



**REVIEW OF DRAFT
"ADDENDUM TO THE
METHODOLOGY FOR
ASSESSING HEALTH RISKS
ASSOCIATED WITH
INDIRECT EXPOSURE TO
COMBUSTOR EMISSIONS."**

**PREPARED BY THE INDOOR AIR
QUALITY/TOTAL HUMAN
EXPOSURE COMMITTEE OF THE
SCIENCE ADVISORY BOARD**



UNITED STATES ENVIRONMENTAL PROTECTION AGENCY
WASHINGTON, D.C. 20460

OFFICE OF THE ADMINISTRATOR
SCIENCE ADVISORY BOARD

EPA-SAB-IAQC-94-009b

July 29, 1994

Honorable Carol M. Browner
Administrator
U.S. Environmental Protection Agency
401 M Street, SW
Washington, DC 20460

RE: Draft "Addendum to the Methodology for Assessing Health Risks Associated with Indirect Exposure to Combustor Emissions"

Dear Mrs. Browner:

On December 3, 1993, the Indoor Air Quality/Total Human Exposure Committee (the Committee) of the Science Advisory Board reviewed the draft document "Addendum to the Methodology for Assessing Health Risks Associated with Indirect Exposure to Combustor Emissions" (the Addendum). In view of pressing EPA and public concerns about incinerators, the Committee sent you an interim letter (EPA-SAB-IAQC-94-009a) on February 15, 1994 to provide you with some of the major findings of the Committee. The attached is a more detailed report of our conclusions and recommendations.

The assessment of risks from stationary combustors entails a complex range of issues, including the existence of many different kinds of combustion devices and raw materials, both direct and indirect exposure routes, concerns regarding transportation and disposal of raw materials and combustion ash, and the need to account for cumulative impacts of multiple combustor sources at the regional, national and international levels. We preface our comments by emphasizing, however, that this report is primarily focused on the questions surrounding indirect exposure assessment which are the subject of the Addendum. For lipophilic contaminants, such as dioxins, furans, polychlorinated biphenyls, certain pesticides, and for metals such as lead and mercury, indirect exposures through food have been demonstrated to be dominant contributors to total dose within non-occupationally



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exposed populations. It is also likely that atmospheric pollution from combustors and other thermal processes significantly contribute to the ubiquitous presence of some of the highly persistent lipophilic compounds.

To grapple with the complex indirect exposure pathway issues, the Agency needs to be able to estimate the environmental fate of combustor emissions and their consequent potential for human exposures. This task requires the development of models to predict accumulations of chemical contaminants in the environment and identification of the chemicals, environmental compartments, and exposure pathways most likely to be of concern so that appropriate actions can be taken before there is widespread and/or irreversible damage.

The Addendum the Committee reviewed is a critical part of the Agency's effort to deal with these very difficult challenges. The effort has significant merit because it is the first attempt by the Agency to put all the models relevant to indirect exposure together in a coherent fashion. The overall exposure model presented in the Addendum represents a major improvement over earlier incinerator risk assessment models, with much potential practical value.

The model is an effort that pushes at the very edge of our current scientific knowledge. This is the source of its considerable merit, but it is also the *source of its most serious limitations*. The Committee's major findings, therefore, concern the proper use, and the potential misuses, of the Addendum as a tool by the Agency and others. During the review, the Committee was left with the strong impression that the Addendum would be the primary or even the sole tool to be used by the Agency to carry out routine evaluations of risks from combustors. This impression caused serious concern in the Committee for two main reasons.

First, the evaluation of *indirect* exposures from *single* sites is too narrow a basis for decisions regarding stationary combustor risks. The potential risks of combustors are multiple, and they include direct and indirect risks. In addition, while a single new facility may not result in a significant risk, the cumulative effect of the addition of a facility to an area with a number of existing combustors may well result in aggregate health risks that reach levels of concern. While the full scope of these issues is beyond a narrow review of the Addendum, the Committee feels strongly that any national exposure assessment model framework for large combustor operations must address both local (i.e., within 50 km) impacts on human health and the environment, and also the contribution of the operations of any single combustor to the more distant (> 50 km) regional impacts. This consideration affects both direct and indirect exposure pathways.

The second and most directly relevant reason for concern is that the Committee has many serious reservations about the possible routine use of *the methodology in the Addendum* as a detailed quantitative exposure model for combustors. These reservations are discussed in some detail in the attached report, and, incidentally, many of them have been noted by previous SAB committees in their review of hazardous waste and domestic waste incineration.

The Committee's principal conclusion is that the Addendum is not ready for release as an "EPA Methodology" for routine regulatory assessment of indirect exposures from stationary combustors due to the substantial scientific uncertainties in the models and the absence of information in the Addendum concerning those uncertainties and limitations. The major scientific concerns were as follows:

1. Lack of validation and reliance on default input values for many of the inter-media transfer factors, for many chemicals, leaving the Committee with many reservations about widespread application of this document as an EPA methodology for all types of combustors and chemicals.
2. Lack of information on incinerator upset conditions, which may contribute significantly to the total emissions from combustors.
3. Insufficient attention to the chemical nature of the emissions and the frequency of upset conditions for the full range of combustors addressed by the Addendum. In particular, species whose exposure potential is determined by multiple chemical forms and physical states, such as mercury, should be explicitly considered.
4. Requirements for the use of site-specific human exposure data which may be impractical and excessively costly to obtain. A balance must be struck between needs for site-specific data with very strong impact on the exposure results and data with less impact where defaults may be applicable.
5. Possibility of the violation of the laws of Conservation of Mass and of chemical thermodynamics in two sub-components of the methodology, that is, in the models for volatilization from soil and plant surfaces and for transfer from air to both plants and soils. This should be checked and corrected if necessary.
6. Insufficient treatment and exposition of uncertainty and variability throughout the Addendum.

Therefore, the Committee makes the following recommendations:

1. Use the Addendum as an *analytical tool* to identify the chemicals most likely to accumulate in the environment, the environmental compartments most at risk of excessive accumulations, and the exposure pathways most likely to lead to aggregate risks of concern. Such analyses will provide strategic guidance in utilizing environmental sampling to obtain actual data on indirect exposure to humans and to ecosystems. We do not recommend release of the Addendum as an "EPA Methodology" for routine, *quantitative*, site-specific risk assessments for incinerators and other combustors.
2. Develop and implement a strategic plan to collect critical input data for the models and to validate the methodology. For this purpose, take advantage of the re-permitting process to collect relevant data to help validate the Addendum's models.
3. Establish a framework to ensure that the entire range of potential risks from stationary combustors are addressed holistically. This must include both direct and indirect risks, as well as local, regional, national and international concerns.

In closing, we wish to emphasize that the Committee is keenly aware of the difficulties inherent in the "state of the science" nature of the work which the Addendum effort entails, especially when the work must be done under the combined pressures of severely limited resources and public demands for "something" to be done quickly.

We urge you to implement these recommendations and would be happy to review a revised Addendum in the future.

Sincerely,

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Executive Committee
Science Advisory Board

Joan M. Daisey
Dr. Joan M. Daisey, Chair
Indoor Air Quality/Total
Human Exposure Committee
Science Advisory Board

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ABSTRACT

On December 3, 1993, the Indoor Air Quality/Total Human Exposure Committee (the Committee) of the Science Advisory Board reviewed the draft document "Addendum to the Methodology for Assessing Health Risks Associated with Indirect Exposure to Combustor Emissions" (the Addendum).

Although the multi-media model of the Addendum is not yet fully developed, the Committee found merit in the model and recommended its use as an *analytical tool* to identify the chemicals most likely to accumulate in the environment, the environmental compartments most at risk of unacceptable accumulations, and the exposure pathways and chemicals most likely to result in aggregate health risks that reach levels of concern. Such analyses will provide strategic guidance for environmental sampling to obtain data on indirect exposure to humans and to ecosystems. However, they did not recommend the release of the Addendum as an "EPA Methodology" for routine, quantitative, site-specific risk assessments for incinerators because of substantial scientific uncertainties in the model and the absence of many important model parameters.

In addition to their general findings regarding the use and possible misuse of the methodology in the Addendum, the Committee addressed numerous specific issues concerning: 1) air emissions and modeling; 2) soil impacts and the food chain; 3) water impacts and modeling; and 4) exposure. Finally, the Committee stressed the need to establish a framework to ensure that the entire range of potential risks from stationary combustors are addressed holistically, including both direct and indirect risks, as well as local, regional, national and international concerns.

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SCIENCE ADVISORY BOARD
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Review of Indirect Exposure Addendum

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TABLE OF CONTENTS

1.	EXECUTIVE SUMMARY	1
1.1	General Findings	1
1.2	Air Emissions and Modeling Issues	3
1.3	Soil Impact and Food Chain Issues	4
1.4	Water Impact Modeling Issues	4
1.5	Exposure Issues	4
2.	BACKGROUND AND CHARGE	6
3.	FINDINGS	7
3.1	Introduction.	7
3.2	General Findings	8
3.3	Responses to Charge to the Committee	14
3.3.1	General Issues	14
3.3.2	Air Emissions and Modeling Issues	16
3.3.3	Soil Impact and Food Chain Issues	23
3.3.4	Water Impact Modeling Issues	29
3.3.5	Exposure Issues	30
3.4	Summary of Major Recommendations	37
APPENDIX A.	Charge to the Committee	38
APPENDIX B.	Interim Letter to Administrator (EPA-SAB-IAQC-94-009a)	39
APPENDIX C	Miscellaneous Comments and Specific Corrections and Suggestions for Clarification of Text	40
APPENDIX D	Earlier SAB Reports Concerning Hazardous and Domestic Waste Incineration.	47

1. EXECUTIVE SUMMARY

1.1 General Findings

The assessment of risks from stationary combustors entails a complex range of issues, including the existence of many different kinds of combustion devices and raw materials, both direct and indirect exposure routes, concerns regarding transportation and disposal of raw materials and combustion ash, and the need to account for cumulative impacts of multiple combustor sources at the regional, national and international levels. This report, however, is primarily focused on the questions surrounding indirect exposure assessment which are the subject of the Addendum. Indirect exposures are those that occur after transfer of airborne contaminants into water, soil and the food chain.

To grapple with indirect exposure pathway issues, the Agency must be able to develop and validate models to estimate accumulations of chemical contaminants in the environment and specifically to identify the chemicals, environmental compartments, and exposure pathways most likely to be of concern so that appropriate actions can be taken before there is widespread and/or irreversible damage. The Addendum is a critical part of the Agency's effort to deal with these very difficult challenges. The effort has significant merit because it is the first attempt by the Agency to put all the models relevant to indirect exposure together in a coherent fashion.

The Committee's principal conclusion is that the Addendum is not ready for release as an "EPA Methodology" for routine regulatory assessment of indirect exposures from stationary combustors due to the substantial scientific uncertainties in the models and the absence of information in the Addendum concerning those uncertainties and limitations. The major scientific concerns were as follows:

1. **Lack of validation and reliance on default input values for many intermedia transfer factors for many chemicals, leaving the Committee with many reservations about widespread application of this document as an EPA methodology for all types of combustors and chemicals.**
2. **Lack of information on incinerator upset conditions, which may contribute significantly to the total emissions from combustors.**

3. **Insufficient attention to the chemical nature of the emissions and the frequency of upset conditions for the full range of combustors addressed by the Addendum.** In particular, species whose exposure potential is determined by multiple chemical forms and physical states, such as mercury, should be explicitly considered.
4. **Requirements for the use of site-specific exposure data which may be impractical and excessively costly to obtain.** A balance must be struck between needs for site-specific data with very strong impact on the exposure results and data with less impact where defaults may be applicable.
5. **Possibility of violation of the Law of Conservation of Mass and the laws of chemical thermodynamics** in two sub-components of the methodology, that is, in the models for volatilization from soil and plant surfaces and for transfer from air to both plants and soils.
6. **Insufficient treatment and exposition of uncertainty and variability throughout the Addendum.**

The Committee found, however, that the Addendum has significant practical potential value. They recommended the use of the Addendum as an *analytical tool* to identify the chemicals from combustors that are most likely to accumulate in the environment, the environmental compartments most at risk of unacceptable accumulations, and the exposure pathways most likely to result in aggregate health risks that reach levels of concern. Such analyses will provide strategic guidance in designing environmental sampling to obtain actual data on indirect exposure to humans and to ecosystems.

In addition, the Committee stressed that the evaluation of *indirect* exposures from *single* sites is too narrow a basis for decisions regarding stationary combustor risks. The potential risks of combustors include direct and indirect risks from single and multiple facilities. While a single new facility may not pose a significant risk, the cumulative effect of the addition of a facility to an area with a number of existing combustors may well result in aggregate health risks that reach levels of concern. The Committee feels strongly that any national model framework to guide exposure assessments for large combustor operations must address both local (i.e., within 50 km) impacts on human health and the

environment, and also the contribution of the operations of any single combustor to the more distant (>50 km) regional impacts.

Finally, if the Addendum is released for use as an analytical tool, the Committee recommended the addition of an integrating introductory chapter to provide additional information which would be necessary to conduct an indirect exposure and risk assessment with the Addendum. This chapter should include a decision tree to assist the user in deciding what models to use and when, guidance regarding how good the models are for which chemicals and which situations/combustor types, and some additional screening procedures to help avoid spending time modeling exposures for pathway-chemical combinations that are likely to lead to extremely low exposures and risks.

1.2 Air Emissions and Modeling Issues

The chemicals identified as of concern from combustor emissions would be more usefully presented if grouped by chemical class and combustor type and/or fuel or waste composition. The grouping by chemical class will provide a more rational framework for both modeling and model validation.

The hierarchical approach for estimating emissions needs some clarification, and the role of upset conditions on total emissions needs to be more carefully defined. Additional measured data describing the emission factors during start-ups, shut-downs, upsets and poor operation are necessary. The re-permitting process for incinerators offers a unique opportunity to obtain such data and the Agency should take advantage of it.

The net effect of using the recommended procedure to estimate the vapor phase/particle phase partitioning of semivolatile organic compounds is to underestimate wet deposition near the source and to overestimate it further from the source. The Addendum could benefit from incorporating more of the recent literature on the issue of adsorption of organics.

Some clarification is needed regarding which particles are being addressed in the particle size distribution of particulate matter from combustors, combustor particle emissions or outdoor aerosols upon which combustor vapor emissions condense. The reasons for selecting a default distribution need to be more clearly stated.

There are shortcomings in the basic transport and dispersion model used in the Addendum--related to calms and terrain-forced changes in wind direction. Explicit guidelines are therefore needed to avoid misuse of the model under certain circumstances. Finally, certain modifications of the COMDEP model (based on the Shulman and Shire model) are needed to correct underpredictions of wake concentrations for short stacks.

Finally, the atmospheric lifetime and environmental fate of different forms of mercury are among the species of most concern and should receive increased attention.

1.3 Soil Impact and Food Chain Issues

It is very difficult to make judgements concerning the recommendations to account for vapor phase impacts to soils because there is such a paucity of environmental measurements available to validate any approaches.

The assumptions for mixing soil depths for tillage and non-tillage situations seemed reasonable. The 30% assumption for the percent of contaminants in wet deposition that is retained on plant surfaces is reasonable. The concentrations resulting from the re-suspension from soils of emission particles should not be extensively modeled if they are orders of magnitude smaller than concentrations resulting from stack emissions.

One of the most significant issues in the methodology that can and should be addressed is the inherent lack of reliability associated with both measured data and models used to determine inter-media transfer factors (ITFs), which are factors that define the partitioning and mass transfer between environmental media.

1.4 Water Impact Modeling Issues

A tiered approach for the evaluation of aquatic impacts is the more sensible approach than the use of alternatives that involve more complex models and numerous inputs, many of which are not available. Consideration should be given to the impacts of multiple combustors impacting coastal regions, where much of the U.S. population resides.

1.5 Exposure Issues

The use of geographically-defined boundaries based on dispersion/deposition models is reasonable for estimating concentrations in soil, water, and air, and direct exposures to these media. However, it may not be sufficient for defining the population at risk from indirect exposure. The recommendation to develop distributions of exposures is a substantial improvement over the use of worst-case scenarios and is commended.

The use of Monte Carlo simulations to estimate the effects of uncertainty is not sufficiently clear because the distinction between variability and uncertainty is blurred. The guidance for the use of Monte Carlo analysis is not sufficiently detailed.

The choice of a model for estimating breast milk concentrations on the basis of maternal intake requires validation by comparison with actual data. For TCDD, the Smith approach appears preferable to the Travis *et al.* model.

2. BACKGROUND AND CHARGE

On December 3, 1993, the Indoor Air Quality/Total Human Exposure Committee (the Committee) of the Science Advisory Board reviewed the draft document "Addendum to the Methodology for Assessing Health Risks Associated with Indirect Exposure to Combustor Emissions" (the Addendum). The charge for this review can be found in Appendix A. In view of pressing EPA and public concerns about incinerators, the Committee prepared an interim letter to the Administrator to provide her with preliminary information on some of the major findings of the Committee (EPA-SAB-IAQC-94-009a). The interim letter can be found in Appendix B. This report is the Committee's detailed review of the Addendum.

3. FINDINGS

3.1 Introduction.

The assessment of risks from stationary combustors, henceforth called *combustors* for simplicity, entails a complex range of issues. A *complete* risk assessment for an individual combustor must, in principle, include direct exposures from both combustion emissions and from accidental releases of fuels (including hazardous waste) and waste ash during transportation and handling at the site as well as disposal of the waste ash. The population density in the area of such a facility, as well as the presence and contributions of other stationary and mobile combustion sources must also be considered. It is thus important to emphasize at the outset that this report primarily addresses only one of these aspects, namely the questions surrounding indirect exposure assessment which are the subject of the Addendum.

Indirect exposures from combustors are those that occur via a transfer of airborne contaminants from a combustor into water, soil, and the food chain. For lipophilic contaminants, such as dioxins, furans, polychlorinated biphenyls, pesticides, and for metals such as lead and mercury, indirect exposures through food have been demonstrated to be dominant contributors to total dose within non-occupationally exposed populations. It is also likely that atmospheric pollution from combustors and other thermal processes significantly contribute to the ubiquitous presence of some of the highly persistent lipophilic compounds.

To estimate indirect exposures to the population, the Agency needs to be able to estimate the environmental fate of combustor emissions and their consequent potential for human exposures. This effort requires the development of models to predict accumulations of chemical contaminants in environmental media and identification of the chemicals, environmental compartments, and exposure pathways most likely to be of concern, so that appropriate actions can be taken before there is widespread and/or irreversible damage. The Addendum the Committee reviewed is a critical part of the Agency's effort to deal with the difficult challenges of indirect exposure pathways.

The remainder of this Chapter describes the Committee's general findings (Section 3.2), the detailed responses to the questions raised in the Charge for the review (Section 3.3), and a summary of the Committee's major recommendations (Section 3.4). Appendix A contains the charge for the review. Appendix B

contains a copy of the earlier interim letter to the Administrator. Appendix C contains miscellaneous comments and specific corrections and recommendations for clarification of text in the Addendum. Appendix D lists earlier reports by the SAB dealing with hazardous and domestic waste incineration.

3.2 General Findings

In general, the Committee found that the multi-media model of the Addendum has merit and practical potential value because it is the first attempt by the Agency to put all the models relevant to indirect exposure together in a coherent fashion. The overall exposure model presented in the Addendum is comprehensive and includes most plausible exposure pathways, with a few exceptions that are discussed later in this report. The integration of surface water, soil, and atmosphere in the proposed water impact model is well done and represents a major improvement over earlier incinerator risk assessment models. The inclusion of the net effect of gaseous diffusion from air to water and volatilization from water to air increases the credibility of the model and should improve its reliability.

The Committee is also very aware of the difficulties inherent in the "state of the science" nature of the work which the Addendum effort entails, especially when the work must be done under the combined pressures of severely limited resources and public demands for "something" to be done quickly. The fact that the Addendum is working at the edge of our scientific knowledge is the source of its considerable merit, but it is also the source of its most serious limitations. The Committee's major findings, therefore, concern the proper use, and the potential misuses, of the Addendum as a tool by the Agency and others.

Our principal conclusion is that the Addendum is not ready for release as an "EPA Methodology" for routine, quantitative, site-specific risk assessments for combustors. We do not believe that the Addendum can be used for calculating absolute values of risks from indirect exposures that can be used with confidence. However, as described below, we also concluded that the Addendum, with a paradigm shift, could be very valuable to the Agency and to others.

During the review, the Committee was left with the strong impression that the Addendum would be the primary or even the sole tool to be used by the Agency to carry out routine, site-specific evaluations of risks from combustors. This impression caused serious concern in the Committee for two main reasons.

First, the Committee has many reservations about the possible routine use of *this specific methodology* as a detailed *quantitative* exposure model for combustors because of its early stage of development. Secondly, in general terms, the evaluation of indirect exposures from single sites is too narrow a basis for decisions regarding combustor risks. These concerns, incidentally, have been noted by previous SAB committees in their review of hazardous waste and domestic waste incineration.

With regard to the first concern, which is the principal subject of this report, the major reservations concerning the Addendum as a tool for quantitative indirect exposure modeling for combustors are as follows:

- a) **Lack of Validation and Reliance on Default Input Values.** There is a general lack of measured data to estimate input parameters and very little validation of the exposure models. The general reliance on default input data and the large number of assumptions left the Committee with many reservations about widespread application of this document as a universal EPA methodology for all types of combustors and chemicals. Sensitivity analysis of the model would help to identify which input parameters and exposure pathways are likely to be most important. For some of the indirect pathways, the exposures are likely to be trivial. For example, how large must emissions from an incinerator be before surface waters are sufficiently contaminated to be of concern with respect to exposure due to eating contaminated fish? And what is the time scale?

The Committee recommends that the Agency develop and implement a strategic plan to collect the most critical input data for the model and to validate the methodology. Sensitivity analysis of the model should be used to help identify the most important data gaps for various classes of chemicals and to develop a strategic research plan. The results of such an analysis should also be compared to experimental measurements wherever possible, as a reality check. This application of the model may also alert EPA to potential accumulations of certain chemicals in key environmental compartments.

There is a related need to have a feedback loop built into the methodology for indirect and direct exposure assessment, including

emissions measurements and a second round of modeling after a facility has been built. Other important measurements would include contaminant deposition and air measurements. These measurements should be conducted for the compounds of greatest concern, and used to validate the initial predictive modeling results.

In this context, we note a short news item in the *Journal of the Air and Waste Management Association*, Vol 42, No. 7, p. 950, 1992, reporting a study done by the Texas Air Control Board (TACB) in Midlothian, Texas, site of the highest concentration of cement plants burning hazardous waste in the U.S. The TACB conducted an extensive environmental monitoring study involving 955 samples of air, soil, water and other materials. Samples were analyzed for about 40 contaminants. All levels were below state thresholds of potential health concern. EPA should investigate the possibility of using these available data in a model validation exercise.

- b) **Lack of Information on Incinerator Upset Conditions.** The Committee is concerned about the lack of information on the frequency and impact of upset conditions (i.e., upsets in the combustion process that result in incomplete combustion), as well as breakdowns of control devices such as precipitators, on both the chemical composition and the quantity of emissions from incinerators. The model relies on measured data from four California incinerators. These limited data suggest that upset conditions may contribute significantly to the total annual emissions from an incinerator. However, the frequency of upset conditions has not been determined for sufficient numbers and types of incinerators to be reasonably confident of the adequacy of the default values recommended for general use. The Committee noted that the EPA's re-permitting process for incinerators offers a unique opportunity to obtain existing data on the frequency and duration of upset conditions for various types of incinerators in the U.S. Other useful data may be available that could be required as part of the re-permitting process. In addition, some incineration studies and emission data sets already exist in other countries. It is strongly recommended that the Agency compile and review these previous efforts as a way of focusing its future directions.

A related issue is the definition of an "upset condition." Is this a failure to meet carbon monoxide (CO) permit conditions? A recent article by Dempsey and Oppelt (*J. Air Waste Manage. Assoc.*, 43:25, 1993) notes that detection of process failure is an important research need.

- c) **Multiple Combustor Sources.** The Committee is concerned that the Addendum does not require the risk assessor to account for the impact of multiple combustor sources. While a single new facility may not result in a significant risk, the cumulative effect of the addition of a facility to an area with a number of existing combustors may well result in aggregate health risks that reach levels of concern. Furthermore, in many areas dominated by mobile combustion processes, stationary combustor sources may be a relatively minor source of total emissions. There is a need for a more regional and multi-source approach to evaluating risks from indirect exposures from combustion sources. There are also concerns that the impact beyond 50 km (the maximum distance considered in the Addendum) may be of concern for areas with significant upwind sources.

- d) **Combustors Addressed by the Addendum.** Although the Addendum nominally addresses all stationary combustors (incinerator, fossil fuel, etc.), the document as now written appears to place more emphasis on incinerators and does not adequately address all combustors. It is known that the chemical nature of the emissions and the frequency of upset conditions will differ substantially among various types of combustors but the document does not reflect this body of information. For example, there is a substantial body of information on emissions from coal combustors that should be referenced if the document is to be all inclusive.

- e) **Use of Site-Specific Data.** Throughout the document there are repeated statements that site-specific data should be used whenever possible. In reality, these recommendations may seldom be followed because of the costs inherent in obtaining site specific information, particularly emission rates and contaminant concentrations in media. There is a need to consider if there should be a requirement (instead of recommendation or preference) for site-specific data only for variables which could have a very strong impact on the exposure

results (e.g., atmospheric conditions, emission data, soil characteristics) as compared to those which may not be as crucial and where defaults may be applicable. Obviously, such recommendations would require that the Agency perform validation studies and sensitivity analysis prior to releasing the Addendum. At a minimum, a set of criteria should be provided to indicate for which variables, parameters, and conditions site-specific data are indispensable. A balance must be struck between needs for site-specific data with very strong impact on the exposure results and data with less impact where defaults may be applicable.

- f) **Conservation of Mass.** Another issue that should be addressed is the potential violation of the law of conservation of mass and the laws of chemical thermodynamics for some components of the model. In general, the proposed transport models are mass conserving. However, it was noted that contaminants are allowed to volatilize from soil and plant surfaces, but it is not clear that the lost chemical is then treated as an input to the atmospheric transport model, thus violating conservation of mass. With regard to chemical thermodynamics, it should be recognized that soils, surface waters, sediments, and (in particular) plant tissues are all secondary to the atmospheric source of contaminants and, thus, are not likely to have an annual average chemical potential (that is, "fugacity") that exceeds the chemical mass potential in the atmosphere. Because of the way contaminants are allowed to transfer from air to both plants and soils by a combination of deposition and diffusion and be lost by a removal process that is defined by a rate constant that is not clearly linked to the mass potential in these compartments, there is a possibility that these compartments could become chemical traps that receive chemicals but do not exchange them in a way that maintains chemical equilibrium. This problem is easy to fix by placing some limits on the chemical potential of any compartment relative to the air. The Committee notes that the model for the surface water is carefully constructed to maintain both conservation of mass and comply with rules of chemical thermodynamics. This care should be extended to the soil and plants compartments.
- g) **Uncertainty and Variability.** The treatment of uncertainty and the distinction between uncertainty and population and environmental

variability should be integrated as much as possible into all aspects of the method and explicitly presented. Numbers that have large statistical variances should not be reported as single values. Regression equations for biotransfer factors should not be presented without including the standard error of the estimator in these equations. The user(s) should be provided with some very explicit guidance on chemicals and compartments for which there are large uncertainties and/or insufficient information to even estimate uncertainties. In addition, the results of any risk assessment involving this model should always include estimates of the associated uncertainties.

The Committee's second major concern involves the need to evaluate the full range of potentially important risks associated with combustors, including indirect exposures, as part of any comprehensive regulatory strategy. A full discussion of this topic is outside the scope of this report, but the Committee wishes to emphasize one aspect because of its critical importance. Any national exposure assessment model framework for large combustor operations must address both local (i.e., within 50 km) impacts on human health and the environment, and also the contribution of the operations of the combustor to the more distant (>50 km) regional impacts. This consideration affects both direct and indirect exposure pathways.

For local permitting purposes, it is both practical and necessary to measure and/or model exposures within a local area (e.g., 50 km). However, for the purpose of protecting the public health and welfare it is also necessary for EPA and the Nation to consider the cumulative impacts of combustor emissions on exposure on a regional, national and international scale. The Addendum does not address the latter issue at all, and it may not be feasible or desirable to revise it to do so. On the other hand, it is essential that EPA's overall risk assessment for combustors address the contributions to both direct and indirect exposures of effluents undergoing longer-range transport, especially of longer-lived trace metals and air toxics in fine particles and vapors. Thus the cumulative exposures of people and ecosystems to combustor effluents need to be examined on both a local and national scale and applied in an integrated risk assessment framework in order to protect the public health and welfare in a responsible and cost-effective manner.

In summary, for the reasons described above, the Committee *does not support the release of this Addendum as an "EPA Methodology" for routine, quantitative, site-specific risk assessments for combustors*. We do not believe that the Addendum can be used for calculating *absolute* values of risks from indirect exposures that can be used with confidence. The Committee agrees, however, that knowledge of the important pathways underlying "indirect risks" from combustors is important, and we concluded that the methodology in the Addendum, with a paradigm shift, may have considerable value *for analytical rather than site-specific regulatory purposes*. That is, the methodology may be valuable for its ability to identify chemicals, environmental compartments and exposure pathways most likely to be significantly impacted by combustor sources and therefore to provide strategic guidance in utilizing environmental sampling to obtain actual data on indirect exposure to humans and to ecosystems.

Specifically, the Committee strongly recommends that EPA consider using the indirect exposure model to identify the environmental compartments in which particular contaminants are likely to accumulate and reach levels of concern. This would provide the Agency with a strategic basis for designing monitoring plans for these compartments in particular regions. The targetted monitoring data, in turn, would provide a means of validating and improving the indirect exposure model, as well as early warning of any accumulations of hazardous contaminants in the environment which may be of concern. Such strategic monitoring might be co-funded by a regional consortium of stationary combustor companies and operated with EPA oversight.

3.3 Responses to Charge to the Committee

The following sections (3.3.1 to 3.3.5) address the questions (highlighted in bold) posed by the Charge to the Committee, which is reproduced in Appendix A.

3.3.1 General Issues

The EPA Working Group recognizes the awkwardness of needing two reference documents to determine procedures for conducting indirect exposure assessments, and has recommended that the 1990 IED [Indirect Exposure Document] be updated with this addendum as a single methodology. In the interim, is the presentation of material in the Addendum clear? And can the SAB make specific recommendations for further clarity and completeness?

At the outset, and consistent with the emphasis in the earlier section and throughout this document, the Committee is of the opinion that the Addendum should be used for analysis rather than prediction, because the model has not been validated and lacks information about many important parameters. If the document is to be released in any official fashion for such uses, therefore, the Committee strongly recommends a revision to make this limitation unequivocally clear to the user(s).

For use in this manner, however, the Addendum is not easy to read or follow nor does it provide the kind of decision-making information necessary to conduct an indirect exposure and risk assessment for combustors in an efficient and effective manner. An integrating introductory chapter should be provided for the reader for this purpose and to place indirect exposure in perspective with direct routes of exposure. A clear definition of indirect exposure is also needed in this chapter, with some reference to EPA documents that are to be used for direct exposure assessment. In the draft Addendum, there is no overall guidance or decision tree to assist one in deciding what models to use and when. This introductory chapter should provide some guidance on this and on how useful the models are for which chemicals and which situations/combustor types. There may be more confidence in their use, for example, for dioxins, than for many other classes. Further, because the IED and the Addendum are to be applied to facilities other than municipal incinerators, a "Practical Decision Tree" is required to define the boundary conditions, complete parameterization, and to identify the chemicals of greatest concern. A decision tree could also provide some indication of the importance of various exposure pathways for key chemicals and scenarios. There is also a need to provide some guidance on the sensitivity of the results to important input parameters and on the overall "errors" which can be expected.

The EPA Working Group recognizes that implementation of the full methodology is very resource intensive and that screening procedures are needed to narrow the scope and level of detail where appropriate. The current draft does provide some general guidance on narrowing the list of compounds of concern. However, in the current form, most decisions about how to establish screening procedures would need to be made within the Agency Programs as part of their implementation guidance. Does the SAB believe this is appropriate or should further screening procedures be included here? If so can such procedures be recommended?

Since the integrated model has many components and many input values (often default), some additional screening procedures would be very useful to avoid spending time modeling exposures for pathway-chemical combinations that are likely to lead to extremely low exposures and risks. Addition of a Decision Tree which includes some guidance based on sensitivity analysis would be very helpful. If the decisions on screening are expected to be made at the Program level, the Addendum should state this explicitly. Furthermore, it should be noted that such an approach could lead to inconsistencies across Programs.

3.3.2 Air Emissions and Modeling Issues

Tables 3-1 to 3-3 present a listing of chemicals that have been measured in the stack gas emissions of a variety of combustion sources and that may be subject to direct/indirect exposure/risk assessment. Is this sufficient in terms of an initial listing and a first step in the process?

Tables 3-1 to 3-3 present over 90 chemicals which have been identified in combustor emissions. For incinerators, the list is probably too long and too detailed; for addressing combustors in general, it is probably not sufficiently inclusive. It would be more useful to present these data grouped by chemical class and combustor type and/or fuel or waste composition, rather than a single listing. This might eliminate looking at all chemicals for each type of combustor or feed composition. For example, the specific metal emissions from coal combustors are probably quite different from those from hazardous waste incinerators. The grouping of chemicals by chemical class will provide a more rational framework for both modeling and model validation, i.e., default parameters for chemicals within a class can often be approximated based on known parameters for one or two chemicals within the class. The criteria for listing compounds for various combustor types should also be explicitly presented. The issue of relative amounts of compounds also needs to be addressed on some common basis, e.g., BTUs generated, as part of the criteria for listing compounds.

For indirect exposures, it is also essential to consider the chemical stability of the chemicals in the environment. For example, 1,3-butadiene emitted from an incinerator into the atmosphere will be very short-lived and, therefore, is not likely to be significant with respect to indirect exposures. It is suggested that the lifetimes of the chemicals in the list be examined and short-lived ones eliminated from consideration for indirect exposure assessments.

With respect to missing compounds, there are some concerns about omission of trace species such as vanadium, from the lists, particularly if the Addendum is to apply to all types of combustors. How about nitrogenous organic compounds? Are the waste streams for incinerators ever high in nitrogen? If so, amines and N-nitrosamines, many of which are carcinogens, may be emitted. Also, cobalt and beryllium are cited as being emitted by sewage sludge incinerators on page 3-12, but are not included in Table 3-3.

Finally, one of the species of most concern is mercury. The atmospheric lifetime and environmental fate of mercury are determined by the chemical form (i.e., water soluble Hg^{+2} or insoluble $\text{Hg}(\text{O})$) and the physical state (i.e., gas or particulate). Unfortunately, data of this kind are sparse and often facility-specific. Consequently, measurements to address this critical issue will be required.

A general procedure is presented that, if applied, on a site-specific basis, could reduce the number and kinds of contaminants subject to review. Is the procedure adequate for this purpose, or should the Agency continue to develop a more detailed/refined screening procedure? If a detailed procedure is recommended, can you provide guidance or suggestions as to a screening method that would be appropriate?

The Committee recommends that the Addendum provide lists of chemicals grouped by chemical class and by combustor type, with chemical selection criteria explicitly stated, e.g., toxicity, concentrations in emissions, likelihood of significant exposures, lifetime in the environment, and potential for bioaccumulation. The recommendation would then be to use the sub-set of chemicals by combustor type on this list PLUS any new ones that meet these same criteria.

The Committee also has concerns about the recommendation to eliminate from consideration those compounds "which lack verified dose-response relationships for specific health endpoints." In fact, this phrase lacks any criteria for "verified dose-response relationships." The proposal for ranking is also problematic. What is the absolute framework in which the ranking would be accomplished? How are different types of health outcomes (e.g., cancer, reproductive effects) to be comparatively ranked? There are probably other sources of dose-response information other than RfD and RfC data in the IRIS that meet criteria that define a verified dose-response relationship. If not, then at a minimum, some uncertainty factor for risk assessment should be developed.

A hierarchical approach for estimating emission rates of contaminants is presented. Is this procedure adequate in terms of providing estimates of average emission estimates. A more difficult issue is the magnitude, frequency and duration of increased emissions that may occur during temporary upset conditions, during start-up and shut-down procedures and during emergency events. The Agency presents default assumptions that address these occurrences based on information developed by the State of California Air Resources Board. Are these default assumptions adequate in terms of reflecting short-term and longer-term increases in emissions when mal-operation occur? Can the SAB recommend additional assumptions, alternative approaches, and/or databases that may be used?

The hierarchical approach for estimating emissions needs some clarification. The data from trial burn conditions should not be used to represent normal operating conditions (temperature, feed, etc.). Furthermore, the Addendum does not reference other studies on emissions from stationary combustion sources which have been completed. This information would be useful to those trying to use this approach. Will the information in AP-42, which "will be available" for MWC (municipal waste combustors), sewage sludge incinerators, and medical waste incinerators, take into account different types of incinerators, air pollution control devices, etc.?

The basis of the default assumptions on incinerator upsets is not clear from the document. For example, how many incinerators were monitored, and for how long? The basis for the assumption of a one-hour upset duration as well as CARB's selection of default values for emissions under poor operation conditions need to be further documented. An hour upset seems quite long. Normally the waste feed would shut-off immediately and the upset time would depend upon the gas and solid residence times in the incinerators as well as how long the contaminants were volatilizing from the solid. In the case of liquids, the time would be only the gas residence time. How well does one hour represent actual conditions? Are there national data on the nature, frequency, and duration of less than optimum operating conditions for different types of incinerators which could be used as a more "realistic" guideline?

The default assumptions that are recommended suggest that upset conditions may have significant impact on the total emissions. If the frequency of upsets is substantially greater than the California data indicate, then the impact of upsets on average emissions could be very significant. If this is the case, then EPA needs to get some real data on what the emission factors actually are during

start-ups, shut-downs, upsets, and poor operation. Normally, incinerators are not allowed to operate with waste materials during start-up (natural gas or fuel oil are normally used). Calculations based just on good operating conditions may be quite misleading. In addition, various incineration studies and emission data sets exist in other countries. It is strongly recommended that the Agency compile and review previous efforts as a way of focusing its future directions. The Committee also notes that the EPA's re-permitting process for incinerators offers a unique opportunity to obtain existing data on the frequency and duration of upset conditions for various types of incinerators in the U.S. Other useful data may be available that could be required as part of the re-permitting process. Data on the frequency of upsets for RCRA units should be available.

A specific procedure (based primarily on adsorption theory) is presented to estimate the vapor phase/particle phase partitioning of semivolatile organic compounds in the ambient air. This V/P ratio is then applied to the air modeling of the contaminant emissions. Specifically, the V/P ratio expected in ambient air is assumed to apply at the stack. Is this method appropriate or can alternatives be recommended?

Some clarification of the underlying assumption about which particles are being addressed--those from the incinerator or those in the ambient atmosphere--is needed in the discussions on pp. 3-30 and 3-31. The assumption appears to be that particle emissions from incinerators are generally removed by control devices and that the combustor vapors that condense the ambient particulate matter are being addressed here. If this is the case, then it should be clearly stated.

The net effect of using the recommended procedure is to underestimate wet deposition near the source and to overestimate it further from the source. This is because p is high near the source and low downwind and therefore f is higher near the source than downwind.

On p. 3-35, Equation 3-12 says that for $p \ll (cS_p)$, ϕ is independent of p . This only applies for adsorption. If the molecules are water-soluble, then absorption is important and ϕ is not independent of p . Similar considerations apply for organics being absorbed into an organic matrix (rather than simply adsorbed on a surface). In this latter case, the weight fraction of organic matter in the aerosol is probably important. Equation 3-12 has the correct limits in either case, i.e., that ϕ approaches 1 at large p and zero at small p . However, to the extent that absorption is important, it does not have the correct form between

the limits. The examples cited in the documents apply to very low values of ϕ , on the order of 10^{-12} atmospheres in some cases. At these low values of p , absorption may be an important process even for very slightly soluble vapors.

In addition, there has been much more work done on adsorption of organics since Junge's publication. This section does not reflect the more recent (1982 to current) literature on this issue. Specifically, temperature has long been known to affect the distribution between vapor and particle phases (See Yamasaki *et al.*, *Environ. Sci. Technol.*, 16: 189-194, 1982; Pankow, *Atmos. Environ.*, 25A: 2229-2239, 1991; Baek *et al.*, *Chemosphere*, 22: 503-520, 1991). Summer-winter differences between the percentages in the vapor phase can differ by a factor of three. Just recently, Pankow *et al.* (*Environ. Sci. & Technol.*, 27: 2220-2226, 1993) reported some evidence that relative humidity affects the partitioning. This effect appears to be smaller than that due to temperature but EPA should be aware of this work.

Also, Whitby's data for surface to volume ratios (p.3-35) are actually based on measurements of particle number concentration. Surface and volume were estimated assuming spherical droplets. Therefore, these data do not reflect actual measurements of surface-to-volume ratio. The actual surface-to-volume ratio in the context of Equation 3-12 could be very different, given that some fraction of the particle mass contains materials with very high adsorptive capacities. Because this fraction is usually small, the overall surface to volume ratio could be highly variable.

Since the vapor/particle partitioning for semi-volatile organic compounds is an input parameter that is used throughout the modeling on atmospheric and inter-media transport, it is likely to have a significant impact on final outcomes of the modeling. Thus, some representative measured data are critically needed. Measurements made out the stack should be made with a dilution sampler and should utilize the more advanced denuder-filter samplers for determining the partitioning wherever possible.

In order to evaluate the potential deposition flux of particles near the source, a particle-size distribution must be known. A default assumption of the particle size distribution of particulate matter in combustor emissions is presented in Table 3-7. Is this default distribution of particulate matter appropriate for use in those situations where site specific information is not available? The emission rate of the particle-bound portion of the contaminant is apportioned to the particle

size distribution based on the assumption that the contaminant will be adsorbed on the surface of the particle. Is the procedure for assigning the portion of emissions to the particulate array logical and technically defensible?

The reason for selecting the size distribution presented in Table 3-7 as the default distribution needs to be justified. How does it compare with whatever size distribution data are currently available? (some of these data may be available at the State level). How sensitive are the exposure predictions to the particle size distribution? If exposure predictions are sensitive to this parameter, then why not simply require some direct measurement? Compared to the cost of building and operating an incinerator, this cost is generally trivial. Also, if combustor particulate emissions are being addressed (see comments above) the method of measuring the particle size distributions from the incinerator should be specified. A dilution sampler that allows for condensation and coagulation should be used as it provides a better measurement of what the size-distribution will look like in the environment.

The Addendum recommends running the COMDEP model twice in order to isolate the ambient air concentration and the wet/dry deposition flux of the particle-bound portion of the contaminant. Is this approach appropriate and sufficient for purposes of providing concentration terms to be used in estimating inhalation exposures, for estimating media concentrations, and for estimating contamination of the human food chain?

It should be noted (p. 3-16) that MPTER-DS is only one of a number of model frameworks for estimating deposition from a point source. To the extent that the vapor/particle partitioning depends upon airborne concentration and, therefore, changes with distance from the source, the MPTER-DS model does not describe this phenomenon. An alternative framework is the segmented plume model approach (e.g., Zanetti and co-workers). This framework can include both deposition (wet and dry) and changes in the vapor-particle partitioning. Another addition worth considering is the adjustment of the sigma-z value below the plume centerline due to shear induced dispersion (see, e.g., Venkatram *et al.*). This phenomenon is important under weakly stable conditions and can result in ground level concentrations that are several orders of magnitude greater than those predicted by symmetry models. Because weakly stable air is a frequent nighttime phenomenon, this adjustment could be important to the predictions of dry deposition on a long-term basis.

The scavenging coefficients in Table 3-4 are not very useful because they are too general. The current version of the model allows the user to specify the coefficients as a function of particle size. A more useful set of suggested default values should be provided here as a function of particle size.

With respect to the vapor deposition, there has been a good deal of work on predicting air-surface exchange during the last decade or so, generally using the multiple resistance analogy, under the National Acid Precipitation Assessment Program; in future generations of this model, perhaps some of that effort could be adopted, so that dry deposition is better treated.

In addition, it has been pointed out that the basic transport and dispersion model is a Gaussian plume type, adjusted to account for some of the inherent shortcomings of that model, and drawing on decades of EPA and air quality community experience with such models. But the model still has two central problems:

- a) It just cannot deal with calms (the algorithm has the wind speed U in the denominator, a consequence of the model's basic assumption that along-wind diffusion is negligible compared to transport with the wind, and so the computations "blow up" as U approaches zero). In some areas of the U. S. (e.g., the southeast), calms are very common -- up to 20% of the time or more -- especially at night. The model simply ignores these periods, which unfortunately may account for the highest concentration levels. This can lead to underestimates of dose.
- b) The model uses "straight-line" winds from a single location to move the plume. This is adequate for relatively flat terrain, especially when averaged over long periods, but it cannot account for terrain-forced changes in wind direction, which are often very persistent. A simple example is the wind within a curving valley, which often simply follows the valley; however, a computed straight-line plume will exit the valley, and produce erroneous concentration predictions, both short- and long-term. If these are put together with actual population patterns (population centers are often within valleys), there is a risk of underestimating the dose. A coastal zone which experiences frequent and repeatable sea/lake breeze patterns is another area where a straight-line model will not perform well. Note: the authors do a good job of emphasizing that on-site

data are highly recommended for the calculations; in a complex terrain site, such data are essential. Data from some airport 50-100 km away are simply irrelevant.

Because of these two shortcomings, it is important that some explicit guidelines be established for use of the model, to ensure that it is not applied in inappropriate circumstances.

It is also recommended that the EPA consider taking into account the substantial progress that has been made in atmospheric transport modeling, and move on in its regulatory models to codes which can deal with calms, and with spatially non-uniform wind fields. This may require (at least in the short term) an increased amount of site-specific wind data to drive the model, but greatly increased realism should result. For the purposes of the combustor document, the Gaussian model is adequate *as long as its application is limited to conditions where its inherent limitations are not exceeded*. This means that it may not be possible to do a reasonable assessment of all sites with this particular tool.

The Bowers et al. algorithm for building wake effects (p. 3-17) has been shown to be flawed in a significant way (Shulman and Scire, *J. Air and Waste Manage. Assoc.*, p. 1122-1127, 1993). The algorithm has nonsensical mathematical limits such that a tall, skinny building can generate wake concentrations greater than the stack gas concentrations! It also misses the fact that a portion of the plume can be trapped in the wake (because it has an "all or nothing" feature) and thus will significantly underpredict the wake concentrations when the stack is short. This latter feature was verified via wind tunnel testing in the paper by Shulman and Scire. It is recommended that the Shulman and Scire model be incorporated into COMDEP. It is a straightforward fix.

There also needs to be some discussion of the methods used to estimate the turbulence parameters, u^* and L (p. 3-22 to 3-23). There are a number of approaches that could be used, ranging from direct methods based upon cloud cover and wind speed to a simple look-up table based upon stability class. The latter method is the least accurate, especially under stable conditions. If one wants to use other methods, there should be a list of those methods.

3.3.3 Soil Impact and Food Chain Issues

Impacts to soils have been modeled as a function of contaminants depositing onto soils in the particle-phase; deposition has included wet plus dry deposition of particle-bound contaminants. A common shortcoming identified is the lack of consideration of vapor phase impacts to soils. This Addendum recommends the inclusion of a vapor-phase diffusive flux term to soils, based on the gas-phase mass transfer coefficient described in the IED and used there to estimate a volatilization rate from soils. Is this approach accurate, appropriate, and sufficiently described?

With regard to deposition, the Committee is pleased that the Agency no longer recommends the use of the ISC deposition model. The new recommended procedure is also troubling, however. Given the present state of knowledge, the recommended procedure is not superior to simply assuming a total particulate deposition velocity on the order of 1 cm/sec (together with a presentation of the uncertainty around this value). For additional information, see Webster and Connett, 1989a.

The Sehmel-Hodgson model is semi-empirical. One of the resistance integrals (box 3) is evaluated using wind-tunnel data. Dry deposition onto plants is of considerable interest for indirect exposure assessment. It is not clear that the surfaces used in the wind tunnel experiments (including artificial grass) adequately model plant surfaces (e.g., waxy hairs).

The boundary conditions of the model may not match. Many risk assessors estimate dry deposition by multiplying a deposition velocity by an air concentration at a reference height (calculated by a plume model). As Sehmel and Hodgson state, their deposition velocity model assumes that "particles diffuse at a constant flux from a uniform concentration of particles..." Although this does not precisely match the conditions in a plume, it is unclear how much difference this discrepancy would make. When a surface depletion model is available, Sehmel and Hodgson note that the box 3 resistance integral can be used directly (Sehmel and Hodgson, 1980).

How well has the Sehmel-Hodgson model been validated with field data? Travis and Yambert (1991) compare the predictions of several such models with various field data. All of the models they analyzed tend to underestimate mean deposition velocities for particles with diameters of 0.05-1 μm (an important range of particle sizes for incinerators). There was great variation in the field data, with deposition velocities as high as about 1 cm/sec for all particle sizes from 0.02 μm to 10 μm . Unfortunately, as is often the case with field data, detailed

meteorological data are not provided. We are left with a difficult problem: how do we validate less than ideal models with problematic field data? Has anybody conducted a more rigorous evaluation of the Sehmel-Hodgson model?

Koester and Hites (1992) measured wet and dry deposition of ambient particulate-bound PCDD and PCDF in Bloomington and Indianapolis. The average dry deposition velocity was about 0.2 cm/sec; wet deposition flux was about the same as dry deposition. McVeety and Hites (1988) estimated dry deposition of PAHs at about 1 cm/sec to water. A number of factors may account for this four-fold difference, including the type of surface, water vs. "frisbee" collectors (neither of which behave like plants).

Mixing soil depths of 20 cm for tillage operations (farming, e.g.) and 1 cm for non-tillage situations (pasture, lawns at residences) were recommended in the 1990 IED, and are also recommended here with additional verbiage on their use and interpretation. Are these depths appropriate, and is the SAB aware of other methodologies to determine more appropriate soil mixing depths?

These assumptions seemed reasonable to the Committee.

A differentiation between below and above ground vegetation is recommended in this document, and a procedure supplied for below ground vegetation is described. Is the differentiation appropriate, and is the procedure for the underground vegetation appropriate?

The Addendum recommends separating the estimation of transfer to above ground and below ground parts of plants. This seems reasonable. However, the proposed model should be evaluated with respect to factors such as: the theoretical basis of the model, the reliability of empirical data, the range of validity, the similarity of modelled situation to experimental set-up, and the validation with field data.

As an example of concerns about data reliability, Travis and Arms' (1988) regression equation used results on TCDD from Helling *et al.*, originally reported in Isensee and Jones (1971). In actuality, no TCDD was detected in the plants, making the point an upper bound. Examination of the regressed data suggests that it may depend to a large degree on one extreme data point for PBB.

The 1990 IED assumed that wet deposition (i.e., deposition of particle-bound contaminants via rainfall) would not impact above ground vegetation (vegetables/fruits, and animal feeds). The reference to this assumption was an internal memorandum which did not include a technical justification. The Working Group recognized that for some compounds evidence exists that wet deposition is roughly equal in magnitude to dry deposition, so that zeroing out wet deposition halves the impact of depositions to plants. Subsequent to the completion of the Addendum document, the Working Group was made aware of a key publication by Hoffman, et al., (1992, Quantification of the interception and initial retention of radioactive contaminants deposited on pasture grass by simulated rainfall. Atmospheric Environment 26(A) (18) 3313-3321). Data from this article suggests that the retention of particles on the vegetation tested ranged between 24 and 37% of all depositing, depending on size of particle and vegetation. Without this information, the Working Group recommended the following assumptions for the percent of contaminants in wet deposition that is retained on the plant surface: 1) 100% for contaminants with high sorptive tendencies and 10% for contaminants with low sorptive tendencies. The Hoffman article suggests that an assumption of 30% retention of particle-bound contaminants, regardless of sorptive tendencies of the contaminant is a more supportable assumption. Which of the two approaches, or other approaches, would the SAB recommend? And is the SAB aware of other information that would assist in developing values for the retention of particle-bound contaminants on plants during rainfall?

The 30% assumption seems more supportable. The previous assumption that the sorptive properties of the contaminant on the particle determine retention does not seem reasonable since it is the interaction between the plant surface and the particle as a whole that is more likely to be the determinant of retention.

After contaminants from stack emission deposit onto soils, they can be resuspended as particles or volatilize into the air. Model testing has shown that resulting air concentrations associated with these soil emissions are significantly smaller in magnitude compared to concentrations resulting from the stack emissions. Accordingly, the Addendum does not recommend increasing modeled air concentrations from stack emissions to reflect additional emissions from soil. Does the SAB concur with this recommendation or can alternative approaches be recommended?

Does "significantly smaller" mean an order of magnitude or two or a factor of less than an order of magnitude? There should be some statement that

provides this information. If it is "order of magnitude" smaller, then the Committee agrees that there is little point in extensive modeling.

Recent literature has emphasized the importance of vapor-phase transfers to vegetation, particularly for dioxin-like compounds. This Addendum recommends substantial changes for estimating vegetative impacts by vapors. These changes include:

- 1) Neglect soil volatilization as an additional reservoir for vapor transfers. The argument was presented that the soil-plant empirical transfer factor already includes all routes of soil to plant transfers.
- 2) Do not include the vapor fraction term, since the air modeling will directly yield a vapor-phase concentration.
- 3) In external model testing, the Bacci azalea leaf model was shown to overestimate the transfer of vapors to plants. An empirical correction reducing the value of the Bacci transfer factor by 90% was recommended based on external validation exercises with dioxin.

Are these changes technically accurate, complete, and sufficiently described?

One of the most significant issues in the methodology that can and should be addressed is the inherent lack of reliability associated with both measured data and models used to determine inter-media transfer factors (ITFs). There are a number of ITFs proposed for use in the Addendum including octanol-water partition coefficients (K_{ow}); organic-carbon partition coefficients (K_{oc}); soil-water and sediment-water partition coefficients (K_d); Henry's law constant; mass-transfer coefficients in water and air; steady-state bioconcentration factors for plant root concentrations relative to soil concentration, plant leaf concentration relative to air concentration, and fish concentration relative to water/or sediment concentrations; steady-state biotransfer factors for milk or dairy-product concentration relative to contaminant intake by cattle; meat concentration relative to contaminant intake by animals; egg concentration relative to contaminant intake by chickens; and breast milk concentrations relative to contaminant intake by mothers; and contaminant biodegradation factors in soil. Recent work on the reliability of methods for measuring and estimating these types of ITFs reveals that the reliability for determining ITF values has an associated error factor in the range of 1.5 to 10 depending in part on whether the ITFs are measured or estimated. McKone has

found that overall variance in quantitative estimates of ITFs comes from several factors including (1) variability among experiments; (2) our ignorance regarding the processes of metabolism and chemical partitioning; and (3) the reliability with which we can measure both the outcome (biotransfer or partition factor) and the explanatory variable (i.e., Kow). (SAR and QSAR, in *Environmental Research* 1,41-51, 1993) It is likely that the lack of reliability for determining these ITFs can be a major contribution to the overall variance in the exposure estimates.

With respect to vapor plant transfer, the Committee is pleased that EPA has considered this potentially important pathway. A thorough evaluation would discuss (as noted earlier) the theoretical basis of the model, reliability of empirical data, range of validity, similarity of modelled situation to experimental set-up and validation with field data.

A comparison between the results of Bacci *et al.* and McCrady *et al.* indicates two problems with current use of the Bacci model;

- a) plants differ in their uptake (k_1) and volatility (part of k_2) rates;
- b) photodegradation appears to be a real phenomenon that should be taken into account.

EPA suggests using the Bacci model with a common correction factor of 0.1 for all compounds. EPA bases this correction factor of modelled and measured average "dioxin" environmental concentrations. There are some problems with this approach. First, the Addendum does not present enough data for proper valuation. Second, even if the data were appropriate, the comparisons should take into account the variance of the data, not just mean values. Third, the sensitivity of various compounds to photodegradation varies (even within the dioxin family).

On the other hand, comparison of the grass and azalea results in Table III of McCrady *et al.* suggest 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.* 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. For instance, Bacci's results may be a better approximation for certain other plants.

Table III of McCrady *et al.* indicates that inclusion of photodegradation reduces the Bv of 2,3,7,8-TCDD onto grass by about a factor of four (relative to only including volatilization in k2). The discrepancy between the results of McCrady *et al.* and Bacci is also due to the use of different plants and isomers. Note that the equilibrium "bioconcentration" factor (Bv) depends on the ratio of k1 to k2. Based on Table III of McCrady *et al.*, k1 for grass (2,3,7,8-TCDD) is about 9 times larger than for azaleas (1,2,3,4-TCDD) while the k2 for volatilization is about factor of 26 larger for grass.

The Addendum's discussion of the time required to reach steady state (p.5-9,10) is somewhat confusing. If vapor to plant transfer is modeled as a first order system,

$$\begin{aligned}dC/dt &= k_1 \cdot C_a - k_2 \cdot C \\C(t) &= k_1/k_2 \cdot C_a \cdot (1 - \exp(-k_2 \cdot t))\end{aligned}$$

then the time required to reach equilibrium depends only on k2, not k1. In Table III of McCrady *et al.*, the k2 value for grass is larger than the k2 value for azaleas, accounting for the more rapid equilibration.

3.3.4 Water Impact Modeling Issues

A three-tier approach to evaluating aquatic impacts, from screening level, Tier 1, to site specific and more complex modeling, Tier 3, was promoted in the Indirect Exposure Document. This has been replaced by a steady-state model for long-term average risks and a storm event model for short-term acute risks. Is the replacement of the three-tier system with these two models appropriate?

A tiered approach would seem to be a more sensible approach since the aquatic modeling is quite complex and requires many inputs. At a minimum, some guidance should be provided concerning the conditions under which one might undertake such detailed calculations. An additional point is that the Addendum appears to suggest lakes or ponds as the bodies of water of concern. Much of the U.S. population resides in coastal cities. Has any thought been given to impacts of multiple incinerators impacting coastal regions, e.g., Atlantic seacoast, and possible impacts of seafood production and health of the ecosystems in these areas?

The previous IED long-term average water model was quite simplistic, accounting for loading and dilution only. The steady-state water model proposed

in the Addendum accounts for several water body loss processes, ... It also allows for formation of a reaction product, thereby maintaining reservoirs of more than one contaminant in the aquatic ecosystem. These processes, of course, require more input data. Does the addition of these processes make the simple screening level approach more appropriate for assessing health risks? And can the SAB provide comments on specific algorithms?

The Committee did not have sufficient information to address this question.

The proposed watershed loading model calculates average soil concentrations in a manner similar to the old IED. One additional process modeled is the atmospheric diffusion loading to the soil. Long-term average erosion of chemical from the watershed is multiplied by sediment delivery ratio and a pollutant enrichment factor before loading to the waterbody. Are the watershed calculations appropriate for calculating long-term average loadings via erosion to the waterbody?

The Committee did not have sufficient information to address this question.

The proposed storm event water model, like the previous version, accounts for loading and dilution only. There are two significant differences. The proposed version includes a watershed sediment delivery ratio, so that predicted loading of sediment and chemical is reduced by typically over 70%. Second, the proposed version calculates a peak instream storm flow that includes storm runoff volume added to the average annual stream flow. The old version diluted storm loads into base flow only. The additional dilution further reduces the calculated peak sediment and chemical concentrations. Is the proposed storm event model appropriate for calculating peak concentrations for assessment of acute or threshold health risks?

The Committee did not have sufficient information to address this question.

3.3.5 Exposure Issues

The Addendum recommends a procedure to define the extent of the study area on the basis of an isopleth corresponding to a de minimus risk level. Is this appropriate; if not could alternatives be suggested?

The use of geographically-defined boundaries based on dispersion/deposition models is reasonable for estimating concentrations in soil, water, and air, and direct exposures to these media. However, it may not be sufficient for defining the population at risk from indirect exposure. The Working Group has partly recognized this by including in the population at risk those individuals who live in areas of negligible risk but spend time within areas of concern. However, indirect exposure to people living outside the areas of concern could also occur because the medium of exposure (e.g., food) is transported from the areas at increased risk to individuals who neither live nor spend time in those areas. It would not be unreasonable for populations residing and working beyond the fifty mile boundary to consume foods such as beef or milk produced within the fifty miles. The criteria for defining the population at risk should consider the complete indirect exposure pathways (e.g., consumption of beef and/or milk from cattle range-fed within the 50 Km) on a site-specific basis. (This approach is apparently suggested in method #2 for estimating population risk on page 15-2-3).

The idea of "isopleth rings" as presented in Figure 2-1 (page 2-5) and Table 2-1 (page 2-7) should be refined. The shape and area of the isopleths derived from the dispersion/deposition models could be quite different from those represented by the "rings," and will depend on combustor-specific characteristics (e.g., stack height), as well as local topography and prevailing atmospheric conditions. A "ring", as represented, could encompass both areas of high and low media contamination, depending on prevailing wind direction, for example. Further, a high (or low) media concentration isopleth could extend beyond a "ring". Therefore, within any given "ring" one could expect to find a distribution of exposures (and risk) depending not only on population activities but also on media concentrations.

Why was 200m selected as the minimum distance for the first "ring" of receptors under all conditions? Is this distance reasonable in situations when downwash/building wake effects occur?

The recommendation to develop distributions of exposures is a substantial improvement over the use of worst case scenarios and is to be commended. The two procedures described are generally clear and reasonable. One clarification - for Method 1 (series of point estimates), how does one calculate an average since the modeling is based on several scenarios? Is the average assumed to be the model outcome when average values are used as input?

The Addendum recommends a procedure to develop distributions of exposure on the basis of either a series of either point estimates of exposure or a Monte Carlo assessment. Both would involve identifying isopleths defining areas of equal media contamination levels and characterizing behavior patterns in these areas. Are these procedures clear and reasonable?

The Addendum provides some general guidance for conducting a Monte Carlo assessment in the areas of separating uncertainty and variability, addressing dependence among variables and selecting distributions. Is this guidance appropriate and sufficiently detailed?

The use of Monte Carlo simulations to estimate the effects of uncertainty is not sufficiently clear. The distinction between "variability" in exposures and risks (due to variability in the location and activities so the population), and "uncertainty" due to uncertainties in measurements and parameterization need to be made clear.

Monte Carlo Analysis is only one approach used to examine variance. It can yield large quantities of data that are not particularly useful. One must also be concerned with the form of the exposure distributions, and examine the high/median/low features of a distribution at a site. These distributional analyses can help define the types of actual measurements which should be taken after the plant is operational, and how they can be used in a follow up assessment.

The Addendum provides two procedures for estimating breast milk concentrations on the basis of maternal intake. Are these the best models available for this purpose? Are the assumptions sufficiently well stated?

Accuracy can only be determined by comparing estimates with actual data. For TCDD, the Smith approach would appear to be more accurate than the Travis *et al.* model, as presented. Since the Travis *et al.* model was largely based on estimates rather than direct measurements of organic compound concentrations in human milk. Can any of their estimates be compared to actual data for those same compounds, if those data are now available? This may explain part of the discrepancy and perhaps assist in adjusting their equation. Anyway, if application of a simpler model results in estimates which approximate actual data better, use of other more complex and apparently less reliable alternatives appears to be unnecessary.

The various approaches--bioconcentration, biotransfer, fraction transferred (McLachlan *et al.*, 1990)--all have an underlying commonality based on pharmacokinetics. Among the factors that should be taken into account in modeling bioconcentration of lipophilic, low volatility compounds are: absorption, metabolism, growth, various routes of elimination (lactation, metabolism, excretion of parent compound across the gut).

Some of the best data on bioaccumulation of dioxin and similar compounds into cow's milk comes from McLachlan (1990, 1993) who examined (presumably) steady-state input and output of xenobiotics from a dairy cow eating feed grown in a relatively unpolluted area. This case provides a closer match to many real-world "indirect exposure" situations than extrapolation from spiking experiments (requiring additional assumptions about the bioavailability to the cow). McLachlan's data are preferable to the TCDD estimate of Fries and Pastenbach (1990) which was not at steady state (Fries and Pastenbach justified the latter on the basis of similarity to BCFs for certain other xenobiotics. However, some of the latter were not persistent; for instance, arochlors contain both readily metabolizable and non-metabolizable congeners). Although some claim that the BCF for TCDD should be the same for beef fat and milk fat, this appears unlikely. Correction for experimental values to steady-state results in higher bioconcentration in beef (Jensen *et al.*, 1981). Lactation provides an efficient means of excreting lipophilic compounds.

For most compounds, many model parameters for bioconcentration are not readily available and structure-activity relationships are recommended in the Addendum as a means of estimation. EPA should make clear that good experimental data are preferable to use of a structure-activity relationship. Second, the use of several of the SARs in the Addendum that estimate bioaccumulation from food or feed to breast milk, cow's milk or beef based on $\log K_{ow}$ (a measure of the lipophilicity of a compound) cannot be recommended for several reasons.

These SARs assume a direct proportionality between bioaccumulation and $\log K_{ow}$. There is some theoretical basis for this relationship for fish based on partitioning between water and fish lipid. But there are several well known limitations to even this situation. It does not work so well for compounds which are easily metabolized. It does not properly take into account biomagnification (which leads to concentrations above equilibrium with water). It does not work well for highly lipophilic compounds. Indeed, the relationship between

bioconcentration, bioaccumulation and K_{ow} seems to plateau and then decrease for these compounds. This may be a function of poor absorption.

For terrestrial animals, partitioning occurs primarily between body lipid and gut contents. The other limitations apply as well. A decrease of bioaccumulation at high $\log K_{ow}$ is usually observed. The ability of an organism to metabolize a compound has no necessary relationship to lipophilicity. For example, some PCDDs are relatively easy to metabolize while others are not; it depends on the number and arrangement of the chlorine atoms. As a result, compounds with similar $\log K_{ow}$ s may bioaccumulate to very different degrees (unfortunately, the more toxic PCDDs are the same ones that bioaccumulate).

The data used in such SARs must be carefully scrutinized. Inclusion of both readily metabolized and poorly metabolized compounds in the SARs--as was done in the models in the Addendum--will bias estimates, generally downward. The data should normally represent steady-state conditions (unless the time required for steady-state is excessively long). Empirical data not corrected to steady state will also bias estimates downward.

Concentrations in beef cattle will be diluted by growth, but a "balance" of these two factors would be coincidence. Finally, the diet of beef and dairy cattle may vary considerably by location and type of farm. For instance, the cattle of "subsistence" or small farmers may consume more pasture than larger commercial herds.

The Addendum describes a procedure for estimating population risk for cancer on the basis of local food production. Is this approach clear and reasonable?

The Committee did not have sufficient information to address this question.

The 1990 IED document used the 1978 USDA food consumption survey to estimate diet fractions for locally grown foods. Some of these estimates do not appear reasonable. The Working Group strongly recommends trying to base these estimates on local surveys, but could not resolve how to make these estimates if this was not feasible. Can the SAB provide any further guidance on how to address this issue?

The Committee did not have sufficient information to address this question.

Indirect pathways are defined as all pathways other than inhalation. This document addresses food chain, water, soil, and breast milk exposures. Should other pathways be considered?

A somewhat revised and expanded definition of indirect exposure is needed that includes the idea of transfer to another medium, for example, deposition of aerosols followed by resuspension and inhalation or ingestion.

Penetration of the incinerator-produced aerosols into buildings and subsequent deposition onto surfaces is mentioned as an exposure pathway (p. 4-1) but is not treated adequately. This pathway may be more significant than suggested in this document for populations living near incinerators. When windows are open, there is quite a rapid accumulation of dust on indoor surfaces. For homes near incinerators, this dust is likely to be relatively enriched in incinerator contaminants (i.e., there will be less dilution by other outdoor aerosols or by soil). Children would be expected to pick up such dusts on their hands and carry them to their mouths. The recommendation that the estimated concentration in the top 1 cm of soil be used as the estimate for this deposited dust from air seems inappropriate as the soil itself will cause a dilution.

Some Additional Exposure Issues

Section 7. Determining Dermal Exposure from Dermal Absorption via Soil, p. 7-1. The conclusion/recommendation for the issue of dermal risks is to apply the USEPA guidance document, "Dermal Exposure Assessment: Principles and Applications." One of our Committee members reviewed this document and noted that the application is not entirely straightforward. Parameters for its implementation may not be available for many chemicals. The equations have not been provided in the Addendum and it is therefore impossible to evaluate effects of uncertainties in these parameters on the outcome of an exposure and risk assessment.

Section 8. Dust Resuspension, p. 8-1. The conclusion/recommendation on estimating exposures to fugitive dusts from tilling and vehicle traffic is to add algorithms for estimating this exposure. As in Section 7, however, documents are cited but no equations or example calculations are provided as the reader is led to expect from the "Example Calculations" heading. Vehicle generated fugitive dust may be 100 times as great as wind-generated fugitive dusts. This exposure pathway is particularly important for chemicals that are carcinogens by inhalation

but not ingestion. For chemicals that are carcinogens by both routes, the larger mass of ingested soil will be the more important pathway.

Section 11. Determining Exposure from Fish Intake, Bioconcentration Factor, p. 11.1. The conclusion/recommendation for this section is confusing. In the first paragraph, it is stated that "The use of the Biota Sediment Accumulation factor (BASF) is recommended for dioxin-like compounds." In the second paragraph appears: "While this recommendation is made here, it should be noted that water column approaches for lipophilic compounds, including dioxins, are currently being used in the Agency and used appropriately." Which is it? BASFs may be hard to come by. It is not a good idea to incorporate terms for exposure assessments for which input values are not available. The models are simply not usable. Parameter values should be provided for at least the top 50 contaminants.

For the second issue on this page. "Alternate approaches for estimation of aquatic bioconcentration and bioaccumulation may also be appropriate," it is suggested that the Food and Gill Exchange of Toxic Substances Model of ERL Athens, GA be consulted. Where is it and how does its output compare to the other suggested methods? What is the overall recommendation? Exposure to toxics by the consumption of contaminated fish is a very important route of exposure. The Methodology for Assessing Health Risks Associated with Exposure to Combustor Emissions will not be very useable until some of these issues have been addressed and examples for many chemicals have been worked through the model.

Section 12. Determining Exposure from Dermal Absorption from Water. p. 12-1. The conclusions/recommendations for the issue of dermal absorption from water are: "This chapter should be replaced by the appropriate sections of the report "Dermal Exposure Assessment: Principles and Applications." Which are the appropriate sections? Where are the equations? Are the parameter values available for a significant number of chemicals?

3.4 Summary of Major Recommendations

The following are the major recommendations arising from this review. The bases for these recommendations are summarized in Sections 3.2 and 3.3 above.

1. Use the Addendum *as an analytical tool rather than a site-specific regulatory methodology*. That is, use the addendum to identify chemicals, environmental compartments and exposure pathways associated with these sources and thus provide strategic guidance in utilizing environmental sampling to obtain actual data on indirect exposure to humans and to ecosystems.
2. Do not release the Addendum as an "EPA Methodology" for routine, quantitative, site-specific risk assessments for incinerators.
3. Develop and implement a strategic plan to collect critical input data that can be used to refine the models and to validate the methodology. Take advantage of the re-permitting process as an opportunity for the collection of such relevant data.
4. Establish a framework to ensure that the entire range of potential risks from combustors are addressed holistically. This must include both direct and indirect risks, as well as local, regional, national and international concerns for both waste combustors and fossil fuel combustors.

APPENDIX A. Charge to the Committee



UNITED STATES ENVIRONMENTAL PROTECTION AGENCY
WASHINGTON, D.C. 20460

NOV 23 1993

OFFICE OF
RESEARCH AND DEVELOPMENT

MEMORANDUM

SUBJECT: Science Advisory Board Review of the Draft Report
"Addendum: Methodology for Assessing Health Risks
Associated With Indirect Exposure To Combustor Emissions
Working Group Recommendations"

FROM: William H. Farland, Ph.D. 
Director
Office of Health and Environmental
Assessment (8601)

TO: Donald G. Barnes, Ph.D.
Director
Science Advisory Board (1400)

The purpose of this memorandum is to outline our charge to the Science Advisory Board (SAB) for review of the subject draft report. This report was developed by a Working Group which began deliberations in February, 1993. The purpose of the report was to evaluate and make recommendations concerning the state of the science with regard to indirect exposure methodologies relating to incinerator emissions. The Working Group was comprised of individuals from the several offices within EPA, including the Office of Research and Development, the Office of Solid Waste, the Office of Pollution Prevention and Toxic Substances, and the Office of Water.

The Working Group selected the document, *Methodology for Assessing Health Risks Associated with Indirect Exposure to Combustor Emissions - Interim Final* (EPA/600/6-90/003; January, 1990; subsequently abbreviated the Indirect Exposure Document or IED) as the best currently available guidance on this topic. Using this document as a starting point, the Working Group identified areas needing updates and recommended ways to make these updates. The recommendations have been compiled into an Addendum to the Indirect Exposure Document.

The Addendum is not meant to be a stand alone document. It is meant to be used in conjunction with this Indirect Exposure Document. The Addendum identifies "Issues" and makes "Conclusions/Recommendations" regarding these

issues. The short-term goal of the Working Group was to provide guidance to risk assessors for conducting indirect exposures assessment using state-of-the-science tools. A longer term goal for the Office of Research and Development is to merge the Addendum with the Indirect Exposure Document to provide a single, updated indirect methodology document. A second long term goal is to develop a companion document which provides guidance on selecting values for parameters used in the methodology.

The Working Group recommended changes to most chapters of the 1990 Indirect Exposure Document. The most significant changes were made in the following three areas:

1) **Emissions, and air dispersion and deposition modeling:** A comprehensive list of potentially emitted contaminants from combustors was compiled and a procedure outlined to narrow the list for a specific facility. An updated version of the COMPDEP model for atmospheric transport, dispersion, and deposition, was described and recommended for use. Procedures to appropriately describe and model vapor phase and particle phase emissions were provided.

2) **Surface water impacts:** The three-tier option for calculating water concentrations in Chapter 9 of the Indirect Exposure Document was replaced with a single model which calculates water and sediment concentrations.

3) **Exposure:** This Addendum updates the procedures used to develop exposure scenarios on the basis of the policies described in the February 26, 1992, Deputy Administrator memorandum "Guidance on Risk Characterization for Risk Managers and Risk Assessors" and the 1992 Exposure Guidelines. Accordingly, an emphasis is placed on developing distributions of exposures and identifying high end and central exposure levels.

We are seeking comment from the SAB in the following five areas. In areas where weaknesses are found, please provide suggestions for improvement.

General Issues

i. The Working Group recognizes the awkwardness of needing to reference two documents to determine the procedures for conducting indirect exposure assessments, and has recommended that the 1990 IED be updated with this Addendum as a single methodology. In the interim, is the presentation of material in the Addendum clear? and can the SAB make specific recommendations for further clarity and completeness?

ii. The Working Group recognizes that implementation of the full methodology is very resource intensive and that screening procedures are needed to narrow the

scope and level of detail where appropriate. The current draft does provide some general guidance on narrowing the list of compounds of concern. However, in the current form, most decisions about how to establish screening procedures would need to be made within Agency Programs as part of their implementation guidance. Does the SAB believe this is appropriate or should further screening procedures be included here? If so can such procedures be recommended?

Air Emissions and Modeling Issues

Chapter 3 of the Addendum presents a framework for estimating emissions from stationary combustion sources, and for estimating the atmospheric concentration and surface deposition flux of the contaminants near the source using the COMPDEP air dispersion model. The Agency is not asking for a critical review of the algorithms inherent to COMPDEP. Instead the Agency would like the SAB to focus on application issues such as selection of the appropriate inputs to the model and proper interpretation and use of the outputs. Specific issues are listed below.

- i. Tables 3-1 to 3-3 present a listing of chemicals that have been measured in the stack gas emissions of a variety of combustion sources and that may be subject to the direct/indirect exposure/risk assessment. Is this sufficient in terms of an initial listing and a first step in the process?
- ii. A general procedure is presented that, if applied on a site-specific basis, could reduce the number and kinds of contaminants subject to review. Is the procedure adequate for this purpose, or should the Agency continue to develop a more detailed/refined screening procedure? If a detailed procedure is recommended, can you provide guidance or suggestions as to a screening method that would be appropriate?
- iii. A hierarchical approach for estimating emission rates of the contaminants is presented. Is this procedure adequate in terms of providing estimates of average emission estimates? A more difficult issue is the magnitude, frequency and duration of increased emissions that may occur during temporary upset conditions, during startup and shutdown procedures and during emergency events. The Agency presents default assumptions that address these occurrences based on information developed by the State of California Air Resources Board. Are these default assumptions adequate in terms of reflecting short-term and longer-term increases in emissions when mal-operations occur? Can the SAB recommend additional assumptions, alternative approaches, and/or databases that may be used?
- iv. A specific procedure (based primarily on adsorption theory) is presented to estimate the vapor phase/particle phase partitioning of semivolatile organic

compounds in the ambient air. This V/P ratio is then applied to the air modeling of the contaminant emissions. Specifically the V/P ratio expected in ambient air is assumed to apply at the stack. Is this method appropriate or can alternatives be recommended?

v. In order to evaluate the potential deposition flux of particles near the source, a particle-size distribution must be known. A default assumption of the particle size distribution of particulate matter in combustor emissions is presented in Table 3-7. Is this default distribution appropriate for use in those situations where site-specific information is not available? The emission rate of the particle-bound portion of the contaminant is apportioned to the particle size distribution based on the assumption that the contaminant will be adsorbed to the surface of the particle. Is the procedure for assigning the portion of emissions to the particulate array logical and technically defensible?

vi. The Addendum recommends running the COMPDEP model twice in order to isolate the ambient air concentration of the vapor-phase portion of the contaminant from the ambient air concentration and wet/dry deposition flux of the particle-bound portion of the contaminant. Is this approach appropriate and sufficient for purposes of providing concentration terms to be used in estimating inhalation exposures, for estimating media concentrations, and for estimating contamination of the human food chain?

Soil Impact and Food Chain Issues

i. Impacts to soils have been modeled as a function of contaminants depositing onto soils in the particle-phase; depositions have included wet plus dry deposition of particle bound contaminants. A common shortcoming identified is the lack of consideration of vapor phase impacts to soils. This Addendum recommends the inclusion of a vapor-phase diffusive flux term to soils, based on the gas phase mass transfer coefficient described in the IED and used there to estimate a volatilization loss rate from soils. Is this approach accurate, appropriate, and sufficiently described?

ii. Mixing soil depths of 20 cm for tillage operations (farming, e.g.) and 1 cm for non-tillage situations (pasture, lawns at residences) were recommended in the 1990 IED, and are also recommended here with additional verbiage on their use and interpretation. Are these depths appropriate, and is the SAB aware of other methodologies to determine more appropriate soil mixing depths?

iii. A differentiation between below and above ground vegetation is recommended in this document, and a procedure supplied for below ground vegetation is described. Is the differentiation appropriate, and is the procedure for underground

vegetation concentration appropriate?

iv. The 1990 IED assumed that wet deposition (i.e., deposition of particle bound contaminants via rainfall) would not impact above ground vegetations (vegetables/fruits, and animal feeds). The reference to this assumption was an internal memorandum which did not include a technical justification. The Working Group recognized that for some compounds evidence exists that wet deposition is roughly equal in magnitude to dry deposition, so that zeroing out wet deposition roughly halves the impact of depositions to plants. Subsequent to the completion of the Addendum document, the Working Group was made aware of a key publication by Hoffman, et al. (1992, Quantification of the interception and initial retention of radioactive contaminants deposited on pasture grass by simulated rainfall. Atmospheric Environment 26(A), number 18: 3313-3321). Data from this article suggests that the retention of particles on the vegetations tested ranged between 24 and 37% of all depositing, depending on size of particle and vegetations. Without this information, the Working Group recommended the following assumptions for percent of contaminant in wet deposition that is retained on the plant surface: 1) 100% for contaminants with high sorptive tendencies and 2) 10% for contaminants with low sorptive tendencies. The Hoffman article suggests that an assumption of 30% retention of particle-bound contaminants, regardless of sorptive tendencies of the contaminant, is a more supportable assumption. Which of the two approaches, or other approaches, would the SAB recommend? And is the SAB aware of other information that would assist in developing values for the retention of particle bound contaminants on plants during rainfalls?

v. After contaminants from stack emissions deposit on to soils, they can be resuspended as particles or volatilize into the air. Model testing has shown that resulting air concentrations associated with these soil emissions are significantly smaller in magnitude compared to concentrations resulting from the stack emissions. Accordingly, the Addendum does not recommend increasing modeled air concentrations from stack emissions to reflect additional emissions from soil. Does the SAB concur with this recommendation or can alternative approaches be recommended?

vi. Recent literature has emphasized the importance of vapor-phase transfers to vegetation, particularly for dioxin-like compounds. This Addendum recommends substantial changes for estimating vegetative impacts by vapors. These changes include:

1. Neglect soil volatilization as an additional reservoir for vapor transfers. The argument was presented that the soil-plant empirical transfer factor already includes all routes of soil to plant transfers.

2. Do not include the vapor fraction term, since the air modeling will directly yield a vapor-phase concentration.

3. In external model testing, the Bacci azalea leaf model was shown to overestimate the transfer of vapors to plants. An empirical correction reducing the value of the Bacci transfer factor by 90% was recommended based on external model validation exercises with dioxin.

Are these changes technically accurate, complete, and sufficiently described?

Water Impact Modeling Issues

- i. A three-tier approach to evaluating aquatic impacts, from screening level, Tier 1, to site-specific and more complex modeling, Tier 3, was promoted in the Indirect Exposure Document. This has been replaced with a steady state model for long term average risks and a storm event model for short term acute risks. Is the replacement of the three-tier system with these two models appropriate?
- ii. The previous IED long-term average water model was quite simplistic, accounting for loading and dilution only. The steady-state water model proposed in the addendum accounts for several water body loss processes, including burial, volatilization, chemical/biochemical degradation in water column and benthic sediment, and sediment burial. It also allows for the formation of a reaction product, thereby maintaining reservoirs of more than one contaminant in the aquatic ecosystem. These processes, of course, require more input data. Does the addition of these processes make the simple screening level approach more appropriate for assessing health risks? And can the SAB provide comments on specific algorithms?
- iii. The proposed watershed loading model calculates average soil concentrations in a manner similar to the old IED. One additional process modeled is atmospheric diffusion loading to soil. Long term average erosion of chemical from the watershed is multiplied by a sediment delivery ratio and a pollutant enrichment factor before loading to the waterbody. Are the watershed calculations appropriate for calculating long-term average loadings via erosion to the water body?
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sediment and chemical concentrations. Is the proposed storm event model appropriate for calculating peak concentrations for assessment of acute or threshold health risks?

Exposure Issues

- i. The Addendum recommends a procedure to define the extent of the study area on the basis of an isopleth corresponding to a de minimus risk level. Is this appropriate, if not could alternatives be suggested?
- ii. The Addendum recommends a procedure to develop distributions of exposure on the basis of either a series of point estimates of exposure or a Monte Carlo assessment. Both would involve identifying isopleths defining areas of equal media contamination levels and characterizing behavior patterns in these areas. Are these procedures clear and reasonable?
- iii. The Addendum provides some general guidance for conducting a Monte Carlo assessments in the areas of separating uncertainty and variability, addressing dependence among variables and selecting distributions. Is this guidance appropriate and sufficiently detailed?
- iv. The Addendum describes two procedures for estimating breast milk concentrations on the basis of maternal intake. Are these the best models available for this purpose? Are the assumptions sufficiently well stated?
- v. The Addendum describes a procedure for estimating population risk for cancer on the basis of local food production. Is this approach clear and reasonable?
- vi. The 1990 IED document used the 1978 USDA food consumption survey to estimate diet fractions for locally grown foods. Some of these estimates do not appear reasonable. The Working Group strongly recommends trying to base these estimates on local surveys, but could not resolve how to make these estimates if this was not feasible. Can the SAB provide any further guidance on how to address this issue?
- vii. Indirect pathways are defined as all pathways other than inhalation. This document addresses food chain, water, soil, and breast milk exposures. Should any other pathways be considered?

APPENDIX B. Interim Letter to Administrator (EPA-SAB-IAQC-94-009a)



UNITED STATES ENVIRONMENTAL PROTECTION AGENCY
WASHINGTON, D.C. 20460

OFFICE OF THE ADMINISTRATOR
SCIENCE ADVISORY BOARD

EPA-SAB-LAQC-94-009a

February 15, 1994

Honorable Carol M. Browner
Administrator
U.S. Environmental Protection Agency
401 M Street, SW
Washington, DC 20460

RE: Draft "Addendum to the Methodology for Assessing Health Risks Associated with Indirect Exposure to Combustor Emissions"

Dear Mrs. Browner:

On December 3, 1993, the Indoor Air Quality/Total Human Exposure Committee (the Committee) of the Science Advisory Board reviewed the draft document "Addendum to the Methodology for Assessing Health Risks Associated with Indirect Exposure to Combustor Emissions" (the Addendum). In view of pressing EPA and public concerns about incinerators, this interim letter was prepared to provide you with preliminary information on some of the major findings of the Committee. A more detailed report is in preparation and will follow soon (EPA-SAB-LAQC-94-009).

The assessment of risks from combustors entails a complex range of issues, including many different kinds of combustion devices and raw materials, direct and indirect exposure routes, and concerns regarding transportation and disposal of raw materials and combustion ash. It is thus important to emphasize, at the outset, that this letter and the report in preparation address only one of these aspects, namely the questions surrounding indirect exposure assessment which are the subject of the Addendum. Indirect exposures are those that occur via transfer of airborne contaminants into water, soil and the food chain. Direct airborne exposures from combustor emissions are being addressed with other methodologies by the Agency and are not the subject of this letter.

To grapple with these complex exposure pathway issues, the Agency needs to be able to estimate the environmental fate of combustor emissions and their consequent potential for human exposures. This task requires the development of models to predict accumulations of chemical contaminants in the environment and identification of the chemicals, environmental compartments, and exposure pathways most likely to be of concern so that appropriate actions



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can be taken before there is widespread and/or irreversible damage. The Addendum we reviewed is a critical part of the Agency's effort to deal with these very difficult challenges.

The Committee's, principal conclusion is that the Addendum is not ready for release as an "EPA Methodology." The major scientific concerns were as follows:

1) There is a general lack of measured data to estimate input parameters and very little validation of the exposure models. The general reliance on default input data and the large number of assumptions left the Committee with many reservations about widespread application of this document as an EPA methodology for all types of combustors and chemicals. The Committee recommends that the Agency develop and implement a strategic plan to collect critical input data for the models and to validate the methodology.

2) The Committee is concerned about the lack of information on the frequency and impact of upset conditions (e.g., upsets in the combustion process that result in less complete combustion, as well as breakdown of filters, precipitators, or other controls on stack emissions) on both the chemical composition and the character of emissions from incinerators. The model relies on measured data from four California incinerators. These limited data suggest that upset conditions can contribute significantly to the total emissions from an incinerator but the frequency of upset conditions has not been determined for sufficient numbers and types of incinerators to be reasonably confident of the adequacy of the default values recommended for use.

3) The Committee is concerned that the Addendum does not require the risk assessor to account for the impact of multiple combustor sources. While a single new facility may not result in a significant risk, the cumulative effect of the addition of a facility to an area with a number of existing combustors may well cause an unacceptable health risk. There is a need for a more regional approach to evaluating risks from indirect exposures. There were also concerns that the impact beyond 50 km (the maximum distance considered in the Addendum) may be of concern for areas with large numbers of combustors.

4) Although the Addendum nominally addresses all combustors (incinerator, fossil fuel, etc.), the document as now written appears to place more emphasis on incinerators and does not adequately address all combustors. It is known that the chemical nature of the emissions and the frequency of upset conditions will differ substantially among various types of combustors but the document does not reflect this body of information. For example, there is a substantial body of information on emissions from coal combustors that should be referenced if the document is to be all inclusive.

5) The Committee noted that EPA's re-permitting process for incinerators offers a unique opportunity to obtain existing data on the frequency and duration of upset conditions for various types of incinerators in the U.S. Other useful data may be available that could be required as part of the re-permitting process, such as emissions measurements.

Many of the above issues have been noted by previous SAB committees in their review of hazardous waste and domestic waste incineration. In addition, various incineration studies and emission data sets exist in other countries. It is highly recommended that the Agency compile and review these previous efforts as a way of focusing its future directions.

In summary, the Committee is very aware of the difficulties inherent in the "state of the science" nature of the work which the Addendum effort entails, especially when the work must be done under the combined pressures of severely limited resources and public demands for "something" to be done quickly. The Committee, however, does not recommend the release of the Addendum as an "EPA Methodology" due to the substantial scientific uncertainties in the models and the absence of information in the Addendum concerning those uncertainties and limitations.

Sincerely,

Genevieve M. Matanoski
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Executive Committee
Science Advisory Board

Joan M. Daisey
Dr. Joan M. Daisey, Chair
Indoor Air Quality/Total Human Exposure
Committee
Science Advisory Board

APPENDIX C Miscellaneous Comments and Specific Corrections and
Suggestions for Clarification of Text

- Page 1-2: last sentence should read "...to determine air concentrations..." instead of "to determine an air concentrations..."
- Page 1-3: third line should read "If one were..." rather than "It one were..."
- Page 2-1: rephrasing the statement "...potential number of health effects cases..." can be more clearly stated as "...estimated number of excess cancer cases..." since it refers to carcinogens.
- Page 2-1: Emphasis is placed on defining the boundaries for the exposed population. Are food distribution patterns such that regional or even national level exposures should be anticipated? As the number of incinerators increases, how would the general population R's level of exposure be affected?
- Page 2-5: the term "non-negligible" should be defined (e.g., exposure less than the RfD).
- Page 2-7: typo on first line of text (population density).
- Page 2-8: It is unclear whether individual or group risk is of interest. Although addressed subsequently, some clarification would be appropriate at this point.
- Page 2-8: A statistically-based survey is recommended. This recommendation, while well intended, may not be practicable. How much data would be needed to identify deviations from larger survey databases. Would it be feasible to characterize all segments of the population including high-risk groups?
- Page 2-9: The "other residents" should be identified. Listing as group I = 1 to N would be preferable.
- Page 2-10: 6th line from the top "individual special interest groups" probably mean "subgroups of special interest" in this context. Also, last sentence on

first paragraph should probably read "Judgment will be needed to decide..."

Page 2-11: 6th line from the top "characterized"; at 14th line from the top "...may change in the future..." and "...may be performed..." instead of "...may be desired..."

Page 3-2: Add a statement to first paragraph under 3.2 "focus on chemicals that are potentially toxic to humans. are relatively long-lived in the atmosphere, and therefore have a definite propensity for bioaccumulating."

Page 3-10, top of page: log K_{ow} can indicate partitioning soils and sediments. Doesn't this also indicate lower bioavailability?

Pages 3-10 & 3-11 - Wouldn't it be better to use as average emission factors, factors weighted according to operating conditions with appropriate weighing for higher emissions during upset and start-up conditions rather than simply some arbitrary arithmetic average?

Also, the next to last sentence on the page "For purposes of exposure assessment, ranges and average values should be developed" also applies to section A.

Operational Facilities. This deserves greater emphasis and should be stated earlier in the discussion rather than at the end.

Page 3-10: on last paragraph, third line "...stack sampling, analytical, and quality control, quality assurance protocols...." should be "...stack sampling, analytical, and quality control/quality assurance protocols and procedures..."

Page 3-10, 11: The wording at the bottom of p. 3-10 is somewhat confusing. When read carefully, it says that if you use concentrations corrected to 12% CO₂ or 7% O₂ in the calculation of mass emission rate, then you should use the corresponding volumetric flow rates corrected in the same way. The words "this calculation" on p. 3-11 could refer to any number of possible combinations of concentration and flow rate.

Page 3-11: 6th line of first paragraph "In cases where..."

- Page 3-13, paragraph 2: Combustion emissions from hazardous waste incinerators are likely to be only part of the concern for local communities. Risk assessments for each facilities should examine probabilities of spills and leakage of hazardous waste as well as accidents during transport.
- Page 3-13: Increased emissions during start-ups and shut-downs, malfunctions and perturbations. Some real-world data are critically needed here. How often do these things occur and how much do they contribute to total emissions? The recommended default values suggest that they re very important (p. 3-15). For nearby populations, increased emissions of irritant chemicals are likely to be of much more concern than dioxin if levels frequently reach thresholds. Also, levels should be kept well below thresholds because such threshold are generally based on data from healthy worker populations and the general population can be expected to include significant percentages of people who are much more sensitive, e.g., by one to two orders of magnitude.
- Page 3-13: Option "D" is not clear. What emission test reports are these? If they are not done via EPA protocol, then how should they be interpreted? Are you suggesting that a mass balance of the system is an acceptable option for estimating emissions. If so, the Committee does not agree but at a minimum, it should be made clear.
- Page 3-14: - Similarly, some information on the removal efficiency of control devices overtime, as they age, is needed, as well as information on how frequently they fail and what occurs when they fail.
- Page 3-16, Section 3.5: Short-Term Impacts Due to Meteorologic Conditions. It is stated that short-term increases in emissions and atmospheric inversion conditions are unlikely to significantly affect indirect exposure, but may be important to consider in evaluation of acute effects from inhalation. Two points here: (1) Is this treated in the guidance for direct exposures? (2) Are chemicals with irritant and other short-term acute effects the focus for these situations?
- Page 3-17, first paragraph of 3.6.1: The text refers to lateral and vertical "concentrations"; I believe "concentration distributions" would fit better.
- Page 3-18: Suggest C rather than X be used for concentration in the equations.

- Page 3-21, first paragraph of 3.6.7: The text says that building wake effects are important if the plume height and the stack height are "greater than" certain values. It should say "less than".
- Page 3-21: In principle, the restrictions on algorithms from building wake effects under complex terrain conditions apply only to those complex terrain receptors located within the building cavity, not to the downwind receptors. In this regard, the Shulman and Scire algorithm is more compatible with complex terrain modeling.
- Page 3-25: The algorithms described in Equations 3-5 to 3-10 do not depend upon the surrounding terrain. The algorithms describe phenomena on a relatively small scale near the surface. Perhaps the turbulence parameters differ in flat vs. complex terrain, but the implication of the statement is that the algorithms would depend upon the terrain.
- Page 3-25, Bottom of page: Definition of Dyw units should be $g/m^2 \cdot s$ not $g/m^2 / s$ which is $g \cdot s/m^2$.
- Page 3-27, third paragraph of 3.7.2: The text refers to "Julienne" day; it should be "Julian". Also in Table 3-5, on p. 3-28.
- Page 3-30: Need to state clearly both here and at the beginning of this major section that vapor phase deposition is not being modeled here. To the extent that it is possible, the error produced by this assumption needs to be discussed and put in context.
- Page 3-30, continuation paragraph at the top of the page: The text recommends model receptor spacing out to 5 km, and from 10 km to 50 km, but doesn't offer guidance between 5 and 10 km. Consider 500m resolution between 1 km and 5 km, and 1000m resolution from 5 km to 10 km. The jump from 200m to 1000m is too severe.
- Page 3-31, continuation sentence at the top of the page: The text talks about airborne vapor absorption into plants, and particle deposition on to both plants and soils. It is not clear why vapor uptake by soils is omitted, or for that matter, why uptake of both gases and particles by water surfaces is not mentioned.

- Page 3-35, last paragraph: The text recommends using the "background plus local source" value for the term S_T for urban incinerators, instead of the "urban" value. Why?
- Page 3-36, first complete paragraph, line 6: The text includes the word "emission" after "partition"; it should be deleted.
- Page 3-36: The conclusion that volatile components are solely in the vapor phase should not be based solely on Henry's constant. An additional factor is the adsorptive properties of the particles.
- Page 3-37, 3-38, particle size distributions and table 3-7: How are these measured? If there are not measured with a dilution stack sampler, the mass in particles will be underestimated and the size distributions will not be correct since condensation and coagulation will occur as the plume cools and becomes detected.
- Page 3-38, table 3-7, column 5: Shouldn't this be "Available surface areas," with units given (μm)?
- Page 3-39, first complete paragraph, lines 7 and 8: The text referring to Column 4 says "... the total mass of particles is 15 μm ". I suggest a rephrasing: "... the total mass of particles is of this size", which meshes with the earlier sentences.
- Page 3-39, first complete paragraph, line 20, first word: "Contaminate" should be "contaminant".
- Page 3-40: What is considered an "adequate general vicinity" when using off-site meteorological data? Should some guidelines be provided to the assessor?
- Page 4-1: For homes located near incinerators, some indoor dust will be incinerator particles that penetrate building shells via infiltration and open windows and settle out as dust. The concentrations of pollutants may be greater in these dusts than those tracked in from outdoors because outdoors dusts will be diluted by soils. It is not appropriate to assume that deposited dusts in nearby homes have the same concentrations of contaminants as the top 1 cm of soil.

Page 4-2, Conclusions/Recommendations: The first sentence is not clear, specifically the term "Volatilization." Isn't "Loss of volatiles due to deposition" meant here?

Page 7-1: Dermal exposure from contaminated soil. Is this ever significant? In the IED document, p. 7-11, even if $AF=1$, DDI, is only 1 pg/kg for BaP in soil at a concentration of $\sim 1 \times 10^{-9}$ mg/g (1×10^{-6} μ g/g) = 1 pg/g soil. At what kinds of concentration would this be important, compared to ingestion or normal background? The calculation suggests that this pathway would only be important if soil levels reach the order of micrograms of BaP per gram of soil. Are soil levels likely to reach this?

Also, is there any evidence that trace metals can be absorbed via the skin?

Page 9-1: The input requirements for the calculations are very substantial and the calculations extensive: Some guidance is needed regarding situations in which these calculations are needed. Is there good experimental evidence that supports the accuracy of that model?

Page 11-2: What kinds of emission rates are needed to get sufficiently high concentrations of something like dioxin into water and then into fish and finally into people so that levels are sufficiently high to matter?

Page 15-2: On the inhalation dose, this assumes that people are outdoors 24 hours a day. In actuality, they spend most of their time indoors. Buildings provide varying degrees of protection. For homes near incinerators, this might make a difference of an order of magnitude in estimated exposures for some chemicals. Why isn't this taken into consideration?

Page 15-8: The discussion on mass balance is welcome. It should be included in the discussions elsewhere in the Addendum.

The citation to Junge is not in the reference list.

Page 5-13: Why assume that free ranging chickens (or chickens raised in an open coop) have a lower soil intake in their diets than pigs? One might assume instead that chickens consume even more soil since they peck directly at the ground and need to ingest soil particles and very small pebbles to aid in the breakdown of kernels in their stomach. Chickens are also raised for their

eggs. Is there transfer to and accumulation of contaminants in eggs? Ignoring a pathway component because there is no data available cannot be easily justified. If the data are not available it should be obtained.

Page 15-15: Characterization of uncertainty. Some of the uncertainties are qualitative in nature, i.e., based on assumption or limited information about the population. These are often more important than those that are evaluated with modeling. This should be discussed.

Chapter 2, Human Exposure Scenarios: Regarding Study Area. Based upon Exposure Potential, not concentration Potential. Therefore, impacts >50 km could be important for some IE situations.

Certain sections of the Addendum are not reviewable as they stand. They should either be deleted or repaired. For instance, the sections on comparing model results for PCDD/PCDF with rural "background" levels in various environmental media (pages 5-10, 15-11,12) contain neither sufficient references nor information to make any reasonable judgement as to their validity. These comparisons begin with a "profile of air concentrations crafted to be typical of rural environments" (p. 5-10). Concentrations of PCDD and PCDF were estimated from these data using the Addendum's models. These results were compared with "background" levels measured in beef. Given the persistence of PCDD and PCDF in the environment, this is not a completely unreasonable idea. However, such comparisons are *not* reasonable unless we have some notion of variability. Where are the error bars on these averages? Furthermore, a major problem with this approach is ensuring that the data are comparable (see Webster and Connett, 1990). This question cannot be evaluated with the supplied data and lack of references (For various reasons, most ambient air samples are taken in urban areas. Comparisons of such values with concentrations in beef grown on feed from rural areas may bias the results. This is why study of a specific setting is probably better, even though field data can be difficult to use under the best of circumstances).

APPENDIX D Earlier SAB Reports Concerning Hazardous and Domestic
Waste Incineration.

U.S. EPA/SAB, Report on the Incineration of Liquid Hazardous Wastes, by the Environmental Effects, Transport and Fate Committee of the Science Advisory Board, April 5, 1985.

U.S. EPA/SAB, Review of Technical Documents Supporting Proposed Revisions to EPA Regulations for the Disposal/Reuse of Sewage Sludge Under Section 405(d) of the Clean Water Act, Report of the Environmental Engineering Committee, SAB-EEC-87-015, January 1987.

U.S. EPA/SAB, EPA'S Risk Assessment Methodology for Municipal Incinerator Emissions: Key Findings and Conclusions, Report of the Municipal Waste Combustion Subcommittee of the Environmental Effects, Transport and Fate Committee of the Science Advisory Board, SAB-EETFC-87-027, April 9, 1987.

U.S. EPA/SAB, Review of the Municipal Waste Combustion Research Plan, Report of the Municipal Waste Combustion Subcommittee of the Science Advisory Board's Environmental Effects, Transport and Fate Committee, SAB-EET&FC-88-023, Final Report, April 11, 1988.

U.S. EPA/SAB, Evaluation of Scientific Issues Related to Municipal Waste Combustion, Report of the Environmental Effects, Transport and Fate Committee of the Science Advisory Board, SAB-EETFC-88-25, Final Report, April 26, 1988.

U.S. EPA/SAB, Review of Proposed Sewage Sludge Incineration Rules (40 CFR Parts 257 and 503), Report of the Municipal Sludge Incineration Subcommittee of the Environmental Engineering Committee, EPA-SAB-EEC-89-035, September 20, 1989.

U.S. EPA/SAB, Review of OSW Proposed Controls for Hazardous Waste Incineration Products of Incomplete Combustion, Report of the Products of Incomplete Combustion Subcommittee of the Science Advisory Board, EPA-SAB-EC-90-004, January 1990.

