answers are included in the attachment.

GENERAL ISSUES

1. *Comment on the organization of the risk assessment document. Does the layout follow a logical format? Is the presentation of the information in the document clear, concise and easy to follow?*

The risk assessment document is well-organized, follows a logical format, and is presented in a clear, relatively concise, easy to follow manner. The document is very well-written. Finding specific information within the document is difficult. While there is a lot of cross-references, additional cross-references (particularly with regard to chemical-specific data), would be helpful. The document could be improved if the noncancer health effects of concern were highlighted for those substances evaluated in depth.

The following suggestions would improve the reviewers ability to analyze the document in depth. The units in the document change between ppm, and mg/m^3. It would be helpful if the other units could be presented in parenthesis so comparisons could be made more easily. The cancer risks of the emitted substances are presented in the form of per mg-kg-day. Since inhalation risks are often expressed as unit risks in the form of per microgram per cubic meter, it would be helpful to have that information available in the tables as well. Many of the tables in the document, such as, Tables III-1 and III-4 have an "NL" or not listed under the slope factor column. The notation refers to whether or not information is available in IRIS or HEAST. It is not clear if the absence of a slope factor is due to it not being carcinogenic, or the unavailability of a number. It would be helpful to clarify this in the tables. The presentation of noncancer health values in Table IV-2 was changed from inhalation RAC values in Table III-1 to RfD values. It is not clear why this was changed, what the conversion factors were, or how it affects the assessment.

2. *Does the executive summary accurately reflect the data and methodologies used and the conclusions derived in the risk assessment?*

Yes, in general, the executive summary accurately reflects the data and methodologies used and the conclusions derived in the risk assessment. Overall, the methodology described for the human health risk assessment appears to be consistent with the methodology used in other risk assessments to evaluate stationary facility emissions.

4. *Are there any major data or methodological gaps that would preclude the use of this risk assessment for decision making? If so, how should they be addressed?*

The primary methodological gap identified was in the use of IDLH values in the accident analysis. As indicated below, alternate values that appear to more accurately reflect the toxicity of the substances are available. Application of these other values may increase the severity ranking of the potential consequences.

Another methodological issue of concern is the absence of evaluating the potential impact of typical emission upsets or excursions above the long-term background emissions.

A third issue would be to more clearly indicate the summation of the cancer risks, including the breast milk pathway.

5. *What long-term research would you recommend that could improve risk assessments of this type in the future?*

The primary research needed is to develop more appropriate health levels especially to evaluate potential acute health effects for both intermittent upset conditions and for accident analysis as well as to assess chronic noncancer risks.

For many of the substances without IRIS or HEAST values, California risk assessment agencies have developed toxicity-based values to allow for evaluation of emissions. Further collaboration between the Cal/EPA and U.S. EPA could improve evaluation of the health effects of complex facility emissions, such as the one described in the report.

Another primary area is to better identify the input parameters and distributions of the parameters.

HUMAN HEALTH RISKS

EXPOSURE

1. *EPA's Exposure Assessment Guidelines identify certain exposure descriptors that should be used to characterize exposure estimates. The Guidelines define high end exposure estimates as those representing individuals above the 90th percentile on the exposure distribution but not higher than the individual in the population who has the highest exposure. Bounding exposure estimates are those that are higher than the exposure incurred by the person in the population with the highest exposure. Central tendency exposure estimates are defined as the best representation of the center of the exposure distribution (e.g., arithmetic mean for normal distributions). Comment on whether or not the WTI exposure assessment properly characterizes each of the exposure estimates in terms of these descriptors.*

The EPA's Exposure Assessment Guidelines are somewhat different from the ones I am familiar with, the California Air Pollution Control Officers' Air Toxics 'Hot Spots' Program Risk Assessment Guidelines. The WTI exposure assessment appears to have properly characterized the central tendency exposure estimates based on EPA procedures, but the results for the high end exposure estimates would be underestimates under the California Air Pollution Control Officers' Risk Assessment Guidelines. Some of the areas of concern for the high end estimate are the assumptions regarding exposure duration, fish consumption, and the breast milk exposure pathway. In particular, it is unclear how the exposure duration, which appears to define the length of time in a single residence, relates to living in the particular community of concern. Furthermore, it does not appear that an evaluation was done on whether there are individuals that might subsist on fishing to a large part residing in the community, and if their exposure has been considered. The fish consumption rate is low in comparison to levels used in the California Air Pollution Control Officers' Risk Assessment Guidelines and it appears to be low due to the 11-county averaging procedure.

2. *The factors that go into estimating a central tendency or high end exposure, once the population has been defined, include the environmental media concentration, the intake rate, and the duration and/or frequency of exposure. Comment on whether or not the WTI exposure assessment does an adequate job of describing the logical procedure of combining these factors to develop central tendency, high end, and/or bounding estimates of exposure for each of the exposed subpopulations.*

The WTI exposure assessment appears to do an adequate job describing the components of the exposure estimates. The breast milk and fish consumption pathways were difficult to evaluate due the extensive material in both the body of the document and in the appendices. It is not clear if the breast milk pathway is incorporated into the final risk estimates.

3. *An important factor in an exposure assessment is identifying all of the important exposure sources. Please comment on the adequacy of the WTI assessment in identifying the important sources and pathways of exposure.*

The risk assessment appears to have adequately identified the important sources and pathways of exposure. The evaluation appears to follow standard U.S. EPA procedures.

The document refers to a concept of potential dose. This concept is confusing and its necessity is unclear. Since essentially all potency values and reference doses are based on potential doses as well, the concept does not seem to clarify the issue.

4. *Have the key assumptions for estimation of chemical concentration and for estimation of exposure been identified? Are the magnitude and direction of effect correct for the assumptions that have been identified?*

The document identifies what appears to be the two key assumptions in terms of impact: fate and transport modeling and chemical specific inputs. In both cases the best available data were used. It is unclear how the best available data result in likely overestimates of risk. Upon review of selected parameters, the values appear be within the reported range of values. Clarification on how the parameters were chosen to be conservative would be helpful.

Due to the voluminous nature of the documentation it would have been helpful to state the importance of specific input parameters on the results of the risk assessment. After reading the risk assessment it was difficult to sense the key drivers of the risk.

5. *Supposedly, conservative assumptions have been applied in this assessment to account for uncertainty. Are the conservative assumptions appropriately factored into the ultimate characterization of what descriptor best applies to each exposure estimate? Please comment on whether the uncertainties were confronted in an adequate manner. If they were not, please state what should be done differently.*

The document discusses the uncertainty in many of the assumptions made in the analysis. However, the document does not extensively discuss the uncertainty with regards to data gaps and the absence of information. This leaves the impression that the uncertainties of over-prediction are emphasized, while those of under-prediction are not mentioned in places like the summary. One example is that reference concentrations are available for only 54 of the 96 compounds listed as fugitive emissions. Consequently, the exposure to these substances cannot be considered quantitatively. Another example is that the summary does not indicate that for 77 of the carcinogenic substances emitted, the U.S. EPA does not have potencies calculated for them, and consequently their contribution to the carcinogenic risk is not considered in the evaluation.

With regard to uncertainties of exposure parameters, the choice of input parameters for Kow being conservatively selected appears unclear. The Kow values used appear to represent measured values in the literature. The range of values for a chemical may occur for a number of reasons. However, since the values chosen were from the range, it is unclear how they tend to overestimate risk as indicated in Table VI-2. Data gaps in the literature are not adequately addressed in the uncertainty evaluation.

Lifetime chronic doses could result in an underestimate of risks for the following reasons. In establishing a chronic RfC or RfD, an exposure of 1 year or more may be used. The experimental dose or study dose from which the RfC or RfD was derived may not have occurred over the lifetime of the test subject. In the risk estimate of dose, doses are averaged over a lifetime of the test subject. Thus, a higher exposure could occur a few times a year and be averaged over a lifetime. If the exposure had not been averaged, it may have exceeded the RfC for some period of time less than a year.

HAZARD IDENTIFICATION/DOSE RESPONSE AND RISK CHARACTERIZATION

1. *To select surrogate compounds for quantitative risk assessment, a two step process was used in which chemicals were ranked on the basis of emission rate, toxicity (both cancer and non-cancer), and bioaccumulation potential. Please comment on this selection process. Are the ranking factors appropriate? Could important compounds have been omitted from the analysis based on the ranking procedure?*

Yes, important chemicals could have been omitted from the analysis and this is a key uncertainty that should be discussed in the document. The current summary does not indicate: 1) many of the surrogate substances emitted (72 of 159 stack emissions, 45 of 96 of fugitive emissions) do not have RfDs or RfCs calculated for them; 2) the 300 substances considered emitted were reduced to less than 100 based on total pumpable feed processed. While this seems reasonable, the substances not considered do constitute an uncertainty.

The procedure to identify substances that bioaccumulate and have potential long-term toxicity appears to be sufficient. However, in many of the risk assessments the inhalation pathway is often the dominant pathway. For this reason, there may be important air emissions that do not bioaccumulate but are significant for the risk assessment. Were any procedures taken to determine if any such chemicals were missed? Does the procedure for selecting fugitive emissions result in identifying the chemicals of concern for stack emissions? Possibly, a calculation of emission rate divided by potency would identify the highest ranking chemicals under those circumstances.

The document acknowledges that some uncertainties are introduced into the process of identifying the surrogate chemicals. The document states that a "conservative" method was used to select Kow values and consequently the risk may be overestimated. It does not appear clear what type of decision process was used in selecting the Kow values from the ranges available in the literature. Furthermore, it is unclear how a risk conclusion can be reached in chemical selection process. It seems possible that the process could have resulted in overestimating the risk in a few chemicals and thus resulting in the non-selection of other important chemicals. Clarification of this issue would be helpful.

The assumptions regarding the selection of surrogate chemicals could use additional clarification. Table IV-13 should be revised to include the uncertainties referred to in the text of nature and the magnitude of the fugitive emissions. Table IV-13 should be revised to include the possibility of overestimating the risk of some chemicals by using high Kow values and consequently missing some important chemicals. The procedure of using the Kow value in the formula appears interesting but new to the reviewer. Other procedures often look at emissions and toxicity only. One uncertainty is whether the introduction of the Kow value into the equation gets one closer to the actual risk or not. It is not transparent to this reviewer.

2. *For the majority of the chemicals of concern, traditional approaches to dose response evaluation were employed (e.g., use of a slope factor for cancer and use of a RfD/RfC for non-cancer). However, for certain chemicals or groups of compounds a different methodology was used. Specifically, dioxins, furans, PAHs, lead, mercury, nickel, chromium, acid gases, and particulate matter were given special consideration. Was it appropriate? Have the uncertainties associated with the methodology been adequately characterized? Comment on the assumptions used due to lack of chemical specific data.*

To see a complete picture of the carcinogenic risk and the uncertainty associated with estimating it, it would be helpful to indicate which of the carcinogenic substances do not have available slope factors.

The document used a relative potency procedure to estimate the risk of PAH exposure. However, it appears that the assessment assumes that the remaining PAHs are not carcinogenic. However, IARC has identified an additional 16 PAHs as possibly or probably carcinogenic to humans. Also, a larger number of PAH compounds exhibit strong genotoxicity. It would be helpful to know how much of the PAH fraction has been evaluated based on these seven compounds. This would allow some quantification of the uncertainty of the toxicity estimation for these compounds. The OEHHA in Cal/EPA (Office of Environmental Health Hazard Assessment (OEHHA) 1993 Benzo[a]pyrene as a Toxic Air Contaminant. Part B. Health Effects of Benzo[a]pyrene. Office of Environmental Health Hazard Assessment, California Environmental Protection Agency, Air Toxicology and Epidemiology Section, Berkeley, CA.) developed relative potency factors for a total of 25 PAHs; such information could be helpful to further refine the risk assessment. An alternate comparison, that has been used by the California Air Pollution Control Officers Association, would be to assume that the remaining fraction is as potent as BAP and use that in the calculations. This could help bound the risk from PAHs and could be used as part of the uncertainty analysis.

Since the carcinogenic activities of the various chemicals are added together, the reasoning provided for not developing a cancer SF for lead does not appear convincing. The document states that since "neurobehavioral effects have been observed in children with blood lead levels below those that have caused carcinogenic effects in laboratory animals, a cancer SF has not been derived by U.S. EPA." This logic would only be applicable if one was trying to determine the most sensitive effect of lead. However, the risk assessment is looking at the toxicity of emissions from a facility. Consequently, the carcinogenic activity of lead is relevant in ascertaining the impact of the facility. Using U.S. EPA's methodology, the OEHHA (Health Effects of Airborne Inorganic Lead (Draft 1993) Office of Environmental Health Hazard Assessment, Cal/EPA) has developed a draft upper bound range of inhalation unit risks of 1.2×10^{-5} to 6.5×10^{-5} $(\mu g/m^3)^{-1}$ for inorganic lead. Such information could be derived to determine the contribution of lead to the overall estimated cancer risk.

It is important that the risk assessment treat inorganic nickel as a carcinogen as it proposes to do so. The IARC classification is based on a study that was co-funded by U.S. EPA (International Committee on Nickel Carcinogenesis in Man (ICNCM) 1990, ISSN 0365-3140 Scand J. Work and Environmental Health 16, no.1). The U.S. EPA apparently has not updated its classification of nickel compounds since the publication of this study. Consequently, it appears that the current U.S. EPA nickel evaluation may be out of date and would underestimate risk based on the best scientific information currently available. The approach taken in the risk assessment attempts to account for the more recent information and appears to be appropriate.

3. *Please comment on the selection of the overall population and the various subpopulations at risk. Were site specific data, such as the informal home gardening survey, properly utilized to identify these subpopulations?*

The document refers to the comparison of background exposure levels to those of the most highly exposed individual. It would be useful to understand how many other individuals may be close to the highest exposed, similar to the analysis done for off-site consequence analysis. Additional information on potential subsistence fisherman would be helpful.

5. *Comment on whether or not the non-cancer risks of chemicals of concern have been adequately addressed by the risk assessment? For example, has an adequate discussion of endocrine disrupters been provided which either characterizes their risks or clearly explains why their risks cannot be characterized? Further, have non-cancer chronic toxicities of dioxins and furans been adequately discussed in the risk assessment?*

Noncarcinogenic risk assessment is a difficult area to address completely due to the substantial data gaps. The report does not address the issue of data gaps to a great extent. Many chemicals of concern have not been thoroughly tested for noncarcinogenic effects. Many substances have not been adequately tested for acute effects, neurotoxicity, reproductive toxicity and many long-term health effects. Consequently, choosing even the more sensitive reported studies may not result in health levels that are protective for the untested health endpoints. This is an uncertainty worth mentioning in the report.

Some of the statements regarding reference concentrations do not completely reflect the uncertainty involved. Page III-3 states that "since the RfD is intended to be adequately protective of sensitive individuals, application of the RfD to the general population is conservative." Sensitive individuals are members of the general population; they generally include pregnant women, children, aged, and individuals with chronic diseases such asthma. Thus, the statement appears to be an overstatement of health protection. It would be better to state that the RfD is designed to protect sensitive members of the population.

It is clear from Table III-1 that RfCs were available for only approximately 41 of the 215 substances listed plus an addition 82 values bases on route-to-route extrapolation. However, route to route extrapolation may miss important irritation effects or may be masked by poor oral absorption.

The non cancer health effects evaluation focuses on chronic exposures. However, for many of the substance short-term excursions may be more important. Exposure to the acid gases and other irritants may exceed irritating levels on a short-term occasional basis, while the long-term averaged exposure is below irritating levels.

For a key group of substances, RfC values are not available for evaluation. The recent U.S. EPA health effects document on chlorinated dibenzo-p-dioxins and chlorinated dibenzofurans related compounds indicated the average levels in human tissue from background exposure was 28 picograms of TEQ/g. The report also indicated that waste incineration was a key source for dioxin emissions. Since non cancer effects are thought to act by a threshold mechanism the emissions could be considered additive to existing background levels. The sum total would be of interest to consider to determine the potential for dioxin-related health effects. The California Air Pollution Control Officers Association uses the reference exposure level of 3.5 10^{-6} $\mu g/m^3$ for the noncancer chronic health effects of chlorinated dibenzo-p-dioxins and chlorinated dibenzofurans.

The document states that it is valuable to compare the incremental exposures to dioxin-like compounds to the expected background exposure levels. However, such a comparison may suggest that background levels are acceptable or at least unavoidable. However, if the recent USEPA health effects document on dioxins and related compounds is correct, then the existing background levels are in large part due anthropogenic practices, then usefulness of the comparison becomes unclear.

For some of the chemicals listed in the document without U.S. EPA reference concentrations, the California Air Pollution Control Officers Association has developed chronic exposure values. These include benzene (RfC of 71), chlorinated dibenzo-p-dioxins (RfC of 3.5×10^{-6}), chlorinated dibenzofurans (RfC of 3.5×10^{-6}), copper (RfC of 180), dimethylamine (RfC of 2), 1,4-dioxane, (RfC of 4.0), ethyl acrylate (RfC of 48), ethylene oxide (RfC of 600), hydrazine (RfC of 0.24), lead (RfC of 1.5), toluene diisocyanate ((RfC of 0.095) and vinyl chloride (RfC of 26). If the emission rates for these substances were given in the report, it would be possible to determine how they would be ultimately scored using the procedures described.

The formaldehyde RAC value of 180 $\mu g/m^3$ in Table III-4 and an RAC value of 175 $\mu g/m^3$ in Table IV-6 (RfC of approximately 720 $\mu g/m^3$) should probably be double-checked. The value is based on a route to route extrapolation in the risk assessment. An inhalation RfC value used by the California Air Pollution Control Officers Association is 3.6 $\mu g/m^3$ or approximately 20 times lower. Logically, it would not appear that formaldehyde should have an RfC that is twice that of acetone or 3 times that of dichlorodifluoromethane. The 3.6 $\mu g/m^3$ value is based on prevention of eye and nose irritation.

In Table III-6, the assumption that RACs are developed from oral RfDs when RfCs are not available states that it overestimates the risk. This does not appear to be sufficiently substantiated since oral absorption may be much less than inhalation absorption and respiratory effects may be significant but not evaluated in oral studies.

7. *Have the key assumptions for estimation of dose and risk been identified? Are the magnitude and direction of effect correct for the assumptions that have been identified? Please comment on whether the uncertainties were confronted in an adequate manner. If they are not, please state what should be done differently.*

In Table III-6 a number of statements regarding uncertainty are made. Their basis is in some cases unclear and they may not be correct. The nickel statement regarding and overestimate of carcinogenicity is not necessarily correct. The U.S. EPA co-funded the human epidemiological reanalysis on which the IARC determination is based. The results indicated that nickel oxides encountered in the nickel refining industry were carcinogenic to humans.

It is unclear why the TEF scheme is thought to overestimate the risk of dioxin exposure. The approach reflects the best use of the data, the compounds are considered to be equal to or less potent than TCDD. It is unclear why the uncertainty is labeled an overestimate.

It is unclear why the PAH relative potency scheme is thought to overestimate the risk of PAH exposure. The approach reflects the best use of the data, the compounds are considered to be less potent than BaP. It is unclear why the uncertainty is labeled an overestimate. It is more likely that the scheme underestimates the risk of PAHs since not all carcinogenic PAHs are accounted for in the scheme.

It is unclear why the use of NAAQS values are considered overestimates of risk. The standards are based on extensive human data. The standards incorporate small margins of safety due to the high quality of the data. However, they could be underestimates of risk if an acute standard is applied to a chronic exposure. Especially in light of the data now available on particulate matter, it is unlikely that use of the current NAAQS overestimates its toxicity.

The table does not discuss several important areas of uncertainty that indicate that the overall risks could be underestimated. These areas include 1) the substances emitted which do not have RfDs or RfCs calculated for them; 2) the focus on chronic exposure effects and not the potential acute effects of repeated excursions above the chronic exposure level; and 3) the incomplete nature of the database for noncancer health effects resulting in the possible omission of critical adverse health effects.

The risk assessment makes the assumption that 100% of the chromium emissions are in the hexavalent form. This appears to be an overestimate of emissions. However, the potency slope used for hexavalent chromium in the risk assessment appears to be based on total chromium exposure. If that is the case, then the overestimate may not be that significant and would represent the relative difference between the hexavalent chromium content in the dose-response study and in the facility emissions.

8. Please comment on the overall adequacy of the risk characterization. Does the risk characterization include a statement of confidence in the risk assessment including a discussion of the major uncertainties. Are the hazard identification, dose-response assessment, and exposure assessment clearly presented? Have sufficient risk descriptors which include important subgroups been presented and discussed?

The uncertainties regarding the data gaps of emitted substances are not substantially discussed in the document as described above. Furthermore, the impact of many substances cannot be estimated since toxicity values have not been established. The document relies on a number of extrapolations from oral to inhalation toxicity. Extrapolation from oral to inhalation may underestimate risk if absorption is greater by the inhalation route, or if the effect if respiratory tract is a target organ, as it would be for irritants. This reviewer believes that such extrapolations are more likely to underestimate risk. For this reason, additional uncertainty factors are often included in such extrapolations. However, the statements in the document suggests cross route extrapolation will overestimate risk, and do not appear to be substantiated.

While the document acknowledges the potential for endocrine disruption, it does not quantify the potential risk and it does not acknowledge that by not quantifying the risk, that the uncertainty is one that tends to underestimate risk.

ACCIDENT ANALYSIS

1. *The WTI accident assessment selected five scenarios for quantitative evaluation that were considered to be of primary concern. The scenarios are an on-site spill, an on-site fire, an on-site mixing of incompatible waste, an off-site spill, and an off-site spill and fire. Please comment on the selection of these scenarios. Were any significant scenarios missed?*

The scenarios appear to be reasonable choices. However, it is unclear why a release associated with a function of the facility, i.e., an equipment failure, was not chosen. The document lists equipment failure as one of the conservative choices. Later in the document it states that the scenarios were chosen based on guidance, WTI design characteristics and accident reports in the industry, combined with the potential for significant off-site consequences and the potential for occurring within 30 years. Thus, it appears that judgment was used to select accidents with greater frequency. One question in this regard is how age of facility was accounted for in the scenario selection analysis. Are there a sufficient number of older facilities to determine the probability of failure in the 10 to 30 year age bracket?

Use of the descriptors of the releases, typical verses conservative, are unclear. They appear to be probability related. Using the FEMA guideline terminology, it seems that scenarios were chosen that were either common (several accidents a year), likely (once every 10 years) or reasonably likely (accidents once every 10 to 100 years). If that is the case it may be clearer to use the descriptors in the FEMA (1993) guidelines of common, likely, or reasonably likely in the initial description of the scenarios.

2. *Specific chemicals were selected to evaluate each scenario. Please comment on the selections. Would other chemicals have been more appropriate?*

The choice of acetone to evaluate on-site spill and off-site spill scenarios may have been inappropriate. Page III-2 states that "a key factor in evaluating the consequences of accidental releases is the acute toxicity criterion." The document further states that "the purpose of the WTI Accident Analysis is to determine the areas where, if any accidental releases were to occur, serious irreversible health effects are possible."

On June 16, 1995, U.S. EPA granted a petition to delete acetone from the list of toxic chemicals under Section 313 of the Emergency Planning and Community Right-to-know Act (Fed Reg. 60(116):31643). The Federal Register states "It was EPA's belief that there was insufficient evidence to demonstrate that acetone causes or can reasonably be anticipated to cause significant adverse human health or environment effects." The Federal Register further states "...acetone (1) cannot reasonably be anticipated to cause cancer or neurotoxicity and has not been shown to be mutagenic, and (2) cannot reasonably be anticipated to cause adverse developmental effects or other chronic effects except at relatively high dose levels." Further it states "...acetone causes adverse environmental effects only at relatively high dose levels." It is my understanding that scenarios developed by U.S. EPA indicated that severe toxic levels would not be expected to occur in any acetone releases. For these reasons it seems that acetone would not be expected to be found a concern in the WTI risk assessment. Thus, if the intent is to determine the impact of a release of commonly transported hazardous substance, another chemical should be chosen.

The choices of formaldehyde, phosgene and HCl appear to be appropriate for the accident analysis.

5. *Please comment on the assessment's conclusions on the severity of consequences and probability of occurrence. Has the report correctly categorized the severity of the consequences of the different accident scenarios? Has the assessment adequately justified the reported probability of occurrence of each of the accident events?*

As indicated below, under question 7, it is suggested that the severity of the consequences be reevaluated for formaldehyde, hydrogen chloride and phosgene. For formaldehyde, reevaluating the accidental impact based on the AIHA ERPG-2 of 10 ppm indicates that the severity ranking may increase for the conservative and typical scenario. For, hydrogen chloride, reevaluating the accidental impact based on the 1-hour SPEGL of 1 ppm or the 1-hour EEGL of 20 ppm may increase the severity ranking for the on-site fire scenario, off-site fire scenario and on-site mixing of incompatible wastes scenario. For phosgene, reevaluating the accidental impact based on the 1-hour EEGL (or AIHA ERPG-2) of 0.2 ppm would likely increase the severity ranking for the on-site fire scenario and off-site fire scenario.

6. *Key assumptions were made in the identification of accident scenarios and the description of the conservative and typical events. Included were a description of the magnitude of the effect of the assumptions and direction of the effect. Please comment on the assumptions. Are they justified? Are the descriptions of the magnitude and directions of the effects correct? Has the accident assessment adequately confronted the uncertainties involved in doing this type of analysis? If not, what else should be done?*

One assumption used in the accident analysis states that "IDLH values can be used as a benchmark to evaluate extent of possible off-site health effects." This assumption is ranked in the report as having a "high" magnitude of effect with and the direction of the effect "may over or underestimate size of area over which effects may be observed, depending on derivation of IDLH value." As discussed in response to questions 5 and 6, it does not appear that the assumption is correctly characterized. The IDLH values are not consistent acute toxicity criteria, and the available scientific data for formaldehyde, hydrogen chloride and phosgene suggest that the IDLH would underestimate their toxicity.

7. *Comment on the appropriateness of using IDLH values for characterizing the severity of consequences in the accident analysis. Comment on the appropriateness of using 10 X LOC for chemical for which IDLH values have not been established.*

Page III-3 of Volume VII of the report states:

Immediately Dangerous to Life or Health (IDLH) values established by the National Institute for Occupational Safety and Health (NIOSH) are used in the Accident analysis as the acute toxicity criteria for evaluating potential off-site consequences, because they represent a consistent, relatively comprehensive set of criteria for assessing the acute effects of short-term exposures. IDLH values are defined by NIOSH as *the maximum airborne contaminant concentrations from which an individual* could escape within 30 minutes without any escape-impairing symptoms or any irreversible health effects (FEMA 1993). This definition is consistent with the purpose of the Accident Analysis. Although the IDLH is primarily used for selection of occupational respiratory protection levels, the IDLH values represent a consistent, relatively comprehensive set of criteria that can be used to estimate the areas in an accidental release situation where people may be potentially exposed to harmful concentrations of hazardous substances. The IDLH values used in Accident Analysis can be found in U.S. EPA (1995c). The document also states the U.S. EPA chose to use the IDLH values for the quantitative evaluation to generate consistent, comparable results.

An evaluation of such an application *[PROBLEMS ASSOCIATED WITH THE USE OF IMMEDIATELY DANGEROUS TO LIFE AND HEALTH (IDLH) VALUES FOR ESTIMATING THE HAZARD OF ACCIDENTAL CHEMICAL RELEASES - George V. Alexeeff, Michael J. Lipsett and*

Kenneth W. Kizer, American Industrial Hygiene Association Journal: 50(11):598-605 (1989)] suggests that it would be inappropriate to classify chemicals for the basis of accidental release planning by using the IDLH.

The paper concluded the IDLH values were developed for the purpose of respirator selection, not permissible exposure. In this study, 84 of the 336 IDLH values were reviewed critically. For 79 of the 84 compounds (94%), the IDLH concentrations did not appear to be adequately protective for a 30-minute exposure. Comparing IDLH values to LC_{50}s, 18 compounds were in the same range as lethal levels for animals. Severe toxic effects might result from exposure to the IDLH concentrations for 45 compounds. All the NAS emergency exposure guidance levels developed for the military were below the respective IDLH values. In comparison to lethal or severe toxicity endpoints, the IDLH varied up to four orders of magnitude; and the IDLH values vary by 200-fold when compared with NAS emergency guidance levels. Thus, IDLH values represent inconsistent estimates of toxicity. Consequently, the use of IDLH values as planning guidelines for accidental releases would appear inappropriate. Several of the IDLH values have been subsequently changed; however, it is likely that the general criticism still holds as indicated below.

Specifically with regard to the substances evaluated in the document, the report made the following points. Acetone's IDLH was similar to the RD_{50} (respiratory rate depression of 50% in mice) reported for the compound. As indicated in the report, exposure to 1/10 the RD_{50} would be expected to be irritating to the eyes, nose, and throat but would be tolerable, while 1/100 the RD_{50} would cause slight to negligible irritation. The IDLH for acetone was also found to be 2.4 times greater than the 1-hour emergency exposure guidance level developed by NAS committee on toxicology to protect military personnel.

With regard to formaldehyde, the IDLH was found to be 32 times greater than the RD_{50} reported for the compound and 1/8 the LC_{50} reported in the rat.

For hydrogen chloride, the IDLH was found to be reasonably below the LC_{50} and RD_{50}, however it was 100 times greater than the SPEGL developed by the NAS.

With regards to phosgene, little was reported except that the IDLH was 10 times greater than the EEGL developed by the NAS.

The Executive Summary of the risk assessment document cites FEMA (1993) as the basis for choosing the IDLH: "FEMA (1993) also presents a four-tier system for classifying the consequences of accident scenarios. This system has been used as the basis for developing the following severity of consequence categories in the Accident Analysis: Minor - No exceedance of an IDLH ... Moderate - Exceedance of IDLH values in inhabited areas over distances of 100 meters or less ...Major - Exceedance of IDLH values in inhabited areas over distances between 100 and 1,000 meters... Catastrophic - Exceedance of IDLH values in inhabited areas over distances greater than 1,000 meters..." However, the FEMA (1993) document actually states:

> "Minor accidents are specified herein as those with the potential to have one or more of the following features: low potential for serious human injuries; no potential for human fatalities; no need for a formal evacuation, although the public may be cleared from the immediate area of the spill or discharge; localized, non-severe contamination of the environment which does not require costly cleanup and recovery efforts; no need for resources beyond those normally and currently available to local response forces.
>
> "Accidents are specified herein as of moderate severity when they have the potential to have one or more of the following features: up to 10 potential human fatalities; up to 100 potential human injuries requiring medical treatment or observation; evacuation of up to 2000 people; localized contamination of the environment requiring a formal but quickly accomplished cleanup effort; possible assistance needed from county and state authorities; only limited need for specialized equipment, services, or materials for a rapid and effective response.

"Major accidents are specified herein as those with the potential to have one or more of the following features: up to 100 potential fatalities; up to several hundred potential human injuries requiring medical treatment or observation; evacuation of up to 20,000 people; significant contamination of the environment requiring a formal and somewhat prolonged cleanup effort; assistance needed from county, state, and possible federal authorities; significant need for specialized equipment, services, or materials for a rapid and effective response.

"Catastrophic accidents are defined as those having the potential to have one or more of the following features: more than 100 potential human fatalities; more than 300 potential human injuries requiring formal medical treatment; evacuation of more than 20,000 people; significant contamination of the environment requiring a formal, prolonged, and expensive cleanup effort to protect human health and the environment; assistance needed from county, state and federal authorities; significant need for specialized equipment, services, or materials for a rapid and effective response."

The FEMA (1993) also suggested that NAS values be chosen as a first priority by stating: "Some *options*, in order of decreasing preference, and by no means mandatory for use, are as follows: use the NAS/NRC SPEGL or the AIHA ERPG-2 value for the material if one has been established; consult a toxicologist or similarly qualified individual for advice based on a formal review of the toxicity of the material of concern; use the highest value among the following:

- IDLH value divided by 10 (with "10" being a safety factor)
- TLV-STEL
- TLV-TWA multiplied by 3 (if a TLV-STEL does not exist)
- TLV-C"

Consequently, the FEMA document does not appear to recommend the IDLH. However, even if the IDLH was used in the screening for surrogate chemicals, other more specific values could have been used in the actual scenario analysis.

SPECIFIC COMMENTS ON THE USE OF THE FORMALDEHYDE IDLH IN THE ACCIDENT ANALYSIS

The choice of the IDLH to evaluate the severe toxicity of formaldehyde may be inappropriate. The IDLH for formaldehyde is provided in Table III-1 as 0.024 g/m^3 (which is equivalent to 20 ppm); it would have been helpful if the document provided the level in ppm in various tables and appropriate places since all the comparisons are made in the document based on ppm. The basis of the IDLH is Patty (1963) reported that exposure to 10 to 20 ppm produces almost immediate eye irritation and a sharp burning sensation of the nose and throat which may be associated with sneezing, difficulty in taking a deep breath, and coughing; recovery is prompt from these transient effects and that exposure for 5 to 10 minutes to 50 to 100 ppm might cause serious injury to the lower respiratory passages in man.

At the present time the AIHA ERPG-2 (AIHA 1991) appears to be a better toxicity criterion for evaluating the serious irreversible health effects of formaldehyde. This value is 10 ppm (12 mg/m^3). The value is reportedly based on Brabec, 1981; Kulle et al., 1987; Sim and Pattle, 1957, and AIHA, 1991.

Sim and Pattle (1957) exposed twelve men to 17.3 mg/m^3 (13.9 ppm) for 30 minutes. This concentration of formaldehyde caused "considerable nasal and eye irritation when they first entered the chamber, but, despite the continued mild lacrimation for some period of time, there was no marked response (pulmonary or cardiovascular) to the exposure." The eye irritation was not severe and was absent after 10 minutes in the chamber (Sim and Pattle, 1957).

The intensity of sensory irritation symptoms diminishes during exposure to formaldehyde at approximately 5 ppm, however, tolerance is lost after 1-2 hours of exposure (Brabec, 1981). The ERPG document mistakenly cites the Kulle et al. study as supporting a 10 ppm ERPG-2. No data on exposures to 10 ppm formaldehyde were available in the Kulle article. Feinman (1988) states that most people cannot tolerate exposures to more than 5 ppm formaldehyde in air; above 10-20 ppm symptoms become severe and shortness of breath occurs. The Sim and Pattle data are based on only 12 healthy men and are poorly presented. In contrast to the Sim and Pattle finding that irritation was absent after 10 minutes, Brabec (1981) states that tolerance to the sensory irritation induced by 5 ppm formaldehyde was lost after 1-2 hours of exposure. Because of the conflicting evidence and interpretations offered by these studies, as well as the failure to adjust for duration of exposure and sensitive individuals, the level may underestimate the impact of exposure to the general population. Despite the shortcomings of the AIHA value and the uncertainties in the conflicting data, the ERPG-2 is a more scientifically credible basis for the severe effects of formaldehyde than the IDLH.

REFERENCES FOR FORMALDEHYDE

American Industrial Hygiene Association (AIHA) 1991. Formaldehyde. Emergency Response Planning Guidelines. AIHA. Akron, OH.

Brabec, J.B. 1981. Aldehydes and Acetals. In Patty's Industrial Hygiene and Toxicology, Volume II A, Toxicology. 3rd. ed., edited by D.C. Clayton and F.E. Clayton. New York: John Wiley & Sons, pp. 2637-2669.

Kulle, J.T., L.R. Sauder, J.R. Hebel, D. Green, and M.D. Chatham 1987. Formaldehyde dose-response in healthy nonsmokers. J. Air Pollution Control Assoc. 37:919-924.

Patty FA, ed. [1963] Industrial Hygiene and Toxicology, 2nd edition, revised. Vol. II Toxicology. New York, NY Interscience Publishers, p. 1971

Sim V.M. and R.E. Pattle 1957. Effect of possible smog irritants on human subjects. J. Am. Med. Assoc. 165:1908-1913.

SPECIFIC COMMENTS ON THE USE OF THE HYDROGEN CHLORIDE IDLH IN THE ACCIDENT ANALYSIS

The choice of the IDLH to evaluate the severe toxicity of HCl may be inappropriate. The IDLH for HCl is provided in Table III-1 as 0.075 g/m^3. (which is equivalent to 50 ppm); it would have been helpful if the document provided the level in ppm in various tables and appropriate places since all the comparisons are made in the document based on ppm. The basis of the IDLH is Patty (1963) stating that according to Matt (1889, yes, over 100 years ago), as cited in Flury and Zernik (1931), work is impossible when one inhales air containing hydrogen chloride in concentrations of 75 to 150 mg/m^3 (50 to 100 ppm); work is difficult but possible when the air contains concentrations of 15 to 75 mg/m^3 (10 to 50 ppm); and work is undisturbed at the concentration of 15 mg/m^3 (10 ppm).

According to FEMA (1993) the best value to use to evaluate serious irreversible health effects of hydrogen chloride is the 1-hour SPEGL (Short-term Public Emergency Planning Level) of 1 ppm. The rationale states "...in connection with community exposure during space shuttle launches, the Committee recommends lower concentrations, to avoid adverse effects that might occur in a more sensitive population..." (NRC, 1987). While it appears that no supporting data is cited to justify the value, the SPEGL essentially incorporates an additional 20-fold safety factor to the Emergency Exposure Guidance Level of 20 ppm, to protect sensitive subpopulations. The EEGL of 20 ppm (29.8 mg/m^3) is based on NRC, 1987; and Kane et al., 1979.

The RD_{50} in mice for a 10-minute exposure to HCl is reported as 309 ppm (460 mg/m^3) (Kane et al. 1979). The NRC applied an uncertainty factor of 10 to the RD_{50} to account for interspecies differences yielding a 1-hour EEGL of 31 ppm. The EEGL was further reduced to 20 ppm because "of the paucity of human data." The 1 ppm value may be an overestimate of the concentration of hydrogen chloride that could produce an serious health impact. Consequently a level closer to 20 ppm or perhaps the range should be considered in the accident analysis.

REFERENCES FOR HYDROGEN CHLORIDE

Flury F and Zernik F (1931) Schadliche Gase Dampfe, Nebel, Rauch- und Staubarten (In German). Berlin, Germany: Verlag von Julius Springer, p. 128.

Kane, L.E., Barrow, C.S., Alarie, Y. 1979. A short-term test to predict acceptable levels of exposure to airborne sensory irritants. Am. Ind. Hyg. Assoc. J. 40:207-229.

Matt L (1889) Doctoral dissertation (in German). Wurzburg, Germany: Julieus Maximillian University.

National Research Council (NRC) 1987. Committee on Toxicology. Emergency and Continuous Exposure Limits for Selected Airborne Contaminants. Vol. 7. National Academy Press, Washington, D.C.

Patty FA, ed. [1963] Industrial Hygiene and Toxicology, 2nd edition, revised. Vol. II Toxicology. New York, NY Interscience Publishers, p. 851.

SPECIFIC COMMENTS ON THE USE OF THE PHOSGENE IDLH IN THE ACCIDENT ANALYSIS

The choice of the IDLH to evaluate the severe toxicity of phosgene may be inappropriate. The IDLH for phosgene is provided in Table III-1 as 0.00081 g/m^3, which is equivalent to 2 ppm.

The best value available to use to evaluate serious irreversible health effects of phosgene would appear to be the 1-hour NRC-EEGL of 0.2 ppm (0.8 mg/m^3). The basis of this level is Cameron and Foss (1941); Cameron et al. (1942). The guideline exposure of 20 mice, 10 rats, 10 guinea pigs, 10 rabbits, 2 cats, and 2 goats to 0.2 ppm phosgene for 5 hours per day for 5 days resulted in no deaths, and minimal pulmonary edema in the majority of the animals. In a small percentage of animals: 1 rat, 1 mouse, 1 rabbit, and 3 guinea pigs, massive pulmonary edema was noted. It was therefore decided that a single 1-hour exposure of humans to 0.2 ppm phosgene would not cause serious health effects. The AIHA-ERPG-2 level is also 0.2 ppm.

However, the NAS document includes a margin of safety of approximately 5, since the 1-hour EEGL is based on a 5-hour exposure, and since the concentration/time product for phosgene has been shown to use an exponent (n) of 1 for the equation $C^n * T = K$ (Rinehart and Hatch, 1964). However, additional safety factors for extrapolation from animal data, for approximation of a NOAEL, and for consideration of sensitive individuals, were not included to protect against pulmonary edema in humans. This level may therefore underestimate the risk and has some uncertainty. It is noteworthy that the results by Hatch et al. (1986), indicate the onset of pulmonary edema in several laboratory species after a 4-hour exposure to 0.2 ppm phosgene and was not considered in the development of the EEGL. It is also of interest to indicate that the AIHA-ERPG-3, has also proposed that the maximum airborne concentration below which it is believed that nearly all individuals could be exposed for up to one hour with out experiencing or developing life-threatening health effects, is 1 ppm (4 mg/m^3). That is below the IDLH of 2 ppm. Based on the above information it would appear to be appropriate to reevaluate the accidental impact of phosgene based on the NAS EEGL (and AIHA ERPG-2) criterion.

REFERENCES FOR PHOSGENE

American Industrial Hygiene Association. 1989. Emergency Planning Guidelines for Phosgene. Akron, OH.

Cameron, G.R., and Foss, G.L. 1942. Effects of exposing different animals to a low concentration of phosgene 1:1,000,000 (4 mg/m^3) for 5 hours. Porton report No. 2349, Washington, D.C.: British Defense Staff, British Embassy. Cited in the NIOSH criteria for a recommended standard for occupational exposure to phosgene. DHEW/PHS/CDC [NIOSH] Pub. No. 76-137). Washington, D.C.: U.S. Government Printing Office, 1976. p.52.

Cameron, G.R., Coutice, F.C., and Foss, G.L. 1942. Effects of exposing different animals to a low concentration of phosgene 1:1,000,000 (4 mg/m^3) for 5 hours. Porton report No. 2349, Washington, D.C.: British Defense Staff, British Embassy. Cited in the NIOSH criteria for a recommended standard for occupational exposure to phosgene. DHEW/PHS/CDC [NIOSH] Pub. No. 76-137). Washington, D.C.: U.S. Government Printing Office, 1976. p.52.

Hatch, G.E., Slade, R., Stead, A.G., and Graham, J.A. 1986. Species comparison of acute inhalation toxicity of ozone and phosgene. J. Toxicol. Environ. Health. 19:43-53.

Rinehart, W.E., and Hatch, T. 1964. Concentration-time product (CT) as an expression of dose in sublethal exposures to phosgene. Ind. Hyg. J. 545-553.

8. *In the accident analysis, IDLH (or 10 X LOC) values were used to determine the downwind distances over which adverse human health effects might occur. To evaluate the uncertainty introduced by using the IDLH, a sensitivity analysis was conducted where these distances were recalculated using the LOC (a more stringent health criteria). Other sources of uncertainty that are identified in the accident analysis include concentration averaging times, chemical concentrations, emission rates, and meteorological conditions. For most of these parameters it is stated that conservative assumptions were used to avoid underestimating risks. Have the uncertainties inherent in the accident analysis been adequately characterized? For those parameters where sensitivity*

analyses were not conducted, is the conclusion that conservative assumptions have avoided underestimation valid?

Use of the LOC values for phosgene, formaldehyde and hydrogen chloride appear to be suitable for the accident analysis. This is based on information provided in response to question 7. The LOC values are very close to the levels suggested in the response to question 7. Consequently, they would be expected to reflect the impact of the accidents more appropriately than use of the IDLH.

The uncertainty analysis considers the implication of a shorter averaging time. This may be a helpful method. Another method would be to consider the relative exposure-dose based on a concentration^n x time metric. The value of n can be based on empirical evidence where the value is 2 for formaldehyde and hydrogen chloride and 1 for phosgene (American Institute of the Chemical Engineers, Guidelines for Chemical Process Quantitative Risk Analysis, Center for Chemical Process Safety, New York, NY,. p.156). The exposure-dose comparisons may provide a helpful way of comparing whether a slight change in the scenario is significant (Alexeeff, G., Lewis, D., and Lipsett, M. (1992) Use of toxicity information in risk assessment for accidental releases of toxic gases, J. Hazard. Mater. 29:387-403). This procedure would take into account the increased importance of concentration relative to the contribution due to time for formaldehyde and hydrogen chloride. The major limitation of this, and the suggested method in the risk assessment, is that at some point exceedance of a certain concentration may result in exceedance of an additional effect threshold.

Mary E. Davis

Risk Assessment for the Waste Technologies Industries Hazardous Waste Incinerator Facility at East Liverpool, Ohio

General Comments

This risk assessment is extensive, in that it covers a wide variety of potential exposure scenarios and develops risk characterizations for them. The uncertainties of the various elements have been considered and are well documented. The organization is reasonable given the mass of the material. Recommendations of the 1993 peer review workshop seem to have been followed.

Hazard Identification/Dose Response and Risk Characterization

1. Selection of surrogate compounds.

The procedure gave equal weight to emission rate, toxicity and bioaccumulation potential. The basis for this is not clear. The three parameters seem to have similar, broad, ranges and so equal weighting probably is reasonable.

There is still some confusion as to which chemicals have actually been detected in stack emissions and which are predicted to be there based upon modeling. It would be less confusing if potential emissions were labeled as such. For example, Table I-1 would be more appropriately labeled "Substances of Concern in Potential Stack Emissions."

2. Special consideration for dose-response evaluation of some chemicals.

The methods used to characterize cancer and non-cancer risks are very different and the expression and interpretation of the results could lead to confusion. The methodologies for PAHs, lead, mercury, nickel, chromium, acid gases and particulate matter seem reasonable. The approach for dioxins and furans is to not estimate non-cancer risks.

The non-cancer risks of dioxins and furans have not been characterized because EPA has not yet determined the reference dose/concentration, at which no adverse effects are expected. In the chapter on toxicity assessment (V/III), the context suggests the reference dose has not been determined because there is not agreement as to which effects are toxic and which effects are ancillary, and not to be considered as toxic and therefore the reference dose cannot be determined. There are data on dose-response relationship of specific toxic effects and a better solution would be to estimate the risks of these specific adverse effects individually rather than not estimate non-cancer risks because the "global" risk cannot be estimated. Reproductive/developmental toxicity is one non-cancer toxicity for which there are good data, and for which there is serious concern. There are data that suggest that reproductive effects occur at lower exposures or body burdens, so that estimating the risk of cancer is not estimating the most sensitive outcome. The problem is the perception that reproductive/developmental and immune toxic effects occur at doses lower than those that cause cancer. Therefore, while the risk from cancer is in the range of $3.2X10^{-5}$ to $5.8X10^{-8}$ the other effects will occur at lesser exposures and therefore will be more prevalent.

3. *Population and subpopulation selection.*

Seems to be appropriate, in that it addressed both a "representative" or likely exposure and an upper end exposure.

4. Appropriateness of risk estimate terms "average risk" and "maximum risk"

While reading the risk characterization chapter, I understood the meaning of the terms "area average" and "maximum concentration" however, one could easily misconstrue the meaning of "maximum concentration" in the tables if one did not read the text closely. Tables should have sufficient headings or footnotes so that one can discern the results being presented without having to refer back to the text. My impression of the text is that the terms "average risk" and "maximum risk" were used with qualifiers so that it was clear that maximum referred to an even smaller region that, because of differences of dispersion, had higher chemical concentrations than the subarea as a whole. I would not use the term "maximum risk" to refer to the risk associated with this exposure, however, as that would be misleading.

5. Non-cancer risks adequately addressed?

The discussion of endocrine disrupters does explain why the risks cannot be characterized. However, it is not clear why endocrine disruption has been considered, and which chemical(s) are endocrine disrupters.

The non-cancer risks of dioxin have not been characterized, as discussed in #2, above. Dioxin is a major concern with this facility and its' non-cancer risks should be addressed.

6. Additivity/synergy uncertainties

This was discussed only briefly in V/VIII. It would be useful to include discussion of what is known about interactions among chemicals, particularly at relatively low concentrations (such as interactions mediated by changes in metabolism are unlikely because at low exposures, the amount of enzyme does not limit the amount of metabolism and therefore formation of reactive intermediates).

7. Identification of key assumptions

Overall, this seems to be well done. The tables are particularly useful.

8. Overall adequacy of risk characterization

The risk characterization as presented is as readable as I suspect such a document could be. Much of the information is in tables and table titles, column headings and footnotes sometimes could be more detailed. The question of non-cancer dioxin risks was not addressed as well as it should have been and that undermines the adequacy of the risk characterization.

Accident Analysis

General comment:

The impact on East End Elementary School is a major concern for the accident analysis. The use of population densities seems to hide the fact that an elementary school full of children is within 1000 feet of the site. How many children are in the school? The truck route also goes by Garfield School. What kind of school is this? How many students? Are deliveries expected during school hours or afterwards? How much is the school exposed under the various meteorological conditions, or under what conditions is the school more likely to be exposed?

The accident scenarios and chemicals selected seem to be reasonable choices for exploring the effects of a range of accidents and conditions under which the accidents occur. In the discussion of the severity of occurrences, I would have liked explicit reference to the East End Elementary School. I suggest that the discussion of the probability of occurrence include a more detailed presentation of the rationale for the final probability result (combining the probability of the accident occurring under the specific meteorological conditions).

The sensitivity analysis of the IDLH vs. LOC was illuminating and disconcerting. It presents a reasonable case for not using the IDLH for this type of analysis. The IDLH is designed to protect healthy adult male workers from severe consequences. Children are likely to be more sensitive. At elementary school age, many childhood asthmatics have not yet outgrown their asthma. Also, elementary school children have colds and other respiratory infections more often and so are less able to deal with additional respiratory challenges. Another component of the IDLH is that the individual will leave the environment so that exposure is of a short duration. The prospect of evacuating an elementary school is daunting, although I suspect a "shelter in place" strategy would probably be the more reasonable response.

Comments on WTI Draft Final Risk Assessment (December, 1995)

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I have focused my attention on the following: Executive Summary (Volume I), Facility Background (Volume II), Human Health Risk Assessment (Volume V), Screening Ecological Risk Assessment (Volume VI), and Accident Analysis (Volume VII). Most of my comments have been directed at these volumes. I have also briefly reviewed Facility Emissions (Volume III) and Atmospheric Dispersion and Deposition Modeling (Volume IV) in order to obtain general view of how certain data and principles applied in the Human Health Risk and Accident Analysis volumes were generated.

I. General Comments:

1. Overall the risk assessment appeared to be well organized and presented in a logical format. I liked the idea of presenting the detailed discussion of certain items and the bulk of the data as Appendices, while focusing on the main and important thrust of the particular volumes within individual chapters. Nevertheless, due to the (mostly) thorough nature of the document and the number of volumes and pages necessary to contain all of this information, I found the document at times difficult to wade through, especially when searching for particular information. Better cross-referencing would have been useful. For example, when a point is made about a particular piece of data, the page number and or Appendix page number would have been useful beyond just giving the Volume or Chapter number. In most cases the text was well written and concise. However, each chapter, for the most part, was very thorough and there was enough redundance of explanation and information to allow each to stand on its

own while referring back to other sections for specific details and data.

2. Executive summary: For the most part the Executive Summary appears to reflect the data, approaches used, and conclusions derived. It is emphasized often, and appropriately, that in most cases conservative approaches and assumptions were used so that the data likely represents an overestimate of the risk.

3. There were several major modifications suggested by the 1993 workshop.

The recommendation was made for additional performance tests to develop more reliable estimates of emissions have been performed. These have added to the reliability and accuracy of the assessment.

Within the context of the Human Health Exposure Assessment, the 1993 workshop also recommended an updating of food consumption data and the inclusion and/or consideration of other exposed population groups. These have been adequately addressed and/or considered by the present document.

The workshop suggested more consideration be given to uncertainties and variability. In the present document, much additional effort was made in this document for assessing uncertainty and variability, in particular which parameters and/or measurements were most likely to be highly variable and the likely degree of variability. There were also many factors which contributed to the uncertaintly of the either the assumptions being made or the data being used. These were, for the most part, also appropriately discussed and/or documented. Some exceptions are discussed in my specific comments below.

A recommendation was made to consider in more detail upset conditions, fugitive emissions, and accidents. For the most part this has been adequately addressed. Some further considerations are detailed below.

For particular chemicals, the physical and chemical form of several of the metals was identified as influencing transport. Where appropriate, and where data was available, this has been considered by the present document. In many of these cases, only limited data was available, and the conservative, but not totally unrealistic, approach was applied.

The workshop recommended that an ecological risk assessment be conducted. A laudable, and what appears to be a reasonably thorough, attempt was made at this risk assessment. However, I have made a number of specific recommendations below.

The consideration of several additional compounds was recommended by the workshop. In some cases, the contributions to the total risk was minimal. However, for others, especially for certain polycyclic aromatic hydrocarbons, the contributed risk was considerable. Thus, this turned out to be a valuable suggestion by the previous workshop.

The workshop also recommended a consideration of the additive and/or synergistic effects. Although the present document has considered these interactions, very little and specific data is available that could be reasonably used for this risk assessment. The method of adding risks for these compounds is actually one of the difficulties which, in my opinion, has not been adequately discussed. My specific comments are noted below.

It would have been useful for a statement to be made if in fact all the recommendations by the workshop were considered in this document. It is indicated on p. IV-1 that "..a concerted attempt has been made to incorporate the recommendations provided by the Peer Review Panel." It would have been useful to indicate, perhaps in a Table, what these recommendations were, if or if not these recommendations were incorporated, why or why not they were incorporated, and if incorporated, what was the overall effect on the risk assessment process. From what has been presented, the reviewer does not know if there were some that the present document did not consider? This should have been more specifically, and likely very simply, addressed.

4. As indicated, "..there are always additional data and method development efforts that could be undertaken to reduce the level of uncertainty". The question has been put forth of whether there are serious data or methodological gaps in this particular assessment that would preclude its use in a decision-making process. Much of the risk assessment described in this document depends on the accuracy and reliability of the models used for predicting concentrations of particular chemicals in environmental receptors, i.e. soil, foodstuffs, water, and in human tissues exposed to these chemicals either directly or indirectly. These predicted concentrations are given in some of the Appendices of Volume V. Realizing that the concentrations used for this risk assessment are those predicted to be contributed by the WTI facility, it would have been extremely useful for an additional section (in the uncertainty analysis ?) to compare these concentrations to present, i.e. "background" levels, in environmental receptors and human tissues. Certainly there is enough literature already available to indicate what these "background" levels might be. If data were available from the locality under consideration, so much the better. The lack of this comparison, in my opinion, decreases the reviewers confidence in the models being used, despite their apparent theoretical goodness. One would predict that the numbers generated from the models would be substantially less, but within at least 1 to 2 orders of magnitude, than the determined background levels. If the predicted numbers happened to be substantially lower or any higher, then it would be apparent that something is wrong with the particular model, the assumptions made, and/or the data used. Again, if nothing else, this comparison would have increased confidence in the models being used. No such comparison was apparent in this document, and I would recommend that such be made for each chemical of concern prior to the use of this risk assessment for any decision

making process.

For the most part very conservative assumptions have been made in this particular risk assessment process. One of the most conservative assumptions is that the risk is additive for all chemical exposures. With some exceptions, i.e. the dioxins and dibenzofurans, for which there is reasonably good data, this is probably an unreasonable assumption and for which there is little or no data. I would recommend that any the risk be based on the most hazardous chemical group, e.g. the dioxins, without the consideration of additivity from the other chemicals unless reasonably good data were available.

5. Long-term research: This reviewer would recommend that more effort be made to gather real world numbers in terms of concentrations in environmental receptors and the contributions of various sources to these concentrations. Although, as indicated above, the models, in most cases seem theoretically appropriate, real world data would be much better not only to estimate risks more accurately but to test the models. Thus, more research should be made into testing the models developed and their parameters under a real world situation.

II. Comments on Volume I, Executive Summary:

1. General Comments: At least for the Human Risk Assessment and Accident Analysis volumes, the Executive Summary is an accurate reflection of these both in specific data and conclusions from the data. Importantly, it has also been emphasized that in general very conservative assumptions have been used, and thus the predicted risks are more likely to be overestimated than underestimated.
2. p. II-3 and elsewhere throughout this risk analysis: Since this is a scientific analysis, the temperatures should be presented as degrees C, rather than degrees F.

3. pp. III-1 through III-3 discuss Incinerator Stack Emissions. If not already done in the specific volume where this is discussed more thoroughly, somewhere in the document it should be indicated how efficiency of combustion changes over time and usage, how often maintenence procedures are performed and how these procedures (or the lack of such procedures) are likely to change the combustion efficiencies and thus the emissions.
4. p. IV-4: It is indicated that the use of the CALPUFF and ISC-COMPDEP models indicates that "..the inclusion of calm wind dispersion and fumigation does not have a significant effect on the peak predicted concentrations from the WTI incinerator stack". This is an important conclusion, but seems counter-intuitive. This might be explained in more detail here. Are there any real data to back up the suggestions of the models? Here again, as noted above, although the models are undoubtedly useful, it seems necessary to consider real data and compare the predicted to actually observed data whenever possible. For such important conclusions, if no real-world data is available a statement should be made indicating such.
5. p. V-3, Fugative Emissions: Here is should be mentioned if there should be any consideration of groundwater contamination and why or why not. In addition, although this might have been considered elsewhere, what are the concerns, if any, to the workers at the WTI site? Are workers who may <u>also</u> be highly exposed by residence or life-style an additional population to consider?
6. p. V-10: Here for "Fugative Emissions" it should be stated specifically for which subgroup the risk estimates are given.
7. V-11:. Regardless of the very conservative assumption of additivity of risks and hazard quotients across all exposure pathways, it seems inappropriate do this simply since there is no evidence to indicate (with the exception of the dioxin-like compounds) that the risks are additive. This seems to be an

overly conservative assumption. If there is any basis for such an assumption is should be clearly explained.

III. Comments on Volume II, Introduction:

1. p. II-2: Although this reviewer realizes that this is not the central focus of these risk assessment document, there is some concern likely to be generated (and there should be some real concern) that regardless of the risk estimates from the WTI facility alone, the facility may be contributing to an existing problem for the population of the area due to the presence of other industries and other sources of area pollution. This is especially true since there are a number of coal-fired power plants in the area. For example, it would be useful to have some data on the relative levels of dioxin-like compounds and PAHs in cattle, milk, and fish in the area. In addition, environmental conditions, especially during inversions, may allow very high air levels of a variety of air pollutants. These points should be addressed, even if only in a general sense, somewhere in this document.
2. p. II-4, line 8: The phrase "..if appropriate.." should be briefly explained here.
3. p. II-5: It should be specified what happens to the sand and activated carbon that is used to treat the contaminated water. Where does this go? Likewise on p. II-7, it should be indicate where the collected fly ash is taken for "..treatment and disposal..".

IV. Comments on Volume V, Human Health Risk Assessment:

Comments Re "Charge to Reviewers"

1. General Comments: Well organized and documented. Methodology and approaches are appropriate. The choice of surrogates is well rationalized. The food consumption data has been updated as requested by the 1993 review. In addition, the document has now considered the PAHs - this is an important addition to the document considering the relative contribution

these compounds have to the total risk assessment. Fugative emmission are considered in more detail, although nothing is mentioned of any potential groundwater contamination. Additive and synergistic effects are considered, although little can be done since little data is available.

2. Characterization of the exposure estimates in terms of "central tendency", "high end", and "bounding" exposures: The terms of "central tendency" and "high end" exposures are certainly well defined at the beginning of Chapter VII on Estimation of Exposure Dose. However, throughout the text the terms of "average exposure" and "maximal exposure" are used in their places. This seems appropriate and clearly understandable. The term "bounding exposure" is rarely used elsewhere in the text. In my opinion, the risk characterization for a "bounding exposure" seems inappropriate and not needed.
3. Procedure for combining environmental media concentration, intake rate, and duration / frequency of exposure to develop estimates of exposure: This procedure was well described, and in most cases was based on sound and documented rationale. Where assumptions were made they appeared to be clearly explained with much consideration of uncertainty analysis. In fact, it took a great deal of effort to wade through the documentation and explanation of models, etc. because it was done very thoroughly.
4. Identification of Important Sources and Pathway Routes: Very thorough and clear approaches. All of the significant pathways of exposure appeared to be identified. With one exception all of the significantly exposed subgroups appeared to have been identified. One subgroup that was not considered was WTI workers who may live in the area. Should this group have been considered?
5. Key assumptions for estimation of chemical concentrations and estimation of exposure: Most of the assumptions are conservative and would tend to overestimate these concentrations. Where there is a possibility of an

underestimate due to an assumption the magnitude of the effect tended to be low. An exception might be the assumption that inorganic compounds do not accumulate in mother's milk. This should not be assumed as milk would be a significant route of exposure especially for compounds such as methylmercury. If this assumption is made it should be backed up with referenced data most of which likely exists. Further literature searching is needed here. In fact, this relates to my main criticism of the document - lack of referencing real world data for which there is likely much available. This referencing would allow us to have greater confidence in the models being used.

6. Uncertainties: This risk assessment process is full of uncertainties and I think that the document does a reasonable job in confronting these and appropriately pointing out what they are, why they are uncertainties (i.e. lack of data, or variation in day-to-day environmental conditions, etc.), how they might affect the process (either an underestimate or overestimate), and what the general magnitude of the effect might be. In most cases a Table is presented at the end of each chapter to identify these uncertainties and indicate their nature and possible affect on the risk assessment.
7. Selection of surrogate compounds: The chosen ranking is appropriate.
8. Dose-response evaluation for chemicals: The dose-response evaluation for the dioxins/furans is based primarily on TEFs. This is appropriate given the mechanistic data, albeit the uncertainties surrounding the values for the TEFs. The PAHs are given relative potencies from zero to one, with benzo(a)pyrene given a potency of 1.0. As an interim approach, this seems appropriate. The lead data is based on a model predicting blood levels in children. Given the greater sensitivity of children, this is entirely appropriate. The considerations for mercury are based on its inorganic and organic forms. Given the inportance of the organic forms and the documented recommendations, these considerations are appropriate. There are also

forms of nickel that may or may not be carcinogenic. The assumptions for nickel that it all exists as an inhalation carcinogen are very conservative, but nevertheless, appropriate for this case. Similarly the assumption for chromium is very conservative. For acid gases and particulate matter the NAAQS values are used. This is appropriate.

9. Identification of subpopulations: In many cases, the use of non-site data was appropriate given the lack of good data and the cost and time of obtaining good data. In other cases, such as for subsistence farmers and the consumption of local meat some site specific data, although some more formalized than others, were used to identify subpopulations at risk and estimate exposure rates. This is actually better than what I would have expected and was glad to see some consideration of local data, although it is not clear how accurate these data are. Nevertheless, it provides some directions for estimation of these parameters.
10. Average vs maximum risks: The term "average risk" is appropriate given the nature of the paramaters, i.e. average emission rates, average air dispersion/deposition, and "typical" exposure factors. There might be some consideration as to whether the "maximum" risk should be based on maximal emission rates. This would be a conservative approach - and this reviewer thinks actually too conservative. Given the facility operations, it is highly unlikely that maximal emission rates will ever be approached. Thus, the use of average emission rates for maximum risks seems appropriate, also given the very conservative assumption of maximum air concentrations.
11. Non-cancer risks: It is this reviewers opinion that given the available data, these risks have been adequately addressed. Given the paucity of data in most cases, the discussion of "endocrine disrupters" would have been inappropriate here. Nevertheless, it is an area of concern that should be addressed at later dates once more specific data is available. For the

dioxins/furans, the numbers used for the cancer risk are likely overly conservative and, at least based on the available data, would likely protect against the non-cancer risks from these compounds.

12. Additivity and synergy: Given the data available, the discussion is appropriate. As indicated elsewhere in my review, it is my opinion that the additivity of risks for all compounds is very very conservative. This should have been given more discussion.
13. Assumptions for estimation of dose and risk: As noted above the assumption of additivity for individual chemicals (p.VIII-55, Volume V) is certainly a very, very conservative assumption. This should have been given more discussion to point this out very clearly. Given the paucity of available data the noncancer health risks due to infant ingestion of breast milk are not considered. This should be given more discussion given the noted effects of some of the metals, i.e. lead, methylmercury, and even dioxins (some recent data) on learning and behavior.
14. Overall adequacy of risk characterization: With the exception of the above - seemingly adequate.

Specific Comments

1. III-5, 1st sentence of first full paragraph: This is not quite true since certain other congeners which do not have chlorines in the 2, 3, 7, and 8 positions also have dioxin-like activity. However, the potency of most of these is very, very low on a relative basis. This reviewer would recommend a qualification of something like "...as displaying dioxin-like activity of significant potency".
2. p. III-6, line 8 from bottom: Eliminate the term "hydroxylase" here. Also the next sentence add "Based partially upon differences..." since the receptor binding data was not the exclusive data upon which the TEFs have been developed.
3. p. IV-2: Here it is indicated that 31 compounds were not evaluated in this

assessment because emission rates were not available. Somewhere in the document a table should show what these compounds are.

4. Chapter VI, Estimation of Environmental Concentrations: As noted in I.4 above, some comparison of the predicted WTI-contributed concentrations to real world concentrations would be useful. In my opinion, this is a very important deficiency of this document.
5. p. VII-18, 1st sentence: For completeness some brief explanation of how it was determined that "..plant uptake of vapor-phase dioxin/furans is the primary contributor to total risk" is needed here. It simply could be indicated that consumption of plants is the major contribution to the body burden of dioxin/furans in cattle. (See p. IX-1- in Volume V as well.)
6. p. VII-18, 4th line from bottom of 2nd paragraph: Shouldn't this be Appendix IV-4 instead of IV-1?
7. p. VIII-1, 1st sentence: In Chapter VII only the dose estimates for the adult subsistence farmer in subarea E1 are presented. Are the data for the other population supgroups somewhere else?
8. p. VIII-2, B, Estimation of Risks Due to Stack Emissions: For the noncarcinogenic effects it would be useful to list for each compound what the particular noncarcinogenic effect was. This might either strengthen or weaken an argument for assuming additivity of risks. Likewise for carcinogenic effects, it should be indicated whether a particular type of tumor or total tumors are being considered.
9. p. VIII-8, c, Potential Health Effects...and Particulate Matter: For completeness, it should be mentioned whether or not the maximum predicted concentrations are during a period of inversions or other environmental conditions. Again, it would also be useful to know the real world concentrations of these pollutants in the particular area of interest.
10. p. VIII-2, I, Estimated Risks Due to Fugitive Organic Vapor Emissions: Data is given for the lifetime cancer risks due to average exposure. What about

maximum exposure? (Are these numbers a concern for workers at the plant who might live close to the facility??)

11. pp. IX-1 and IX-2: Here again, some comparison with real data would assist in the uncertainty analysis.
12. p. X-3, Cancer Risks, 1st paragraph: There should be some clarification here to distinguish more specifically the average total cancer risk for each of the subpopulation groups vs the highest cancer risk for the subsistence farmer. For the latter, shouldn't this be "the cancer risk for maximal (or highest end) exposure"? The way is is presently worded it sounds like of the subpopulation groups the subsistence farmer has the greatest risk for the average total cancer risk.
13. Appendix V-5, p. 2, Individuals Who Work at WTI: It is stated that "evaluation of worker exposure ....is beyond the scope of the WTI Risk Assessment". What if those same workers also live in the area that is predicted to be the most heavily contaminated? It would seem that this is a population that should be considered. If indeed this is beyond the scope of the Risk Assessment, specific reasoning and rationale should be given.
14. Appendix V-8, p. 1, bottom 7 lines: It is not clear why only 6 year exposure is used for chilhood exposures. Shouldn't this be extended to cover the age until the end of high school. Is this inconsistent with what is stated on the top of p. 12?
15. Appendix V-8, p. 40, Table 17: It is not at all clear why 1) (as noted above) ED is 6 years for both the child resident and school-age children, 2) the assumed BW for these groups are different, and 3) the LT value is same here across all subgroups. This is not obvious here and should either be explained or cross-referenced to somewhere else in text where it is explained.

V. Comments on Volume VI, Screening Ecological Risk Assessment:

1. p. I-2, bottom 8 lines: The differences among SERA, PERA, and DERA should be explained more thoroughly in terms of the data base used, the assumptions made, etc. This is especially important since the present SERA indicate possible risks for certain chemicals that would have to be considered in more detail by the PERA and DERA.
2. p. I-6, line 5 from top: It has not been explained why a 30-year accumulation of the chemicals has been assumed.
3. p. II-9, Indicator Species: Based on the chemicals of interest, would it not be good to choose a most sensitive species? The indicator species chosen, although perhaps representative of certain groups, may not truly cover a most sensitive species. Data should be available, again based on the chemicals of interest, for a most sensitive species for each chemical. If these species are within the area under consideration than this most sensitive species should be represented.
4. IV. Identification of Ecological Chemicals of Concern: Is there any consideration of the possibility that the existing levels of certain chemicals may already be high in the area of concern due to the amount of industrialization present? Will the WTI facility contribute to a problem that already exists? This should be addressed somewhere in the document. As this reviewer noted for other volumes, some data on already existing levels of certain chemicals already existing in the area of concern would be useful.
5. p. IV-4, B, "Development..., last 2 sentences of 1st paragraph: Despite the discussion, it is not clear to this reviwer why the emission rates used in the SERA differ from that used in the HHRA. The discussion should be more precise - but obviously brief.
6. p. IV-11, 1st paragraph: For the aquatic organisms, the discussion should mention if all stages of development are considered vs just adult animals. For example, it is well known that developing fish are extremely sensitive to

dioxins/furans. I believe that the endpoints examined (in Appendix VI-22) do cover fish embryo, eggs etc, but this should be mentioned here as well.

7. V, Characterization of Exposure: Here it is indicated that "the potential for adverse effects to ecological receptors....is a function of.....(2) the concentrations of chemicals in the media to which the receptor is likely to be exposed...". Without a consideration of what is already there from other contributing sources, this analysis might be considered somewhat useless. It would be worthwhile to have some real world data regarding the concentrations of some of these chemicals that may already be present in the particular environment. This data might also help to have some confidence in the modeling that is being used to estimate exposures.
8. p. V-61 and in other tables in this chapter: For the total dioxin/furans - is this as TEQs?

VI. Comments on VII, Accident Analysis:

<u>Comments Re "Charge to Reviewers"</u>

1. Selection of Scenarios: A number of scenarios were initially considered as potential accident scenarios. These included the ones finally selected for analysis as well as others including, for example, failure of air pollution control equipment. A number of criteria were used to make the final selections. These were well justified and had good rationale. The final selections were appropriate.
2. Selection of Chemicals: The selection of chemicals was based on the substances handled at the WTI facility, acute toxicity indices, volatility, and estimated maximum concentrations in waste.These criteria were appropriate. It might have been useful to consider <u>persistence</u> as an additional criteria for off-site spills, especially where ground and/or surface water might be contaminated.
3. Chemical Release Rates: With two exceptions the methodology used and

assumptions made seemed to be appropriate and mainly conservative. The exceptions are the assumptions surrounding ambient temperatures and roadway sites. In both cases worst-case, but nevertheless real, parameters should have been used.

4. Atmospheric Dispersion Modeling: The selections appeared to be well-justified and appropriate. However, this is somewhat beyond the limits of this reviewers expertise.
5. Severity of Consequences and Probability of Occurrence: Based on the use of the IDLH values and the assumptions used to determine release rates, the categorization of severity and probability appear to be correct. Notably, the probabilities are likely an overestimate based on some historical data presented.
6. Key Assumptions: I have commented on the use of IDLH and the roadway sites above and below. The other assumptions seem reasonable, but not without a significant amount of uncertainty. For example, the 30-minute exposure assumption may not necessarily be the worst-case assumption. This might be altered since it may have a significant effect on the ranking of events. The others are fairly conservative assumptions.
7. Use of IDLH Values: I have commented on these above an below. There is no specific rational presented for using 10 x the LOC values for chemicals for which IDLH values have not been established. This seems to be even more reason for using the LOC values for the analyses.
8. Uncertainties: For the most part the uncertainties appeared to adequately addressed. As noted in my other comments, there are other areas where, in my opinion, the best conservative assumptions have not been made to avoid underestimation.

<u>Specific Comments:</u>

1. p. I-5: The use of IDLH values seems to be inappropriate in as much as these values, as the document states, "..were originally developed to be

protective for healthy adult male workers..” and have not been adjusted to account for heterogeneous populations. I would have recommended strictly using the LOC values. Nevertheless, a “sensitivity analysis” was performed using the LOC values.

2. II, Accident Scenarios: Good rationale has been used for the scenarios used here. These appear to likely account for most, if not all, of the expected (predicted) accidents. Indeed, some historical data was presented to indicate that this would be the case. Fairly conservative assumptions have been made.
3. III, Chemicals of Potential Concern: The selection of chemicals is well documented and based on sound and appropriate criteria. This is based on part on the use of the WTI facility.
4. IV, Characterization of Accidental Releases: Here although the assumptions are mainly conservative, for some parameters only the average and not worst-case values are used. For example, 68°F is used as the ambient temperature. In fact temperatures have been documented in the 80s. Depending on the scenario and particular chemical in question the higher temperature should have been used as a more conservative and worst, but real, case.

Ecological Risk Assessment

P.L.deFur

WTI Review -- SERA -- December 1995
Peter L. deFur
Environmental Defense Fund

Overall issues from reviewing the Screening Ecological Risk Assessment (SERA)

1. The exposure data are limited by the paucity of real data from a range and variety of real operating conditions. Granted that the facility has only been operating for a limited period of time, but the reliance on a small number of test burns under fairly known and controlled conditions is problematic. Some correction needs to be made for upset conditions, emergency shut-downs, start-ups, and other non-normal operating conditions that would increase or change the composition of the emissions.

2. The SERA does not seem to take into account the contribution of the facility to continued degradation or prevention of restoration and recovery of sites (habitats) within the impact region. Specifically, the SERA notes that bald eagles do not occur within the site, but does not consider the contribution of the facility to preventing bald eagles from returning. Other species, both terrestrial and aquatic may be similarly affected.

3. It is not clear that the SERA considers the combined effects of all of the chemicals released, or even all the chemicals selected for analysis.

4. The SERA explains that a 20 km was selected for analysis, but does not explain the deference that might be expected if the largest region (50 km) had been selected.

5. The analysis uses arithmetic means in a number of places, e.g.

of the test burn data, yet no analyses are mentioned that other means (e.g. harmonic, geometric) are not as accurate in representing the data.

6. The emissions estimate cannot take into account chemicals for which there are no quantitative estimates, understandably. But is this not an omission that undermines the validity of the outcome?

7. The SERA does not account for the process used here. Were outside parties used to suggest approaches, data, species, end-points, etc. Recognizing that this represents but one part of a multi-year and multi-stage effort, the process for including interested and affected parties is still a necessary one in the SERA. Where is it?

Answers to Reviewers Questions:

1. Does anything undermine the scientific validity of the SERA? The estimates of emissions rates, including the omission of chemicals is undermining. Are there other facilities similar to this one the could be used to provide surrogate data for comparison?

2. Is the organization clear and does it follow the Framework? Yes, the organization is fine and this SERA does seem to follow the Framework quite well.

3. Are all the uncertainties included? To the extent that chemical interactions are uncertain, the SERA does miss some important ones. The uncertainty of emissions is at least discussed.

4. The weakest and strongest points in the SERA: The weakest points in the SERA are the emission estimates and the unknown animal data. The former are addressed above; the latter will remain unknown.

5. The major elements in section II seem adequate. The 5 scenarios for emissions are not convincing in terms of completeness. I am left wondering about other ash emissions and about total loadings

to the environment from this facility via all routes, both intentional and accidental. The SERA does not account for the unexpected.

6. The site characterization seems adequate, but not overly so. I do not get a good sense of the interactions within the area as a watershed. What type of water flow drains across the land? How much flooding is there? Are there numerous diverse habitat niches?

7. The screening method in section IV seems to be adequate, but I have to see where the question "What did we miss?" is answered. The SERA cannot capture everything, therefore it must include a section that looks for the missing parts. It is not clear that the tiered approach does provide a "thorough screening-level evaluation." The approach may be more accurate termed "representative" than thorough. The approach is one based on indicators and such is limited.

8. Are the exposure and effects adequately characterized? The characterization may be adequate, but key elements are not obvious. The species selection, section V, indicates only part of the explanation for choice. Do these species represent most or some of the total number of individuals that are resident? Why is the 1 km distance used here, when the site was described as 20 km?

9. Section IX is adequate in describing the uncertainties, but it is not a complete risk characterization. A more complete risk characterization needs to address some risk estimation questions, process questions and the issues raised by interested and affected parties.

10. Does the SERA not underestimate risk?

There are several omissions that raise the possibility that risk is actually greater than indicated. First, the chemical by chemical approach does not include the interactions among chemicals, especially metals and organics. Second, if one of the chemicals not included in the SERA has a high impact, then is not the risk underestimated? Third, the exclusion of amphibians does not offer evidence that this group will be unaffected. Organics washed from surrounding areas into numerous small wetland areas would be an effective exposure pathway should be investigated.

Pim Kosalwat

COMMENTS ON THE SCREENING ECOLOGICAL RISK ASSESSMENT FOR THE WASTE TECHNOLOGIES INDUSTRIES (WTI) HAZARDOUS WASTE INCINERATOR FACILITY

1. **Are there any components of the SERA which you feel undermine the scientific validity of the assessment? If so, what are they and can you provide suggestions to strengthen the identified components?**

   No, I think the SERA was well conducted and scientifically defensible.

2. **Is the organization of the document clear and does it present the material in a clear and concise manner consistent with the *Framework for Ecological Risk Assessment* (EPA, 1992)?**

   Yes, the organization of the document is clear and follows the EPA's *Framework for Ecological Risk Assessment*. However, the information presented is too repetitive which makes the report long and tedious to read; and at times, creates confusion.

3. **Uncertainties are discussed in numerous sections of the SERA and compose Section VIII of the SERA. In each case, do these discussions cover all relevant and important aspects of the uncertainties which you think should be addressed in the SERA?**

   Yes, uncertainty analyses presented cover all relevant and important aspects that should be addressed. When uncertainties occur, the most conservative assumptions are usually chosen.

4. **In your opinion, what is the weakest and what is the strongest aspect of the SERA? Can you make any suggestions on how the weakest parts can be strengthened by the Agency?**

The weakest aspect of the SERA is the data gap, particularly the toxicity values used to derive the benchmark values. To strengthen this aspect, the Agency should ensure that all data bases have been exhausted and if feasible, fund toxicity testing for chemicals that are deemed critical for this risk assessment.

The strongest aspect is the conservative approach to ensure that the ecological risks from the facility will not be underestimated.

5. **In Section II, are the stressors, ecological effects, and both the assessment and measurement endpoints adequately characterized? Are the five emission scenarios adequate to characterize the exposure for the WTI facility? Are there other emission scenarios which you think should be included in the SERA?**

Yes, they are adequately characterized. The scenarios chosen would probably overestimate the risks and adequately protect the ecosystem in the area.

6. **In Section III, is the site characterization adequate to support the SERA? Why or why not?**

Yes, except for the land use statistics which contain 15-year-old data, the rest of the site characterization is considered adequate to support the SERA. This section has enough details on physical and biological descriptions of the area for an ecological risk assessment.

7. **In Section IV, is the tiered process used to identify the ecological chemicals of concern (ECOC) from the initial list of potential chemicals considered scientifically defensible?**

Yes, the tiered process used is appropriate for a pool of potential chemicals this size.

Does application of this tiered approach support the statement made in the SERA "by focusing on the potential risk from the selected ECOCs, the SERA provides a thorough screening-level evaluation for the WTI facility?"

Yes.

8. **In Sections V and VI, are the exposure and ecological effects adequately characterized? Are the most appropriate estimation techniques available used? Are the assumptions clearly stated?**

All exposure and ecological effects are adequately characterized using appropriate estimation techniques with clear assumptions. However, I do not feel comfortable with some of the uncertainty factors (UF) used to extrapolate toxicity values in Table VI-1. In particular, an interspecies uncertainty factor of one is used if NOAEL values are available for three or more species within a class. Since species' responses to chemicals are highly variable, using one as the UF to extrapolate toxicity values among species seems inappropriate; an uncertainty factor of at least 5 should be used.

9. **In Section VIII (*sic*), are there any major elements missing from the risk characterization which you think need to be included or which would strengthen the risk characterization?**

In Section VII (Risk Characterization), it would be interesting to determine cumulative risks from exposure to all selected ecological chemicals of concern (ECOCs) for each scenario for each indicator (e.g., by adding hazard quotients for all chemicals for animal).

Does the risk characterization support the summary and conclusions presented in Section IX?

Yes.

10. In Section IX, given the assumptions made and the processes used to select and evaluate chemicals, receptors, and exposure pathways, do you think the SERA adequately met its objective of not inadvertently underestimating risk?

Yes; in my opinion, this SERA is more likely to overestimate the risk.

Other Comments:

As stated in Comment No. 2, the information presented in this SERA report is too repetitive. For example, in Section IV, the three paragraphs under Toxicity on pages IV-23 and IV-24 are identical to the three paragraphs under the same heading on pages IV-10 and IV-11. The report can probably be condensed to about two-third of the current size without losing any crucial information. Also, there are a few grammatical errors in the report. For example, the word "cannot" should be written as one word, not the two words (i.e., can not) which appear throughout the report.

Steven C. Peterson

Technical Workshop on WTI Incinerator Risk Issues
Washington, D.C., January 11-12, 1996
Draft Review Comments on Volume VI - Screening Ecological Risk Assessment (SERA)

General Issues

1. Overall Organization

With exceptions as noted, the draft SERA is generally well written and logically organized, if somewhat lengthy and cumbersome. The document could be streamlined by (a) transferring some of the detailed tables and discussion of methodology to appendices, and (b) attempting (in the problem formulation phase) to identify a narrower list of potential concerns, thereby focusing the risk assessment on the issues of greatest significance. Examples of tables that could be placed in appendices include the listings of locations of state parks and other areas provided in Section II; log K_{ow} and persistence values in Section IV; modeled concentrations in Section V; and summaries of effects in Section VI. In addition, some of the detailed discussion of methodology, such as the contaminant of concern screening methods in Section IV and the risk analysis calculation methods in Sections V through VII (which are repeated again in Appendix VI-26) could be placed in appendices. Further comments on focusing the problem formulation are provided below under the heading, Specific Issues.

2. Executive Summary

The executive summary is adequate.

3. Adherence to Recommendations of 1993 Peer Review Workshop

Not reviewed.

4. Data/Methodological Gaps

The SERA fails to compare predicted site-related risks to background (or reference area) conditions. Because of the many conservative assumptions used to account for uncertainty in the assessment, it is possible that some of the predicted site-related risks are not significantly greater than background risks. This might lead to a faulty interpretation of the significance of potential risks, and preclude the use of the SERA for decision making. Further comments on background comparisons are provided below in remarks on Specific Issues.

5. Long-term Research Needs

A significant problem faced by ecological risk assessors at WTI and other sites is the lack of comprehensive guidance. The available guidance is too general and does not provide detailed methodology. Consequently, WTI is similar to most ecological risk assessments in the "recycling" of information from other government reports and risk assessments, many of which have not undergone serious peer review. In my view, two of the critical areas of research in the development of guidance are (1) further development, compilation and evaluation of toxicity benchmarks for wildlife and other useful values such as BAFs (bioaccumulation factors); and (2) development of methods for extrapolating from potential effects on individual organisms to the prediction of population and community-level effects.

<u>Specific Issues</u>

1. Scientific Validity of the SERA

There are many areas of contention over approaches for evaluating ecological risks of chemicals in the environment. For example, risks to populations and communities frequently are not directly addressed, yet these are the levels of biological organization of fundamental importance in ecosystems. However, the SERA is no more remiss in this regard than hundreds of other ecological risk assessments conducted for regulatory purposes. In fact, the level of scientific rigor in the SERA is above average, in my experience. While I have a number of specific technical concerns with various aspects of the SERA, as indicated in the following comments, none of them seriously undermines the scientific credibility of the report.

2. Consistency with EPA Framework

While the SERA is generally consistent with the organization and process of ecological risk assessment as presented in the EPA *Framework for Ecological Risk Assessment*, the framework calls for selection of endpoints and development of a conceptual model based on identification of stressors and ecosystems of concern. Hence, these should be presented at the conclusion of the problem formulation and should be drawn more directly from the information presented in Sections III and IV (see comments on Sections II through V, below).

Besides its adherence to national guidance, the SERA follows Region V ecological risk assessment guidance, which calls specifically for a tiered approach. According to the regional guidance, more detailed risk assessments should be conducted if potential risks are identified at the SERA stage. However, the philosophy of taking a highly conservative approach at the SERA stage to avoid underestimating risks can be taken to an extreme. This approach implicitly assumes that resources will be available to conduct further, more detailed phases of ecological risk assessment, in which overestimation biases presumably will be uncovered and corrected. Given the need to make timely and cost-effective decisions, time and resources are not always available for additional tiers of analysis. Therefore, it is imperative to evaluate risks conservatively but realistically, so that unnecessary additional analysis or unwarranted remedial measures are not deemed to be required. Further suggestions for incorporating more realism into the SERA are provided below in comments 5 through 10.

3. Uncertainties of the SERA

The identification of uncertainties is comprehensive, with the exception of uncertainties regarding the chemical form of metals in stack and fugitive emissions (discussed below in comment 9).

4. Weakest and Strongest Aspects of the SERA

The weakest aspects of the SERA are the methodological problems with selection of ECOCs (ecological contaminants of concern), and the lack of consideration of background exposure to ubiquitous chemicals. Comments 5 though 10 offer suggestions for strengthening these aspects of the SERA.

The strongest elements of the SERA are its comprehensive site characterization and consideration of exposure pathways, and the sophisticated incorporation of exposure models.

5. Section II

There are several areas requiring clarification and reorganization in this section.

The preliminary screening process used to select ECOCs should be moved from Section B.2b to Section A.1, Stressors, and the ECOC selection process should be more clearly explained. The methods used to identify potential receptors within the assessment area should be identified in Second A.2, Ecological Components.

The results of the site characterization and selection of ECOCs should be used to refine the conceptual model presented in this section. For example, Section III identifies the virtual absence of ecologically significant habitats within a 1 km radius of the facility. Yet according to the dispersion modelling this is the area where maximum air concentrations and deposition occur. Given the lack of significant ecological receptors within the 1 km radius of likely exposure, further explanation is needed to justify the evaluation of a broad range of ecosystem components within this area.

It appears from Section IV that many chemicals have been selected as ECOCs even though they have not been detected in emissions tests. As discussed below in comments on Section IV, the first screening step typically taken is to remove non-detected chemicals from consideration. In addition, as stated below in remarks on Section V, chemicals with predicted concentrations less than background could also be removed from consideration. Further narrowing of the ECOCs by eliminating non-detected chemicals and chemicals with concentrations below background might lead to a more focused identification of relevant pathways and receptors.

In Figure II-2, Conceptual Site Model diagram, the exposure pathways and receptors identified for stack and fugitive ash appear to be identical and could be combined. Moreover, the text should state why deposition to soil is not shown as a relevant pathway for organic fugitive ECOCs (I did not notice any mention of the reason for this until later in the report, page VII-2).

6. Section III

This section is comprehensive and well-written. The information could be utilized to refine the assessment endpoints identified in Section II, as described above.

7. Section IV

There are several problems with the overall organization and methodology of this section. As mentioned above, there is too much detailed discussion of the methodology used for ECOC selection - because of this, it is difficult to develop and maintain comprehension of the principal findings. In this regard, it would be useful to provide an overall summary at the end of this section. An overview of the ECOC selection process also is needed, perhaps presented diagrammatically, to capture the overall purpose and flow of the tiered selection process. In addition, some of the material included in this section is not really relevant to the ECOC selection process. For example, methods of estimating emission rates for metals are provided in Section B3, yet metals are selected as ECOCs on the basis of the need to evaluate permit limits and thus, none of the emissions rate data is relevant to ECOC selection. Rather, the emission estimates are needed for the exposure assessment, and should be placed in Section V or in an appendix.

Non-detected chemicals are included as ECOCs. As stated above, chemicals that are not found to occur at a site typically are not selected as ECOCs. The approach taken in the SERA of evaluating non-detected chemicals using the detection limit as an estimate of the emission rate is certainly conservative, but is it realistic or necessary? If the detection limits are not felt to be sufficiently low to evaluate ecological risks, this can be pointed out and perhaps data quality objectives could be set to allow their detection at ecologically-relevant concentrations.

The ranking algorithms are potentially misleading in that no attempt is made to express input parameters on a uniform scale. This leads to an inadvertent bias because the calculation of overall scores is particularly sensitive to extreme values of any given parameter. I believe this accounts for some of the apparent biases in the ranking, such as the effect of the very low ingestion TV (toxicity value) on the overall score for dioxin/furans. I would emphasize that the selection of ECOCs for WTI is probably not unduly affected by this statistical bias, but I am concerned about the need for further review of this methodology, since acceptance by the workshop and inclusion of this approach in the final report is likely to set a precedent that will be followed by others.

Of greater significance are the methods used in the derivation of toxicity values. In general, the toxicity values used in the ECOC selection algorithms are poorly documented in the report - the values are provided in tables, but the source and toxicological basis of the TV is not identified for each value. I was able to identify the basis of several of the TVs by inspection of Appendix VI-24 and VI-25, where oral toxicity values are provided for ECOCs and benchmarks are derived for use in the risk assessment. For discussion purposes, some of the oral toxicity values and their basis are provided in the following table.

Selected Oral Toxicity Values Used in Screening ECOCs

Chemical	TV used in screening	TV used in the risk characterization	Receptor and Effect
Benzo(a)pyrene	10	10	Mouse LOAEL - reduction in fertility and reproductive capacity
Bis(2-ethylhexyl) Phthalate	200	5	Rat LOAEL - maternal effects
2,4-D	0.2	10	Mammal NOAEL - effects not specified
Hexachlorobenzene	1	1.6	Rat NOAEL - reproduction
Pentachlorophenol	3	1.2	Rat - no effect
Polychlorinated Biphenyls	0.1	0.32	Rat NOAEL - developmental effects, decreased litter size
2,3,7,8-TCDD	0.00001	0.000001	Rat - no reproductive effect

The table illustrates two important issues related to the selection of TVs for screening ECOCs. First, there appear to be discrepancies in the TVs selected for screening and the TVs used in the risk characterization portion of the risk assessment. Some of these discrepancies are quite significant (e.g., Bis(2-ethylhexyl) Phthalate) and could influence the results of the screening. Second, there is no uniformity in the selection of NOAELs and LOAELs as the standard for evaluating effects. According to the statement on page IV-10, NOAELs were selected if available. However, using LOAELs for some chemicals and NOAELs for others introduces a serious bias in the ranking, because for any given chemical the LOAEL and the NOAEL can differ by an order of magnitude. Since it is customary to use safety factors to estimate NOAELs from LOAELs if a measured NOAEL is unavailable, this approach could have been used to provide a uniform basis for comparison among the TVs.

8. Sections V and VI

Comments on Section V

The first few subsections (A through G) of Section V represent a refinement and completion of the problem formulation phase (see comments on Section II). In addition, references should be provided to justify some of the statements made in these subsections, namely: page V-4, the list of metals "known" to bioaccumulate; page V-6, the importance of "dermal" exposure for earthworms; and page V-11, the higher lipid content of earthworms as compared to seeds.

There is a shift in emphasis in this section from the 20-km radius assessment area to the area of maximum deposition within 1-km of the site. I agree that this is appropriate based on the air modeling, but the point should be made more explicitly earlier in the document.

Although I cannot comment on the details of the modeling presented in other volumes of the report, the application of the models in the SERA is sound and presented clearly and logically. My main concern with this section relates to the lack of consideration of background concentrations of naturally occurring substances such as metals, and general ambient concentrations of other widely occurring pollutants such as PAHs and PCBs. For example, many of the metals concentrations in soils and sediments predicted for both exposure scenarios are well below background concentrations. It is not uncommon to eliminate chemicals from further consideration in risk assessments if their site-related concentrations are below background levels. The selection of ECOCs could be refined by eliminating metals that do not occur above background. In addition, some of the organics are predicted to occur at concentrations substantially below levels typically found in developed areas such as the Ohio River valley. Further discussion of background comparisons is provided in comments on Sections VII & VIII, below.

Additionally, a default value for small mammal whole-body tissue BAFs of 1 is used, because these are "generally unavailable for most chemicals" (page V-26). However, soil to mammal BAFs are available for some metals, and small mammal tissue levels of many contaminants have been reported from controlled laboratory feeding studies.

Finally, incidental ingestion of soil and/or sediment can be an important pathway for many wildlife receptors. Default estimates of soil ingestion are available in EPA guidance. I recommend the inclusion of this pathway in the SERA.

Comments on Section VI

The selection of the lowest reported effects concentration, use of uncertainty factors, and other conservative approaches can result in calculated toxicity benchmarks that are unrealistically low. While the methods used in this section are generally appropriate, toxicity benchmarks for wildlife are not well established and professional judgement plays a larger role in their selection than in the selection of benchmarks for aquatic life. See comments on Section VII, below, regarding an approach to determine if the toxicity benchmarks are overly conservative. In addition, the chemical form administered in the toxicity test used as a basis for the selected toxicity benchmarks should be identified, for reasons given in comments on Section VII below.

AWQC (Ambient Water Quality Criteria) should be adjusted as described in recent EPA guidance for evaluating dissolved metals.

9. Section VII & VIII

While a conservative approach is warranted, a "reality check" is needed or predicted risks will be unrealistic. This is particularly true for naturally occurring metals, for the following reasons. One of the major factors affecting toxicity of metals to aquatic life and wildlife is the form of the chemical administered. Typically, highly bioavailable forms such as metal salts are used in toxicity tests, yet metals in nature assume a variety of less soluble and available forms. As a result, there is an inherent bias toward overestimation of risks of exposure to metals. The SERA fails to identify this as a source of uncertainty with a potentially large effect on overestimation of risks. The exposure assessment does not identify the likely form of the metals emitted by the facility, nor does the toxicity assessment identify the form of the chemical used in toxicity tests to establish benchmarks.

In order to provide a frame of reference for the predicted site-related risks, it is important to evaluate the potential risks occurring through exposure to background levels of metals and other ECOCs. In the SERA, for example, some of the toxicological benchmarks for plants and wildlife are below levels of exposure to metals in soils likely to occur at background (e.g., aluminum, arsenic, chromium, mercury, etc.). In general, the evaluation of risks relative to reference conditions is an accepted approach for ecological risk assessments and is advocated by EPA guidance. I recommend that metals predicted to be below background levels in soils be removed from the assessment entirely during the ECOC screening process. Metals predicted to be above background levels of exposure should be evaluated relative to background risks to provide the needed perspective on the risks and to allow interpretation of the significance of overestimation biases inherent in the SERA.

10. Section IX - Does SERA Inadvertently Underestimate Risk?

As mentioned in the report and in my previous comments, the SERA appears more likely to overestimate than to underestimate potential risks. While this may be consistent with a conservative approach, it is equally important not to unduly overestimate risks if this can be avoided. Resources are finite and the overestimation of risks could be interpreted as a need to proceed with costly, additional tiers of more detailed risk assessments when in fact they are unnecessary. I believe this could be the case with the SERA - the predicted risks for metals are likely overestimated, based not only on the use of operationally unlikely emissions scenarios, but on the overestimation bias of the screening methods and the lack of consideration of risks relative to background.

Scientific Peer Review of the Ecological Risk Assessment Sections of: Risk Assessment for the Waste Technologies Industries (WTI) Hazardous Waste Incinerator Facility (East Liverpool, Ohio)

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December 1995

The following comments address each of the general issues and the specific issues related to the screening ecological risk assessment contained in the charge to reviewers.

General Issues:

1. The ecological risk assessment (ERA) is generally clear but is not concise. It would have been nice if someone had taken the time to reduce the repetitions and to make the text more focused. I lost track of the number of times that the reason for not assessing risks to reptiles and amphibians was repeated.

   The "IDENTIFICATION OF ECOLOGICAL CHEMICALS OF CONCERN' was the most difficult section to follow. It is in effect a preliminary screening assessment that is performed prior to the main screening assessment. However, its logic is not clearly risk-based. Rather, it is presented as being based on an "exposure analysis" (although exposure is not estimated but toxicity is) and "professional judgement."

   ERAs that have multiple endpoints are often hard to follow because the reader has to go through the exposure analysis for each endpoint, then the effects analysis for each endpoint, and finally the risk characterization for each, so that the continuity is lost. At ORNL we have found that reviewers and stakeholders prefer an organization by endpoint. For example, you present the analyses of exposure and effects and the risk characterization for plants, then for piscivorous birds, etc. I believe that would have helped here, but it is probably not worth doing at this point.

2. The executive summary adequately reflects the conclusions.

3. Performance of an ecological risk assessment was recommended. The response was to perform a screening ecological risk assessment. I am not sure that the intent of the reviewers was satisfied.

4. The obvious impediment to using this assessment for decision making is the fact that it does not reach a conclusion about the ecological risk. A definitive assessment is needed.

5. Screening assessments should be routine, quick, and concise. A program is needed to address this problem by developing default methods, data sets, models, and assumptions for screening ERAs. We then need methods to perform conclusive ERAs for these types of actions, the permitting of future complex effluents.

ERA Workgroup-Specific Issues:

The major problem that I have with this assessment is that it has no clear purpose. A screening assessment should either conclude that there is no credible hazard or should lead to a definitive assessment that actually estimates risks of the hazards that are retained by the screen. This screening assessment retains some hazards, but it does not prompt a definitive assessment.

1. See the specific comments below.

2. See general comment 1. Although the problem formulation is not identified as such (the first four sections constitute a problem formulation), that is not really a problem. The presentation is consistent with the framework.

3. The discussions of uncertainty do not address the magnitude of the uncertainties and they do not consistently address the significance of the uncertainties. There should be an analysis of the magnitude of the uncertainties relative to the magnitude of the conservatism. However, in fairness to the authors, I should point out that the narrative treatment of uncertainties presented in this document is typical of current ERAs.

4. The weakest aspect of the ERA (ignoring the apparent lack of purpose discussed above) is the selection of ECOCs. This is a difficult task and would benefit from a careful validation of the method. The strongest aspect is the well researched paramaterization of the ecological exposure models and ecotoxicological benchmarks.

5. a. Stressors appear to be adequately characterized.

b. Effects are adequately characterized for a screening assessment.

c. Assessment and measurement endpoints are not adequately characterized. The assessment endpoints are rather vague statements and the "indicators" are closer to being assessment endpoints. The assessment endpoints should be the things for which risks are actually assessed and the measurement endpoints should be numerical summaries of actual measurements used as estimators of the effects. Ask yourself the following questions, for ecological concerns what is equivalent to lifetime cancer risk of a maximally exposed individual (the assessment endpoint) and what is equivalent to the cancer slope factor (the measurement endpoint)? By answering those questions, you can come up with useful endpoints and eliminate the need for "indicators." For example, you really do not assess risks to all birds and mammals as a group (first assessment endpoint). You assess risks to particular species as representatives of taxonomic and trophic groups (e.g., kingfishers as representative of piscivorous birds) just as the human health assessors estimate risks to a reasonable maximally exposed adult as a representative of all adult humans. A measurement endpoint is not an "evaluation" and it is impossibly optimistic to require that it be a measurement in your indicator species (Table II-1). It should be something like "the lowest oral NOAEL for mortality, growth or reproduction in an avian species." Specific comments:

i. The assessment endpoints are different is Table II-1 and Figure II-1.

ii. The "intact and productive food chains" endpoint seems pointless. Some species are related to this endpoint but not others even though all species are part of food chains. Earthworms/soil fauna are the only "indicator group" that is not protected except as contributors to food chains. Given the ecological importance of these organisms, why not protect them for their own sake?

iii. How do the indicator species represent the Ohio River (p. V-11)?

iv. Red-tailed hawks are not "top predators" (p. V-12). They feed largely on herbivores so they are no higher than, for example, warblers.

v. What is the passerine/woodpecker group? It is not a taxonomic group, trophic group, or guild.

vi. Contrary to the statement on p. V-13, small mammals are not a taxonomic or ecological group, particularly when bats are included as here.

vii. Shrews are not surrogates for bats that feed on emergent aquatic insects or lepidoptera. Shrews are a worst case for exposure via the soil invertebrate pathway, but that does not help with bats that consume very few soil invertebrates. If you can not assess bats, just say so, as you did with reptiles and amphibians.

viii. The large size and small metabolic rate of deer would make them more rather than less sensitive based on conventional wisdom (p. V-14). This metabolic correction is what makes humans more sensitive than mice in standard risk models.

d. The logic for selection of the five emission scenarios is unclear. Why have a high emission and expected scenario for metals in the stack emissions and not the others? Also why was the metal stack maximum scenario so much more conservative than the high emission scenarios for the other classes of emissions? Consistency would seem to be desirable for comparing the emissions and their constituents. Finally, why were the risks from the accident scenarios not assessed?

6. The site characterization is adequate but unfocused and excessively long. The discussions of wetlands, parks, fauna, and flora and associated tables are not really used in the analysis and are more like the kind of thing that pads EISs. Risk assessments should be more focused.

7. I believe that the results of the selection process are acceptable based on my professional judgement, but I am not sure that I agree with all of the particulars. As stated earlier, the logic is not clearly presented. The assumption behind the "exposure analysis" seems to be

that relative risk is proportional to emission rates times some physical-chemical property that controls exposure divided by toxicity. The most questionable part seems to me to be the selection of the physical-chemical property which, in effect, substitutes for the entire transport and exposure model.

A. The authors justify the use of K_{ow} rather than log K_{ow}, (which is conventionally used in estimating bioaccumulation, because the log-scaled relationships are linear), by indicating that they want to increase the influence of bioaccumulation on the scores (p. V-11). Did they do a sensitivity analysis to determine that to be appropriate? How influential is K_{ow} in the scores now?

B. Is it reasonable to use solubility in scoring aquatic contaminants (p. IV-15)? Is solubility really the physical-chemical property limiting risks to aquatic organisms under these conditions? I doubt that it is. I believe that K_d is more likely to be a controlling factor, as is degredation rate.

C. Freon-like chemicals are eliminated because they are highly volatile (p. IV-19). However, earlier the authors deliberately include volatile chemicals that had not been included by the procedure. This does not appear to be consistent. Freons can be eliminated based on their extremely low toxicity.

D. Selection by chemical group as well as by "exposure analysis" is said to increase confidence that these chemicals represent the greatest risk potential (p. IV-20). Unless I missed something, the same criteria are used in both cases. Therefore, there is no real increase in confidence.

E. Why are volatile organics included in sediment but not soil? Given that the chemicals are originating in air, are they any less likely to partition to solids that are damp than those that are saturated?

Validation of the scoring method would increase confidence in its results. One could apply the scoring method to a set of reasonably well-characterized chemicals, then do a risk analysis on them, and finally determine whether the scores and risks are correlated (at least rank order correlated).

8. The analysis of exposure and effects is generally appropriate. However, I have some disagreements and questions. My comments do not include the transport modeling component of the exposure analysis, only the exposure models.

A. Equation V-6 (p. V-27) does not make sense to me. I think the authors modeled it too closely on the dose rate model. The BAF is simply an empirically derived quotient of TCx/MCx. It is not clear to me that the accumulation due to diet should be diminished by FF or that the BAF is applicable to drinking water. Water does not appear to be a significant source of exposure for wildlife. If that is correct, then in this model the water is just acting as a diluent of the dietary exposure. Mammalian BAF studies with which I am familiar did not include water as part of the exposure, so the model is not equivalent to the situation in which the BAF was derived. If you can leave respiratory uptake out of the model as negligible (assuming that is the reason), you can leave water out as well.

B. The paragraph at the bottom of p. V-27 is unclear since it implies that incidental soil ingestion is handled separately from the model in Eq. V-6 (which does not mention soil). Apparently soil is one of the dietary items, which is acceptable. However, one should not have to refer to the tables to figure out what was done.

C. The parameters identified as percentages in Eq. V-6 and V-7 are proportions. I assume that this is only an error in wording.

D. The selection criteria for toxicity data to be used as wildlife screening benchmarks is unclear. On p. VI-11 the most sensitive species available is indicated. On p. VI-9&10 wildlife are preferred over laboratory species in the first paragraph of Sec. F, but in the next paragraph "the lowest available and most applicable toxicological value" are indicated. How are these potentially conflicting criteria prioritized? Other considerations are mentioned, but their relative importance is not indicated. For example, is an acute or subchronic test with an "applicable" species preferred over a chronic test with a less applicable species? If you simply used professional judgement to select the best benchmark value, taking certain factors into consideration, just say so.

E. The frequent lack of toxicity data is dismissed rather lightly on p. VI-11. See the response to question 9, below.

9. A. The uncertainty due to lack of toxicity data for some chemicals is not treated in a consistent rigorous manner. The authors should either prorate the uncharacterized chemicals among the characterized ones based on similarity or greatest toxicity (which is what we did in the synfuels technology ERAs, performed for the EPA ten years ago) or conduct a consistent analysis similar to the one presented verbally for risks of organics in air to plants

(p. VIII-3). That analysis would address the following questions: (a) what is the concentration of the uncharacterized chemicals relative to the characterized ones, (b) what is the level of relative toxicity that would be necessary for the uncharacterized chemicals to pose a significant risk, and (c) what is the likelihood that the chemicals would have the necessary relative toxicity?

B. The risk characterization supports the conclusion that most but not all hazards have been screened out.

10. I believe that the assessment does not underestimate risks.

This is probably the most difficult type of ecological risk assessment to perform, a predictive assessment with a large number of potential contaminants in an atmospheric emission. There is no opportunity to focus on the properties of a single chemical as in registration of pesticides or industrial chemicals and no opportunity to collect, analyze and test contaminated media as at a contaminated site. In addition, the atmospheric route of exposure is the most poorly characterized route for ecological receptors. Although the comments presented are largely negative, that is because like most reviewers I did not take the time to point out the many good and correct analyses and statements. The assessment is, as I expected, competently performed but not ground-breaking or flawless.

APPENDIX E

REVIEWER WORK GROUP ASSIGNMENTS

United States
Environmental Protection Agency
Risk Assessment Forum

Technical Workshop on WTI Incinerator Risk Issues

Holiday Inn - Georgetown
Washington, DC
January 11-12, 1996

Final Workgroup Breakout Sessions

Combustion Engineering

Barry Dellinger, Workgroup Leader
Elmar Altwicker

Air Dispersion/Deposition Modeling and Accident Analysis

Walter Dabberdt, Workgroup Leader
Mark Garrison
Halstead Harrison
Jerry Havens
Geoffrey Kaiser

Exposure Assessment

George Fries, Workgroup Leader
Thomas McKone
James Butler

Toxicology

Mary Davis, Workgroup Leader
George Alexeeff
Thomas Gasiewicz

Ecological Risk Assessment

Glenn Suter, Workgroup Leader
Peter deFur
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APPENDIX F

FINAL OBSERVER LIST

United States
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Technical Workshop on WTI Incinerator Risk Issues

Holiday Inn - Georgetown
Washington, DC
January 11-12, 1996

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APPENDIX G

PRINTED MATERIALS DISTRIBUTED BY OBSERVERS

The following materials were distributed by observers Terri Swearingen and Paul Connett. These observers also distributed nonprint materials (e.g., a video tape and a ruler); these could not be reproduced in this written report.

TRI STATE ENVIRONMENTAL COUNCIL

For immediate release: January, 10 1996
Contact: Tom Webster (617)-638-4641 (Office), (617)-542-1676 (Home)
Ellen Connett (315)-379-9200
Richard Wolf (216)-385-2133

WTI: EPA RISK ASSESSMENT OBSCURES OBVIOUS DANGERS

While 21 expert peer reviewers are flying into Washington, D.C. to attend the **Technical Workshop on WTI Incinerator Risk Issues, January 11 & 12, 1996,** citizens from East Liverpool, Ohio will have driven 8 hours through appalling weather conditions to attend this same workshop. They will be presenting their criticism of the USEPA's Health Risk Assessment for the WTI hazardous waste incinerator.

Spokesperson, Terri Swearingen, said "We weren't going to allow this weather to keep us from pointing out that the EPA's Risk Assessment is an enormously expensive exercise to obscure the obvious! Anyone who has visited East Liverpool and is not "beholden" to the hazardous waste industry can see with their own eyes that the incinerator is located ridiculously and dangerously close to where people live and where children go to school."

According to Dr. Paul Connett, Professor of Chemistry, St. Lawrence University, who has been helping citizens unravel the complexities of several thousand pages of the Risk Assessment, "The strength of a chain is the strength of its weakest link. In this case the weakest link is clearly the accident analysis. This analysis is full of holes. The authors did not even consider the track record of WTI's sister plant in Biebesheim, Germany, which has the same Von Roll technology. This is a serious omission because the German plant has had a long history of accidents and fires."

School Nurse, June Connolly, said that if one of the major fires that had taken place in Biebesheim occurred at WTI, the children for whom she is responsible could've been killed. "It astounds me," she said, "that the authorities here have an emergency plan for these children which consists of herding them into the gymnasium and duck taping the windows."

According to Dr. Michael McCawley, Professor of Air Pollution Engineering, Dept of Civil Engineering, West Virginia University, Morgantown, WV, "The accident analysis was deficient on three major fronts: 1)It did not include a "fault tree analysis". 2) It did not include an accident analysis of chemical plants which, in theory, should be safer than hazardous waste facilities. 3) While they modeled a fire of spilled liquids, they did not consider the vaporization of these same liquids prior to ignition. Such a scenario could produce a giant explosion, much like a gasoline bomb."

Tom Webster, School of Public Health, Boston University, was able to demonstrate that the food chain dioxin exposure analysis was seriously underestimated.

Alonzo Spencer, Save Our County, stated, " The question comes down to two things: 1)Whether or not these peer reviewers, with their lofty credentials, can empathize with the ordinary person, the person who lives with the dangers every day. 2)Whether the EPA, even at this late hour, can admit they have made an horrendous mistake and shut this plant down immediately."

In addition to their oral testimony, the citizens gave each of the peer reviewers a copy of a 17 minute videotape they had prepared, documenting an explosion in an American hazardous waste incinerator; a list of accidents and fires occurring in the Biebesheim facility(WTI's sister plant in Germany); a series of video clips showing the proximity of the WTI incinerator to people's homes and the elementary school. Citizens also gave each member of the panel a list of statements made by scientists, regulatory officials and politicians who have visited the location., a copy of the Ohio State Law passed in 1984 which would have prevented the siting of the WTI facility if a permit was applied for today, and lastly, a ruler for each member to remind them of Terri Swearingen's comment on CBS 60 minutes, "You don't have to be a rocket scientist to work out that the WTI location is unacceptable. You only need a ruler."

Ladies and Gentlemen,

I am a life-long resident of East Liverpool, Ohio. I am a high school graduate and had one year of business college. I don't have letters following my name to signify a degree, like all of you. But I do have the important title of MOM. My husband, Bob and I have five children. Matthew is nineteen and a freshman in college. Ryan is seventeen and a junior in high school. Elizabeth is fourteen and a freshman in high school. Sierra is nine and Alex is six.. They are in third grade and Kindergarden at East Elementary School, only 1100 ft. away from WTI. Our home sits on the bluff over looking WTI, 800 ft. away, which is the subject of your latest science project.

From this little bit of information, you all can see that I have a vested interest in the outcome of your work. If you inadvertently leave one T not crossed or one I not dotted,it could mean a life or death sentence for my family. I am not some hysterical housewife with nothing better to do. Over the last few years, I have had to learn as much as I could about this most unwelcomed neighbor. I have had to do a lot of reading about lead, mercury and dioxins (something I knew nothing about before WTI). This has taken cherished time away from my family. I have experienced the terror of being evacuated from my home and pulling my children out of their school to run out of harm's way. During construction of this plant, workers struck a gas main and it ruptured. Even our policemen didn't know what the problem was.

I have also been chased from my neighborhood because of the very nauseous fumes from a chemical spill. Image your entire town smelling like cat urine. Not a very pretty picture, is it?

Or imagine driving across a bridge and looking across the river towards your home, and seeing the red flashing lights of fire trucks. I didn't think we would ever get home. Our hearts were racing faster than the engine to our van. We had left our kids at home for a little while to visit friends. Upon our arrival home, we saw firetrucks everywhere at WTI. Remember, we have a bird's-eye view of the plant. Panic tries to take over at this point, because we have no where to call for answers. My little ones seem excited over the sight of all the firetrucks and commotion, but my older kids are giving me that all to familiar look. "Are we going to have to leave home again?" Our house is filling with concerned friends and neighbors now and the phone is ringing off the wall. We can't take it off the hook, because that might scare someone even more when they can't reach us.

Last January, we were taking down our Christmas tree, when we hear this very loud roar. We look outside and see what looks like Old Faithful to us. I"m going crazy running for the video camera and the phone together. My oldest son says "Mom, WTI's blowing again." The phone starts ringing "No , I don't know what's wrong".

Vallentine's Day, and the day after too, more firetrucks..What's wrong now? This is getting ridiculous!

July 18th, a truck of "caustic WASTE" was delivered to WTI by the same trucking company that delivers their "caustic MATERIAL". The truck driver knew the company was expecting the 'CAUSTIC MATERIAL' AND WAVED THE TRUCK THROUGH WITHOUT AN INSPECTION. The material was then put into the pollution control system. As the truck was leaving the plant, an employee looked at the truck manifest and realized the mistake. The plant was shut down and the pollution control system had to be drained and cleaned. I brought all of this up to Gary Victorine of USEPA and he said it was just a miscommunication. Would an air traffic controller accept that excuse?

I could go on and on and on with stories like this. But frankly, I"m tired of this game. I don't want to play any more. The stakes are too high.

I would like you to look at this map of the US and tell me where each of you live.

I bet you are all a safe distance away from the world's largest hazardous waste incinerator. I want my family to be a safe distance away, too. Moving is not an option. This is my home. I am an American citizen, not some wealthy, foreign-owned corporation.

PLEASE, when you are doing your job on this risk assessment, remember each of those numbers on your paper has a name and a face and a life behind it.

PLEASE, FOR OUR CHILDREN'S SAKE !!!

Sincerely,
Sandy Estell, MOM

CITIZENS CRITIQUE OF THE ACCIDENT ANALYSIS IN THE U.S. EPA's RISK ASSESSMENT FOR WTI

1) Introduction

While we have a number of concerns and criticisms about many parts of the U.S.EPA's Risk Assessment, our major concern is for the chapter on Accident Analysis (Chapter 7). The strength of a chain is the strength of its weakest link, in our view, the accident risk analysis is by far the weakest link in the entire assessment. It is grossly inadequate! At the end of this analysis we attach a few comments on other parts of the risk assessment including, comments by Tom Webster on the food chain exposure analysis.

2) Weaknesses of the accident risk analysis

2.1 The accident history of the Von Roll hazardous waste incinerator in Biebesheim, Germany was not reviewed even though it has almost identical technology to the WTI plant. Moreover, it has had a long history of accidents and fires which could have thrown important light on the potential for fires and accidents at the WTI plant (see video).

2.2. A number of American hazardous waste incinerators were not reviewed even though their operation history would be significant for accident analysis. For example,

a) PCI, Shakopee, MN, was not included even though it was destroyed in an explosion which led to its closure by local authorities (see video).

b) Caldwell Systems, Lenoir, N.C. was not included even though it was also destroyed in an explosion and closed down by local authorities.

c) LWD, Calvert City, Kentucky. LWD was listed as Clay County but this small incinerator was closed many years ago. On the other hand, LWD has a major and troublesome incinerator in Calvert City which did not appear on the EPA list.

2.3 Where American facilities were reviewed, the list of accidents was incomplete. For example, the ENSCO incinerator in El Dorado, Ark. had a major explosion in December, 1994. This was not included (see attached newspaper account).

2.4 Since the analysis was prepared there has been a major explosion in the Ross incinerator in Grafton, Ohio.

2.5 The authors relied heavily on self reporting by the facilities and the records of regulatory authorities. These two sources frequently combine to give a very cosmetic account of accidents with respect to off site damage and impacts on citizens. To get a more balanced picture, the reviewers need to examine local newspapers and to interview residents who have often kept extensive records of plant accidents, including photographs, videos and diaries.

2.6 The accident history of chemical plants should also have been reviewed. This is a critical omission in our view because a hazardous waste incinerator is really a chemical plant handling dirty chemicals. One would anticipate fewer accidents to occur in chemical plants because the operators are dealing with known chemicals with precisely known dangers. Knowing why these accidents occur could throw important light on potential accidents in a plant handling hazardous waste. Moreover, studying the impact of fires and explosions involving known chemicals would be very valuable in attempting to estimate the impact of similar chemicals handled by WTI. We have attached a list of accidents in the chemical industry by R. Andurand.

2.7 The scenarios chosen for analysis were not worst case even though the authors claim they were. For example, the fires examined involved liquids burning. The authors did not consider a situation were the liquid first vaporizes and the condensate aerosol becomes ignited causing a gasoline bomb type explosion. Such an explosion would have devastating consequences in the East Liverpool location.

2.8 The scenarios did not include a major fire in the bunker where solid hazardous waste is dumped and co-mingled prior to burning. Again, this was an important omission because numerous fires have occurred in the bunker in WTI's sister plant in Biebesheim, Germany including two large fires (see video). This becomes even more important when one discovers that WTI has already had several smaller fires in the bunker in its short time of operation.

2.9 In considering the experience with American incinerators the reviewers have contented themselves with the notion that when major accidents have occurred, even those involving deaths of plant workers, little or no off site damage has occurred, however, such analysis ignores the unique location of the WTI incinerator. The reviewers before concluding no off site damage, should have compared the location of each plant vis a vis housing, schools and topography with the location of the East Liverpool plant.

2.10 The authors of this report appear to be insensitive to the very special problem of the location of the East Elementary School. In particular, they did not appear to be aware that the emergency evacuation plan for these children is to herd them into the gymnasium and duck tape the windows. Had they been more sensitive to this issue, they would've realized how inappropriate it was to project casualties based upon IDLH values. Since these IDLH values are derived from the levels from which a healthy adult worker can escape within thirty minutes. Even a factor of ten applied to these values would probably not be adequate to estimate a level a small panicking child could withstand. Replacing the IDLH values divided with a more protective value of a 100 would make a huge difference to their analysis.

2.11 We are told by a professional in the field that an accident analysis is not really an analysis unless a fault tree analysis is constructed. Such an analysis would require a much greater in depth understanding of each step in the WTI process and each aspect of operator training. Someone once said, "Those who do not learn from history, are doomed to repeat it." Prior to the Three Mile Island accident, risk experts had predicted that the chance of such an accident occurring would have been once in a billion years. A post-mortem fault tree analysis of the plant's operations greatly reduced this prediction!

2.12 No scenario was considered in which radioactive waste was accidentally burned at the plant, even though it has been well established that radioactive waste has been burned at numerous incinerators, including Rollins, LA, and ENSCO, El Dorado, AR.

2.13 Despite two requests by citizens, the dispersal of chemicals liberated in an accident was not modeled using a wind tunnel experiment, even though equipment had been set up to model stack and fugitive emissions. Citizens were told that it would have been too expensive. Instead, the authors have relied on a computer model.

2.14(a) When the authors modeled fires involving organochlorine wastes, they failed to acknowledge that such fires have the potential to create considerable quantities of dioxins and furans and other toxic by-products. While these products may not pose an acute effect at the time of the accident, they could considerably contribute to contamination of residents' back yards and the playground of the elementary school. Even if these risks were not calculated in the accident analysis, they should have been passed on to other sections of the risk assessment.

2.14(b) We are puzzled why the authors estimated that the burning of 5,000 gallons of liquid waste (tetrachloroethene and toluene) should take 117 minutes on site, but only 7.2 minutes off-site, especially since the burning of 100 gallons of this waste took the same length of time (2.8 minutes) in both locations.

2.15 Unfortunately, the accident analysis reads more like a PR exercise than a genuine attempt to gauge the likelihood, and the impact, of a major accident occurring at WTI. The use of words like "moderate" to describe an accident involving up to 10 fatalities may be appropriate when considering federal emergencies, but it is highly insensitive and inappropriate when discussing the chances of local residents or children being killed by an operation they have not chosen.

2.16 Even if we take the accident analysis at its face value with all its many shortcomings, it is highly unlikely in our view that any permitting body in possession of this analysis would have allowed WTI to have been built at this location. The state of Ohio, the Tri-state region, the United States of America are simply not that small that residents should have to live near or children go to school with this kind of risk hanging over their heads. A more appropriate and thorough accident analysis, in our view, would make the location of this plant unthinkable. We wonder how long our regulatory agencies can support the unthinkable.

2.18 We invite all the peer reviewers to consider for a moment what it would be like if they had to live in Sandy Estell's house, or if they had to send their children to the East Liverpool elementary school (see video). At this point, we are asking more than expertise, we are asking for empathy. It's time to call this agony to an end. The plant must be shut down.

2.19 Whatever the financial remuneration to WTI by the U.S. government to compensate for their costs would be a small price to pay to remove the shadow from the lives of the residents who live near this facility. Simply put, no child in the United States should have to go to school 1100 feet from a hazardous waste incinerator.

2.20 No amount of number crunching on other parts of this risk assessment can compensate for the totally unacceptable risks posed by accidents at this location. To dwell on other risks, until the accident analysis is satisfactorily completed is to obfuscate the issue. These other risks are somewhat like choosing what kind of deodorant you'll wear to your own funeral.

3. Attachments

We are submitting the following additional material:

a) A list of statements made by scientists, regulatory officials, politicians and others who have commented upon the location of this facility.

b) A 17 minute video tape which illustrates the kind of accidents that have occurred at American incinerators (Shakopee, MN), a list of accidents which have occurred at the Biebesheim incinerator (sister plant to WTI), as well as shots depicting the location of local residences and the school.

c) A copy of the Ohio state law which was passed in 1984, seven years before WTI was built, which forbade the building of such a facility within 2000 feet of schools, homes, hospitals or prisons.

d) A ruler!!

e) Pat Costner's (Greenpeace scientist) comments on the dioxin emissions.

f) Tom Webster's (Department of Public Health, Boston University) comments on risk assessment methodology and food chain exposure analysis.

g) A list of accidents in the chemical industry published in 1979.

h) An article from an El Dorado newspaper on the December 1994 explosion.

i) An article from an Ohio newspaper on the explosion at the Ross incinerator in Grafton, Ohio.

j) Correspondence from the Tri-State Environmental Council to the EPA which raised many important questions about the risk assessment prior to its completion, including requests to model accidental releases of pollutants using wind tunnel experiments. Sadly, most of these questions and issues were ignored. So much for citizen participation.

k) An important statement made by Deputy Surgeon General, Dr. Barry Johnson, to the U.S. Congress, January 24, 1994, where he indicates that of 72,000 papers published on incineration, only one discussed the conduct of a population based study conducted in a community living in the vicinity of an incinerator. In other words, the claims that an incinerator poses no health threat to a community is a theoretical statement which has not been validated, as is the case with the current risk assessment. To quote Dr. Johnson, "The scientific information on human health impacts of incineration isn't often available because the relevant studies haven't been conducted."

Note 1. The deposition velocities illustrated in figure II-3, page II-54, Volume IV, are based upon wind tunnel experiments. Field data give much higher values for particles in the 0.01 to 1 micron sized particles. See figure 6 and reference 10 in Chapter 3 in Health Effects of Municipal Waste Incineration, ed. Hattemer-Frey and Travis.

Note 2. The extrapolation in figure III-1, page III-22, Volume IV, is mathematically invalid.

WTI, EAST LIVERPOOL, OHIO

EPA RISK ASSESSMENT OBSCURES OBVIOUS DANGERS

We believe that the EPA's Risk Assessment for the WTI hazardous waste incinerator has been an extremely costly exercise designed to obfuscate the obvious.

The obvious (common sense) analysis of the WTI incinerator is that no matter how good the technology, it should not have been built where it is: in a flood plain, 300 feet from the nearest homes and 1100 feet from an elementary school, where the authorities have a totally inadequate evacuation plan.

This obvious (common sense) analysis has essentially been confirmed by numerous scientists, regulators, politicians and others who have visited the location. Below, after giving the key section in Ohio State Law, which was passed in 1984, and would prevent the siting of such a facility at this location today, we offer a series of statements made by those who have either visited the site or have responded to a description of the location.

WHAT THE GOVERNING LAW SAYS ABOUT THE LOCATION:

Ohio Revised Code Section 3734.05(d)(6) states "The Board shall not approve an application for a hazardous facility installation and operation permit unless it finds and determines:....(g) that the active areas within a new hazardous waste facility are not located or operated within:

(i) two thousand feet of any residence, school, hospital, jail, or prison;

(ii) any naturally occurring wetland; or

(iii) any flood hazard area if the applicant cannot show that the facility will be designed, constructed, operated, and maintained to prevent washout by a one hundred year flood or that procedures will be in effect to remove the waste before flood waters can reach it."

WHAT SCIENTISTS HAVE SAID ABOUT THE LOCATION:

The United States is not so small that it should have to site one of its twenty commercial hazardous waste incinerators so close to where children go to school. The ultimate fail safe of a facility like this is the location where you put it. Here, we have no fail safe. WTI would have to be a perfect machine, run by perfect people. Such a machine does not exist. It is only a matter of time before a serious accident will engulf these children. Only those who have not visited the location, or those who are totally devoid of empathy, could tolerate allowing WTI to run for even a single day.

Dr. Paul Connett, Professor of Chemistry, St. Lawrence University, New York.

The WTI facility is the worst siting decision I have seen in my 25 years of practice in public health. Locating a major hazardous waste incinerator 300 feet from the nearest residence and 1100 feet from an elementary school with 400 children amounts to administrative incompetence if not malfeasance in office and does violence to commonsense. ....It is well known that the area where the WTI facility is located is prone to frequent episodes of atmospheric stagnations, conditions which prevent pollutants released into the atmosphere from dispersing, instead resulting in sudden high pollutant buildups......It is truly hard to imagine a more inappropriate region for such a facility, to say nothing of the additional matter of placing it next to an elementary school.

What if an accident were to happen at WTI? How would the nearby residents and the children in the school escape? It is likely that there would be no time for an evacuation, and this problem has been recognized for the school (I don't know what the residents, who are even closer, are to do.) The emergency "plan" calls for a strategy of "sheltering in place." An examination of the details of this plan reveals in the starkest fashion the underlying futility of truly protecting these small children.. The plan assumes that all 400 children can be herded safely and efficiently into the school cafeteria within 3 minutes, the room sealed by stuffing wax paper and tin foil in the cracks and taping with duct tape, and the air conditioning, heating and ventilation systems turned off so that outside air would not be entering the building. If an explosion were to shatter even one of the windows, sealing the room would be impossible. Even without a broken window, however, it is unlikely that toxic gases could be kept out of the room, now making it more a tomb that a safe haven...

Dr. David Ozonoff , M.D., MPH, Professor of Public Health and Chair, Department of Environmental Health, Boston University School of Public Health, June 21, 1993

Considering all of the information available as to effects of exposure to the various chemicals that WTI proposed to handle at the facility, I fail to see how morally or ethically, they can go ahead.....Should a for-profit corporation be allowed to force this degree of hazard upon a community?..

Of greatest concern are the adverse health effects to children living and going to school in the area. It is inconceivable that such arrogance, to site a toxic waste handling facility in an inhabited area and close to a school, would persist.

Janette Sherman, M.D., Internal Medicine, specializing in Occupational Medicine and Toxicology and author of the book, "Chemical Exposure and Disease :Diagnostic and Investigative Techniques.

While it is true that one can criticize the specific relevance of all of these studies [toxicological studies of substances like dioxin] in one respect or another, the bottom line is still the potential risks which they imply, risks which are not discountable, partly because they are not quantifiable. It is obvious that the WTI incinerator could create a high risk situation for the valley, and it is also obvious that there remains a tremendous number of unknowns.

Daniel Vallera, Professor and Director, Experimental Cancer Immunology, Department of Therapeutic Radiology, University of Minnesota, Minneapolis, MN 15 April 1992.

[T]he physical proximity of the facility to an elementary school (1100 feet) and a residential neighborhood (300 feet) violates reasonable norms for siting an operation that processes massive amounts of hazardous materials and emits some of them at biologically significant levels.

Barry Commoner, Director, Center For The Biology Of Natural Systems, Queens College, CUNY, Flushing, N.Y. in a letter to President Bill Clinton, June 23, 1993

[T]he location of the incinerator in a river valley 300 feet from houses and 1100 feet from an elementary school could turn an accident into a catastrophe.

Tom Webster, Research Associate, CBNS, Queens College, CUNY and Member, U.S.EPA Dioxin Peer Review and Risk Characterization Committee, in a letter to Pres. Bill Clinton, 23 June 1993

The location of the incinerator in a flood plain makes a mockery of siting standards adopted in this country. In addition, the top of the stack is located 300 feet from a residential community and 1100 feet from an elementary school. Theoretically, the emissions may be low enough so that levels of metals and bioaccumulative substances will not reach critical levels in 30 years. However, the experience with virtually every other commercial incinerator in the United States is that there will be accidents and periods when emissions far exceed permitted levels. It is likely that in the next 10 years the children in that elementary school will be subject to acutely dangerous emissions from the incinerator. If the explosion that rocked the Waste Management incinerator in Chicago last year would occur at the WTI facility, those children would have been enveloped by a cloud of hydrochloric acid. Such an accident should not be permitted to be a possibility.

Robert Ginsburg, Ph.D., Environmental Health Consultant, Chicago, Illinois, 17 June 1993 *letter to President Bill Clinton*

I have read the materials that you sent me on the WTI proposed incinerator. My particular interest is in the area of neurotoxicology of lead in children. ...The company's statement is that the annual emissions of lead will be 4.7 tons. ...I can say that the risk to the brains of the children attending school near the stack, during their most critical period of development, will be put at substantial risk. These children have no say in this matter and it is our responsibility as adults and government to speak for them.

Herbert Needleman, M.D., Professor of Psychiatry and Pediatrics, University of Pittsburgh, December 4, 1991.

[The number of days with valley stagnations here is very bad..Donora, Pennsylvania is not that far and the weather conditions that occurred in Donora in 1948 that resulted in 20 deaths due to air pollution can occur here. If you couple that with any accidents.... then you have an incident similar to the one that happened in Bhopal, India.

Michael McCawley, Ph.D., Professor of Air Pollution Engineering, Department of Civil Engineering, West Virginia University, Morgantown, WV

WHAT REGULATORY OFFICIALS SAY ABOUT THE LOCATION:

Arthur Davis, Chief of Pennsylvania's Department of Environmental Resources since 1987, in a March 29, 1993 *Pittsburgh Post Gazette* interview concerning the WTI incinerator, told how he would handle a situation like WTI *in the state of Pennsylvania:*

Post Gazette: Would you have approved the WTI incinerator as presently sited, if it were located in Pennsylvania?

Davis: *As I understand the thing, at that location, WTI would not qualify for a permit under our law. It would be disqualified because of its location in a flood plain and its closeness to a school. So on those two grounds, the exclusionary criteria would deny WTI the opportunity to file a full application.*

As a human being, I would never agree to the school being that close.

Valdas Adamkus, Regional Administrator, U.S. Region V, *Chicago Tribune*, responding to calls for his resignation, May 7, 1993.

From June, 1988 through January, 1991, I chaired the Ohio Hazardous Waste Facility Board which permits all hazardous waste facilities in my home state. It was one of my predecessors who made what I consider to be the most irresponsible environmental decision in Ohio history with the permitting of the WTI hazardous waste incinerator in East Liverpool, in 1984. Had it come before me, I am confident in saying that my four fellow Board members and I would have laughed the proposal right out of the room.

In my opinion, WTI's East Liverpool site is one of the worst possible sites in all of Ohio for storing and incinerating toxic chemical compounds. It is on the banks of the Ohio River that is a drinking water source for millions. Any mishap at the site, either accidental or through operational violations, will immediately impact the River. Because of the River and the residences and school immediately adjoining the site, there is absolutely no margin for error at WTI. For this reason alone, it was both technically and legally indefensible for the State of Ohio to permit WTI to operate there. The fact that the Ohio River Valley surrounding East Liverpool is prone to atmospheric inversions that trap air emissions is a separate ground which, standing alone, should have led to the site's prompt disapproval...

Attorney Richard Sahli, Former Chairman, Ohio Hazardous Waste Facility Board from June 1988 through January 1991. December 6, 1993

In summary, regardless of site preparation, flood hazard areas are inappropriate for a hazardous waste management facility. Based on this and other considerations, it appears that siting considerations and alternatives were not fully and properly evaluated in this case.

Ohio Department of Natural Resources, in a letter concerning WTI, December 29, 1982.

A top of the line, quality waste analysis plan is really needed, especially when one considers that a school is located only 1100 feet from the site. There can be no significant margin for analytical or data interpretive errors, given that human error will always manifest itself in the form of routine facility accidents. ....What may be "okay" for storage facilities is insufficient for a major commercial incinerator parked next door to a school. As a Waste Management Division Director employed in Region V is reported to have stated, "Let's put the "E" back in the EPA".

Allen Debus, U.S.EPA Region V, in a memo concerning WTI, March 19, 1992

The following is a statement by John Higgins, Regional Environmental Engineer for the State of Massachusetts, in a November 20, 1986 letter explaining the rejection of a permit to build a solid waste incinerator in Holyoke , Massachusetts, because of the location:

The proposed facility would be located in a river valley surrounded by elevated terrain. This topography would augment any air pollution problems from the facility. The residents of the elevated areas surrounding the plant would be exposed to higher levels of pollution from the plant than if the land were level. The upper valley meteorology of the area could lead to persistent trapping of pollutants, which would raise short term exposures. The pollutants would not disperse as quickly as they would if the plant were located on level terrain. The health effects of the pollutants emitted from this plant would be heightened by its location because residents of impacted areas would be exposed to higher levels of pollutants for longer periods of time..

WHAT POLITICIANS SAY ABOUT THE LOCATION:

The potential impact on the people of this community...on their health, on their children's health, on the investment they have made in their homes and businesses...is too great to proceed without study and caution.

And the very idea of putting WTI in a flood plain you know its just unbelievable to me!

Vice Presidential candidate Al Gore, July 19, 1992

I mean the federal government should not permit incinerators where you are going to have on-site storage of garbage in a flood plain...that should not be done. You ought to have some jurisdiction at the state level about how close they get to schools and other things which are really troubling.

Presidential candidate Bill Clinton

The fact is the USEPA has been doing an end run around the regulatory process for 10 years, thinking the Ohio Valley is already so polluted no one would notice a little more.

An overwhelming majority of East Liverpool's citizens do not support WTI. In fact, many elected City Councils in Ohio, West Virginia and Pennsylvania on on record as opposing WTI, as are health groups, labor unions(UAW, Steel Workers, Communications Workers) and others.
Encouraging environmental tech is one thing, building a hazardous waste incinerator on the banks that supplies drinking water for 10 million people, next to an elelmentary school and neighborhood, is just absurd.

Senator Howard Metzenbaum, Ohio, December 15, 1992

Be advised that I have never agreed with the location of the facility and have stated that the students at East End Elementary School should be relocated. If a problem at WTI should happen to occur during school hours, what plan would be sufficient enough to ensure the safety of the children? Just the mention of a "safe room" with tape around the doors is enough to fuel the thoughts of a prudent person to consider the relocation of the students.

James Traficant, Member of Congress, in a letter to a member of the East Liverpool Board of Education, Re. the WTI Evacuation Plan, April 13, 1993.

We don't want WTI! The dream of the residents of the state is to leave it better than it was found. Our dream and WTI are absolutely inconsistent. ...We are all entitled to our health. [T]his affects all generations. This [threat] is the result of big business and greed.

The Honorable Gaston Caperton, Governor of West Virginia, June 2, 1991.

U.S.EPA wants this facility as much as WTI does...this permit is too important to the agency to deny no matter what the consequences are to East Liverpool.

Doug Applegate, US House of Representatives, 7/25/83

WHAT OTHERS SAY ABOUT THE LOCATION:

I believe the widespread effects of a potential disaster on the site are too great and too many questions remain unanswered. Moreover, the board herein, seems blinded by the technological sophistication of the plant when time and time again human error has managed to overcome "fail-safe" technology from Bhopal to Chernobyl to the Rhine. One cannot and should not presume the worst will occur. However, proper planning requires fallback measures to ensure that even minor accidents by plant management standards do not turn the southern portion of this state into a wasteland. There cannot be any reason good enough in my view, for the plant's current proposed location.

Justice J. Locher, Ohio Supreme Court, dissenting opinion

The WTI case reveals something of a noble lie in our country. Citizens are assured that the federal government will require careful permitting and operation of hazardous waste facilities. They are encouraged to participate in the review of such facilities and promised that they will have a voice.in decision of siting and permitting. Most communities do little to test these promises. The WTI families did and discovered that there is little that a community can do to oppose powerful interests supporting the hazardous waste market. What we do in this case and dozens like it will determine whether these promises will be finally honored or discarded by the government.

WTI is an embarrassing example of lax siting regulations for waste facilities. Located on the banks of the Ohio River, WTI was placed in a residential area only 1100 feet from an elementary school......WTI is about money!.....There is no better case for new environmental equity laws than WTI. Ultimately, WTI sends a chilling message that individuals are insignificant players in the global marketplace.

Jonathan Turley, Professor of Law and Director, Environmental Crimes Project, George Washington University, Congressional Symposium on the WTI Incinerator, December 6, 1993.

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5 Jan 95

Dear Paul, Ellen and Terri,

Attached are two brief discussion papers that I regard as pertinent to the WTI risk assessment:

* WTI Dioxin Emissions Before and After Carbon Injection
  This paper documents that at least the first series of tests conducted after installation of the carbon injection system were conducted at much lower feedrates than the feedrates of the pre-CIS trial burn. I do not know whether similarly low feedrates were also used during the subsequent test burns.

* Limitations of Historic and Current Methods for Measuring Polychlorinated Dioxins and Furans in Incinerator Emissions.
  Once again, I suggest that the quantification of PCDD/Fs in stack gases suffers from great uncertainty.

In the absence of data such as waste composition, waste feedrates and stack gas flowrates, it is not possible to reach any useful conclusions about the validity of the identities or quantities of PICs used in the risk assessment. Obviously they have presented an extensive list of PICs ... about 137, I believe. Since they report PIC emissions, grams per second, rather than PIC concentrations, grams per cubic meter, it is not possible to compare their data with those of other trial burns or published studies.

Given the conclusion by Huang and Beukens (1995) that de novo synthesis from carbon particles is the dominant mechanism for PCDD/F formation, I would be very curious about the characteristics and fate of the injected carbon. Where does it end up? [Huang, H., and Beukens, A. On the mechanisms of dioxin formation in combustion processes. Chemosphere (1995) 31(9): 4099-4117]

Based on their highest TEQ emission rate, they are releasing PCDD/Fs from the stack at a rate of about 0.12 gram per year. This exceeds the acceptable annual dioxin intake for more than 400 million people, based on the currently proposed risk-specific dose of 0.01 pg/kg/day and assuming that one cancer death in a million is acceptable.

In their estimate of fugitive emissions, they have failed to consider contributions from many potential sources, including the largest likely source -- the kiln. On page V-13 of Vol. II of the risk assessment, they admit as follows:

> Kiln overpressure events trigger automatic waste feed cutoffs (AWFCOs); ... due to the frequent occurrence of kiln overpressures (WTI 1994), a detailed evaluation of these events has been conducted ... However, since these emissions occur from the kiln seals, PCDD/PCDF are unlikely to be associated with these releases ..."

Of course, such AWFCOs can be expected to be highly significant sources of fugitive emissions, including products of incomplete combustion, such as the PCDD/Fs as well as many others. Contrary to their conclusion that PCDD/Fs are unlikely to be formed, the conditions, which include high soot formation, are extremely conducive to PCDD/F formation, as described by Huang and Beukens (1995).

In general, fugitive emissions are considered to be much greater sources of wastefeed components than are the stack gases. For example, at Chem-Security's PCB disposal facility in Swann Hills, Alberta, Canada, fugitive emissions accounted for over 98 percent of the PCBs released at the site in 1994. Of some 95,116 kilograms of PCBs processed, the facility released 34 kilograms into the environment at that site, a loss rate of approximately 0.036 percent. [Clearstone Engineering Ltd., An Assessment of Fugitive PCB Emissions, Calgary, Canada, June 9, 1995.] Obviously, such losses of more volatile waste components can be expected to be much higher.

I hope these comments are of some use. Proper detailing of the emissions component of the risk assessment would have required prior detailing of all of the trial and test burns.

Best regards,

Pat Costner

Preliminary Comments on the "Risk Assessment for the Waste Technologies Industries (WTI) Hazardous Waste Incinerator Facility (East Liverpool, Ohio): Draft"

10 January 1996

Tom Webster
Department of Environmental Health
Boston University School of Public Health
80 East Concord St., Boston MA 02130

Invited Participant. Indoor Air Quality/Total Human Exposure Committee of the Science Advisory Board, reviewing the USEPA's "Addendum to the Methodology for assessing health risks associated with indirect exposure to combustor emissions."

Member, Peer Review-Risk Characterization Committee, USEPA Dioxin Reassessment

General Comments

The WTI risk assessment initially gives the appearance of extreme thoroughness. A great deal of time and money has clearly been put into this effort. But my general impression is that this attention to detail obscures the bigger picture. The apparent lack of attention to these larger issues contrasts sharply with the growing data requirements and sophistication--or over-sophistication--of risk assessment techniques. I will address some of these larger issues first.

It should be recalled that indirect routes of exposure were omitted from the preliminary risk assessment for WTI released by EPA a few years ago. Partly in response to public concern and legal controversy, indirect exposure assessment is a major thrust of the draft health risk assessment. While it is certainly true that the public has grown increasingly concerned over exposure via the food chain, these concerns need to be seen from a larger perspective. Health risk assessments for incinerators have evolved from simply examining inhalation--as was common practice a decade ago--to a more thorough consideration of exposure pathways. However, at the same time the public has grown more skeptical of the value of health risk assessments and their use by regulatory agencies. This skepticism has at least two components. First, the public is mistrustful of the policy uses of risk assessment. Indeed, many suspect that health risk assessments are used to justify policy decisions that have already been made. Second, the public is becoming more aware of the scientific limitations of health risk assessment, limitations which are often not clearly acknowledged. Unless these problems are addressed, I believe that such assessments will be largely a wasted effort.

Health risk assessments that contain the correct caveats may provide some useful, albeit limited, information, but only if put in the right context. Unfortunately, I believe that the current uses of risk assessment has tended to usurp other modes of judgment. In my opinion, risk assessment is illegitimate unless conducted in conjunction with other considerations. First, alternatives must be considered, a notion which often seems forgotten these days despite the language of the National Environmental Protection Act. Why should a facility be considered "acceptable" if a viable, less damaging alternative is available? Critics have long maintained that alternative policies for managing hazardous waste were never seriously considered in the case of WTI; they are not addressed in the current document either. There are also important political and ethical issues surrounding the notions of "acceptable." In a country where individual rights are supposed to be protected, risk assessment/management appears willing to sacrifice the health and well-being of some for the profit of others with little in the way of due process. How are decisions about "acceptability" made? Who makes them? Shouldn't the public, i.e., those who will bear the consequences, make such decisions? The opportunity to speak at public hearings and provide written comments is not sufficient. Finally, although many try to distinguish between risk assessment technique and "risk management", this is largely a false dichotomy. Risk management decisions are embedded throughout risk assessment, including the document under review.

An example of the latter is the primary reliance of NIOSH's IDLH values for assessing the potential effects of accidents. These values are designed for workplace hazards and are based on the ability of healthy young men to flee the immediate dangers posed by the accident. Application of them to the general public is inappropriate. They should certainly not be used in relation to school age children, elderly or the infirm. It's true that the risk assessment later uses the more conservative levels of concern but relegates them to the uncertainty analysis. Indeed, the risk assessment has a general tendency tends to bury the bad news or split it up into smaller, perhaps more "acceptable" pieces. Another example is the decision to treat breast-feeding as a risk to a limited number of people associated with a specific activity, placed in the same category as subsistence fishing. However, most of the medical community believe that breast-feeding is or ought to be a normal part of every human's life. Such decisions do little to inspire public confidence.

Non-Cancer Effects of Dioxin-like Compounds

In my view, one of the serious flaws of the WTI risk assessment is its treatment of the potential non-cancer effects of dioxin-like compounds. The document states that "Because the threshold levels for exposure to 2,3,7,8-TCDD and dioxin-like compounds below which toxic effects are not observed has not been established, the USEPA does not currently list RfD or RfC values for dioxin-like compounds"(V:III-6). This is at best a half-truth and has the appearance of hiding unpleasant information. The dioxin reassessment actually concluded that any RfD would likely be below "background" exposure. This conclusion was not changed materially by the last year's SAB review. The risk assessment cannot hide behind the fact that the agency has not promulgated an official RfD as these results are widely known.

Having side-stepped this issue, the risk assessment instead estimates an incremental dose. After comparing this with the estimated average background, they conclude that any incremental effect is very small. This is not a completely useless comparison, although it has its flaws. First, as noted below, I think there is good reason to believe that the risk assessment underestimates exposure to dioxin-like compounds. Second, a proper comparison needs information regarding both variability in the background exposure and the shape of the dose-response curve, about which we know little.

But there is a larger issue. With these sorts of comparisons, individual sources look smaller as the background gets bigger, hardly the macro picture that EPA should be encouraging. Indeed, I believe that the "background" exposure is due to the combined emissions from a large number of sources, mixed together due to the relative environmental persistence and long-distance transport of these compounds. Examination of the short-range impact of a single source is an inherently flawed. This problem is not unknown to the agency: "Evaluation of indirect exposures from single sites is too narrow a basis for decisions regarding stationary combustor risks" (USEPA 1994).

General limitations of incinerator risk assessment which need to be acknowledged up front.

Public skepticism of risk assessment is also fueled by growing awareness of its scientific limitations. These limitations are not usually acknowledged in a forthright manner. They do not prevent many risk assessors from making fairly strong conclusions about the public health and environmental impacts of a facility. If usurpation is the first flaw of risk assessment, hubris is the second. These limitations must be stated openly and up front, not buried in the back chapters on uncertainty. Here are a few

1) Limited knowledge of what is emitted. The document acknowledges that only some of the emissions have been characterized. The proposed accounting for other dioxin-like compounds--brominated, but not mixed halogenated, etc.--is not very convincing.

2) Limited knowledge of fate and transport: Many of these compounds are chemically reactive in the environment, producing other compounds which may be more or less toxic.

3) Limited knowledge of the toxicity of individual compounds. With no or incomplete data, the toxic effects of a compound are treated as zero. This is apparent in the discussion of compounds for study. The discussion of the possible risks of endocrine disruptors, a very hot topic in environmental health (and of great concern to the public) is a good illustration.

4) Limited knowledge of dose-response and human effects.

5) Limited knowledge of the toxicity of mixtures of compounds.

6) Lack of attention to distant impacts.

7) Lack of attention to cumulative effects.

Technical Comments on Indirect Exposure Methodology

The limited time I had to review the risk assessment prevents me from giving a full technical review here. Discussed here are some problems. I would refer the committee to another USEPA document (USEPA 1994) for more technical comments on methodology for indirect exposure.

Emission Factors

The analysis does not for account for upsets, which may lead to substantial increases of emissions of dioxin-like compounds. As a result, the range of emissions appears quite small.

Vapor/Particulate Partitioning

Bidleman's review provides a reasonable starting point for thinking about vapor/particulate partitioning, remembering that partitioning depends on the ambient temperature. If a pollutant is particulate-bound at stack temperatures, then use of the stack PSD seems appropriate. However, if a substantial fraction of a pollutant is vapor at ambient temperatures, it should equilibrate between vapor and ambient particulate. The stack PSD may not be very relevant in this case. The dry deposition rates implied by the air modeling appear unreasonably low.

Biotransfer of dioxin-like compounds from vapor to plants

The risk assessment uses the Bacci model divided by an "empirical" correction factor of 40 applied to all dioxin-like compounds. I do not think this is justified, leading to a possibly substantial underestimation of exposure, especially for congeners other than 2,3,7,8-TCDD. EPA examines this issue in Estimating Exposure to Dioxin-Like Compounds (EPA/600/6-88/005Cc), Volume III. In particular, EPA contrasts the results of McCrady et al with Bacci. Comparison of the grass and azalea results in Table III of McCrady et al. suggests that application of the Bacci model overestimates 2,3,7,8-TCDD vapor transfer to grass by about an order of magnitude or so (about 40 using the expected value for 2,3,7,8-TCDD from Bacci's regression model). The results of McCrady et al. do seem superior for estimating vapor deposition of 2,3,7,8-TCDD onto grass, but the generalizability of this ratio is unclear. To do so we would need to know more about photodegradation of other compounds and the differences between plants. McCrady et al assert that the photodegradation of 2,3,7,8-TCDD accounts for much of the discrepancy from Bacci's results. If this is the reason, it is likely to be less of a problem for many other congeners which are thought to be less susceptible to photodegradation. (Also see USEPA 1994).

EPA attempts to validate their overall air-to-beef model in the same document by comparing rural air samples and beef samples (from different locations). I consider the data used to be far too sparse to place much confidence in these comparisons.

Uncertainty analysis

Although the uncertainty analysis is billed as presenting probable bounds on risk, it should be clearly noted that it actually looks at parameter uncertainty AND variability in exposure. Uncertainty in biological models, typically the overwhelming source of uncertainty, is not addressed. It mixes in variability of population based factors (e.g., consumption) which is OK for population risk estimates but not necessarily justified for examining susceptible groups. It does not address model specification error, a potentially huge problem. The variance in crucial parameters (e.g., biotransfer) is based on the variance in Kow, etc., rather than also taking into account the uncertainty of the model itself (often large for such correlation models). Correction of these problems would lead to much wider overall variance.

REFERENCES

USEPA (1994). Review of Draft "Addendum to the Methodology for assessing health risks associated with indirect exposure to combustor emissions." EPA-SAB-IAQC-94-009b.

Annex

LIST OF ACCIDENTS IN THE CHEMICAL INDUSTRY
by
R. ANDURAND

in "The safety report and its application in industry", *Annales des Mines*, 7-8, July/August 1979, 115-38

Location	Country	Date	Product involved	Damages	Causes
Hull	GB	1921	Hydrogen	Windows shattered within 3 km radius. Pressure felt within a 7 km radius and tremors up to 70 km.	Explosion of confined gas
Cleveland	USA	1944	LNG	136 deaths. Nearby roads swept by burning gas. Windows shattered, pavements ripped up, drain covers blown across rooftops. One fire engine blown into the air.	Explosion of confined gas, fireball
Manhattan District Project	USA	1946	Uranium hexafluoride	Two deaths, three people seriously injured, thirteen slightly injured. Explosion of UF_6 and very hot water in a laboratory. HF aerosol carried up to 100 m by the wind.	Explosion
Ludwigshafen	FRG	1948	Dimethyl-ether	245 deaths, 2,500 injured. Wagon ruptured near a dimethyl-ether factory followed by explosion and fire (Cost: 80 million FF).	Explosion of non-confined vapor cloud
Newark (Warren Oil Port)	USA	1951	Not specified	No record	Explosion of non-confined gas cloud
Wilsum	Germany	1952	Chlorine	Seven deaths in an escape of 15 tonnes, coming from a storage tank.	Toxic product
Whiting, Indiana	USA	1955	Not specified	Two deaths, thirty injured following a detonation in a pressurised container. Storage tanks pierced by the burst burned for eight days (Cost: 80 million FF).	Detonation
New York	A	1956	Ethylene	1,100 m^3 of ethylene escaped into the atmosphere causing an explosion in the air.	Explosion of non-confined vapour cloud

ion	Country	Date	Product involved	Damages	Causes
Niagra Falls, N.Y. State	USA	1958	Nitro-methane	Two hundred injured when a tank wagon detonated and caused a big crater (Cost: 5 million FF).	Detonation
Signal, California	USA	1958	Not specified	Two dead when vapor coming from an overflowing tank ignited and ravaged 70 per cent of the installation.	Fireball
La Barre, Los Angeles	USA	1961	Chlorine	One dead in a cloud of 27.5 tonnes, released by a tank wagon.	Toxic product
Kentucky	USA	1962	Ethylene oxide	One dead, nine injured. Explosion 'equivalent' to 18 t of TNT.	Explosion of non-confined vapour cloud
Berlin, NY State	USA	1962	Propane	No records.	Explosion of non-confined gas cloud
Louisiana	USA	1963	Ethylene	Long lasting fire	Explosion of confined vapour
Texas	USA	1963	Propylene	Fire and explosion in a low-pressure polymerisation unit for polypropylene (Cost: 30 million FF).	Explosion of confined vapour
Texas	USA	1964	Ethylene	Fire explosion subsequent to an escape of gaseuous ethylene (Cost: 15 million FF).	Explosion of confined gas, fireball
Texas	USA	1964	Ethylene	Two dead in a fire following rupture of a high-pressure ethylene pipeline (Cost: 20 million FF).	Explosion of non-confined vapour
Massachusetts	USA	1964	Vinyl monomere chloride	Seven dead, 40 injured. Rupture of an observation window under tension and pressure. The escaping gas caught fire and exploded (Cost: 25 million FF).	Explosion of confined vapour
Pierrelatte	France	1965	Uranium Hexafluoride + fluorising agent	No deaths of injuries or evacuations. Fire subsequent to escape of 300 kg of a mixture caused by chemical corrosion of a sleeve of a distilling column at the CEA pilot plant. Pilot run stopped for eight days. Aerosol of hydrofluoric acid carried over 200 metres by the wind. No external contamination: uranium remained confined in the building natural uranium).	Product of of only chemical toxicity
Louisiana	USA	1965	Ethylene	12 injured by fire and explosion of ethylene from ruptured pipes (Cost: 15 million FF).	Explosion of confined gas
Texas	USA	1965	Propylene	Explosion and fire subsequent to pipe break in a propylene polymerisation plant (polypropylene) (Cost: 30 million FF).	Explosion of confined gas and fireball
Feyzin	France	1966	Propane	16 dead and 63 injured. A valve blocked by freezing during sample-taking from a storage sphere permitted formation of a gas cloud which exploded near a motorway.	Explosion of non-confined gas cloud
La Salle	Canada	1966	Styrene	11 dead after explosion following the breaking of an observation window (Cost: 20 million FF).	Explosion of confined vapour
Not specified	FRG	1966	Methane	Three dead, eighty three. injured. Circumstances not specified.	Explosion of confined gas
Fernhald, Ohio	USA	1966	Uranium Hexafluoride	One injured (Six days in hospital). Eight people under observation. Human error: the maintenance man thought he was unscrewing the top of a valve but unscrewed the valve itself. Escape of 1.7 t of UF_6 in vapour. Duration forty minutes (Lead Company of Ohio).	Toxic gas + dangerous derivatives (HF)
Santos	Brazil	1967	Coal gas	300 injured, eighty buildings of various sizes within a radius of 2 km either destroyed or damaged.	Explosion of confined gas
Hawthorne, N.J.	USA	1967	Not specified	Two dead, sixteen injured. Building explosion.	Not specified
Buenos Aires	Argentina	1967	Propane	100 injured. Fire destroyed four hundred houses in the neighbourhood.	Fire
Antwerp	Belgium	1967	Vinyl monomere chloride	Four dead, 33 injured. Fire lasted three days.	Fire
Lake Charles Louisiana	USA	1967	Isobutane	Seven dead. A leaking 10 inch shutter valve released a cloud which exploded. Secondary fires and explosions continued for two weeks.	Explosion of a non confined gas cloud
Bankstown, New South Wales	Australia	1967	Chlorine	Five people intoxicated by evaporations. Evacuation of a large part of the town.	Toxic product
Perris	Holland	1968	Light hydrocarbons	Two dead, Seventy five injured. Pressure wave broken windows 2 km away.	Explosion of non-confined vapour cloud
Not specified	GDR	1968	Vinyl chloride monomere	24 four dead.	Toxic product
Paris	France	1968	Petro-chemical products	400 people evacuated. Explosion shook houses in the neighbourhood.	Explosion of confined vapours
Hull	GB	1968	Acetic acid	Two dead, thirteen injured.	Explosion of confined vapours
Rjukan	Norway	1968	Gas	Windows of cars and shops shattered. No statement on type of gas or circumstances.	Explosion of confined vapours

Location	Country	Date	Product involved	Damages	Causes
Soldatna, Alaska	USA	1968	Pressurised liquified gas	Two people seriously injured.	Not specified
Tamytown	USA	1968	Propane	3,500 people evacuated.	Not specified
Lievin	France	1968	Ammonia	Explosion of road tanker in the process of unloading. Escape of 19 tonnes. Six dead, twenty people living in the neighbourhood hospitalised for poisoning.	Formation of a toxic aerosol
Grandes Armoises	France	1969	Ammonia	During transfer of NH_3 from a fixed to a mobile tank a hose ruptured. Escape of 4 tonnes.	Formation of a aerosol
Teeside	GB	1969	Cyclohexane	Vegetation burned over a surface of 2,000 × 450 metres. Sixteen cows, one dog and various chicken killed near living quarters.	Fireball
Not specified	Libya	1969	LNG	Two dead, twenty three injured.	Not specified
Puerto la Cruz	Venez-uela	1969	Light hydro-carbon	12 injured.	Not specified
Long Beach, California	USA	1969	Mineral Oil	Five dead. Considerable damage to windows and ceilings in town.	Explosion of confined vapours
Escombreas	USA	1969	Petroleum	One dead, eighty three injured. The cover of a 2,600 t tank was blown off in a suburban area.	Explosion of non-confined vapours
Repesa	Spain	1969	Pressurised liquified gas	Four dead, three injured, 5,000 people evacuated. The shock wave broke windows within a radius of several km.	Fireball
Crete, Nebraska	USA	1969	Ammonia	An escape of liquified propylene gas caused a refinery fire that burned for six days.	Toxic product
Basle	Switzer-land	1969	Liquified nitric product	Six dead. Escape of 64 t of ammonia from a wagon.	Detonation
Philadelphia	USA	1970	Petrol products	Three dead, twenty eight injured. The pressure shook windows up to 1 km away.	Detonation
Osaka	Japan	1970	Gas	Five dead, twenty seven injured. Explosion in an oil refinery.	Detonation of confined gas
				92 dead. Gas explosion on a subway construction site in Osaka.	
Mitcham, Surrey	GB	1970	Propane,	Substantial destruction of private property in the neighbourhood: roofs cracked, windows broken, fences overturned, dwellings destroyed by fire, two cars destroyed.	Explosion of confined gas
St. Thomas	Virgin Islands	1970	Natural gas	25 injured. The explosion shook practically the whole island.	Explosion of
New Jersey	USA	1970	Petroleum products	40 injured. The shock waves shook windows in an area of 150 km^2.	Explosion of non-confined vapours
Port Hudson	USA	1970	Propane	No human casualties. Windows were broken up to 18 km away. The derivative cloud was ignited by an electric motor at a cold storag eunit in its trajectory.	Explosion of non-confined gas cloud
Blair, Nebraska	USA	1970	Ammonia	Overflow of a dryogenic tank of 32,000 t for two and a half hours. Escape of 145 tonnes. Animals and fish killed. Three foliage burned over 40 hectares of woodland. Low cloud of 2.50 to 9 metres thickness stretching over 365 hectares at 2,500 meters from the tank. Affected area: one house, one farm; two dogs killed at 1,770 metres distance.	Toxic product
Crescent City	USA	1970	Propane	Derailment of a wagon; the commercial centre of the town was destroyed.	Explosion of vapours emitted by flash fire and boiling liquified gas
Emmerich	FRG	1971	Not specified	Four dead, four injured, many buildings in the area damaged.	Explosion of confined gas
Not specified	Holland	1971	Butadiene	38 dead, seventy five injured, five hundred people evacuated; window frames dislocated within a radius of 15 km. Accident in a 20-story tower.	Not specified
Not specified	Holland	1972	Hydrogen	230 injured; windows damaged up to 3 km from the site of the accident which occurred during shunting of a wagon.	Explosion of confined gas
Sao Paolo	Brazil	1972	Gas	21 dead, twenty injured, an island put out of action completely.	Explosion of confined vapours
St. Louis	USA	1972	Propylene	350 injured. Thirteen tank wagons of butane exploded.	Explosion of non-confined vapours
Virginia	USA	1972	Gas	One dead, sixteen injured.	Explosion of confined gas and fire
Not specified	Mexico	1972	Butane	Seven dead. Vapours released from a reactor exploded. Evacuation of hundreds of people within a radius of several hundred meters (Cost: 10 million FF).	Explosion
Not specified	Japan	1973	Vinyl monomer chloride	1,000 people evacuated.	Explosion of non-confined vapours
Lodi	USA	1973	Methanol	Four dead, twenty four injured. The explosion damaged buildings over a large area and blew in hundreds of windows. Cars were covered with a shower of debris and crashed by enormous pieces of concrete.	Explosion of non-confined vapours

Location	Country	Date	Product involved	Damages	Causes
Gladbeck/Ruhr	FRG	1973	Cumene	Four dead, two missing, thirty seven injured following the overturning of a truck carrying liquified presurised gas.	Not specified
Sheffield	GB	1973	Gas from	One dead, four injured.	Explosion of non-confined gas
St. Amand les	France	1973	Propane		Explosion of non-confined gas
Tokuyama	Japan	1973	Ethylene		Explosion of non-confined gas
California	USA	1973	Vinyl monoor chloride	200 litre steel containers of chemicall products where hurled on top of houses, fields and into the bay.. Thousands of windows were broken and at least eight small houses seriously damaged. The shock wave was felt 80 km away.	Unknown
Cologne	FRG	1973	Vinyl monomor chloride	Rupture of a joint caused escape of 10 t of product.	Explosion of non-confined gas
New York	USA	1973	Pressurised liquified gas	40 dead.	Explosion of non-confined gas
Western Bohemia	Czecho-slovakia	1973	Gas	47 dead in a factory.	Explosion of non-confined gas
Potchefstroom	South Africa	1973	Ammonia	18 dead of which six were outside the factory. Sixty five injured. Release of 38 t of ammonia. The aerosol cloud of 20 metres thickness and a diameter of 150 metres drifted on to the neighbouring town.	Aerosol of toxic product
Falkirk	GB	1973	Inflammable liquid	Destruction of a tar factory.	Fireball
Texas	USA	1974	Isoprene	Twelve injured. Windows broken over a large area.	Explosion of non-confined gas cloud
Los Angeles	USA	1974	Organic peroxides	Run-away road tanker carrying organic peroxides exploded, causing 250 million FF damge.	Detonation
Beaumont, Texas	USA	1974	Isoprene	Two dead, ten injured: explosion of a vapour cloud which followed a big spillage of isoprene (Cost: 80 million FF).	Explosion of non-confined gas cloud
Not specified	Czecho-slovakia	1974	Ethylene	14 dead, 79 injured.	Explosion of non-confined gas
Flixborough	GB	1974	Cyclohexane	28 dead, 104 injured, 3.000 people evacuated, 10 houses damaged; fishing in river Trent banned.	Explosion of non-confined vapour
Rotterdam	Holland	1974	Petro-chemical products	Enormous fire.	Fireball
Not specified	Romania	1974	Ethylene	One dead, 50 injured.	Explosion of non-confined gas cloud
Nebraska	USA	1974	Chlorine	500 people evacuated. Toxic vapour clouds spread about.	Toxic product
Floride	USA	1974	Propane	Two storages destroyed. Cars crushed and windows broken within an area of four blocks of buildings.	Explosion of non-confined gas cloud
Wenatchee	USA	1974	Monomethyl aminonitrate	Two dead, 66 injured in the explosion of a wagon.	Detonation
Not specified	Holland	1975	Ethylene	Four dead, 35 injured.	Explosion of non-confined gas cloud
Marseille	France	1975	Petro-chemical products	One dead, three injured: the explosion broke windows in a large area around the complex.	Explosion in confined area
Not specified	South Africa	1975	Methane	Seven dead, seven injured. Whole town gas supply was cut for two days.	Unknown
Antwerp	Belgium	1975	Ethylene	Six dead, 13 injured. Ethylene escaping from a compressor exploded, causing extensive damage to buildings and at the plant.	Explosion of non-confined gas cloud
Philadelphia	USA	1975	Crude oil	Eight dead, two injured. Vapours from a storage tank exploded in a boiler house when a marine lighter was refuelled. Cost: 50 million FF.	Explosion of confined vapour
Not specified	Holland	1975	Propylene	14 dead, 104 injured.	Explosion of non-confined gas cloud
Oak Ridge	USA	1976	Uranium hexafluoride	Reaction of UF_6 and oil from a vacuum pump in a type 30 (2 tonnes) container during transport: liquid UF_6 under pressure. Two injured. Eight days' stoppage.	Explosion of UF_6 + hydro-carbon product
Seveso	Italy	1976	TCDD	Complete evacuation of the area until now (1979). Abortions authorised exceptionally. Decontamination made very difficult because of the non-soluble nature of the product.	Aerosol of solid toxic product

Location	Country	Date	Product involved	Damages	Causes
Beek	Holland	1976	Naphta	14 dead, 30 injured when an escape caught fire. The explosion damaged windows of shops and houses. Cost: 100 million FF.	Explosion of non-confined vapour cloud
Baton Rouge, Louisiana	USA	1976	Chlorine	10,000 people evacuated. Mississipi banned for navigation over 80 km to the north.	Toxic product
Sandelfjord	Norway	1976	Inflammable liquid	Rupture of underground piping: fire and explosion killing six people and causing 100 million FF damage.	Fireball, explosion of confined vapour
Pierre Benite	France	1976	Acrolin	Escape from container of a wagon in the Rhone following human error (21 tonnes(. River fauna destroyed from Pierre Benite to Vienne (320 tonnes).	Toxic product
Brachead	GB	1977	Sodium chloride	Fire and explosion. Circumstances not specified.	Detonation
Mexico City	Mexico	1977	Ammonia	Two dead, 102 people treated for poisoning. Gas entered the drainage system.	Toxic product
Umm Said	Quatar	1977	Pressurised liquified gas	Seven dead, many injured. The explosion superficially burned the villages up to 2 km around. The international airport of Doha was closed for two days.	Fireball
Mexico City	Mexico	1977	Vinyl monomor chloride	90 injured. No details.	Unknown
Not specified	Taiwan	1977	Vinyl monomor chloride	Six dead, 10 injured.	Unknown
Pierrelatte	France	1977	Uranium hexafluoride + hydro-fluoric acid	One dead, nine injured.	Chemically toxic product: fluorhydric acid
Jacksonville	USA	1977	Pressurised liquified gas	Neither deaths nor injuries nor poisoning. Comhurex factory. Subsequent to human error rupture of a valve in "6 o'clock" position on a type 48 container. Expulsion of 7.1 t of UF_6 liquified under pressure.	Not specified
Rockingham North Carolina	USA	1977	Uranium hexafluoride	2,000 people evacuated. Derailment of a 29 train. Four type 48 (12 tonnes) containers of UF_6 involved in the accident. Fire of ammonium nitrate, fertiliser and ground nuts. The containers held: no escape of UF_6.	Derailment
Gela	Italy	1977	Ethylene oxyde	One dead, two injured	Explosion of confined vapour
Not specified	India	1977	Hydrogen	20 injured. The explosion shook a fertiliser factory, an oil refinery and a village.	Explosion of confined gas
Not specified	Italy	1977	Ethylene	Three dead, 22 injured. Shop windows and doors smashed. Car blown several metres up into the air.	Explosion of confined gas cloud
Pierrelatte	France	1977	Uranium hexafluoride + hydro-fluoric acid	Neigher deaths nor injuries nor poisoning.f Break of a shutter clamp on a tank that was overfilled with UF_6 under hydrostatic pressure, in the course of warming up. The passage from the solid to the liquid state causes a volume increase in the order of 25-30 per cent. Release of 1,200 kg of "natural" UF_6, confined in the building.	Toxic product: gasuous hydro-fluoric acid
Pasacabalo near Cartagena	Columbia	1977	Ammonia	30 dead, 22 injured. The villagers in the neighbourhood suffered the effect of the gas. The installations of the state factory Acobal were destroyed at the time of shift change (some hundred workers were then present) It has not been proved that NH_3 was the cause of the accident but rather a fire.	Toxic product
Cadarache	France	1977	UF_6 + hydrofluoric acid	Neither deaths nor injuries nor poisoning. During warm-up of a thermic trap which was overfilled with UF_6 following human error a crack developed in the partioning wall through hydrostatic pressure which put UF_6 in contact with cooling fluid and produced an aerosol of hydrofluoric acid and UO_2F_2 which moved the filters of the extractor fan: two neighbouring workshops, becoming depressuriesed, were invaded by HF aerosol. Restart of the workshops after one week. UF_6 released: 20 kg. Natural uranium.	Toxic product: gaseuous acid hydrofluoric
Seoul	South Korea	1977	Explosives	58 dead, 1,300 injured. Explosion of train. Circumstances not specified.	Explosions
Los Alfaques, South Tarragona	Spain	1978	Propylene	216 dead, many disappeared, several hundred injured following an escape of liquified propylene under pressure, following road accident of a tanker near a camping site in summer.	Explosion of non-confined gas
Not specified	USA	1978	Grain dust	Explosion followed by large fire. Number of dead and injured not specified. Criminal attack not excluded.	Dust explosion
Portsmouth	USA	1978	UF_6 + hydrofluoric acid	Number of people poisoned not specified No deaths, no injuries. Rupture in the piping of a hydraulic jack of a lorry which carried a 48G type container (thin partioning wall, not used in France) which held 9.6 t of liquified UF_6 under pressure. In the fall of the container a cylinder clamp cuased a 19 cm long fissure in the partioning wall when it struck the ground violently and expelled the whole UF_6 content.	Toxic product: aerosol of hydrofluoric acid
Waverley, Tennessee	USA	1978	Propane	12 dead and at least 50 injured when a tank wagon which had derailed exploded.	Explosion of vapour emitted by flash of pressurised

Location	Country	Date	Product involved	Damages	Causes
Youngstown, Florida	USA	1978	Chlorine	Eight dead, 10 injured, evacuation of 3,500 people in an area of 10 km^2 following spread of chlorine escaping from a derailed tank wagon. The enquiry concluded that a criminal attempt was likely.	Toxic product
Baltimore	USA	1978	Sulphur trioxide	Toxic fumes drifting up to 15 km. Mor ethan 100 people treated fro nausea.	Toxic product
Paris(Passy)	France	1978	Gas	13 dead, 13 injured, 60 flats destroyed. Cars damaged by flying debris. Series of explosions in a building and in underground piping after rupture of a gas pipe.	Explosion of confined gas
New York	USA	1978	Not specified	130 injured: explosion of a deep-freeze lorry near Wall Street.	Not specified
Pierre Benite	France	1978	Acrolein	Escape of some 100 kg of acroleine into the atmosphere. Inconvenience for several thousand people at Pierre Benite and Oullins (tear gas and nauseous gas).	Toxic product
Xilatopec	Mexico	1978	Presurised liquified gas	100 dead, 150 injured. Explosion of a lorry carrying 10,000 litres of LPG in a collision of 12 vehicles on a motorway 85 km north of Mexico City. 85 people died within minutes from the explosion.	Explosion of non-confined gas
Bantry Bay	Ireland	1979	Hydro-carbon vapours	48 disappeared (41 sailors and seven workers) in the explosion of the oil tanker Betelgeuse at the quayside. The oil tanker was not equipped with an inering system to inject inert gas as the tanks are emptied. An efficient inverting system only became obligatory when the "convention of safeguarding human life at sea" of 1974 came into force (only 15 countries, France among them, out of 25 ratified the convention).	Explosion of gas in confined volume, perhaps preceded by fire
Warsaw	Poland	1979	Not specified	41 dead, 77 injured, several hundred people evacuated. Under the effect of the explosion most of the windows of the neighbouring buildings were sent flying in splinters within a radius of 200-300 metres. The accident occurred in the basement of the savings bank building. There were no gas pipes in the building. Welding work was going on in the basement.	Explosion of confined gas probably
Islamabad	Pakistan	1979	Not specified	26 dead, 50 injured, several buildings affected by the pressure. Explosions of an artisan shop in the Raja Bazar at Rawalpindi.	Explosion of instable solid chemical products
Crestview, Florida	USA	1979	Ammonia, Chlorine	4,500 people evacuated within a radius of 2,500 metres. Derailment of a convoy of 28 tank wagons (NH_3Cl_2) on leaving the bridge over the Yellow River.	Toxic gases

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A publication of *Work On Waste USA, Inc.*, 82 Judson, Canton, NY 13617 315-379-9200 APRIL 1994

The Health Impacts of Incineration

Excerpts of Testimony
by Barry L. Johnson, Ph.D.
Assistant Surgeon General, Assistant Administrator
Agency for Toxic Substances and Disease Registry, Public Health Service,
U.S. Department of Health and Human Services
Before the
Subcommittee on Human Resources and Intergovernmental Relations
Committee on Government Operations, United States House of Representatives
January 24, 1994

[The Subcommittee's transcript of the Hearing on "The Health Impacts of Incineration" is not yet available. To request a copy, call 202-225-2548, and ask to be put on mailing list.]

Part 1 of 2

Good morning. I am Barry Johnson, Ph.D., Assistant Administrator, Agency for Toxic Substances and Disease Registry (ATSDR). I am accompanied today by Maureen Lichtveld, M.D., Senior Biomedical Officer for Public Health Practice, ATSDR. We welcome this opportunity to present testimony on the health impacts of incineration: what we know and what we don't. Our testimony is derived from ATSDR's responsibilities and findings under the Comprehensive Environmental Response Compensation, and Liability Act, as amended (CERCLA, or Superfund) and the Resource Conservation and Recovery Act (RCRA, Section 3019).

I will endeavor to respond to the eight issues listed in your letter of invitation to ATSDR. Because ATSDR is a federal public health agency, our responses to your issues will be given in a health context. As the Subcommittee knows, there are many scientific, technology, and policy issues that attend incineration of wastes. Our focus will be only on the public health issues. But as preface, I can share with you that many communities have expressed concern to ATSDR about the potential implications of incineration of wastes. Their concerns are usually expressed as health effects questions about their health. As this testimony will describe, ATSDR often finds itself unable to answer citizens' questions about associations between incineration of wastes and public health impacts. **The scientific information on human health impacts of incineration isn't often available because the relevant studies haven't been conducted.**

Incineration of wastes should be viewed from a public health perspective in the larger context of generation and management of wastes. Wastes become a public health concern when they are improperly managed and disposed of. Therefore, in a public health context, the most protective action is not to produce waste. Waste elimination or minimization comports with prevention or reduction of health consequences of wastes...

3 **What data currently exist on health impacts from incinerator emissions of dioxin, furans, lead, mercury, and other chemicals you think most relevant? What is the range of health effects and their intensity at likely emission levels?**

There are very few data on the actual human health impacts of incinerator emissions on the health of communities near incinerators. Epidemiologic investigations have rarely been conducted, nor have studies of disease and illness patterns been undertaken. For example, ATSDR staff conducted a recent literature search of the 10 most frequently used computerized environmental data bases. As part of the search over 1,000,000 entries were identified. Approximately 72,000 of those entries dealt with incineration. **Of these only one single entry discussed the conduct of a population-based study conducted in a community living in the vicinity of an incinerator.** That study of residents living near Caldwell Systems Incinerator in North Carolina was conducted by ATSDR. *[See Notes in Waste Not # 277.]*

In the absence of human health data, reliance is placed on using toxicity data for individual substances released into the environment. The effect of any toxic substance depends on factors such as duration of exposure, concentration of the substance in the environment, biological uptake, and persons' susceptibility factors (e.g., age). All these factors have to be considered in any estimate of impact of incinerator emissions...

Adequate information does not exist to support speculation on what, if any, human health effects might be associated with incinerator emissions. However, our experience with public health associations related to hazardous waste sites would suggest the need to conduct two kinds of human health investigations. One kind of investigation would look at cancer, birth defect, and respiratory disease rates in areas thought to be impacted by releases from incinerators. These studies would combine health data from many geographic areas. A second kind of study would be site specific. Community health surveys would help clarify whether any unusual exposure or morbidity is occurring that might be associated with a given incinerator.

4 **What data do you have or gather on additive, multiple, and synergistic impacts when there is exposure to more than one chemical, as would be the case with incinerator emissions? Do you expect those impacts would be greater than from single chemical exposure alone?**

There are few data available in the scientific literature on specific interactions of the contaminants that may be released from waste incinerators (dioxin, furans, lead, mercury). In the absence of specific studies using combined contaminants, and limited understanding of the mechanisms of actions for some substances, it is prudent to assume that the effects of exposure to these contaminants is additive.

5 **What data exists on the sensitivity of various populations, by age, gender or ethnic background, to these chemicals?**

Infants and children are arguably the most sensitive segment of the human population to toxic exposures. Infants and children are at special risk because they play outdoors, they ingest or mouth foreign objects, they are smaller (greater chemical doses per pound) than adults, they breathe more air (greater volume and breathing rate per pound) than adults, they are nutritionally challenged (because of protein-calorie requirements to support rapid physical growth) and they are undergoing developmental changes that make them especially vulnerable to chemical exposures. Moreover, they have the longest life expectancies, during which long-term adverse health effects may become manifest. Certain disorders may not become evident until a child reaches a particular developmental stage, which may occur long after the damage was done. Some of the largest environmental health programs (e.g., lead, asbestos) are directed at children.

People of reproductive age. All women of reproductive age must be included in this population because the most severe effects usually occur during the very early stages of pregnancy, often before a woman knows she is pregnant. In addition, pregnant women, especially those with multiple pregnancies, as well as the developing fetus have increased protein-calorie requirements to support rapid physical growth.

The developing fetus is particularly sensitive to chemical exposures. Exposure to chemicals has the greatest impact on those functions undergoing the most active development at the time of exposure. Animal studies and some human studies show that there are critical fetal developmental stages during which chemical exposure can cause permanent and devastating effects.

There is also a small, but growing, scientific literature that implicates some toxicants as causing effects on male reproductive processes. For example, laboratory animal studies have shown that exposure to lead causes adverse reproductive outcomes in male rats, leading to effects on neurologic function in the offspring of the males. Similarly, PCBs in fish and waterfowl have been reported to cause feminine features in males of these species.

CONTINUED TO WASTE NOT # 277

WASTE NOT # 276. *A publication of Work on Waste USA, published 48 times a year. Annual rates are: Groups & Non-Profits $50; Students & Seniors $35; Individual $40: Consultants & For-Profits $125; Canadian $US50; Overseas $65.*
Editors: Ellen & Paul Connett. 82 Judson Street. Canton. NY 13617. Tel: 315-379-9200. Fax: 315-379-0448.

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The Health Impacts of Incineration

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Part 2 of 2

5. (Continued): **Elderly persons and persons with chronic illnesses.** Elderly persons and the chronically ill tend to be more susceptible to respiratory irritants. Long-standing public health policies such as immunization guidelines for influenza support this notion.

The elderly are also nutritionally challenged often due to reduced protein-calorie intake and combined with the metabolic changes that occur during this life stage. Underlying illnesses such as is the case in the chronically ill may increase their susceptibility to particular toxicants. For example, persons with chronic diseases of the kidney system may experience more harmful effects from exposure to renal toxicants such as lead and cadmium compared to a healthy individual.

Moreover, elderly persons and those with chronic illnesses are often socially isolated and potentially less aware of environmental emergencies. Because of special physical challenges, they may also require special services during time of evacuation in the event of such an emergency.

Minorities. Preventing adverse health effects in minorities exposed to hazardous substances is a priority for the Agency for Toxic Substances and Disease Registry (ATSDR). Minority populations, particularly African Americans, Hispanics, and Native Americans suffer disproportionately from preventable morbidity and mortality. Regardless of income, education, or geographical locale these populations are often in poorer health than their white counterparts. This disparity is often associated with inadequate access to health care for preventive services as well as early diagnosis and treatment of disorders including those that may be associated with exposure to hazardous substances. Their disadvantaged economic status also frequently affects priorities on nutritional status. Occupational chemical exposures may increase this population's susceptibility to adverse health effects resulting from other exposures to hazardous substances. In addition, certain pre-existing genetic disorders (G6-PD deficiency, sickle cell anemia) may compound the impact of such exposures...

7. What are the most serious data gaps that prevent us from determining the exact health impacts from incineration?

The data that impede an accurate assessment of the public health impact of incineration can be divided into two categories.

* those associated with the technology and the facility itself
* and those related to environmental health.

Following are examples of some key data gaps in both categories. Also listed are actions that should be considered in order to ensure the protection of the public's health. These data gaps and recommended actions are based on ATSDR's experience in providing consultations concerning hazardous waste incinerators. Key data gaps associated with the incineration technology/facility include:

1. The often inadequate identification and quantification of waste feed as well as fugitive emissions associated with specific incineration facilities.
2. The deposition rates to soil and water for all the potential incinerator stack emissions are not well known.
3. The identification and quantification of emissions during incinerator process upsets are frequently not measured.
4. When stack emissions are analyzed for metals the specific metal compounds or species present are not usually identified.
5. Concentrations of contaminants in environmental samples around incinerator facilities, e.g., soil, water, and ambient air are typically not measured.
6. There are limitations in the current stack testing, air monitoring, and air modeling methods. Some of these methodologies needed further validation.
7. Often there is a lack of data on the concentration of contaminants present in foods that are grown near a facility, such as vegetables from gardens, cattle, fish or shellfish, etc.

...The second category of data gaps concerns the area of environmental health. Key data needs in this area include:

* limited demographic and health data on the surrounding community.
* lack of environmental data such as the types and concentrations of contaminants present and the

environmental media contaminated.

- limited number of exposure, health monitoring and surveillance activities in communities living near operating incinerator facilities.
- data gaps in our knowledge about adverse health effects from specific hazardous substances.
- toxicologic data on the mixtures of substances released from incinerators.

Efforts by federal and state environmental and health agencies are underway to address a few number of these data gaps. In addition to these efforts, attempts should be made to coordinate and collaborate in order to maximize the results in each individual area of data need...

Comments from *Waste Not:*

We thought that the testimony of Dr. Barry Johnson, Assistant Surgeon General, on the Health Impacts of Incineration at a Hearing held by members of the U.S. Congress was significant enough for us to reprint. From Dr. Johnson's testimony it is clear that when the industry says that incineration is a proven technology, they are clearly not referring to the health impacts. Also, it is important to remember that the U.S. EPA does not have a shred of scientific evidence to support their often repeated refrain that "the proper operation of a well-designed, well-maintained and properly operated incinerator would not endanger human health and the environment"

Dr. Johnson refers to the Caldwell Systems hazardous waste incinerator that operated in Lenoir, Caldwell County, N.C., from 1978 to 1988. During the time this incinerator operated, citizens bitterly complained to every agency responsible for their health and environment that the incinerator was making them sick. The reaction to their complaints was that state officials repeatedly told the community that the Caldwell incinerator operated in accordance with all state and federal regulations. By the time the incinerator was shut down, at least five incinerator workers suffered permanent and irreversible brain damage. Community residents say they have a high rate of cancer and suffered severe respiratory problems while the incinerator operated. Many of them have now moved away. The group which has the worst health problems are the employees who worked at the facility. ATSDR conducted a health study of the Lenoir community three years after the incinerator stopped operating. According to L.C. Coonse, a major watch-dog of the Caldwell incinerator:

the ATSDR's health study of residents who lived near the Caldwell Systems incinerator in N.C. compared the health impacts by using, as a control community, another incinerator community only 7 miles away.

As Dr. Barry Johnson stated, this was the only study available in the ATSDR literature search for health impacts on an incinerator community. In the "control community" that ATSDR used, the Broyhill furniture company operates a hazardous waste incinerator. According to L.C. Coonse, "We would have preferred comparisons to a pristine population" instead of comparing health impacts to another community where thousands of pounds of volatile organic compounds are burned in the Broyhill incinerator each year. L.C. told us that he informed ATSDR that a hazardous waste incinerator was operating in the control community, but ATSDR knowingly dismissed this fact, thus compromising the integrity of the ONE study now available to current and future researchers. L.C. noted that in ATSDR's first report, the Caldwell Systems community, compared to the control community, "still showed increased respiratory problems."

For more information:

1. Request a copy of ATSDR's report, Study of Symptom and Disease Prevalence. Caldwell Systems Inc.. Caldwell County. N.C., January 1993. ATSDR's tel #: 403-639-0700.
2. For the transcript on the hearing, The Health Impacts of Incineration, held on January 24, 1994, call the U.S. Congress, Subcommittee on Human Resources and Intergovernmental Relations at tel # 202-225-2548. We were told it would take six months to a year before the transcript is available, but you can call and asked to be placed on the mailing list.
3. See *Waste Not # 163.*
4. See Scandal in North Carolina, a 31-minute video produced in July 1990 (before ATSDR considered doing a health study in the community). Available for $25 from Video-Active Productions . Rt. 2, Box 322, Canton, New York 13617, Tel: 315-386-8797.
5. Contact L.C. Coonse, 71 Pinewood Road., Granite Falls, N.C. 28630. Tel: 704-396-3288.

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Tri-State Environmental Council

October 10, 1994

Rd #1 Box 365
Chester, WV 26034

Dorothy Patton, PhD
Executive Director and Chair
Risk Assessment Forum
Office of Research and Development
U.S. EPA
Washington, DC 20460

RE: U.S. EPA Phase II Risk Assessment for the Waste Technologies Industries (WTI) Toxic Waste Incinerator

Dear Dr. Patton,

We hope that you will help us obtain information about EPA's Phase II risk assessment for the WTI incinerator. It is well known that there are insurmountable shortcomings in all risk assessments because of the enormous uncertainties in the risk assessment process. One such uncertainty is the absence of information about chemical effects on human health. In his book *Calculated Risks*, even successful risk assessor Joseph Rodricks acknowledges how little data is available by stating: "Toxicologists know a great deal about a few chemicals, a little about many, and next to nothing about most." These uncertainties lead to the development of assumptions, which are nothing more than formalized guesses to fill in gaps in the science. Since sets of assumptions can be chosen so as to reach virtually any desired conclusion, the uncertainties become political opportunities. The risk assessment may become, in borrowed words, "*a pseudo-scientific rationale for a political decision that has already been made.*"

Given our knowledge and understanding of current risk assessment practices, we have many concerns about the Phase II risk assessment for WTI. We hope that you will be able to secure answers to our questions and provide us with information regarding the risk assessment. Following are a few of our questions and concerns:

1. What is the current status of the Phase II risk assessment?

    - When can we expect a draft?

    - When will it be completed?

2. An August 10, 1993 letter from Dorothy Patton, Executive Director of U.S. EPA's Risk Assessment Forum states that in the second part of the peer review process, EPA will convene a peer review workshop of up to 15 experts to review the draft.

    - Will the citizens have an opportunity to nominate peer reviewers or have any input?

    - When will the workshop be held?

3. Exactly what will be considered in the Phase II risk assessment?

- Non-cancer health effects such as effects on the endocrine, reproductive, immune and neurological systems?

  How will the risk assessment address emissions of chemicals such as cadminum, dioxin, lead, mercury, PBBs, PCBs, pentachlorophenol, styrenes, hexachlorobenzene, phenol, furans etc., that are reported to have reproductive and endocrine-disrupting effects?? (Because effects are being seen at or near existing background levels, current thinking is that we must limit emissions of these substances to zero. Any additional release of these types of substances into the environment is unacceptable.)

- Actual exposures in various distinct situations? Other factors during which time individuals may receive higher exposures such as spills, leaky valves, equipment maintenance procedures, planned or unplanned start ups and shut downs, automatic waste feed cut offs, etc.? Effects of fires, accidents and explosions occurring at the plant?

- Hazards to humans and animals varying greatly in genetic characteristics, age, sensitivity, and pre-existing health conditions? Exposure and effect on infants, small children, the elderly, and people with chronic illness such as asthma, emphysema, heart conditions, etc? Effects on the fetus and consideration that infants and fetuses are generally more susceptible to toxic effects of chemicals than adults.

- Additive, cumulative and synergistic hazards of daily exposure to WTI emissions? Simultaneous exposure to other pollutants in the Ohio Valley environment? The ability of many chemicals to enhance or amplify (promote) the effect of past exposure to carcinogens?

- Relationship between dose and response at low level exposure?

- Effects from substances not yet studied, i.e., products of incomplete combustion (PICs) that have never even been identified? (Between 20-70% of the PICs have never been identified, let alone what effects they have on human health and the environment.)

4. In order to keep the assumptions conservative, will the emission levels in the risk assessment equal the permitted levels in the permit for each substance?

In light of the fact that we will be exposed to WTI's toxic emissions on a continuous basis, without our consent, we believe that these are reasonable questions to which we deserve an answer. We sincerely appreciate your help and efforts to secure the answers for us. We look forward to receiving the information.

Respectfully,

Terri Swearingen

Terri Swearingen

Tri-State Environmental Council

October 11, 1994

Rd #1 Box 365
Chester, WV 26034

William Snyder
Chief Fluid Modeling Branch
U.S. EPA
Research Triangle Park, North Carolina 27711

Dear Mr. Snyder,

We are in receipt of a copy of the video tape of the wind-tunnel model of the WTI incinerator. Have you conducted an experiment using the same WTI wind-tunnel model for a ground level release as a result of a mixing accident or spill involving a volatile substance? We request that you conduct an analysis for a ground level release during an accident or explosion involving a substance such as hydrogen cyanide or hydrogen sulfide (or a combination of substances) to determine if the concentration would be such that it would be a killing concentration for the children at the school or in their homes nearby.

Given the extremely sensitive location of the WTI incinerator, and the nature and volume of toxic waste to be stored and treated on site, we cannot afford to ignore the probability (a statistical determination based on an actuarial study or actual experience) of fires, explosions and accidents at the site. In fact, the history of the incineration industry proves that these are common at these facilities. What is unique about the WTI toxic waste incinerator is that it is located in the middle of a residential neighborhood and parked next door to a school. To protect the children, we must consider the worst case scenario for an accident at WTI. We want to know what would happen as a result of an accident involving the release of poisonous gases such as phosgene, methylisocyanate or hydrogen cyanide. We also need to know how likely it is that such an accident will occur over the next 20 years?? (What is the probability of a serious accident at WTI over the next 20 years?)

In light of the fact that you already have the WTI wind-tunnel model built, we hope that you will conduct the analysis we have suggested. You owe it to the children and their parents to do so.

We look forward to the results of your study, and express our sincere appreciation in advance.

Sincerely,

Terri Swearingen

Terri Swearingen

cc: Senators
Representatives

Tri-State Environmental Council

December 13, 1994

Rd #1 Box 365
Chester, WV 26034

BY FAX 3:30 P.M.

CERTIFIED MAIL TO:
Dorothy Cantor, PhD
Chair, WTI Technical Workgroup
Office of Solid Waste and Emergency Response
U.S. EPA
Washington, DC 20460

RE: WTI Wind Tunnel Study

Dear Dr. Cantor,

Thank you for your November 21, 1994 response to our letters concerning the WTI Risk Assessment and the wind tunnel study. This follow-up letter concerns the WTI wind tunnel study.

Today's newspaper headlines in Arkansas concern the explosion and subsequent fire at the Ensco toxic waste incinerator in El Dorado that resulted in at least three immediate injuries. According to the El Dorado, Arkansas *News-Times*, "An **explosion** at the Ensco hazardous waste incinerator rocked houses in El Dorado and southeast Union County and was heard as far away as Farnerville, LA." One witness to the explosion said, "*It mushroomed just like a hydrogen bomb. If there wasn't anyone hurt, it was a miracle.*" The explosion, which reportedly occurred in the rotary kiln unit in the waste feed system, was heard as far as 35 miles away. Residents who live nearby said the explosion violently shook houses, knocking pictures off of walls and items off of shelves. The cause of the accident and the extent of the damage have yet to be determined.

Today's newspaper headlines in Iowa concern an **explosion** at a farm chemical plant that produces nitrogen-based fertilizers near Sioux City, Iowa. According to Associated Press (AP) reports, "An explosion rocked a farm chemical plant south of Sioux City today, killing at least five people and rupturing huge ammonia tanks. Hundreds of people were evacuated." Although it has yet to be confirmed, it is believed that there are at least five dead and 17 injured. The explosion knocked out four nearby electricity generating stations, ruptured two ammonia tanks capable of holding one million gallons, and sent up a cloud of ammonia gas. Hundreds of people were evacuated from an Indian casino about **10 miles** away, and the 500 people in Homer, Nebraska, to the south, also were evacuated. The cause of the accident and the extent of the damage have yet to be determined.

In our letter of October 11, 1994 to which you responded, we requested that the EPA conduct an analysis for just such an accident or explosion causing a ground level release of poisonous gases such as phosgene, methylisocyanate, hydrogen cyanide or hydrogen sulfide at the WTI facility. As we stated in our first letter, we also **need to know** the probability of such an accident at WTI over the next 20 years.

We have several comments and questions relative to your response. We would very much appreciate answers to **each** specific question.

1. What are the chances during the next 20 years for the occurrence of a serious accident involving the release of poisonous gas at WTI?

2. Does the EPA believe that it would be a beneficial and worthwhile effort to conduct a wind tunnel analysis to determine the impact of a ground level release accident as described above?

3. We appreciate the fact that you have said that a scale ratio of 1 to 100 would be more appropriate for the analysis of an accidental ground level release than the scale of the existing wind tunnel model which is 1 to 480. But would it not be possible to do a **first level approximation** or a **preliminary screening analysis** using the existing model to get some indication of the impact of such an accident? Wouldn't it be more scientific to conduct such a study **first** before making a sweeping judgment that it could not be done using the existing model? The preliminary analysis may raise a red flag signaling the need for further study using a more accurate scale ratio.

   In rationalizing why EPA is not doing the ground level release study, you also indicated that the model is at the wrong wind direction. Why not just rotate the model? There is no unidirectional aspect unless the original was a very crude model that did not account for all the hills. Why can't the model simply be rotated to correct for wind direction? It would seem that with the use of computers, the entire model could easily be transferred from one scale to another, and wind direction and speed could be adjusted. The difficult part of the task has been completed. Now just repeat it on a scale of 1 to 100.

4. Obviously it suits WTI's purposes for EPA to focus entirely on stack emission numbers, which can easily be manipulated. But look at todays headlines! Our research of the track record and history of incinerators, including those similar to WTI, indicate that fires, explosions and accidents at these facilities are common, and that accidental releases pose a greater threat to public health than routine emissions. Has the possibility of using a model for accidental releases been discussed or rejected? What would the cost of such a study be? How much would it cost to do it right? Considering what is at stake, can we afford NOT to conduct such a study?

We are interested in **very specific answers** to **each** of the questions we have raised. We look forward to your response.

Sincerely,

Terri Swearingen

Terri Swearingen

cc: Senators, Congressmen

Tri-State Environmental Council

January 3, 1995

Rd #1 Box 365
Chester, WV 26034

BY FAX AND MAIL

Dorothy Canter, PhD
Chair, WTI Technical Workgroup
Office of Solid Waste and Emergency Response
U.S. EPA
Washington, DC 20460

RE: Waste Technologies Industries (WTI) Risk Assessment

Dear Dr. Canter,

This is a follow-up to our October 10 letter and your November 21, 1994 reply. We are disturbed by your response, including the fact that you chose not to answer all of our questions. Given what is at stake here, we have a right to be concerned. We are not trying to be difficult or cause trouble — we just want to be sure our children and our families are not at risk from WTI. Following are additional questions, comments and concerns raised by your response.

1. Why is WTI allowed to operate prior to the completion of the Phase II risk assessment?

2. Will the risk assessment consider the psychological impact on the children who attend school and live beside WTI, or the psychological impact on their parents?

3. How much money has the U.S. EPA spent to date on WTI risk assessment activities, including but not limited to the construction and analysis of a WTI wind tunnel model at RTP; activities related to the Risk Assessment Forum and peer review process; all meetings and workgroups conducted for the WTI risk assessment; all telephone communications; payments for services of A.T. Kearnay, Environ or any other consultants; etc.?

4. When it is available, will you please send a copy of the draft Phase II risk assessment report that is currently expected to be distributed to interested parties in Spring 1995?

5. Will you please notify us of the date when the external peer review panel will hold the open workshop to review the report?

6. In your November 21 response, you state that "*both cancer and non-cancer health effects of chemicals of concern are being evaluated in the Phase II Risk Assessment.*" Please identify which chemicals are considered to be "*chemicals of concern,*" and specifically, which chemicals will be considered in the WTI risk assessment?

7. You failed to answer our previous question concerning endocrine disrupters. How will the risk assessment address emissions of chemicals such as cadmium, dioxin, lead, mercury, PBBs, PCBs, pentachlorophenol, styrenes, hexachlorobenzene, phenol, furans, etc., that are reported to have reproductive and endocrine-disrupting effects??

8. How will the results of EPA's dioxin reassessment affect the WTI risk assessment, especially since it has been reported that the adverse effects of dioxin are being seen at or near current background levels?

9. We are aware that certain chemicals have toxicity profiles but do not have a RfD. You state that lead is one such substance for which there is no threshold, but a computerized model has been developed for establishing potential risks from multiple source environmental lead exposure. This model will be used to estimate what impact exposure to lead emissions from WTI will have on the children around the facility. Is it possible to develop a similar computer model for some of the other chemicals of concern, including the endocrine disrupters?

10. You state that "*routine non-stack releases*" will be included in the risk assessment. Does this include **fugitive emissions** of chemicals from storage and handling?

11. In your November 21 response you state, "*Chemicals for which verified data exists only on cancer effects will be evaluated for cancer effects but not for non-cancer toxicities. Likewise, chemicals for which verified data exist only on non-cancer toxic endpoints will be evaluated only for those endpoints. Also chemicals for which the agency does not have any verified toxicity data will not be evaluated in the risk assessment.*" You further state, "*EPA will not be evaluating potential synergistic hazards because the current scientific data base on synergistic effects (or on antagonistic effects) of exposure to multiple contaminants is not robust enough to determine if such effects are occurring.*" Are we to understand that if the EPA does not have **verified** data on a chemical then you assume the risk from that chemical is zero? If you do not evaluate the chemical in the risk assessment then you are assuming the risk is zero. In addition to the chemicals for which EPA does not have **verified** toxicity data, what about the 20 to 70% of the PICs that have not even been identified yet? We must assume that at least some of the unidentified substances are at least as toxic as dioxin. Doesn't this risk assessment method leave out many risk factors? How can you say that EPA is using conservative assumptions when you are ignoring so many risk factors?

12. What potential products of incomplete combustion (PICs) were present in the stack samples during the February 1994 emissions testing at WTI, and at what levels were they present?

13. You stated that in August 1994 the facility tested stack emissions for the presence of more than 80 possible organic PICs during their performance test. Please identify which PICs were present and in what quantity.

14. Please provide the PIC emission estimates that **have been developed** for contaminants of concern based on the **expected** combustion efficiency of the facility and the known waste feeds from the first year of operation. When completed, please also send the comparison between the **measured** PIC emission rates and the **predicted** value.

15. You failed to answer our previous question concerning permit limits for chemicals evaluated in the WTI risk assessment. To keep the assumptions conservative, will the emission levels *used in the risk assessment* be incorporated into WTI's permit as the emission limit for each substance?

We really hope that you will take the time to answer these important questions. You just don't know how much we appreciate honest, complete and straightforward answers. We hope we can count on you to provide them.

Sincerely,

Terri Swearingen

cc: U.S. EPA Administrator Carol Browner
U.S. EPA Deputy Administrator Fred Hansen
U.S. EPA Assistant Administrator Elliott P. Laws
Senators
Congressmen

Tri-State Environmental Council

January 3, 1995

Rd #1 Box 365
Chester, WV 26034

BY FAX AND MAIL

Carol Browner
Administrator
U.S. EPA
401 M Street, SW
Washington, DC 20469

RE: U.S. EPA Risk Assessment Activities for Von Roll/WTI Toxic Waste Incinerator, East Liverpool, Ohio

Dear Ms. Browner,

We have some serious concerns to which **we would like you to respond.** On October 10, 1994, we wrote to the U.S. EPA regarding the Von Roll/WTI Phase II Risk Assessment. Nearly six weeks later on November 21, 1994, the Chair of the WTI Technical Workgroup, Dr. Dorothy Canter, responded. We are distraught with the quality of EPA's reply. After reading Dr. Canter's response, it is evident that in conducting a risk assessment EPA has thrown us a 20 foot rope over the edge of cliff when they are fully aware that we are 25 feet away! In our October 10 letter, we asked very direct questions. Dr. Canter's reply did not answer some of our most important questions, and she failed entirely to address our concerns about endocrine disrupters. The document she sent was completely oblivious to all of the new information on environmental hormones. It is dreadful that EPA has provided such a shoddy answer. Dr. Canter's response was an insult to our intelligence. The same response with an explanation of RFDs and cancer potencies alone might have been acceptable three or four years ago. It is not acceptable today with our current knowledge and understanding of the effects of environmental hormones. Dr. Canter totally ignored our specific questions related to endocrine disrupters, which are some of our greatest concerns.

After reading Dr. Canter's response, we question whether EPA is making a good faith effort to determine the real risk from WTI. In her reply, Dr. Canter reveals that:

- EPA will **not** evaluate effects of chemicals for which the agency does not have verified data.
- EPA will **not** evaluate non-cancer toxicity of chemicals for which verified data exists only for cancer effects.
- EPA will **not** evaluate cancer effects of chemicals for which verified data exists only for non-cancer effects.
- EPA will **not** evaluate synergistic hazards of exposure to multiple contaminants.

- EPA will **not** evaluate antagonistic effects of multiple contaminants.

As Dr. Canter states, *"the current scientific data base ... is not robust enough to determine if such effects are occurring"*. In essence, if EPA does not know about the chemicals in question, they just assume the risk is **zero!** EPA's solution to their lack of knowledge on many chemicals is to **ignore** them!

It appears that the EPA is spending taxpayer money to complete a risk assessment only to prove the facility is safe rather than to prove it is not safe. The EPA's complicated risk assessment attempts to provide a veneer of science while creating a huge bog of impenetrable material in order to **obscure the obvious.** The EPA is going to enormous effort and expense to protect this permit. With the kind of **taxpayer** dollars EPA is spending on risk assessment activities to justify WTI, we at least expected **specific** answers to **our** questions.

WHY is the EPA spending millions of **American** taxpayer dollars to promote the interests of a **foreign** owned hazardous waste company that is being investigated by the SEC and FBI, and whose executives are under indictment by their own government?

The public needs to know whose side the EPA is on. Is the EPA on the side of **the American taxpayer**, or is it on the side of **this perverted foreign corporation** that is currently under investigation for corrupt and illegal practices involving organized crime?

Frankly, we are baffled! We hope that you will address these issues, and answer **each specific question**. Attached also is our most recent correspondence to Dr. Canter. We would be grateful if you would see that we get a satisfactory response. Given what is at stake here, we are gravely concerned. We are not trying to be difficult or cause trouble — we just want to protect our children and our families. We truly look forward to your response.

Sincerely,

Terri Swearingen

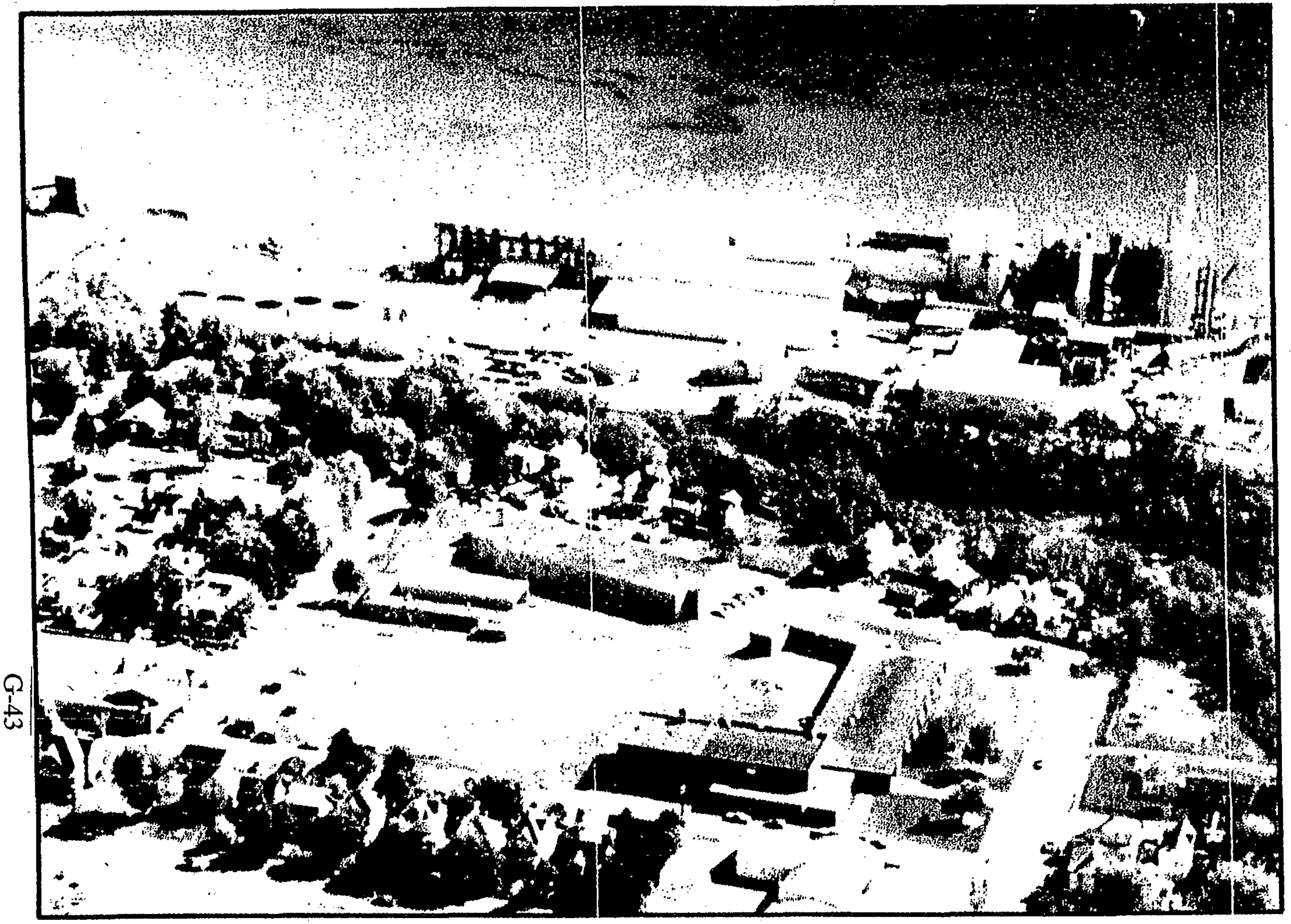
cc: Senators, Congressmen

Enclosures:

Wall Street Journal, December 2, 1994: **Von Roll Finds Fresh Headaches as SEC Investigates Unit's Alleged Mafia Links**

New York Newsday, December 30, 1994: **Did Swiss Firm Pay Off Mob?**

The Daily Times, December 16, 1994, Editorial: **Doubts Cast on Fitness of WTI Ownership**



THEY BUILT IT HERE.

ON PURPOSE.

FOR THE MONEY.

ILLEGALLY.

THE EAST LIVERPOOL, OHIO HAZARDOUS WASTE INCINERATOR

THE GOVERNING LAW:

Ohio Revised Code Section 3734.05(D)(6): "The Board shall not approve an application for a hazardous facility installation and operation permit unless it finds and determines: . . . (g) that the active areas within a new hazardous waste facility . . . are not located or operated within:

(i) **two thousand feet of any residence, school, hospital, jail, or prison;**

(ii) any naturally occurring wetland; or

(iii) any flood hazard area if the applicant cannot show that the facility will be designed, constructed, operated, and maintained to prevent washout by a one hundred-year flood or that procedures will be in effect to remove the waste before flood waters can reach it."

— ***Effective August 1, 1984***

WHY DOES THE STATE OF OHIO IGNORE ITS OWN LAWS?

"[The investigative report of the Ohio Attorney General released in June, 1993] concludes that WTI's changes of ownership have resulted in **unlawful installation and operation** of the facility by the current owner, in violation of the three Ohio provisions of law which prohibit ownership and operation with out a permit." — Jack Van Kley, Environmental Enforcement Section, Ohio Attorney General's Office, Columbus, Ohio, on September 22, 1993.

"[T]he WTI facility is the **worse siting decision** I have seen in my twenty-five years of practice in public health. Locating a major hazardous waste incinerator 300 feet from the nearest residence and 1100 feet from an elementary school with 400 children . . . amounts to administrative incompetence if not malfeasance in office . . . and does violence to common sense."

— Dr. David Ozonoff

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El D(
NEWS-
Volume 106, Number 160 El Dorado,

One hurt in Ensco

By BILL FERGUSON
News-Times Staff

An explosion at the Ensco hazardous waste plant Monday night rocked houses in El Dorado and southeast Union County and was heard as far away as Farmerville, La., but only injured one plant employee.

The plant, located on American Road just outside the El Dorado city limits, was swarmed by emergency personnel from around the county shortly after the 7:53 p.m. explosion. El Dorado police, the Union County Sheriff's Office, the El Dorado Fire Department and the Arkansas State Police responded to the incident.

"It mushroomed just like a hydrogen bomb. If there wasn't nobody hurt, it was a miracle," one witness to the explosion said.

Christy Ibert, the plant employee injured in the blast, was being treated in the emergency area of the Medical Center of South Arkansas for burns at 10:15 p.m., a hospital spokesman said. The spokesman said Ibert was in stable condition, but did not know if she would be admitted to the hospital.

Ibert, according to sheriff's deputies, was in the plant control room at the time of the blast. The control room is near where the explosion occurred, according to deputies.

A state police spokesman said about two hours after the blast that there appeared to be no danger to anyone in surrounding areas and no evacuation was carried out or planned, according to the Associated Press.

"State police (at the site) said that the threat of problems has been eliminated," state police spokesman Wayne Jordan said at 10 p.m. "The fire is probably out to the extent it's not going to cause any danger."

Union County Sheriff Huey Havard echoed that judgment.

"At this time, there's no danger, there's no toxics in the air, there's nothing been released," Havard said.

Operations at the incinerator had been shut down, and "we have been informed there's no danger whatsoever, at this time."

The sheriff said the explosion was heard as far as 30 or 35 miles from El Dorado.

Jordan said a trooper at the scene reported that acetylene gas tanks "that are used to fire the burning process exploded."

"There are still oxidizers burning," he said about 1½ hours after the blast.

State Trooper Roland Ponthieux, a hazardous-materials expert, responded to the scene, Jordan said.

He said Ponthieux reported that the oxidizers that were burning were used in combination with the acetylene to create a hot fire in the incinerator, but he didn't know what the oxidizers were.

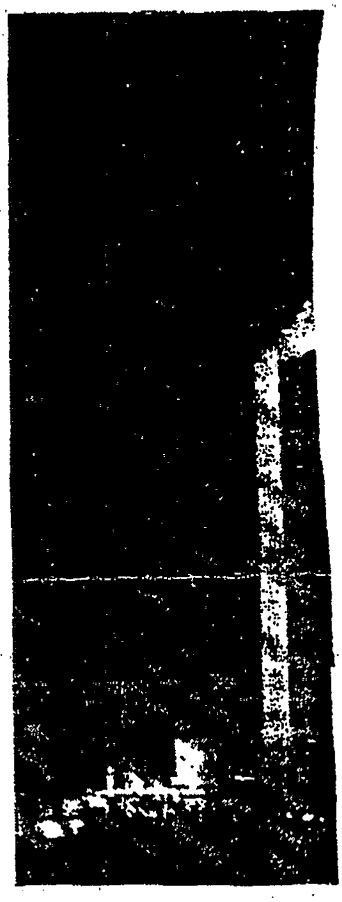
Sgt. David Smith of the El Dorado Police Department said authorities planned to "try and let it burn out" before moving in closer to the explosion scene. He said the department was deluged with calls right after the explosion.

Plant general manager Steve Darnell said the explosion occurred in the rotary kiln unit of the waste feed system. The system was described by another employee as the part that grinds up waste before it is incinerated.

Darnell was interviewed at the plant's front gate about 9:45 p.m.

The general manager said "organic-type wastes" were being burned at the time of the blast. Several small fires burned into the night. He said an explosion "fail-safe" system prevented any chemical release and channeled the explosive power upward and away from areas where employees were located.

"There was no release of any



Blast site

Shortly after the explosion, sm

chemical fumes from the plant and there was no danger to the community," Darnell said. "Our first concern was to make sure all of our employees were out of the area and the area was secured."

Darnell added that an inspector from the state Department of Pollution Control and Ecol-

College, 8A | Tuesday | December 13, 1994

ɔrado
TIMES

Cloudy
Details, 2A

Arkansas 28 Pages In 2 Sections — 35 cents

plant explosion

News-Times/Jim Lemon

ɔke rises (center) from the blast site, to the right of the taller stack, at the Ensco plant.

ogy was on the scene.

He said that as soon as the explosion occurred, plant personnel immediately put the company's emergency plan into operation.

Another plant employee, on site at the plant, said he had been told the incident happened when too much oxygen was mixed into the system.

"I thought someone had picked the building up and moved it," the employee said of his station several hundred feet from the rotary kiln.

El Dorado Fire Chief Ben Blankenship said "at this time it appears to be stabilized. We're working with Ensco, but we'll be on the scene for a while."

Havard, who was at the Union County Criminal Justice Facility just one block away at the time of the blast, said "I thought they had blown the side of the jail off."

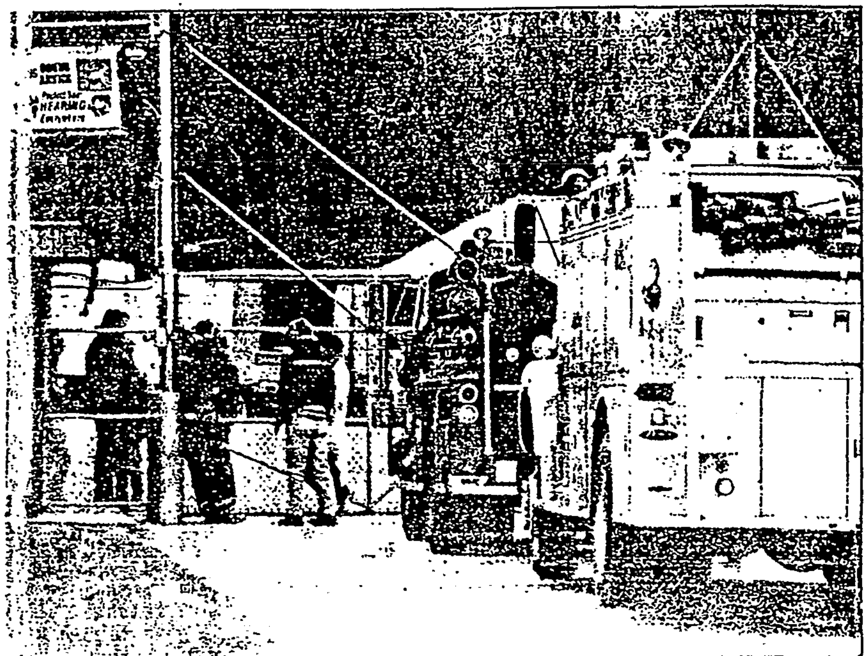
See ENSCO, Page 3A

G-48

THE MORNING JOURNAL

68 PAGES WEDNESDAY, DECEMBER 6, 1995 50 CENTS

WEATHER
Windy, flurries
High 29, low 17
See page A2



MORNING JOURNAL/TOM WHITTINGTON

LAST — Eaton Township firefighters in ·in County look at Ross Environmental Services, where an explosion shook the area yesterday.

Toxic blast rocks area

By JOHN REYNOLDS
Morning Journal Writer

EATON TOWNSHIP — An explosion in the hazardous waste incinerator at Ross Industries yesterday sent a cloud of greenish-yellow smoke into the air and caused extensive damage to the incinerator's kiln, state officials said.

Maggie Kelch, spokeswoman for Ross Environmental Services, said no one was injured in the explosion. She said that people living nearby were not in any danger. Last night, she said the Ohio Environmental Protection Agency and employees from

Ross, EPA seek cause

Ross were still trying to determine what caused the blast.

Nearby houses on Giles Road were rocked by the explosion, and the blast was heard as far away as North Ridgeville.

Thinking the explosion might have been in the city, the North Ridgeville Fire Department sent two trucks out which were recalled when they learned the blast was at Ross, the fire department said.

Immediately after the explosion, Mrs. Kelch said, Ross began calling concerned neighbors who wanted to know what had happened. The green cloud people say they spotted most likely came from the incinerator's steam stacks and didn't contain any hazardous material, Mrs. Kelch said.

Some hazardous material did escape from the incinerator during the explosion in the form of ash, but Mrs. Kelch said it was contained to Ross property. The ash had already been burned in

Please see BLAST, [illegible]

Blast felt like an 'earthquake,' neighbor says

By SHARON TURCO
Morning Journal Writer

EATON TOWNSHIP — Windows rattled and houses shook yesterday, and residents on Giles Road knew there was an explosion at Ross Environmental Services.

"The blast felt like an earthquake," said Vicky Brown of 36625 Giles Road, six houses from the plant. "But I knew it came from Ross. I was alarmed when I saw it wasn't just fire, but a huge green cloud of smoke. You can't help but wonder what went into the air."

Other residents living near the hazardous materials incinerator also expressed concerns for their health and safety following yesterday's explosion.

"There was a big boom and when I looked outside there was a huge cloud of green smoke," said Teresa Jancura, 36520 Giles Road. "I'm concerned about all these accidents, there are too many for this to be safe."

Beatice Conner, 36975 Giles Road, said Ross Environmental Services is a big worry for people who live close to the plant.

"It's right across the field from me," she said. "After the boom I looked out the front window to see a big cloud of blackish smoke creeping across the field toward the houses. One of these days it's going to kill someone."

"It really jarred the house hard, the windows rattled," said Juanita Williams, of 37190 Giles Road. "It was so much harder than the other explosions."

BLAST

From the front page

the kiln and therefore should not have presented a danger, Mrs. Kelch said.

When asked what was being burned at the time of the incident, Mrs. Kelch said she did not know. The Ohio EPA also said it did not know what substances the explosion might have emitted into the air.

"Without knowing what was in the incinerator, it is too early to speculate on what the dangers might be," said Ohio EPA spokesman Rob Berger.

While the cause of the explosion is not known, Mrs. Kelch said it happened next to a 40-foot-long rotary kiln in a section of the incinerator called the ash drop box.

Mrs. Kelch said that immediately after the explosion the plant was evacuated and the company's emergency team began to fight the resulting fire. The Eaton Township Fire Department was called to the scene, but when they arrived, they discovered they weren't needed.

"We weren't exactly sure what they were burning in there," Eaton Township Fire Chief Melvin Ternes said.

Ternes said he was allowed to go through the guarded gate and examine the scene. He said the Ross employees had everything under control, and he saw no reason to bring his firefighters inside the grounds.

"We could have gone onto the grounds, but rather than expose our men to chemicals, we would rather let them (Ross) handle it. They have more training in dealing with hazardous material," Ternes said.

Standing with the firefighters were a few onlookers who wanted to find out what was going on. Guards stood at each of the gates, and within two hours, passers-by couldn't tell anything was amiss because the view of the incinerator was partially blocked from the road.

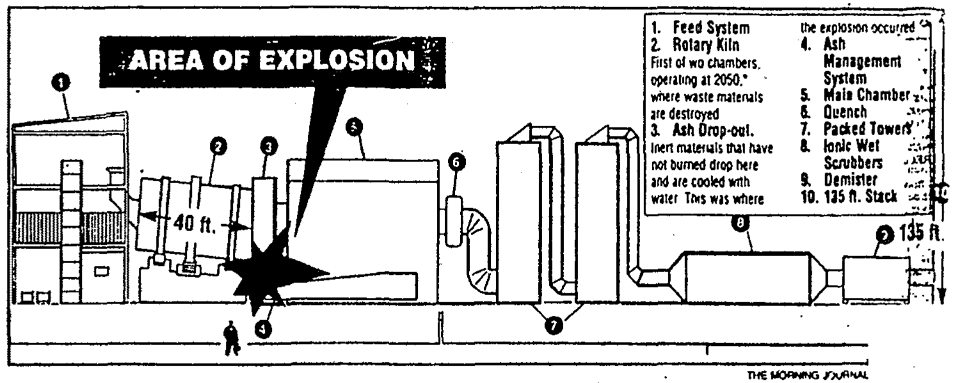
About two hours after the explosion, the company talked with reporters about what was taking place inside the plant.

Yesterday wasn't the first time the spotlight has been turned on Ross Environmental Services. Ternes said he has been chief for 35 years and has been out at Ross about "half a dozen times."

The most recent trip he made there as fire chief was in September when two garages caught on fire, he said. That time, firefighters fought the blaze which was not near any hazardous material, Ross reported.

Before that, the fire department was called to Ross in April when there was an explosion. That explosion was caused by a mechanical malfunction in a different section of the incinerator and had nothing to do with yesterday's incident, Mrs. Kelch said.

"We made changes so that the conditions that led to May's explosion can't reoccur," Mrs. Kelch said.



APPENDIX H

WRITTEN STATEMENT SENT AFTER THE WORKSHOP

The following statement was sent by a member of the public who was unable to attend the workshop due to severe weather conditions along the East Coast.

Testimony of Alonzo Spencer, President
Save Our County, Inc.
P.O. Box 1242
East Liverpool, Ohio

Before the WTI Risk Assessment Peer Review
January 11 and 12, 1996
Georgetown, Washington D.C.

Mr. Chairman, Members of the Committee;

My name is Alonzo Spencer. I am President of Save Our County, Inc., a local grassroot environmental organization, located in East Liverpool, Ohio.

For almost fifteen years, we have been struggling to protect our community and our people from the deadly threat presented to us by the construction and operation of the Waste Technologies Industries (WTI) Hazardous Waste Incinerator located in our community.

All across the country low income populations and communities of color have been targeted for the location of hazardous waste facilities. While such facilities frequently arrive promising jobs and prosperity to a community, what they really bring is a threat to the health, environment and safety of families that live in these areas.

The main reason for addressing you today is to inform you that the EPA maybe using you to try to justify that which is unjustifiable.

The EPA, thus far, has been unable to produce evidence that can be used to justify allowing WTI/Von Roll to be built, let alone go into limited commercial operation. They have failed from a legal, moral or ethical position, and now they are turning to the scientific community seeking help to make that which is unholy to somehow become holy.

The WTI case suggests something of a noble lie in our society. We have even a scheme to transfer R.C.R.A. permits without authorization from the EPA, to transfer operational control of a facility without authorization from EPA, to handle hazardous waste without a R.C.R.A. permit, to fail to disclose one of their corporate family member's environmental compliance record and connections to organized crime, to approve a R.C.R.A. permit without the landowner's signature, and to add the landowner's signature despite the landowner's objection.

EPA officials have played a significant role in the irregularities in the WTI permitting process. The culpability of particular individuals must be addressed for allowing this plant to be built on the banks of the Ohio River, a source of drinking water for literally thousands of families. For knowingly building it in a flood plain, over two high yielding aquifers, one being contaminated by a previous spill of toluene, ethylbenzene and xylene. To this day no remediation has taken place to clear up that contamination. It should also be taken into account that this plant is within a few hundred feet of the nearest resident. The plant is located in an area that experiences air inversions approximately once every six days.

The most astonishing collateral effect of this environmental misconduct is the fact that this plant sits less than eleven hundred feet from an elementary school, kindergarten through fifth grade. Those children are exposed to the dangers of this plant from stack emissions and accidents threatening their lives each and everyday they attend school.

We now have the EPA turning to risk assessments for some justifications for its actions. Let me give you a brief history of the origin of risk assessments.

HISTORY - a reminder; Risk assessment began in secret in the late 1940's. When atmospheric testing of nuclear bombs became more and more intensive in the 1950's there was a build-up of radioactive strontium (and other radioactive elements) in the atmosphere of the northern hemisphere. Strontium-90 and company were being washed down in rain and drifting down in dew. They entered the food chain. In the early 1950's (reference Merrill Eisenbud) some scientists in New York City began to worry. They went to the market and bought some lamb chops and some Muenster cheese, ran them through their still somewhat rudimentary analytical equipment and found that they contained radioactivity. The U.S. Atomic Energy Commission embarked on the most expensive and extensive monitoring study ever launched to that time, to find out the extent of strontium contamination of the bones of people. WHY? Because strontium was chemically similar to calcium and of course it would go into the skeleton of animals and people along with the calcium in food. There was also the very disquieting knowledge that radioactivity in human and animal bones caused cancer.

This was knowledge then 25 years old. Many of the women who had painted watch dials with radioactive radium paint and tipped their paintbrushes in their mouths had been slowly dying from the early 1920's and still developing cancers in the 1950's. The animal radium experiments of the previous 25 years had shown the same thing. The relationship between the amount of radium and the dose of radiation to the skeleton and the appearance of bone cancer and other forms of cancer was well known.

The strontium levels in children's skeletons particularly, were obviously increasing as the Cold War grew worse. Was there a way to get a risk assessment (a term not yet invented) on the number of cancers that might result from the radioactive strontium in human bones? How high could the level go? How many cancers could result? This was a period of high secrecy - the public afraid but unaware of the research efforts about radioactive fallout for years.

The answer to the questions was to use the data from the radium dial painters (information still in use to this day) and from old and new animal radium experiments added to new experiments on radioactive strontium in animals. The comparison could then be made between the radiation dose relationship between human and animal data for radium with animal data for radiation dose from strontium - then there was only one unknown in the equation - the effect of the radioactive strontium dose on human populations. Enough money was spent, enough scientific observation went into that equation to provide a reliable cancer risk assessment for radioactive strontium in fallout. The studies also contributed to the evidence for the halting of Russian, British and American atmospheric testing. Strontium-90 did not reach dangerous levels in human populations, remaining below the level of natural radioactivity that has always been part of the human skeleton. That experience showed that with a reliable scientific foundation, risk assessment for human cancer was appropriately done and appropriately used.

How does this experience relate to the effluent from the WTI Hazardous Waste operation.

FOR CANCER RISK ASSESSMENT PURPOSES DO WE HAVE? A human population under surveillance for a period long enough to know the cancer rate in relation to their exposure to a single component of the effluent? Dioxin? No. Any other component of the effluent? Only Radioactive elements. Conclusion - Except for radioactive effluent, assumptions based on animal studies of varying and unpredictable chemical mixtures from the WTI Hazardous Waste plant, do not provide a useful scientific basis for human cancer risk assessment.

IS RISK ASSESSMENT RELIABLE FOR DISEASES OTHER THAN CANCER? Respiratory disease - The long history of acute, high level exposure to factory and power plant exposures associated with immediate death and long-term chronic respiratory disease - e.g. Donora, Pa., (1948), Meuse Valley, Belgium (1934), London, England (1950) etc where air inversions were the precipitating events have all had exposures to unidentifiable mixtures of hazardous chemicals.

There is some reliable prevalence data for chronic respiratory disease in human populations, good animal data for exposure to specific respirable chemical compounds. But there is no reliable way to compare a dose response relationship as in the radium/strontium relationship with variable, non-reproducible mixtures of chemicals coming from the WTI stack. The assumptions to be added to any model of risk assessment for the latter situation are imaginary and unscientific.

There is no available comparison data for assessing the effects of acute exposures on the chronically ill population particularly those with chronic respiratory disease and the newborn (Harrisburg, 1968)

Reproductive Effects - The reliable scientific foundation for developing a risk assessment model for the multitude of reproductive effects does not exist in terms of human/animal comparison for most specific chemical compounds and exists not at all for variable mixtures of compounds in effluents under varying climatic conditions and operating conditions.

Immune diseases - No reliable epidemiologic data that can be applied to a scientifically based risk assessment process.

CONCLUSION: The further we move from the firm scientific foundation established for the early development of risk assessment methods for radioactive fallout, the weaker the conclusions become for providing a reliable and credible understanding for the public, for regulators, for operators of hazardous waste plants.

The most serious problem of all and the most serious and unpredictable danger we have is NEVER knowing what is coming out of the WTI stack. What is the use of a risk assessment using as many imaginary assumptions as any hysterical neighbor can dream about when the next Donora type inversion comes our way. When that happens as it inevitably will there will be no analytical data available to describe what is being added to the atmospheric soup from the WTI stack. There will be no monitoring on the synergistic effect of the mix of hundreds of chemicals coming out of the stack right next to the homes and the school.

To use risk assessment judgments for policy decisions concerning the WTI plant flies in the face of rational scientific conclusions.

☆ U.S. GOVERNMENT PRINTING OFFICE: 1996 - 750-001/41006