

ornl

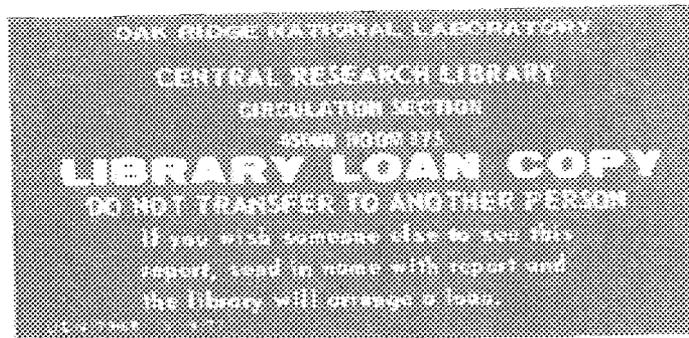
ORNL/TM-10879

**OAK RIDGE
NATIONAL
LABORATORY**

MARTIN MARIETTA

Comments by a Peer Review Panel on the Computerized Radiological Risk Investigation System (CRRIS)

David A. Baker



OPERATED BY
MARTIN MARIETTA ENERGY SYSTEMS, INC.
FOR THE UNITED STATES
DEPARTMENT OF ENERGY

Printed in the United States of America. Available from
National Technical Information Service
U.S. Department of Commerce
5285 Port Royal Road, Springfield, Virginia 22161
NTIS price codes--Printed Copy: A06; Microfiche A01

This report was prepared as an account of work sponsored by an agency of the United States Government. Neither the United States Government nor any agency thereof, nor any of their employees, makes any warranty, express or implied, or assumes any legal liability or responsibility for the accuracy, completeness, or usefulness of any information, apparatus, product, or process disclosed, or represents that its use would not infringe privately owned rights. Reference herein to any specific commercial product, process, or service by trade name, trademark, manufacturer, or otherwise, does not necessarily constitute or imply its endorsement, recommendation, or favoring by the United States Government or any agency thereof. The views and opinions of authors expressed herein do not necessarily state or reflect those of the United States Government or any agency thereof.

COMMENTS BY A PEER REVIEW PANEL
ON THE COMPUTERIZED RADIOLOGICAL
RISK INVESTIGATION SYSTEM (CRRIS)

David A. Baker, Ed.*

*Pacific Northwest Laboratories

August, 1988

Prepared for the
the U.S. Environmental Protection
Agency under a Related Services
Agreement with the U.S. Department
of Energy Contract DE-AC06-76RLO 1830

Prepared by the
Oak Ridge National Laboratory
Oak Ridge, Tennessee 37831
operated by
MARTIN MARIETTA ENERGY SYSTEMS, INC.
for the
U.S. DEPARTMENT OF ENERGY
under Contract No. DE-AC05-84OR21400

MARTIN MARIETTA ENERGY SYSTEMS LIBRARIES



3 4456 0283882 9

DISCLAIMER

Although the research described in this report has been funded by the United States Environmental Protection Agency through Interagency Agreement Number AD-89-F-2-A106 (formerly EPA-78-D-X0394) with Oak Ridge National Laboratory, it has not been subjected to the Agency's required peer and policy review and therefore does not necessarily reflect the views of the Agency and no official endorsement should be inferred.

CONTENTS

DISCLAIMER	ii
ACKNOWLEDGMENTS	iv
ABSTRACT	1
INTRODUCTION	2
TOPICS	4
GENERAL COMMENTS	4
UNCERTAINTY ANALYSIS	11
HEALTH EFFECTS	18
MODEL AND COMPUTER PROGRAM DOCUMENT COMMENTS.....	19
AN INTRODUCTION TO CRRIS - ORNL/TM-8573	19
ANDROS - ORNL-5889	27
ANEMOS - ORNL-5913	34
MLSOIL - DFSOIL - ORNL-5974	54
PRIMUS - ORNL-5912	56
RETADD-II - ORNL/CSD-99	59
SUMIT - ORNL-5914	64
TERRA - ORNL-5785	65
PARAMETERS AND DATA BASES	73
AGRICULTURAL PRODUCTION - ORNL-5768	73
A REVIEW AND ANALYSIS OF PARAMETERS (SITE DATA BASE) - ORNL-5786	74
REFERENCES	82
APPENDIX A BRIGGS LETTER	85
APPENDIX B RESPONSES TO COMMENTS BY PEER REVIEW PANEL	94

ACKNOWLEDGMENTS

This is to acknowledge the persons who took some of their valuable time to render assistance in this review. These persons and their affiliations are listed below.

Lynn R. Anspaugh	Lawrence Livermore National Laboratory
Edward F. Branagan, Jr.	Nuclear Regulatory Commission
Gary A. Briggs	National Oceanic and Atmospheric Administration/EPA
Keith F. Eckerman	Oak Ridge National Laboratory
Samuel L. Finklea, III	Conference of Radiation Control Program Directors, Inc.
Jerome L. Heffter	National Oceanic and Atmospheric Administration
Bruce B. Hicks	National Oceanic and Atmospheric Administration
Rayford P. Hosker, Jr.	National Oceanic and Atmospheric Administration
Bernd Kahn	National Council on Radiation Protection and Measurements
Thomas B. Kirchner	Colorado State University
Walter L. Marter R. W. Kurzeja	Savannah River Laboratory
Charles W. Miller	Illinois Department of Nuclear Safety
Samuel C. Morris L. Dupuis V. Fthenakis C. Miles J. Nagy M. Rowe H. Thode	Brookhaven National Laboratory
Yook C. Ng	Lawrence Livermore National Laboratory
John M. Palms	Emory University
Harold T. Peterson, Jr.	Nuclear Regulatory Commission

Rodican P. Reed

Tennessee Valley Authority

Andrea Sjoreen

Oak Ridge National Laboratory

Dennis L. Streng

Pacific Northwest Laboratory

D. Bruce Turner

National Oceanic and Atmospheric
Administration/EPA

James R. Watts

Savannah River Plant

Carl G. Welty, Jr.

Department of Energy

Chris G. Whipple

Electric Power Research Institute

William H. Wilkie

Carolina Power and Light Company

The patience, skill, and hard work of Ms. Ronda R. Weston that went into the production of this report are acknowledged.

ABSTRACT

This document represents the comprehensive review by experts of the documents describing the models, computer programs, and data bases making up the Computerized Radiological Risk Investigation System (CRRIS). The CRRIS methodology has been produced for the U.S. Environmental Protection Agency's (EPA) Office of Radiation Programs (ORP) by the Health and Safety Research Division of Oak Ridge National Laboratory (ORNL) to assess the significance of releases of radioactive material from facilities handling such materials.

The comments covered a wide range of aspects of the CRRIS models. Special review topics covered were uncertainty, validation, verification, and health effects. The reports making up the CRRIS documentation were reviewed in detail. The following are some of the more frequent comments about the methodology.

This is a very comprehensive work, but too complex and hard to use.

Too little explanation of some of the assumptions taken such as variance from standard ICRP organ weighting factors.

Overly complex model for soil to root transfer and interception fraction.

Gaussian plume model was used, when more state-of-art models are available.

Extensive comments were made on the dispersion models used in ANEMOS and RETADD-II, such as the treatment of reflections, building wake, resuspension, and deposition.

This review provides guidance for EPA in the continuing development of a comprehensive and consistent methodology for assessing airborne emissions of radionuclides.

*Research sponsored by the Office of Radiation Programs, U.S. Environmental Protection Agency under Interagency Agreement AD-89-F-2-A106 (formerly EPA-78-D-X0394).

INTRODUCTION

This document represents the comprehensive review by experts of the documents describing the models, computer programs, and data bases making up the Computerized Radiological Risk Investigation System (CRRIS). The CRRIS methodology has been produced for the U.S. Environmental Protection Agency's (EPA) Office of Radiation Programs (ORP) by the Health and Safety Research Division of Oak Ridge National Laboratory (ORNL). The CRRIS is a system of eight computer codes and associated data bases designed to assist EPA in determining the health significance of airborne radioactive emissions, pursuant to its regulatory authority under the Clean Air Act (as amended August 1977).

The following questions were to be addressed by the review committee.

1. Are the mathematical models employed in the CRRIS appropriate for meeting the stated objectives of each computer code?
2. Are the mathematical models used in the CRRIS representative of the state-of-the-art in environmental radionuclide assessment modeling?
3. Are the data bases that are an integral part of the CRRIS appropriate for the purposes stated?
4. Are the reports that document the CRRIS clear and understandable so that someone outside EPA or ORNL could successfully execute the computer programs involved?
5. Are there any significant environmental pathways of exposure for radionuclides that have not been considered in the CRRIS?
6. Have appropriate numerical and computing techniques been employed in transforming the mathematical models into working computer codes?
7. How can EPA best address the uncertainty inherent in any calculations made with the CRRIS or any similar system of assessment models?
8. What changes would be required to make the CRRIS a useful assessment tool for your organization?

The following documents were reviewed by the members of the panel.

1. An Introduction to CRRIS: A Computerized Radiological Risk Investigation System for Assessing Atmospheric Releases of Radionuclides, ORNL/TM-8573.

2. ANDROS: A Code for Assessment of Nuclide Doses and Risks with Option Selection, ORNL-5889
3. ANEMOS: A Computer Code to Estimate Air Concentrations and Ground Deposition Rates for Atmospheric Nuclides Emitted from Multiple Operating Sources, ORNL-5913.
4. MLSOIL and DFSOIL - Computer Codes to Estimate Effective Ground Surface Concentrations for Dose Computations, ORNL-5974 (with revised appendices)
5. PRIMUS: A Computer Code for the Preparation of Radionuclide Ingrowth Matrices from User-Specified Sources, ORNL-5912.
6. RETADD-II: A Long Range Atmospheric Trajectory Model with Consistent Treatment of Deposition Loss and Species Growth and Decay, ORNL/CSD-99,
7. SUMIT: A Computer Code to Interpolate and Sum Single Release Atmospheric Model Results Onto a Master Grid, ORNL-5914.
8. TERRA: A Computer Code for Calculation of the Transport of Environmentally Released Radionuclides Through Agriculture, ORNL-5785.
9. Agricultural Production in the United States by County: A Compilation of Information from the 1974 Census of Agriculture for Use in Terrestrial Food Chain Transport and Assessment Models, ORNL-5768.
10. A Review and Analysis of Parameters for Assessing Transport of Environmentally Released Radionuclides Through Agriculture, ORNL-5786.

This report is a compilation of comments submitted by members of the review panel on topics and documents pertinent to the CRRIS program. Topics addressed by reviewers include general comments on CRRIS, uncertainty, and the treatment of health effects.

Following the above general topics, are the comments on the specific model and computer program documents. Comments received were both of a technical and editorial nature. Only technical comments have been reproduced in this report; editorial comments (typos, misspellings, etc.) have been submitted to ORNL separately. Technical comments are grouped as either "general" or "page-specific". They encompass such problems as inaccurate or ambiguous language, thought, and other content-related matters.

Note that individual sections contain minimal explanation or overview. Comments are taken verbatim from the reviewers' letters and are allowed to "stand alone". Further, no attempt was made to resolve differences of opinions between the reviewers. For extended commentary, the reviews of separate authors are divided by solid lines.

TOPICS

GENERAL COMMENTS

This section reports general review comments on topics related to the CRRIS program. The entire CRRIS program was a massive undertaking. It certainly is a comprehensive and detailed program of almost every aspect of environmental dose assessment methodology; that is the problem. It is too comprehensive, too elaborate and just too mammoth to deal with in a practical way. The program needs a narrative which will assist the potential user in choosing aspects of the formulations which will most effectively meet the users needs. For example, the program as it stands could not be effectively used for quick analysis or screening. As a minimum, extensive user instructions, especially as they relate to site specific data, are an essential need.

As one of my colleagues commented, "Although the authors are to be commended for their efforts to produce a flexible tool, the result is a nightmare of complexity that will surely limit its utilization and complicate its interpretation."

The vehicle for bringing all aspects of environmental dose assessment together in "one place" seems to have been the formulation of computer codes. The writing, debugging and checking of these codes no doubt consumed most of the time. It seems we have covered our fundamental ignorance of environmental transport and dose assessment with still another layer of computer sophistication. Incidentally, the computer hardware requirements are cost prohibitive for the average potential user in the commercial world. ANEMOS requires 900K to run on an IBM 3033 or 3701.

The CRRIS is designed to be used as an assessment tool for radiation standard setting purposes and we believe it is basically adequate as such, subject to the comments provided below. It is not, however, a practical engineering tool for the nuclear industry for determining routine compliance with already established regulations, such as 40 CFR Part 190 or Appendix I to 10 CFR Part 50.

Also, the general consensus of my co-workers is that the assumptions and results of the dispersion modeling portions of the CRRIS tend to be somewhat crude compared to the state-of-the-art models. Details are provided in specific comments given below.

The CRRIS codes tend to be cumbersome to run for routine assessment applications and require highly trained personnel with a detailed knowledge of the codes to successfully execute them. Will it be possible to maintain the necessary expertise within user organizations to run the CRRIS? We recommend that where feasible, CRRIS be made more "user friendly," particularly with regard to input parameters. For example, it is unrealistic to assume, in general, that one knows particle sizes and clearance classes for radioactive materials in atmospheric effluents (for routine nuclear plant effluents, even physical and chemical forms are not generally known). In practice, such

information would be difficult and costly to obtain and its collection probably could not be justified. Whereas CRRIS does provide for default parameters, every effort should be made to simplify input requirements. We believe the modularity of the CRRIS is a positive feature that can result in enhanced usefulness in certain applications.

With regard to the whole package, the Oak Ridge group, which put CRRIS together, did a good job of explaining what each section of the CRRIS models covered, and how the models work together. In response to your question 5 (Are there any significant environmental pathways of exposure that have not been considered in CRRIS?), I note that surface water pathways do not appear to be treated, either for drinking water or irrigation. It may be that exposures from surface waters are often small for air releases, but if this was addressed, I did not see it.

My major criticism of the package taken as a whole is the relative lack of detail about the operational limits of CRRIS with regard to the time frame of effects it is capable of computing. It would be particularly useful to have a single section or report that summarizes these limits for each component of CRRIS, and hence for CRRIS as a whole. Because it would probably be difficult to generalize about these limits for all conceivable cases that might be run on CRRIS, it would be particularly useful if, in addition to practical advice on limiting times and distances, this section indicated the theoretical or analytical limits that are constraining. This could permit users to judge the appropriateness of various applications.

With regard to calculations for which CRRIS is suited, the reports make it clear that the models are designed for long-term, rather than short-term, effects. However, few examples are given of the types of problems appropriate and inappropriate for each model. While it is clear that CRRIS is not meant to provide assessment of the short-term local effects of an accidental release, it is less clear for any specific case what practical limit this imposes. Specific questions deserving attention include: Is CRRIS limited to steady state releases, or can it handle one time or time-varying releases? The time frame limitations imposed by the valid ranges of the models are unlikely to be uniform, (e.g., exposures from agriculture are clearly long-term). Are the air transport models RETADD-II and ANEMOS equally limited in their ability to calculate short-term effects? Are there limits on calculations of short-to-intermediate effects from PRIMUS, SUMIT, or, for air exposures, ANDROS? Does CRRIS overlap in its calculation capabilities with the family of CRAC codes? If so, have the two been compared? [This should be done if possible, and the reason that this is or is not possible should be spelled out in the reports.] If CRRIS and CRAC do not overlap, is there a gap in available models for radiation calculations? Are there meaningful problems in this gap, if it exists?

As the introductory materials point out, the individual modules of CRRIS perform calculations that are often done in isolation; the significant contribution of this effort is the integration of the various modules to provide a consistent and universally applicable

calculation. Therefore it seems that CRRIS will be useful to state radiation control programs. However, many agencies that might wish to use CRRIS capabilities do not have the computer support to adapt the codes to the available computing resources, and this forms the basis for many of my comments.

The environment needed to run CRRIS should be spelled out in more detail. Is there a minimum hardware configuration needed? What is required in the way of main memory, disk storage, or tape drives? What about language versions, subroutine libraries, and data bases? This information is inferable from the JCL provided for each module, but is not gathered in one place for easy study. Has the complete CRRIS package been exported to any computer centers outside the Battelle Northwest¹ and ORNL facilities? If so, what difficulties were encountered, and what was required to resolve them?

Will states be able to have CRRIS problems run routinely at PNL, ORNL, or EPA computer centers? Will there be any mechanism for state access to EPA resources? If so, would it be possible to provide some fill-in-the blank parameter selection format or check-off menu for each module, rather than requiring the full jobstream listing? Is it possible to estimate costs in time and computational resources to run the various modules? Does it make any practical difference whether all the modules are run for any given problem, or are there advantages to selecting specific modules? How will CRRIS be distributed? Are the SITE, DRALIST/CRRIS-1, and meteorological data sets included in the distribution tapes? Are there plans to extend CRRIS to allow calculations of doses from fish and shellfish? Can the output be tailored to limit it to state boundaries? Is it possible to extract seasonal data?

In summary, I believe that the integrated computations that CRRIS provides will be useful for state radiation control programs that are faced with decisions about low-level waste burial sites, power plant decommissioning, and other similar problems relating to management of large diffuse sources of radioactive materials. However, the code as it is presently configured is less accessible than perhaps it could be for the reasons given above, and this will limit the amount of use it gets from state programs. Installation of CRRIS, together with the associated data bases, at national or regional computer centers, or its use by contractors preparing reports for state radiation control programs, may alleviate some of these problems and facilitate the use of CRRIS.

Before using these models for regulatory purposes, EPA should commission an independent review that involves actually setting up and running the codes and carrying out some test cases and sensitivity analyses. Such a review would require several person-years of effort.

¹ As of March 1986, the CRRIS has not been implemented at Battelle-Northwest (ed).

One general comment pervades the entire series of documents: The CRRIS methodology has been created to provide the EPA with the means to implement the Clean Air Act. No further discussion of the Clean Air Act nor of EPA's responsibilities has been provided. The absence of a foreword or introduction that provides this background information is a serious deficiency, I feel. This deficiency makes it difficult if not impossible to adequately review the CRRIS documentation.

Overall, you have a reasonable approach which should provide an effective procedure for estimating or predicting the passage of airborne radioactive effluents through the environment and the resulting doses. It is not possible by reading the documents to determine whether the system works, however; are some test runs available? Situations at places like Hanford and the Savannah River Plant that permit comparison of computed with measured airborne radionuclides, deposited radionuclides, and associated radiation doses can be used to test the applicability of the program.

In general, these documents seem to be quite well written and self-consistent; I believe an uninitiated user could understand and use the codes without too much difficulty. There are a few exceptions (the area source section, for example) and I have indicated these in my comments. The portions of these codes dealing with atmospheric transport and dispersion appear to follow normal practice; I found nothing startling. The authors have documented their sources quite well; their principle contribution is to have taken useful procedures from many sources and molded them into a coherent assessment system. The authors have been careful to say that these methods are suitable for long-term radionuclide releases; this is important because the models and techniques used effectively preclude their realistic application to any short-term events. Detailed comments are given on the attached sheets.

Overall, I thought the documentation for the models was good. I think the attempt to integrate the six models into a single system makes a lot of sense. In terms of meeting the objective as user manuals, however, I am not convinced that an outsider would be able to execute the programs. Such documentation would require many specific examples, and should clearly show how to replace default parameters values with user specified values.

The system of models and data files that constitutes the CRRIS is presented as the state-of-the-art in assessment modeling. However, I question whether that claim is valid. In many ways, the current product seems more like an integration of earlier software than an improvement on earlier models. Even at that, the choice of Fortran 4 (FORTRAN 66) for the models would negate the idea that the software itself is a state-of-the-art product. FORTRAN 66 has been widely replaced by FORTRAN 77 at most installations, with support for FORTRAN 66 being

dropped. The use of NAMELIST I/O (non-standard within FORTRAN) also limits the portability of the code. Thus, the CRRIS is constrained to work in a limited and generally outmoded computer environment.

Another major failing of the models seems to be in the lack of adequate consideration of time dependent processes and age and sex specific factors in the models. Work done as part of the Off-Site Radiation Exposure Project (OREP) has clearly shown the importance of seasonal, sex, and age specific factors in determining dose to individuals from fallout. The integrated concentrations of some radionuclides in milk have been shown to vary over 2 orders of magnitude due to seasonal variation in the diets of dairy cattle. Human diets, in terms of source of foods and storage times, also may vary seasonally. The quantities and types of foods consumed vary with age and sex. Activity patterns depend upon both age and sex of individuals, and change across seasons for many demographic groups, thus affecting exposure from external sources. I am not convinced that such time dependent factors were adequately represented in the models.

The other major fault I find with the CRRIS is the lack of documentation on analysis of the models. I assume that the current review is, in part, an attempt to establish the "face validity" of the model. However, as these models have been around for quite some time, one might expect that some effort would have been put into performing uncertainty, sensitivity, and especially quantitative validation analyses. The models are complex enough that trying to judge their utility in risk assessment problems would be better substantiated by statistical comparisons of model predictions against real world observations for known scenarios (e.g., Kirchner and Whicker 1984). Such comparisons apparently were not done. Sensitivity analyses would at least help identify those situations where the models may be likely to fail, and also indicate where further research would be most beneficial. Uncertainty analyses would also help show the level of uncertainty associated with model predictions, given that all of the parameters in the model are not constants known with great precision and accuracy. Monte Carlo techniques for doing uncertainty analyses would not necessarily be difficult to implement, although the analyses could prove to require a lot of computer time.

It is not clear to what degree the atmospheric dispersion models account for complex terrain or other site-specific conditions. Will CRRIS be compatible with the output systems of local computer codes that do adequately account for complex terrain or other site-specific conditions? The computer program documentation does not completely satisfy the accepted guidelines for computer program documentation given by the American National Standards Institute, Inc. in ANSI N413-1974. No sample problems have been included to demonstrate the operation of the computer programs or to use for benchmarking and verification purposes.

Generally, I found that the models and parameter selections represented what I believe to be at least "state-of-the-art" and in some cases, they go beyond previous modeling capabilities. I was particularly impressed with the documentation of the parameter selections and site-related parameters in ORNL-5786.

Although the models and associated parameters generally are technically well thought out and documented, in at least two cases the models appear to go beyond available experimental confirmation or practical data availability. These are the theoretical models for deposition interception by vegetation and MSOIL, the ground-water soil infiltration model. The deposition models appear to require experimental confirmation before they can be applied in practice. The multi-layer infiltration model appears to be "overkill" considering the lack of specific parameters for infiltration for each of the soil layers and the homogeneous soil layers assumed by the model. In both cases, the modeling exceeds the parameter availability and probably the assessment needs.

The codes should be very useful to EPA in preparing its dose assessment. However, because of their size (in terms of computer memory), input flexibility and complexity, I do not foresee widespread adoption by other potential users such as NRC licensees.

Validation and Verification

The documents do not appear to include any consideration of the very important topic of verification and/or validation of the computer codes or the models in general. By verification is generally meant the process of assuring that the computer codes do in fact perform as they are expected. By validation is meant some test of the overall model predictions against actual experimental data.

My personal opinion is that no model should be used for regulatory purposes unless it has been thoroughly verified and validated. This is particularly true in this case, because there are many sets of data that could be used for this purpose. Further, the regulations under the Clean Air Act could result in the expenditure of hundreds of millions of dollars for control technology. It would not be fair to undertake such an expenditure on the basis of an unvalidated and/or unverified model.

In the material that I read, there was not even an example of the application of this model to any real or hypothetical situation. My impression is that this model is now so highly fragmented that it is very difficult to determine exactly what it does or does not do. Therefore, I feel it is mandatory that several examples, or benchmark calculations, be included in the documentation.

The major void in environmental transport technology is not the lack of computer models. It is model driven field verification studies. The computer codes that make up CRRIS provide an opportunity to define such studies in terms of the types of complete data sets that are

required. Virtually all of the field data in the literature requires some extrapolation and assumptions of unmeasured parameters to make it usable. This subject needs to be addressed.

UNCERTAINTY ANALYSIS

The comments below analyze the topic of uncertainty in the CRRIS models. Conceptually, considerations of uncertainty enter CRRIS in several ways:

- 1) Uncertainty in the data, including "default" data or parameters contained in the system and data entered by the user for a particular application;
- 2) Uncertainty in the various models, including errors due to the fact that models always simplify reality and errors due to the possibility that the models may misrepresent reality;
- 3) Methods by which consideration of uncertainty are passed from one component to the next, that is, the propagation of uncertainties through the system;
- 4) How shortcomings in the treatment of uncertainty in CRRIS affect the use of the results.

In addition to review of these aspects, a further consideration is raised in Nelson's letter of 2 October 1985.

- 5) "How can EPA best address the uncertainty inherent in any calculations made with the CRRIS or any similar system of assessment models?"

This question goes beyond a review of the status of consideration in CRRIS and asks for a recommendation on how uncertainty should be treated in such a model. Each of these considerations is discussed below with reference to particular computer codes as appropriate.

Uncertainty in the Data

Results of a model can be no better than the data that go into it. In CRRIS, these data are of two general kinds:

- 1) the data that form the set of parameters built into the models and the data associated with CRRIS such as SITE, ORNL 5768 and ORNL 5786;
- 2) the problem-specific data entered by the user.

The various codes in CRRIS contain tens, if not hundreds, of parameters. Examples include the roughness parameter for simulating building wake effects in ANEMOS, the indoor air exchange rate and indoor removal rate in ANDROS, and plant uptake rates in TERRA. Some (i.e., radioisotope decay rates) are generally applicable and have little error. Others are highly uncertain. In addition to what is usually considered uncertainty, many parameters are subject to variability. That is, there is no "true" value with error bars, but different values

that apply in different places or under different circumstances. For TERRA, an attempt to consider this variability was made by assuming a lognormal distribution. There are many coefficients treated this way and it appears a lot of "fiddling" was required to obtain "reasonable" results. The problem is that data are sparse and often no one knows what the appropriate ranges are. This is probably more true in the food chain model than elsewhere, and sensitivity analysis is probably the best way of determining how much this lack of knowledge contributes to the uncertainty in the overall results.

Of the supporting data bases, only SITE will be discussed. This includes data on population and agricultural production on a 0.5 degree grid. To examine uncertainty, one must go back to the raw data. Errors in data collection and changes since the data were collected should be considered. Then uncertainties introduced by mapping the data into the grid must be examined. Average county data are proportionally mapped into the grid. Since this will produce different results than the actual situation (say several small population clusters in a large rural county) an error or uncertainty is introduced. Finally, the error introduced by the use of the grid itself, as opposed to actual data, should be considered. The nature of the uncertainty will depend on the application. If the SITE data are used for a specific location, the error is the difference between the results produced and what would have been produced with actual detailed local data. If the application is a hypothetical location designed to represent an area or a class of sources, the "error" is the range of results that would have been obtained at various sites within the area or class. This data base has been applied in many applications and such an analysis may already have been done.

The data specified by the user to describe each case also contains uncertainty. The system should assist the user in describing this uncertainty as well as providing the opportunity of including it in the analysis.

What Can/Should Be Done?

Uncertainty in the data and parameters is the simplest kind of uncertainty to include in an analysis. Sensitivity analysis, varying each parameter or input data element in turn to reflect the bounds of possible values is an appropriate approach where the parameters are poorly known. Where interaction takes place among parameters, various values must be selected to test sensitivity.

Where parameters are better understood, uncertainty can be expressed by describing them as a probability distribution. While this should rely on objective data as much as possible, expert judgment is important in selecting the shape and range of the distribution. The entire system or individual models can then be run in a Monte Carlo mode to describe the results as a distribution rather than a point value.

The feasibility of this depends on the computer run time of the models. Pseudo Monte Carlo techniques are available that can be done at lower cost. Computer codes are available (e.g., Vaario, 1982) which will identify a group of the most important input variables of a code that has many variables with uncertainties, thus simplifying the task by allowing concentration of effort on only the most critical parameters.

Uncertainty in the Models

There is a natural variability and stochasticity in nature that cannot be captured in any computer model. Indeed, the function of a model is to simplify nature to the barest essentials and thus eliminate complexity. This underlying variability must be recognized, however, in interpreting results. Beyond this inherent uncertainty in any model is uncertainty introduced by possible faulty or missing relationships in the model. This is harder to deal with than data problems. Our understanding of the science upon which the models are based is often incomplete. At times, alternative hypotheses exist on a given relationship; implications of these alternatives can be explored. In some cases it may be appropriate to provide ranges of results under the various alternatives as a sensitivity analysis. In other cases, discrete alternative results are called for. Missing relationships should be sought (and presumably will be by those who are reviewing each model). The 1983 workshop on food chain modeling (Breckland and Baes, 1985) identified several missing relationships in TERREX, a derivative of TERRA.

What Can/Should Be Done?

Validation studies, where possible, are invaluable. In many cases, however, they are difficult or impossible. Expert review of the models to evaluate inherent uncertainties, possible alternative hypotheses in the underlying science, and missing relationships are important. The results of the current review should be evaluated in this light, but a more extensive review, conducted in close cooperation with the model builders, is probably indicated. Finally, sensitivity analyses should be run to evaluate the effect on the results of uncertainties uncovered.

Propagation of Uncertainty

Within a given code, and among the different codes in CRRIS, there appears to be no method for carrying forward uncertainty. Yet, the results reflect all the uncertainties introduced in the entire system.

The makers and users of such a complex system must find themselves in a box. The purpose of the system is to aid decision making, yet, if all uncertainties are included and propagated through the system, the results are likely to span a range so great as to make them useless for decision making. Yet, the alternative, and likely the existing state of things, is that the need to ponder what to do in the face of such uncertainty is lifted from the decision makers and embedded in the myriad assumptions in the model. There are two results. First, a degree of conservatism or lack of conservatism unknown to anyone is

built into the results. Second, and more important, this degree of conservatism is not constant. For example, it is different in the food chain and the direct inhalation models; different cases, which rely to different degrees on these different exposure routes will have different (but still unknown) levels of conservatism built into them. This, of course, leads to inconsistencies in regulation.

What Can/Should Be Done?

While for regulation it often seems necessary to have a single number, it is important that regulatory decisions be based on a full understanding of the uncertainty associated with that number (if for no other reason than, without demonstration of such an understanding they are not likely to stand up in court). Our inclination would be for a full Monte Carlo simulation on all components of the system, with all data and model uncertainties appropriately quantified. This approach was recommended by a recent comparison of methods for uncertainty analysis in probabilistic risk analyses for nuclear power (Martz et al., 1983) but is probably impossibly complicated for CRRIS. There may be other ways to propagate probabilistic results. There seem to be only four transfers of results between codes. Suppose we only wanted to know the 95 percentile point of the outcome. If we selected the $(1-p)$ points of n distributions and combine them, we get the $[1-(1-p)^n]$ point of the combination (e.g., three 95% points combined yield the 99.9875% point). This demonstrates the problem with using 95% upper confidence limits as parameters. An alternative would be to decide what level of "conservatism" was desired in the final results (say the 95 percentile bound) and determine what the appropriate level of upper confidence bound should be used for the various parameters. The earlier formula can be used to select this level, p , from the equation

$$(1 - p)^{1/n} = 0.05$$

Since CRRIS has 1-, 2-, and 3-level combinations, the general form is:

$$(1 - p_1) + (1 - p_2) + (1 - p_1)(1 - p_2) + \dots = 0.05$$

One approach to allocating relative contributions of each component would be to require that the same part of the distribution be used for each component. Another would be to require that each transfer to man should have the same relative contribution to the overall uncertainty, possible using some weighting scheme.

Uncertainty and Consistency of Results

A detailed discussion of the likely accuracy of CRRIS and of the degree to which this accuracy has been independently established is needed. As written, short comments regarding accuracy and uncertainty are sprinkled throughout the reports in a way that does not permit the cumulative uncertainty to be judged. The Introduction to CRRIS report

refers to one area of analytical inconsistency (page 28), and Table 2.1 of MLSOIL (page 11) indicates another; there must be others. The best description of uncertainty in CRRIS is Table 9 in ANEMOS (page 66); similar sections are needed for each part of the model. Where uncertainties can only be guessed at, this should be noted and the guesses given. While it is clear from the Introduction to CRRIS report (page 47) that the calculations are based on best estimates of parameters, there is no attempt that I found to provide a general sense of the most important sensitivities for CRRIS results. The objective of using reasonable values (noted above, page 47) in contrast to conservative values seems inconsistent with MLSOIL Table 2.1. ANEMOS is significantly better than the other reports in its descriptions of uncertainty and validity.

Uncertainty in ANEMOS and PRIMUS

It is important to provide those responsible for standard setting with the uncertainty bounds on final results. Such information is essential for informed regulatory decision-making. Also, for these reasons, results should never be reported to more than one significant digit, as it is misleading to regulators to imply greater precision.

Several people (Hoffman, Dunning, and Schwarz) at ORNL have pioneered the development of techniques for the analysis of uncertainty in complicated models of this sort. Whicker and Kirchner at CSU have also developed and published techniques for the analysis of uncertainty in such models. It seems obvious to me that a proper analysis of uncertainty must be included for a model intended for application in a regulatory process. It should have been done a long time ago. The necessary techniques exist and are available at ORNL.

Some Preliminary Comments on ANEMOS Uncertainty

- 1) What is the sensitivity of the code to the roughness parameter in simulating complex conditions such as conditions with building wake effects?
- 2) Describe conditions of complex wake effects (e.g., high structure abnormality, too many structures), for which assuming a uniform roughness coefficient may be preferable than trying to simulate building wake effects.

Page 67 of ANEMOS, Last Sentence

Regarding uncertainty associated with calculation of deposition, the authors state that "Miller and Little (1983) suggest that one cannot specify the accuracy associated with a calculation of concentration

on ground or with an air concentration involving significant plume depletion". If this is indeed the case, it is all the more reason to reflect uncertainties in the overall assessment process to regulators who must use the results to make decisions regarding radiation standard setting. To the extent possible, overall uncertainty associated with the output results of the CRRIS should be estimated and reported. We commend the authors for their discussion of uncertainty in the ANEMOS code documentation (ORNL-5913). Such a discussion should be included in the other modules of the CRRIS.

Uncertainty in PRIMUS

The eventual output of this module is problem-specific decay chain activities. The input data are radionuclide decay rates and branching fractions. These values are known much more precisely than any of the other parameters in the whole assessment model. The disparity is so large, in fact, that uncertainties in decay rates and branching fractions need not be considered at all when addressing the problem of uncertainty in the final model results and the sensitivity of the final result to variations in model parameters. (The procedure for using zero half-life for some radionuclides which have very short half-lives described on page 9 and 10 of PRIMUS has no effect on final results or their uncertainty in environmental assessments. These nuclides aren't actually removed though, are they?)

The terms in the equations used in this module can be divided into two kinds: those that may be considered God-given, exact, and true, and those that are more typical in environmental assessment which are, at best, crude approximations to reality. An example of the first kind is the exponential decay of radionuclides. This is known to hold exactly even at nucleus stresses far exceeding those experienced in the earth's environment. (The same is true for the values of the decay rates and branching fractions.) As above, any uncertainty in this formulation need not be considered in result uncertainty or sensitivity. The only examples of the second kind that I could find are the two "S" vectors, "deposition rates of elements" (page 7, equation 2), and "source vector of constant input to system of release rate for nuclide" (page 10, equation 5). Both these vectors are assumed to be constants and the methods used by PRIMUS do not work if they are not constants. However, if the radionuclides of interest have half-lives much longer than any periodicity in elements of either of these arrays, then time averages can be substituted and the correct results obtained. If this condition is not met, then PRIMUS cannot be used and the danger exists, as always of a model being used outside its range of applicability.

Sources of Error

Nowhere in the document is past, current, or future validation of the PRIMUS code and alphanumeric input library discussed. If the CRRIS models will be a primary assessment tool used by EPA for many years, a suite of sample cases should be designed which, to the extent possible,

exercise all nooks and crannies of the code. The output files of these test runs can then be compared with previously prepared files which contain the known results. This is especially important, for example, if the code will be upgraded (i.e., changed) on an ongoing basis or if the input library is transferred by methods which are not error checking.

Nowhere is validation of the run input (PRIMUS.DAT) discussed. Page 24 in the PRIMUS document says that only nuclides contained in the input library are permitted. Does this mean that mistyped nuclide names are ignored, or does the program stop (as it should)?

There is actually very little that can be done to validate the input. A priority, any combination of released radionuclides with any values of magnitude is OK. The procedure has great potential for human error and all that can be done is inveighing the user to change his input and provide tools for him to do this. One example is a REQUIRED echo of "released" nuclide list and magnitude including translation of the element symbols to english (e.g., H = hydrogen). I can easily see people getting cesium and cerium or protactinium and polonium mixed up.

KTRUNC, MAXTRUNC -- I do not understand the utility of these. Speeding up a program is not an end in itself and there seems to be a danger here of eliminating significant nuclides from consideration.

NUMEXP, EXPSYM -- These also leave me with the impression that significant nuclides may be omitted. I probably don't understand something.

HEALTH EFFECTS

The following is a general assessment of the treatment of health effects in the CRRIS models.

I could not tell from the report what dose-response assumptions are contained in ANDROS; these should be displayed clearly and compared with reference radiation risk assumptions from BEIR III and ICRP. While it is clear that ANDROS uses a linear dose-response form (since everything is converted to a lifetime equivalent dose), the parameters are not given, nor is it clear whether a relative or absolute risk model is used. This inscrutability is a serious drawback, in my view. A full discussion of the analytical assumptions for risk estimation is clearly needed; it should describe how the assumptions regarding population age distributions (static, based on constant birth rates and age-specific death rates) differ from actual data and how these differences affect risk estimates. It should also discuss how population mobility could affect calculated risk.

Given the many pathways and long-term focus of CRRIS, it seems likely that significant portions of the calculated population dose will come from large populations at low dose. It is noted that ANDROS can describe its calculated results for a selected or average individual or for collective population dose. Is a histogram of population exposure also possible with ANDROS?

The CRRIS method for estimating doses and risks uses a data base developed by the old RADRISK computer program. The RADRISK program is known to depart from methods and parameters in ICRP Publication Nos. 26 and 30 relative to lung mass, organ weighting factors and the 50-year dose commitment period. It is not possible to tell from the documents if other differences exist between CRRIS and ICRP Publication Nos. 26 and 30. The use of methods and parameters different from those of ICRP (and proposed by NCRP) is unscientific, detracts from the credibility of CRRIS and compromises the integrity of the ICRP and NCRP standards and recommendations.

It is of utmost importance that the ICRP models and parameters be utilized in dose assessments to assure the credibility of the dose estimates and to protect the integrity of the ICRP and NCRP scientifically based dose system and standards.

MODEL AND COMPUTER PROGRAM DOCUMENT COMMENTS

INTRODUCTION TO CRRIS

The following are review comments on the "Introduction to CRRIS," (ORNL/TM-8573).

Technical Observations

It is difficult to make a specific assessment of CRRIS when it is not clear for what purpose it was developed. To say that it was designed "to assist EPA in determining the health significance of airborne radioactive emissions pursuant to its regulatory authority under the Clean Air Act as Amended August 1977," just is not specific enough. Is the purpose to estimate the average population exposure? Will it be used to estimate exposures to critical groups? Will it be used to judge whether a facility is in compliance with regulations? To answer each of these questions would require different criteria with different data bases.

It is difficult to relate the reference list in Section 11 to the text since reference numbers do not appear in the text.

(Pages 9-10)

The Clean Air Act is barely mentioned in passing. It is stated that AIRDOS-EPA and DARTAB have provided an interim methodology for performing nuclear assessments. It is implied but not explicitly stated that CRRIS is intended to be the "state-of-the-art" radiological risk assessment methodology that is referred to in the Introduction to CRRIS report. CRRIS has been designed to be a very versatile system. Does it have applications beyond that of somehow implementing the clean air act? Specific applications of CRRIS should be discussed and its relationship to the Clean Air Act Clarified.

(Page 7, Bottom)

Discussion of $Q(0)$ refers to emission rates in Bq/s or Ci/s; shouldn't $Q(0)$ be in Bq or Ci?

(Page 24)

Food and feed crops have been considered in considerable detail, but only milk and beef have been considered among animal products. Pork, poultry, and eggs, and perhaps to a lesser extent, lamb are important animal products that should be considered. Translocation of surface deposited radionuclides from nonedible to edible parts of plants should be included. (This process is important for nuclides such as Cs-137.)

(Page 28)

The authors state that B_{iv1} and B_{iv2} are directly comparable, but it is not clear (to this reviewer at least) why. Fundamentally they are not directly comparable. Adoption of B_v and B_r over B_{iv1} and B_{iv2} is an interesting approach. It would be useful to compare the distributions of the two sets of parameters statistically.

(Pages 36-37)

The description of the procedure for estimating intercompartment transfer coefficients is rather vague.

(Pages 43 and 47)

ANDROS uses the results from the other codes in the CRRIS and calculates doses and health risks for either a selected individual, or the entire population. The default parameters chosen for the CRRIS are mostly median estimates and reflect an effort to choose reasonable values. ANDROS will calculate doses and health effects for a selected individual provided the user incorporates the appropriate parameter values specific for the individual and site. ANDROS will, after a fashion, calculate doses and health effects for an "average" individual when the default parameter values are incorporated. However, because geometric means of geometric means of plant-to-soil concentration ratios and other geometric mean or median values were adopted as default parameter values for TERRA, doses and health effects for an "average" individual could be biased on the low side. ANDROS will not calculate doses and health effects for a population when the default values are incorporated in TERRA because calculation of a population dose requires the use of arithmetic mean parameter values. Calculation of doses and health effects for members of a critical group also requires selection of parameter values in place of the default values.

(Page 3)

Figure 1.1 is titled "The six computer codes..." There are eight codes shown in the figure and "NAMELIST" still appears to be required if the user wishes to deviate from default parametric values.

(Page 4, Last Paragraph)

An extensive "Users Manual" is a must, including numerous samples input listing to exercise the various options. Such a manual, along with a copy of the programs and sample input fields on magnetic tape, must be available to those potentially affected when the EPA publishes their intent of rule making in the Federal Register. Ample time must be allowed for computer testing of the codes which is essential in the review, interpretation, and impact evaluation of the proposed rule making.

(Page 6, Equation 2.1)

The definition of a_{ii} in Equation 2.2, Equation 9.1 and Equation 9.2 all seem to be missing summation signs in my copy.

(Page 6, Equation 2.2)

The variable λ_{ri} is not defined.

(Page 22)

Define ZAS number (or at least refer to a document defining ZAS...perhaps the MLSOIL and DFSOIL report.)

(Page 36, Equation 7.1)

I think this equation may have several errors in the last term. First, it appears to be missing a summation sign. Second, if b_{ij} is a branching fraction, I would expect it to be multiplied by λ_j and b_{ij} by C_j (not C_i).

(Pages 43-46)

It appears from this discussion that the dose factors and food ingestion rates used in the ANDROS computer code are only appropriate for adults. It is wrong to apply these factors, which were derived for adult occupational workers, to a general off-site population. There is ample evidence that the dose to an infant's thyroid, for example, may be a factor of 10 higher than that to an adult. Other examples for other radionuclides, while not this extreme, amply illustrate that serious errors can accrue from the use of non age-adjusted dose factors and food ingestion rates.

I know from past experience that the EPA health-risk codes go to great length to include a "life table" analysis, which pays a great deal of attention to the varying cancer incidence with age. It seems very inconsistent to devote this much attention to one variation with age and then to ignore the substantial variation in doses with age. (Page 25) (May also appear in every other document--noted in ORNL-5785, page 5, and in ORNL-5786, page 2.)

This pathway diagram is incomplete and inaccurate as shown. It does not even match the pathways that are modeled in the computer codes. For example, it does not show the pathway of resuspension that is actually modeled (incorrectly, in my view) in the documents. The model, as presented later, does consider the resuspension of deposited radioactivity and its subsequent deposition on food crops and forage.

(Page 6, Equation 2.1)

The definition of a_{ii} in Equation 2.2, Equation 9.1 and Equation 9.2 all seem to be missing summation signs in my copy.

(Page 6, Equation 2.2)

The variable λ_{ri} is not defined.

(Page 22)

Define ZAS number (or at least refer to a document defining ZAS...perhaps the MLSOIL and DFSOIL report.)

(Page 36, Equation 7.1)

I think this equation may have several errors in the last term. First, it appears to be missing a summation sign. Second, if b_{ij} is a branching fraction, I would expect it to be multiplied by λ_j and i_j by C_j (not C_i).

(Pages 43-46)

It appears from this discussion that the dose factors and food ingestion rates used in the ANDROS computer code are only appropriate for adults. It is wrong to apply these factors, which were derived for adult occupational workers, to a general off-site population. There is ample evidence that the dose to an infant's thyroid, for example, may be a factor of 10 higher than that to an adult. Other examples for other radionuclides, while not this extreme, amply illustrate that serious errors can accrue from the use of non age-adjusted dose factors and food ingestion rates.

I know from past experience that the EPA health-risk codes go to great length to include a "life table" analysis, which pays a great deal of attention to the varying cancer incidence with age. It seems very inconsistent to devote this much attention to one variation with age and then to ignore the substantial variation in doses with age. (Page 25) (May also appear in every other document--noted in ORNL-5785, page 5, and in ORNL-5786, page 2.)

This pathway diagram is incomplete and inaccurate as shown. It does not even match the pathways that are modeled in the computer codes. For example, it does not show the pathway of resuspension that is actually modeled (incorrectly, in my view) in the documents. The model, as presented later, does consider the resuspension of deposited radioactivity and its subsequent deposition on food crops and forage.

Neither the diagram nor the model considers other resuspension pathways such as the inhalation of resuspended aerosols by humans or by beef and milk cattle. In some cases these may be significant pathways, especially the direct inhalation pathway by humans.

It is incorrect to show the weathering of radioactivity from forage and food crops into a "sink." This material will obviously be available to cycle again.

Another serious conceptual error concerns the milk cows and beef cattle pathway. As shown, the only output from the cows and beef cattle goes to humans. Obviously, this is not true as most of the activity leaves the cows and cattle via urine and feces and is redeposited on the soil and vegetative surfaces.

(Page 3, Figure 1.1)

Add arrow from PRIMUS to RETADD-II (this applies to all reports--some have a line, some do not).

(Page 6, Equation 2.1)

Summation signs appear to be missing here and in subsequent equations (for example, page 9, equation 3.1).

(Page 9, Last Line)

Indicate that x is in meters.

(Page 15, Last Line)

Where is λ^d in Equation 3.6; also λ^w on page 16.

(Page 21)

How consistent are RETADD-II and ANEMOS at overlapping locations?

(Page 26, Equation 6.1)

How is the change of C with time handled in this equation?

(Page 31, Equation 6.6)

Equation 6.6 is wrong (I think P and I should be in the numerator).

(Page 35, Second Paragraph)

Why use top 1 cm instead of top 5 cm?

(Page 46)

Why is drinking water intake not given in Table 9?

(Page 12, Equation 3.4)

The use of this expression for the lofting dispersion case, although correct for the ground level air concentration, will underestimate the external gamma dose from the elevated plume and the washout deposition from rain or snow.

(Page 24)

The use of isotope specific transfer factors based upon half-life is a significant refinement over the Reg. Guide 1.109 (NRC, 1977a) approach using stable element transfer rates without correction. That approach tended to overestimate the food concentration of short-lived radionuclides.

(Chapter 7 - MLSOIL Code)

The seven compartment model appears to be a bit of overkill as the unavailability of site specific parameters for various soil types and depths makes the multicompartmental model mathematically elegant but practically unworkable. Among the factors that contribute to this are the following.

1. One would need a data base of infiltration coefficients as a function of soil type and depth. As noted in Section 7.2, the data on infiltration coefficients is limited and not characterized by isotopes for various soil types.
2. Soils are characterized by layers (horizons) having different physio-chemical characteristics. This means that the transfer characteristics of each layer may have to be separately defined. As noted in item #1, the available data do not permit such sophistication.
3. The relative uptake of nutrients (and radionuclides) by plants varies with depth. This means that in terms of plant uptake, radionuclide concentrations in different soil layers will not be assimilated into the plant to the same extent. As the bioaccumulation step is modeled using a single equilibrium transfer factor, the complexity of the soil distribution model is not warranted for this pathway.

An approach using a single "leaching coefficient" and two soil layers, "root zone" and "other", seems to be adequate for using equilibrium soil-to-plant transfer factors. Assuming a uniform radionuclide concentration with depth is adequate for tilled agricultural land. For untilled land, an exponential fall-off in concentration with increasing soil depth would be a reasonable model.

(Page 46, ANDROS Code)

These weighting factors are slightly different from those of the ICRP as expressed in ICRP Publication 26. Disregarding the merits of any improvements in the derivation of the risk factors that enter into these equations, the uncertainties arising from relative versus absolute risk, linear versus linear-quadratic dose response models, revised dosimetry, etc., more than mask possible differences between the EPA approach and the ICRP 26 weight values. For consistency with the rest of the world, use the ICRP 26 values as EPA has in the occupational radiation protection federal guidance.

(Page 9, Equation 3.1)

It might be helpful to the reader if the term $Q_{i\text{pr}}(x)$ were defined for a constant release rate as:

$$Q_{i\text{pr}}(s) = f_{\text{pr}}(s)Q_i$$

where $f_{\text{pr}}(x)$ is as defined for equation 3.6 on page 15.

(Page 27, Equations 6.2 and 6.3)

It would be helpful when using the dry weight plant-to-soil concentration ratios to standardize the terminology on "concentration ratio", CR, as agreed to in the ERDA "Workshop on Environmental Research for Transuranium Elements," ERDA (1975) and reserve the B (or B_{iv}) notation for the bioaccumulation factors based upon net plant mass to dry soil. As there may be as much as a factor of 20 difference between CR and B_{iv} , the symbol should clearly differentiate between the two parameter sets.

(Page 44, Chapter 9, The ANDROS Code)

The unit of the internal dose factors (mrad/y per pCi/y) appears to be incorrect. While the statement that "the dose commitment per unit intake is numerically equal to the dose rate in the 70th year of a continuous intake at a unit rate" is correct, the dose commitment from a single intake (not a continuous intake rate) is the time integral of the dose rate with time, i.e., it is dose (equivalent) not dose rate. The more conventional terminology for such intake-to-dose factors is mrad per pCi. This distinction may appear to be trivial as the per year terms used in both numerator and denominator cancel giving the correct mrad per pCi. However, in one case multiplication by the annual radionuclide intake in pCi will give dose commitment in mrad while use

of an equivalent pCi/y for an annual intake would give a dose rate in mrem/yr. As the dose commitment is properly characterized as "representing total dose over a 70-year period following [a] unit intake", the units should match the definition (i.e., mrad per pCi).

(Page 46, Table 9.2)

The reference to Guimond et al., 1979, for the breathing rate does not appear to be to the original source. The value quoted, 8035, is simply the daily air intake values from the ICRP Reference Man (ICRP 23) averaged over adult males (23 m³/day) and females (21 m³/day) = 22 m³/day average x 365.24 days/year = 8035.28. The citation should be ICRP 23, page 346.

(Pages 23-24)

It is not completely clear how TERRA calculates root zone soil concentrations. It appears to do so by simply dividing the deposition by the mass of soil in the first 15 cm of depth. This is likely adequate for plowed fields, but not for unplowed fields (orchard crops). Depletion via leaching out of the root zone is considered as it should be.

(Page 27, Section 6.2, First Paragraph)

The validity of the last two sentences is questionable. What technical explanation can be given?

(Page 29, First Paragraph)

The use of geometric means as "best estimates of parameters" (because the data appears to be lognormally distributed) must be done with much caution. A common reason for "an order of magnitude span" in reported data is the fact that the experimental data was developed in entirely different manners for different reasons.

(Section 6.3)

This data base should prove most helpful to the user; but he should be cautioned that it must be confirmed on a site specific basis.

ANDROS

This section compiles comments on the CRRIS model, ANDROS, (ORNL-5889)

Technical Observations

This is the most poorly written and least informative document of the group. It never really describes how doses (or dose conversion factors) are calculated nor provides clear references to the numerous parameters required to calculate dose. Presumably they use ICRP-30 methods but they never say so.

I would strongly recommend that it be rewritten with complete equations for all dose calculations and the rationale from which they were derived. Parameters used in the calculations can be listed or referenced.

The review is based on a general audience with limited background in previous EPA models. The overall pathway assessment capability of this set of codes will enhance the capability of many assessments. However, the dose document lacks adequate description and details. The mathematical models must be more clearly defined. The document presupposes a thorough knowledge of DARTAB, AIRDOS-EPA, and other previous codes for EPA use. The general use of the codes require more complete documentation of the codes included in this report. This particular document is less complete than others of the set. There is a general lack of details of models and parameters of models. Someone outside of EPA or ORNL can execute the code but there is a problem with interpretation of the results based on this documentation. Examples of problem areas are shown below.

ANDROS apparently assumes lifetime exposure for computation of somatic risks and a 30-year period for genetic effects due to internal and external exposure of individuals. This is not always appropriate.

For example, uranium mines usually operate for only 15-20 years. Facility operating life must be considered in order to make realistic estimates of risk.

ANDROS appears to use ICRP 30 methodology and breathing and food consumption rates for only one age group, i.e., adults. What about other age groups that may be more critical, e.g., teens, children, and infants?

The word dose is used frequently throughout the document without any modifiers. In order to avoid ambiguity, dose should be modified with absorbed or equivalent and annual or commitment.

New concepts and methods are sometimes given only by reference. At a minimum, the new concepts and methods should be briefly described in the text for those readers who are not familiar with the references.

No comments will be given for Appendices A, B, or D because they were not included.

(Page 15, Equation 6)

The physical significance of S should be defined.

(Page 4, Second Full Paragraph, Line 3)

To be truly comprehensive, ANDROS should account for morbidity (incidence), e.g., effective workdays lost, etc., as well as mortality risks from radiation. Incidence can be an important measure of total health impacts, especially for organs with relatively low cancer mortality rates (e.g., thyroid).

(Page 16, Equation 7)

- a. Y_{out} should be defined.
- b. The difference between λ^v and λ_i^r is not clear.
- c. For the parameter f, what is f_{ii} ? Should it not be 1 instead of 0 otherwise?

(Page 17, Equation 9)

The parameter $h(i, j, l)$ should be defined.

(Page 17, Paragraph 2, Line 10)

The rationale for calculating indoor air concentrations using effective air concentration from finite gamma plumes is not clear. It is not appropriate to use equation (8) to calculate indoor air concentration Y^{in} when no radioactivity is actually present in the air. Instead, a correction should be made for reduction in direct radiation from finite plumes due to shielding by the structure.

(Page 20, Section 2.2.3, Line 4)

Why does ANDROS not calculate skin dose from beta particles?

(Page 21, Table 3)

In Table 3, what assumptions were made for occupancy time indoors and equilibrium fraction for radon progeny? Do the values apply to indoor or outdoor exposure?

(Page 5, End of Top Paragraph)

Regarding "years of life lost per average premature death", shouldn't this be cancer death?

(Page 17, Equation 8)

Equation 8 of the ANDROS report looks strange. Why doesn't it contain an exponential term as would be expected from the solution of a differential equation of the form of equation 7. Daughter ingrowth and decay does not seem to be considered for this calculation.

(Page 20, Paragraph 2)

The use of a default value of a 70-year-committed dose is overly conservative except for a selected individual. A more realistic mean individual or collective population dose is a committed dose for the life expectancy less the population weighted age. The 70-year-committed dose adds unnecessary conservatism.

(Page 22, Paragraph 3, and Page 23, Table 4)

The use of weighting factors different from those recommended by the ICRP should be more adequately explained. I assume that these are the EPA values and are to be used for EPA studies, but more information is required.

(Page 4, Paragraph 1)

It is not adequate to state that the code is analogous to DARTAB in its combination of dosimetric and health effects. The author presupposes an intimate familiarity to DARTAB which may not exist. The description must stand alone since this set of documents is to be a description of the methodology.

(Page 11, Section 2.1.3)

The description of the RADRISK computer code must be explained in some detail if the results shown are based on it. This statement assumes too much familiarity with that model.

(Page 17, Section 2.2.2.4 and Page 18, Section 2.2.2.5)

The conceptual description is generally unsatisfactory when there is no discussion of the model used. In addition, no model parameters are given. A suggestion is to describe the model and place an appendix of calculation parameters. This is particularly true since there are the models and parameters accepted by EPA and ICRP and variations of these for certain nuclides based on experimental results.

(Page 22, Section 2.2.4)

This section is adequate for describing the risk modeling used but it could be more detailed, possibly with some numerical examples.

(Pages 22 and 23)

Given the size of the documentation for CRRIS (9 volumes), it is surprising that the risk values to be used in CRRIS are only briefly described. For example, on p. 22, 3rd paragraph, the text very briefly describes the default weighting factors (Table 4, p. 23) that are to be used in calculating effective whole-body dose equivalent. The text does not describe the bases for the factors (e.g., absolute or relative risk model; linear, linear-quadratic or pure quadratic model) or the uncertainty in these factors. Since the end product of the CRRIS computer code is an estimation of risk, we recommend that the bases for the risk factors be more thoroughly described and an approximate range for the factors given.

(Sections 2.2.4 - 2.2.6)

The mathematical models described in Section 2.2.4 through Section 2.2.6 are appropriate for estimating risk; however, the bases for calculating some factors needs to be more thoroughly described. For example, the risk to an individual over the remainder of the individual's life from a unit intake at age "t" is denoted by $r(t)$ and is used in Eq. 15 on page 24; however, the text does not explain how $r(t)$ is calculated nor provide a table of values for $r(t)$. In a similar manner the genetic risk coefficient, $r(g)$, is used in Eq. 26 on page 28, but the method used to calculate it is not described.

(Pages 5 and 48)

The CRRIS computer program include provisions for estimating doses to individuals and populations from the major pathways of exposure (e.g., see p. 5, last paragraph, and Table 6 on p. 48). CRRIS contains appropriate provisions for separately estimating doses from exposure to low-LET radiation and high-LET radiation.

(Page 23, Table 4)

Organ dose weighting factors are provided in Table 4, p. 23, to 3 significant figures. The text should state that 3 significant figures are provided only for internal consistency in calculations; a range of uncertainty should also be provided.

(Page 56)

A users guide should be provided as the program would be difficult to use by someone unfamiliar with it. For example, page 56 would seem to indicate that job control and input for the entire CRRIS system?

(Page 5, Paragraph 2)

This section specifies air immersion as a pathway but does not list the alternative for a finite plume where doses/health effects are caused by gamma shine rather than immersion.

(Page 7, Table 1)

Although Table 1 includes a list of many of the notations used in the report, it is incomplete. Some of the notations used in equations in the report are missing in the table. Each equation should be examined to see if the notation is adequately covered in Table 1. One problem encountered was the occasional use of a notation for more than one purpose. Also, it would be useful to the reader to show the units for the various notations used.

(Page 9, Paragraph 3)

Since no example is given of the input stream, it is not clear how a selected individual is selected. Is he selected on the basis of a specified geographical location, on the basis of maximum X/Q, or some other method?

(Page 11, Paragraph 1)

A brief description should be given of the method used to develop dosimetric and health effects data rather than relying entirely on a reference which may not be available to the reader.

(Page 17, Paragraph 3)

Is there an option for using other than average breathing rate to make calculations for a maximized individual?

(Page 18, Paragraph 2)

Is there an option for using other than an average utilization rate to make calculations for a maximized individual?

(Page 20, Paragraph 2)

It would be useful in Section 2.2.3 on dose to explain the different types of doses calculated and the reasons for each calculation. For example, 1) why are low-LET and high-LET doses calculated separately, 2) why are both absorbed dose and dose equivalent calculated, 3) why is a 70-year commitment used for somatic dose, and 4) why is a 30-year commitment used for genetic dose?

(Page 27, Paragraph 2)

More explanation is needed for the term environmental accumulation time.

(Page 44, First Paragraph)

Their definition of dose commitment is puzzling. Do they really mean that it is the dose rate in the 70th year as a result of continuous intake at a unit rate for 70 years. If that is the case then the end result is grossly overly conservative for long biological half-life radionuclides. No one stays in the same location for 70 years. Risk is associated with dose rate (which increases to a maximum at the end of the assumed intake period) or total dose (which also increases with time faster than the dose rate). Neither is the same as the dose rate in the 70th year.

ICRP-30 (1979) defines commitment (committed dose equivalent) as the total dose equivalent averaged throughout any tissue over the 50 years (occupational period of interest) after an actual intake. The ICRP-30 approach is not without fault in that it requires calculation of a 50-year committed dose equivalent for each year of intake, presumably adding them together as time goes by. If the last year of intake is at age 65 (occupational retirement) the calculation is through age 115!

If it must be assumed that the individual remains at the same location for 70 years, then the total dose over 70 years should be calculated and this related to risk, not the dose rate in the 70th year.

(Page 44, Second from Last Paragraph)

The basis for deviating from ICRP-30 weighting factors should be stated or referenced.

(Page 44, Last Paragraph)

It is not clear how the data in Table 9.2 is "adjusted" for the various user selected inputs (fraction of food from home gardens, fraction of population -- rural, non-farm, urban).

(Page 17, Line 2)

Is the beta-particle immersion dose not considered?

(Page 17, Line 5)

Why use indoor air concentrations for immersion dose calculations since the appropriate geometry for indoor air exposure is not considered anywhere, as far as I can see?

(Page 16, Equation 7)

I found the notation in this equation confusing. I could not find a definition for either a_{ii} or f_{ii} .

(Page 22, Paragraph 2)

Population dose here ignores age and sex specific differences between individuals. I think this could be significantly improved.

(Page 23, First Paragraph)

What consideration is given to "plant physiology"?

(Page 24, Section under Equation 15)

I think it would be more accurate to define i as a rate of intake, rather than the intake over the interval $t+dt$. i must have units of year^{-1} .

(Page 26, Equation 21)

This formulation seems to assume that the age at first reproduction for humans is 0, and survivorship is constant. These simplifying assumptions seem excessive, and unwarranted. I don't believe that stable age distributions are dependent on these assumptions. In any event, human populations are unlikely to have stable age distributions. What kind of bias results from making these assumptions? This also seems to be inconsistent with the age of reproduction used for risk assessment (30 years).

(Page 28, Paragraph 1)

Some parameters in the model, like t_i , appear to be constants. I had the impression that the model could be used for specific individuals. Perhaps wording needs to be changed to indicate what the "default values" are, where appropriate.

(Page 17, Equation 8)

There seems to be a problem with Equation 8. Why doesn't this equation contain an exponential term as would be expected from the solution of a differential equation of the form of Equation 7? Daughter ingrowth and decay do not seem to be considered for this calculation.

ANEMOS

The following are reviewers' comments related to the CRRIS model, ANEMOS, (ORNL-5913).

Technical Observations

The ANEMOS model appears to be a fairly standard straight-line Gaussian plume simulation, with few embellishments. It is not apparent what unique feature or set of features makes this model specially appropriate to the subject in question (dispersion of radionuclides), in contrast to other models of the same genre.

The Gaussian approach introduces problems that are common to all such models. The straightline extrapolation of local winds means that there are severe limitations on the locations of applicability. Light wind situations (and especially calms) cannot be handled.

The level of funding provided to support this review did not permit an examination of such factors in more than a cursory fashion. I am interested also in how plume rise and entrainment are handled, but have not addressed these questions. The points listed below relate firstly to the matter of deposition formulation, and secondly to a couple of problems with the physics of the "mother" code.

All in all, the same criticisms could be leveled at almost any Gaussian plume model of the same general variety as this code. I suspect that the generic problems of Gaussian plume approaches are also likely to be appropriate in this specific case. In particular, we might draw attention to the inability to handle light winds and calms, the neglect of influences of topography (mountain ranges, river valleys and coastlines), and the matter of plume rise as it is influenced by emission characteristics. It is possible that none of these matters will constitute a fatal flaw in the model; however I am sure that some observers would need to be convinced of this by a comparison against the output of some more sophisticated approach. In any case, the acceptability of the model will be determined by the applications to which it is put. The usual caveats and qualifications concerning the ranges, averaging scales, and spatial resolution of Gaussian plume models are certainly applicable in the present case, perhaps with the additional proviso that the assumptions made about wet deposition might cause some surprises.

ANEMOS is certainly a very important code in assessing air concentrations and deposition rates for emitted nuclides. The mathematical models and data bases employed seem appropriate for meeting the stated objectives. The report is in general clear and understandable.

I will attempt to answer your eight questions.

- 1) The mathematical methods described in the ANEMOS document appear to me to be appropriate.
- 2) The models used in ANEMOS are representative of state-of-the-art dispersion modeling.
- 3) There are no data bases included in ANEMOS.
- 4) The ANEMOS document contains sufficient technical description for a reader to understand what is supposed to be contained in the model. I am not sure that sufficient information is contained in that document for a user to prepare input data and run the model. Some comments were made on unclear descriptions.
- 5) I'm not familiar with radionuclide environmental pathways so am unable to answer.
- 6) I can not vouch for that which is included in the computer code.
- 7) We have had difficulty in both trying to determine uncertainty and in trying to express it in terms useful to model users.
- 8) It would seem to me that the system could be useful to us on an "as is" basis.

Additions would make ANEMOS more useful.

- 1) I would like to see a priority list of the different user options for different weather and source conditions based on existing validation studies. This will assist the user with limited time and money resources to decide what options to use in specific simulation studies.
 - 2) A figure/figures presenting an aggregated flowchart of ANEMOS with more details than Figure 2 will be very useful. The level of detail presented in Section 7.1, pages 79-80 will be adequate.
-

(Section 2.2.4.2)

This portion of the atmospheric dispersion model is appropriate for use in meeting the objectives of the CRRIS project as I understand them. The Huber building wake correction factors defined in this section are state-of-the-art for Gaussian dispersion plume calculations.

As part of my review, I discussed with Mr. Alan H. Huber the use of his enhanced dispersion parameters in the CRRIS/ANEMOS context. Mr. Huber agreed that this application is appropriate. He indicated that although his corrections were developed in a laboratory environment which approximated slightly unstable dispersion conditions (C), they were generally applicable for estimating the enhanced dispersion from building wake effects during all dispersion conditions.

(Section 2.2.4.2, Paragraph 3)

The first three sentences are somewhat confusing. One possible rewording is the following: Equation (9) should be applied for sources located within $2H_b$ of a building and lower than $2.5H_b$ or, in cases where $H_w < H_b$, a height of $H_b + 1.5H_w$ (Huber, 1979). In these cases H_b should be replaced by H_w in Equation (9) because the width scale H_w is likely to be more significant

(Section 2.3)

The mathematical approach selected is correct and consistent with CRRIS/ANEMOS objectives.

Figure 10 needs to be a better reproduction of the same drawing from the Mills and Reeves (1973) references because some of the "A's" appear to be "delta's."

Several of the radical signs [page 41 and Equation (24), page 42] are drawn incorrectly where: $1/2 \sqrt{A/\pi}$ and $\sqrt{A/\pi}$ are intended. The title on the cover of Draft ORNL-5913 is incorrect.

ANEMOS does not consider terrain effects. A number of methods are available that would be compatible with the current coding and could improve the model significantly. Also, the ANEMOS model is not capable of treating flow reversal effects that often occur in valley flows. During the night and early morning, low level releases would be transported down-valley. After sunrise the flow often refers to the upvalley direction and transports the plume back over the source and contributes to the concentrations from emissions currently released. Terrain and valley flow reversal effects can be significant at some sites and can even have a substantial impact on annual average concentrations.

A more thorough presentation of the theory on which the ANEMOS model is based would be useful. This could be added as an appendix.

ANEMOS, which is recommended for use within 100 km of the source is a statistical model. RETADD-II, intended for use at distances greater than 100 km, is a trajectory model. Both models, according to the documentation, are intended for use in estimating long-term impact (a month or more). This being the case, it seems inappropriate to

calculate trajectories for estimating impact at points beyond 100 km when a joint frequency distribution is used for those points within 100 km of the source.

At 100 km do ANEMOS and RETADD-II models give similar results? Evidence should be provided that the results from both models are consistent at that distance.

(Pages 36-42)

The advantages of the technique used to handle area sources in the ANEMOS model are not apparent. Why not simply integrate over the area source to determine the impact at a receptor (e.g., as in the PAL model)?

(Pages 54-56)

For wet deposition, the ANEMOS model assumes that it is raining constantly during stability categories C and D. These two stabilities account for about 60 percent of the hours in a year, and wet deposition is efficient at removing material from the atmosphere. At most locations, rain occurs during 10 percent or less of the hours in a year. Therefore, this will result in overestimation of deposition near the source and underestimation at more distant points.

Some of the tables and figures in the ANEMOS document seem to be out of place; that is, they do not follow in the order presented in the text (see page 18 - Table 3 is listed before Figure 5 in the text but follows it in the page numbering).

(Pages 24-25)

An explanation for the selection of the Smith-Hosker values for σ_z should be provided. These are based on surface roughness factors, and comparison with the σ_z values from Turner's workbook, with which air pollution scientists are most familiar, would be useful.

(Page 63 and 73)

The approach used by ANEMOS to calculate doses from finite gamma plumes appears to be of very limited usefulness and yet is by the authors own admission "a very computer-intensive calculation". On page 73, Section 6.2.6, the report indicates that NUMOVP, the number of nuclides for which gamma air dose from overhead plume will be calculated, is equal or less than 5. This represents very limited capability.

A better approach would be to generate correction factors for finite versus semi-infinite plumes for various nuclides, meteorological conditions, and downwind distances. These correction factors could be read by ANEMOS from a file, thus eliminating the need to repeat finite gamma plume dose calculations every time the ANEMOS code is run.

(Pages 74, Section 6.2.8)

The report indicates that the default number of wind speed classes is six, but only five values are listed for UIN. Is a value missing?

(Page 47, Paragraph 2 and 3, and Page 48, Table 8)

The discussion on clearance class does not fit at this location. It is more appropriate for an input in the dose assessment code, ANDROS. However, as the present system is structured, the change to the dose assessment section may be difficult.

(Cover Page)

Use sources, not resources

(Page 8)

It would be clearer if the nature of the dependency of concentration on x and t (through the sigmas) was described.

(Page 31)

This problem regarding the syntax error message.

(Pages 54 and 55)

This is a much better attempt to deal with uncertainty and validity than elsewhere in report, but is confusing because page 54 suggests about an order of magnitude uncertainty, while page 55 indicates several orders of magnitude

(Page 57, bottom)

How much less accurate is fast solution, particularly in comparison to other uncertainties?

(Page 49, Equation 33)

Decay and daughter ingrowth during downwind transport of activity is stated to be calculated using a matrix form of equation 33. It is unclear how this calculation is set up to be consistent with the solution to the basic differential equations for chain decay. Even if the assumption is made that distance is equivalent to time (constant windspeed), the system of equations does not have constant coefficients, because the effective removal rate constant for dry deposition is a function of time (distance). The description of the decay calculation scheme indicates that the method of solution requires constant coefficients (i.e., decay constants). The RETADD-II report describes a solution to the problem which involves evaluation of an effective dry deposition removal constant for each time increment. Is this method also used by ANEMOS? Does ANEMOS use one time increment or multiple time increments?

(Pages 15-20)

The reference to the roughness length, Z_0 , is somewhat confusing to the nonexpert who will naturally think of Z_0 as the actual roughness height, h , and will be alienated by the small numbers assigned to Z_0 .

(Pages 15, Equation 6)

I suggest that a definition of Z_0 be given at page 15 instead of p. 43. Equation 6 applies only within the boundary layer. A provision in the program for an upper limit of Z_2 and subsequently U_2 is required (i.e., $Z_2 < \Delta$; $U_2 < V_0$ where Δ is the b.l thickness and V_0 the free stream velocity).

(Page 24, Line 2)

Change "Basic Parameters" to "Basic Parameter" to be consistent with the table of contents.

(Page V and 34, Section 2.2.6.1)

Change "Gravitational Fall" to "Gravitational Effects."

(Page 41, Last Line)

R and r denote the same radius. Change all "r" to "R", or better (although it takes more changes) change all R to r to distinguish from the annual rainfall which is also denoted as R on pages 56, and 117.

(Page 52)

The curves of Figure 11 are for stable stratification and a friction velocity of 50 cm/s -- seemingly almost a contradiction in terms. This value of the friction velocity is representative of 5 - 6 m/s winds over grassland in daytime. Even over forest, 50 cm/s is hard to find at night.

(Page 53)

A von Karman constant of 0.35 is quoted. Since I was part of the team that conducted the Australian (and Russian) experiments that succeeded in verifying that $k = 0.4$ (actually, 0.41 now seems better), I am sensitive about the acceptance of $k = 0.35$ on the basis of one single American experiment. The experimental evidence drawn from around the world is overwhelmingly in contradiction to 0.35.

(Page 53)

This page gives some recommendations regarding V_d . For reactive gases and for nondepositing gases, the values seem about right (although the diurnal cycle is such that all V_d might well approach zero at night and hence the tabulated values are probably intended to be long-term averages). The value for methyl iodide seems very low.

(Pages 54 and 56)

The discussion of wet deposition clearly differentiates between in-cloud rainout and sub-cloud washout processes, of which rainout is the more efficient. Yet the equations are descriptive of washout, not rainout. No reason is given. I suspect that the model will cause wet deposition to be underestimated in all areas where precipitation is dominated by convective processes.

There are some earlier difficulties that I have with the model, that perhaps might be of interest, in addition to the problems mentioned concerning the deposition parameterizations.

I am a complete disbeliever in the power law wind profile. It was developed as a direct result of the availability of log-log graph paper to those early English workers who first mounted two anemometers on the same tower. The power law profile was effectively disproven by Len Deacon in the Thetford experiments in the early 1930's. The power law survived only as long as workers could only afford two anemometers. As soon as Len erected a third anemometer, a straight line on log-log paper no longer worked.

In theory, the power law profile has few redeeming features. It is trivial to show that at neutral the exponent at height z is related to the friction coefficient C_f and the von Karman constant k via $q = C_f/k$. A quick bit of arithmetic shows that the exponents quoted by Irwin are the local slopes at some height near 50 m (for the neutral case). The values quoted for E and F stability appear to be computed for some other height. All in all, I am not a fan of this approach. The errors appear large, and the consequences are difficult to assess. There are better ways to go, so why not use them?

I feel that the state of the science has progressed far beyond the A - F stability categorization schemes. I would be happier if objective, quantified stabilities were used, based on properties such as Z_i/L , Z/L , R_i , etc.

(Page 8, Top)

y and z should also be defined here.

(Page 8, Equation 3)

Rewrite the equation so that the variables included in the radical can be seen.

(Page 10, Middle paragraph)

Rather than use $m = 10$, a value of 3 or 4 is probably quite sufficient. We check the sum of the four terms on each successive iteration and if insignificant, we drop the calculation by adding in these last four terms.

(Page 12, End of second paragraph)

Hourly data used 16 sectors for wind direction through 1964. Since that time, 36 wind directions have been used. The National Climatic Center has used 16 sectors in preparing joint frequency distributions of wind direction, wind speed, and stability converting the 36 directions to 16 directions since 1964. This resolution should be sufficient.

(Page 13, Definition of Q')

Much later in the document it is revealed that the direction, speed, and stability frequency is incorporated in this variable. There should be a few words of explanation here as to the complete contents of Q'.

(Page 13, 2/3 down)

What is the "multiplication factor"?

(Page 13, 4th line from bottom)

States, "there will never be uniform vertical mixing..." The point that is being attempted to be made is not clear from the statement. Uniform mixing vertically through the mixing depth is quite common.

(Page 18, Line 3)

If z_0 not less than 0.01 m to be used implies that ANEMOS is not to be used over sea, sandy desert, or short grass. (Table 3) Is this the intent?

(Page 18, Middle of page)

Won't weird results occur if z_1 is set to 10 m when the input data actually are from a height lower than 10 m?

(Page 31, 3/4 down)

Does part of the material written here (syntax error, etc.) result from the word processor creating the text?

(Page 41)

A strange notation $A/\pi/2$ is used twice on this page and once on the next page. What is meant? Should this perhaps be written as $A/(2\pi)$

(Page 43, Line above Equation 27)

Should "u'" be "u*'"?

(Page 46, Line 8)

Would be clearer if replace "size with "particle size."

(Page 49, Equation 33)

Need to define u_r in list below this equation.

(Page 49)

As stated in comments related to page 13, need to hint at some of the material that is given on this page back on p. 13.

(Page 57)

Would it be clearer to replace "all sources" with "all elements"?

(Page 70)

Would this diagram be clearer if the boxes labeled 21 and 22 were changed to "Point Output File" and "Sector Output File"?

(Page 72)

The descriptions accompanying the following names are not clear and need further elaboration: HTMAX, DSKIP, CONSHT, and AVGG.

(Page 74, UIN)

There are five speeds given here; I thought there were six classes.

(Page 75, Line 2)

Is the notation in parenthesis supposed to indicate that the entire array is set equal to zero.

(Page 91, Bottom)

OUTHG1 should be with its description on the next page.

This document describes the computer code that performs the atmospheric dispersion calculations much like NRC's XOQDOQ. It has many of the same features but superior dry and wet deposition and depletion routines. It also considers the effect of "capping" inversions above mixing zones which the NRC code does not. It makes use of the straight line Gaussian plume model which is adequate for reasonably level terrain, but inadequate for such areas as river valleys. It will handle finite plume gamma dose rate calculations which XOQDOQ does not.

It is not clear whether ANEMOS will handle multiple sources or whether multiple runs must be made with the outputs combined by SUMIT. (Section 2.5, page 45 says the latter.)

User input requirements are extensive. Considerable study and training is required to run code and understand output. It is hard to understand why so many options are incorporated in the code.

(Pages 9-17)

Is it possible that the size of this code and the online computer memory it requires (900K) is significantly affected by the "Multiple Operating Source" feature? If so, consideration should be given to limiting it to a single source per run with output to an on-line file(s). The combining could be (and may be) done by SUMIT.

The summation symbols are missing in Equation 3.1.

The "straight-line Gaussian plume" model is adequate for reasonably regular terrains. Locations such as river valleys (sites of some commercial power plants) require the use of such models as "puff advection."

(Pages 13-14, Section 3.4)

This is a good example of the burden placed on the user in selecting options and specifying input parameters, not to mention the size of the computer program. Are all the options really necessary? Example: Large particles and plume "titling." Large particles are not tolerable for significant lengths of time since they indicate a breakdown in the off-gas clean up system. They are not efficiently inspired and "fallout" near the source, principally within the exclusion area.

(Page 16-17, Section 3.6)

The user should be warned that this is a time consuming calculation (graphical integration). It should definitely be limited to noble gases by the program. One cannot help suspecting that there are "approximate" methods available that would prove adequate for the limited cases requiring them.

I have spent a good deal more time than I wanted to on the documentation received from ORNL concerning the CRRIS code. In general, CRRIS seems to be a standard Gaussian plume model, presumably with additional bells and whistles to make it somehow specially appropriate to the concerns at issue -- the long-term dispersion of slowly-emitted radionuclides. I could not find a clear listing of these bells and whistles. Consequently, I suspect that the model shares precisely the same faults as its kin. It is properly applicable only in conditions such that straight-line transport is a respectable approximation of the average velocity field. This eliminates application in areas of complex terrain and coastal regions. It also means that the distance of application should be small in comparison to one day's transport.

I suspect that the legislative mind-set of regulatory agencies (especially EPA but also NRC) causes fundamental difficulties for all of us who are involved in model development. In essence, there is a strong reluctance for any regulatory agency to accept a new modeling approach, once an initial approach has been "blessed", even though the initial technique may have been put forward only as a stop-gap measure in the first place. The Gaussian plume methodology was developed at a time when direct measurements of several fundamental properties were not yet technologically feasible, and expressions for wind speed are probably not generally applicable under stable and extremely stable conditions because of the likelihood of multiple layers in the atmosphere, with very strong wind direction and speed shears. Long-term time averaging probably helps with these phenomena except when they occur regularly because of some local peculiarity (terrain effects, for example). Some caution seems advisable.

Thus, it is an unfortunate reality that scientists working at the forefront of model development are sometimes forced to cast their advances in the context of obsolete philosophies and approaches. I am reminded that the legislation enacted in some countries in response to the general acceptance (in the late 1800's) of the phlogiston theory of disease transfer is still in place, and that modern office and home design must still meet the standards imposed by that theory. Scientists, engineers, and architects must all bastardize their own knowledge of their own specialties in order to satisfy the requirements imposed by the legal acceptance of early temporary measures imposed in response to an incomplete understanding of the relevant natural phenomena.

In the context of the CRRIS model, I detect evidence that an already accepted modeling approach has been adapted to address a new problem, rather than starting with a new methodology that might be more appropriate. There may be few significant differences between the outputs of the old and new approaches in this particular application, but we must expect that this will need to be demonstrated to those who criticize the basic foundations of the straightline dispersion methodology and question its applicability to areas in which radionuclides emissions are likely.

Scientific credibility and the defensibility of the scientific product are at stake. Those who developed CRRIS are in danger of needing to support an approach which is very easy to criticize now that new generations of measurement techniques are in use and the resulting data are widely known. I would not enjoy being in this situation.

First, I cannot endorse the continued use of the outmoded equation for final rise of buoyant plumes in A-D condition from the 1970 Clean Air Congress (Briggs, 1971). This is adopted with some modification in the ANEMOS document as Eqs. 13-15. However, these equations increase the final Δ_h by including a momentum term that did not appear in the 1970 recommendations for final, buoyant plume rise; this increase can range up to +26%. The 1970 paper gave Eq. 13 with the possibility of

both $F > 0$ and $F_m > 0$ in the rising stage, but for final rise either F or F_m was neglected, whichever was the weakest term, as there is no experimental proof that the terms combine in this case.

Beginning around 1970, very substantial advances were made in the understanding of atmospheric boundary layer turbulence; much of this improved understanding was applied to final rise predictions for A-D conditions in Briggs (1975) and (1984). These are the most important changes from the 1970 model. The 1975 paper is referenced here, but it is not used in any significant way. To reference it in the context of p. 29 implies the use of a more advanced state-of-the-art than the ANEMOS code actually uses.

My present recommendations for final use of buoyant plumes are derived from the 1975 and 1984 papers and are set in their simplest forms in a memo to D. B. Turner dated April 1983 (see Appendix A). Very briefly, two equations are needed for A-D stability conditions. For A-C conditions (unstable), the most elementary form of the equation is $\Delta h = 30(F/u)^{3/5}$ (mks units). However, because this assumes a standard value of the surface heat flux (responsible for the ambient turbulence which limits plume rise), for mid-latitude applications it can be improved by making seasonal adjustments of +30% (winter) and -30% (summer).

For D conditions (neutral), the simplest equation that can be recommended, provided that $H > 0$, is $\Delta h = 30(F/u)^{3/5} H^{2/5}$ (any consistent units). Use of u at the source height, rather than at the plume height, actually improves the fit of this equation to the more exact formula. Because ambient turbulence and plume rise in D conditions is affected by surface roughness, Δh could be 30% higher than that given by the above equation in open countryside with low or little vegetation, and it could be 30% lower in very rough areas (urban and forested). The 1970 equation assumed that Δh is proportional to u^{-1} for both neutral and unstable conditions. This is too much an oversimplification, and is particularly apt to lead to the underestimating of surface concentration for $H < 100\text{m}$ sources and high wind speeds. With moderate wind speeds, it is apt to overestimate concentration (see Table 2 of Appendix A).

Another issue is whether capping effects should be included for the unstable cases. Above the mixing depth, z_i , the air is stable, regardless of the stability at source height. If z_i exceeds the source height, H , by only 0 to 300m, a buoyant plume may be able to escape the mixing layer by penetrating the stable capping layer, depending on F , u , and the strength of the stable capping; methods for calculating the degree of penetration were given in Briggs (1975) and (1984). More often, z_i exceeds H enough that no significant penetration occurs; in such cases, lidar observations have shown that Δh does not exceed $0.62(z_i - H)$, as the plume top "bumps" against z_i (Briggs, 1984). However, there are other significant buoyancy effects on dispersion when "bumping" occurs.

The residual buoyancy of the plume causes increased lateral spread, like that of hot smoke on a ceiling, and delays downward diffusion (Briggs 1985). These effects can be modeled in terms of F , u , z_i , and the convective scaling velocity, w_x . These phenomena require more complex modeling than the conventional two-step approach, $h_e = H + \Delta h$ followed by passive diffusion. Ironically, models which ignore z_i altogether may do better in this case than those which include capping at z_i but ignore the residual buoyancy effects. Therefore you should evaluate whether this likely to be an important case in present applications or not (for long-term dose calculations, I think not). If not, then you may not want to add the modeling complexity that proper solution of these cases demand.

It is also within the prerogative of ANEMOS authors to stay with the cruder 1970 equations for final, buoyant plume rise, if Δh is not a critical consideration in the overall model. If this path is chose, the following comments apply to the ANEMOS document:

(Page 29, Section 2.2.5.1, Line 3)

Cross out 1975. None of the 96 equations from this paper are used, except for those essentially similar to those in the 1971 references.

(Page 30, Equation 11a)

I calculate transitions at $F_c = 0.07216 x^{4/3}$ and $0.01395 x^{5/3}$, in which case the numbers should be rounded to 0.072 and 0.014. The numbers should be checked.

(Page 30, Equations 13-15)

As mentioned in the first paragraph of this review, when $x' = 3.5 x^*$, these equations can give up to 26% larger Δ_h than recommended in Briggs (1971) unless F_m is set = 0 in this case.

(Page 31, Equation 16)

The "1/3" exponent applies to the whole right side rather than to the \cos^3 argument. Alternatively, cross it out and change the left side to Δh^3 . The β_1 in the F_m term is not consistent with Eq. 13, where it was β_j .

(Page 31, Equation 17a)

The correct definition of S is:

$$S = (g/\theta_a) \partial \theta_a / \partial z$$

$$= (g/T_a) (\partial T_a / \partial z) + 0.98^\circ \text{C}/100 \text{ m.}$$

This differs from Eq. 17a by the factor $T_a/\theta_a = (p/1000\text{mb})^{0.14}$, which is not significant at low altitudes.

(Page 32, Top Line)

This is true for β_j in Eq. 13, but not for β_1 in Eq. 16 (which really should be β_j). I believe that Hoult and Weil (1972) are the ones who suggested that $\beta_j = 0.6$.

(Page 32, Lines 3-5)

This statement is too broad. Eq. 13 has been validated for both buoyant and nonbuoyant plumes (although only laboratory data are adequate for validation for nonbuoyant plumes). I would say instead that "Eq. 16 is a theoretical formulation with partial experimental validation; however, for nonbuoyant plumes there are no appropriate data available for validation."

(Page 7, Paragraph 1)

Gaussian plume models are not "state of the art", as I am sure the authors realize. The last sentence in the paragraph is a poor justification. It suggest their use is acceptable only when the environmental effect is minor.

(Page 8, Last Paragraph)

The use of a reflecting solid lid is unrealistic and will contribute to overly large surface concentrations, particularly at long distances from the source and convective conditions.

There is precedent for assuming multiple reflections from the mixing layer and the ground, but it appears to lend legitimacy to a necessary evil, i.e., the errors introduced by the first 2 reflections will not be alleviated by adding additional ones.

(Pages 15, Paragraph 3)

The power law profile is not as rigorous as say similarity scaling but is probably OK for applied models.

(Page 31, Equation 17a)

What does "syntax error..." mean?

(Page 32, Last Paragraph)

...Equation (18) should not be...

(Page 45, Second Paragraph)

Why justify how TERRA models resuspension? The report is about the ANEMOS model.

(Page 50, Section 2.6.1)

It would be useful if ANEMOS could calculate the deposition velocity given particle size, density, composition, etc. Many users would appreciate default values.

(Page 65, Section 5.1)

A comparison of this model with data would establish confidence in the overall code quality. The authors' discussion of the accuracy of Gaussian models in general does not necessarily describe how all their model will perform. A comparison of the model with data and/or other model results should be a part of the document.

(Page 67, Section 5.2)

See the comment regarding Section 5.1

(Pages 68-88)

The description of the computer code is good as far as it goes. Important questions such as: Is the code clearly and logically written? Has sufficient care been exercised to minimize programming errors? Is it modifiable?

(Page 95, First paragraph)

ANEMOS does not break new ground with respect to diffusion meteorology. Nevertheless, it could still find many applications, provided the nuclear safety community was convinced of its reliability. It is this area that could have benefited from additional tests.

(Page 59, Last Paragraph)

How do these calculations compare with Regulatory Guide 1.111 under various stability categories as function of distance?

(Page 60, Equation 47)

Are values of λ^w available with this report?

(Page 66, footnote)

How are the "extremes of a log-uniform probability distribution" used to obtain the variance?

The basic dispersion model is fairly routine and non-controversial. The text does not list the mean wind speeds used as default values for the various speed classes in the discussion of these; one has to go all the way to page 74 to find them. This seems needlessly obscure.

The model uses the P-G "category F" wind speed power-law exponent in "category G" as well. It should be made clear that power-law expressions for wind speed are probably not generally applicable under stable and extremely stable conditions because of the likelihood of multiple layers in the atmosphere, with very strong wind direction and speed shears. Long-term time averaging probably helps with these phenomena except when they occur regularly because of some local peculiarity (terrain effects, for example). Some caution seems advisable.

Briggs has undoubtedly thoroughly reviewed the plume rise expressions, and I will not attempt this. However, I wish to point out some extra text on page 31, immediately before Equation 17a. Also, Equation 17a has a typographical error; temperature should be in the denominator. This entire section would benefit from some brief discussion of the conditions and restrictions appropriate to the various equations.

Stack tip downwash is apparently used only when building wake effects are not included, allegedly because downwash is implicitly included in the building wake calculation. I am not sure this is correct. If I recall correctly, Huber and Snyder's wind tunnel work underlying their building wake model did not deal with a range of effluent exhaust speed to wind speed ratios. Further-more, stack tip downwash is a very local effect. It is another form of wake-induced perturbation, in that the plume is drawn down into the wake of the stack if the plume's exit momentum is too low. It seems to me that it may always occur, although it may be difficult to calculate because the wind speed at the stack tip is often poorly known. I will listen to contrary opinions on this point. Anyway, this may be a fairly small effect compared to the serious vortex-induced downwash that can occur on the roof and in the lee of a building when the wind approaches at an angle; ANEMOS does not even deal with this case, so an extended discussion of stack tip downwash is probably unwarranted.

Terrain effects seem to be treated by subtracting one-half the maximum terrain height between the source and receptor from the effective stack height, subject to the requirement that the result must be greater than or equal to zero. What happens if the terrain is more than twice as high as the effective stack height? Presumably ANEMOS is not an appropriate model for such cases; a plume impaction model is needed instead.

(Page 43)

The reference to Sehmel and Hodgson (1980) is not very appropriate for Equation 25; any standard text on micrometeorology will give this expression. The use of 0.35 for the von Karman constant remains controversial; 0.4 might be preferable, and is adequately accurate in this section, considering the other uncertainties.

In the first full paragraph the statement about "relations similar to Equation 25" seems to imply that the stability-dependent results were derived specifically for use in ANEMOS by Businger et al.; they were not.

In the discussion of saltation preceding Equation 27, the term u' should be u'^* .

(Page 44)

Equation 29 is superfluous.

In Equation 30, K should be k .

The section on source term modifications also has a few questionable areas:

(Page 51)

The discussion of the method for estimating V_d and V_g for particles is far too terse, especially since the reference for the V_g procedure is a "personal communication" not available to the reader.

(Page 53)

With regard to the discussion of the dry deposition of gases, it should be noted that this is a topic of considerable ongoing research. While good values for V_d are difficult to obtain for more than a few chemical species and receptor surfaces, it has become clear that the values depend on the type and extent of surface cover, surface wetness, gas reactivity and solubility, and a host of other factors. The few values for V_d suggested in ANEMOS (borrowed from AIRDOS-EPA) do span the likely range, but don't offer much explicit help to the unwary user.

(Page 55)

Dennison's name is misspelled.

Heffter et al. (1975) and Draxler (1976) used Equation 37 long before Hosker's review article appeared.

(Page 55)

ANEMOS assumes that the default value of the wetted plume thickness is the "lid height"; is this term equal to the mixing layer depth discussed earlier in the text and shown in Figure 3?

(Page 56)

The method used in Equation 38 to calculate precipitation rate distributes the rain evenly over the entire year's quota of near-neutral and neutral conditions. This produces low values of J_0 that occur over a relatively large fraction of the year. Actually, precipitation

occurs in episodes. Can the authors demonstrate that their computationally simple method is equivalent in the amount of material scavenged to the more complicated but physically realistic method of summing over all rain events using actual precipitation rates?

(Page 57)

Evaluation of the dry deposition depleting integral is discussed here. ANEMOS offers user options for Simpson's rule and for a faster but less accurate approximate method. It should be noted that the integrand involves the product of σ_z^{-1} and $\exp(-h^2/2\sigma_z^2)$; because $\sigma_z(x)$ goes to zero as x does, the integrand may be very sharply peaked close to the origin. My own experience with dry deposition depletion integrations suggests that a very careful (fine mesh) numerical evaluation is necessary near the origin, and that a non-zero lower bound on σ_z (perhaps related to the initial plume diameter at the stack) may also be a good idea. It is not clear whether either or both integration options in ANEMOS treat this problem.

(Pages 65-67)

The discussion of ANEMOS accuracy seems adequate as far as it goes. In particular, I agree with the authors' reluctance to ascribe any particular accuracy to the dry deposition calculations; there is simply too much uncertainty in the science at the present time. Can the authors say anything about wet deposition modeling accuracy for this long-time-average approach? Also, a comment about the usual difficulty of accurately specifying source terms might be appropriate, although it may be easier to do this for radionuclides than for simple chemical releases such as combustion products.

(Page 28, Section 2.2.4.2)

In estimating wake effects, the method of Huber and Snyder (1976) and Huber (1979) is used. In ANEMOS, this model adjusts the vertical diffusivity (the sector-averaging removes the crosswind diffusivity) according to the size of the obstructing building for receptors within about 10 building heights of the structure, and uses a diffusivity appropriate to an equivalent virtual source further downwind. Some restrictions and modifications apply under certain circumstances, and these are adequately described in the ANEMOS documentation.

Several things should be kept in mind about this model, and indeed about the entire difficult problem of accounting for building wake influences on dispersion:

First of all, the most practical problems will be associated with a group of buildings, rather than a single isolated structure. A large number of models have been suggested in the literature for estimating concentrations in the intermediate and far wakes of buildings; none of these distinguishes between a single building and a building complex. In particular, the effects of jetting of flow between neighboring structures and of the organized vortices in their wakes are presumed

to have dissipated at distances beyond a few building heights downwind. This premise may be valid in many cases, but building arrays that produce very strong jetting, or vortex wakes, or that contain a few very large structures may violate the assumption. The models cannot be legitimately applied in such cases.

The practical question arises of what to use for the effective height and width of a building complex. The height and width of the crosswind-projected area of the complex have been suggested, but because of the complicated flows around and through the complex, a convincing answer cannot be given. This makes such wake models difficult to apply with any degree of confidence; the ANEMOS scheme is no better or worse than any other in this regard. However, a few words of warning to the user should probably be inserted in the text.

At least one fairly extensive test (Fackrell, 1984) of the commonly-used Gaussian-based wake concentration models against laboratory and field data revealed no "best" model. The wake concentrations seem to depend more strongly on building geometry than these simple models would predict. Fackrell's study suggests that a slightly simpler diffusivity adjustment due to Ferrara and Cagnetti (1980) works well for ground level releases when the wind approaches at right angles to the building or cluster. A slightly more conservative (predictions higher than observed) result is obtained with Barker's (1982) virtual source model, which, by the way, has recently been recommended (Jones 1983) for routine radiological releases estimates in the U.K. For elevated releases, models (such as the Huber-Snyder model, as well as others) that can account for the effective source height at least show the correct trends in concentration with changes in building geometry, even though the predictions may err for very wide or very narrow buildings. None of the models tested by Fackrell attempts to predict the dependency of wake concentration patterns on incident wind direction, even though significant increases in the ground level concentration may occur because of enhanced downwash when the wind approaches at an angle to the building. The simplest conservative way to account for the effects of wind at an angle to a building is to assume that a rooftop or stack source is actually located at ground level behind the building, regardless of the wind angle.

Fackrell (1984) and others have pointed out the great scatter in field measurements of wake concentrations. The scatter can be as much as two orders of magnitude, even under nominally identical wind directions and stability conditions. The reasons for this scatter are not clear, but they may be related to phenomena such as slight shifts in plume transport relative to discrete sampler locations, as well as the normal variability of atmospheric conditions within a specified class.

The above discussion is meant simply to indicate that the effects of building wakes on pollutant concentrations can be quite complex, and that no present-day models can provide adequate predictions in all cases. The uncertainty associated with such predictions must be assumed to be rather large. In particular, certain wind directions may give rise to much higher concentrations than others. The model used in

ANEMOS cannot cope with these complexities--but neither can any other. The Huber-Snyder method has its good points, but the user of ANEMOS should also be aware of its substantial short-comings. These should be described in the text; presently, they are not.

(Pages 36-42)

The method for dealing with area sources follows Raridon et al. (1984) in mapping the real source on to an equivalent area shaped for use with the polar coordinates appropriate to wind rose-based calculations. Unfortunately, while the transformations shown in ANEMOS are demonstrably mathematically consistent, the reader is never given a clue as to the rationale behind them. The casual reference to earlier ORNL reports is not a satisfactory substitute. This section could usefully be rewritten following Raridon et al. (pages 4-9). Incidentally, there appears to be no reason to designate the subtended angle of the equivalent area source by ΔT ; the angle is not an increment, nor is it necessarily small. The delta prefix should be dropped for clarity.

(Page 49, Equation 33)

It is stated that radioactive decay and daughter ingrowth during downwind transport are calculated using a matrix form of Equation 33. It is not clear how this calculation is set up to be consistent with the solution to the basic differential equations for radioactive chain decay. Even if the assumption is made that distance is equivalent to time (for a constant windspeed), the system of equations does not have constant coefficients, because the effective removal rate constant for dry deposition is a function of time (distance). The description of the decay calculation scheme indicates that the method of solution requires constant coefficients (i.e., decay constants). The RETADD-II report describes a solution to the problem that involves evaluation of an effective dry deposition removal constant for each time increment. Is this method also used by ANEMOS? Does ANEMOS use one-time increment or multiple-time increments? Further explanation is needed.

I have reviewed the ANEMOS document. Comments are on separate sheets. From the listing of the test run, there appear to be some problems with the code since there are 5 occurrences of underflow and complaints concerning RELS during, what appears to be, the compilation. Information on running time is desirable.

MLSOIL AND DFSOIL

The following are reviewers' comments related to the CRRIS models, MLSOIL and DFSOIL, (ORNL-5974):

These two programs in combination compute the dose rate, one meter above the ground from radioactive material that is deposited thereon and migrated into the soil. A good summary is provided in the paragraph starting in the middle of page 4.

The "leaching coefficients" (transfer rates) are estimated by eq. 2.2 (page 9) from soil/water "distribution coefficients" which are available for far more elements than are transfer rates. See the first paragraph on page 10. More comparisons of this type should be done. Agreement in Table 2.1 is not very impressive. Values of K_d used are presented in Table 3.3. (page 32) but no reference is given. No doubt they are from reference 1 (page 47). This is probably as reasonable an approach as can be devised for a large number of elements, but caution is in order per ORNL-5786, (SITE data base) Section 2.4.1.

The model used by MLSOIL for the transport of radionuclides in the soil does not take into account any upward movement of the nuclides. Our measurements of the activity of naturally occurring radionuclides such as Pb-214, Ac-228 and Th-232 seem to indicate that these are distributed more or less uniformly in the soil. This would be the case if upward transport mechanisms existed, which is almost certainly true. The action of worms and plants, especially in the upper soil layers, would seem to be important. A comparison of calculated activities and measured activities at the same site would be helpful in assessing the validity of a model which ignores all possibility of upward transport.

The upper compartment in the MLSOIL model is the soil layer from 0-1 cm. At forest sites this layer will be mostly litter and humus, that is mostly organic matter whose density will be much lower than that of the underlying soil. Use of the default soil density 1.35 g/cm^3 will be inaccurate at these locations. Table 2.1, a comparison of measured and computed leaching coefficients for plutonium shows remarkable disagreement. The best agreement between measured and computed values is to within an order of magnitude. Some comment on the sensitivity of the computed effective ground surface concentration to the values of the input parameters perhaps would be helpful.

We question the extensive degree of effort which has been made regarding modeling of direct radiation doses from ground contamination. For the nuclear power plant industry, dose due to direct radiation from ground contamination is almost always at least an order of magnitude lower than projected doses from other, more critical ingestion and inhalation pathways.

(Page 7, Paragraph 1, Line 8)

It is stated that in modeling radionuclide transport in soil, "there is no upward transfer." What about production Rn-222 from Ra-226 which has deposited onto the ground or leached into soil? Rn-222 so produced would diffuse upward and out of the ground, subjecting individuals to inhalation exposure and enhanced direct radiation. An example of such a pathway might be Ra-226 released from coal plant stacks.

(Page 11, Table 2.1)

The agreement between measured and computed plutonium leaching coefficients appears to be rather poor. Were no measured data available below 30 cm?

Why isn't the available data used in the model, given the apparent error in the computed leach coefficient?

Appendix H and Errata

What is the explanation for the differences in dose conversion factors given in Appendix H and in the Errata dated November 6, 1985? Values in the Errata appear to peak for Layers 1-2, whereas values in Appendix H generally increase with increasing layer number.

(Pages 35-38)

It would appear that this code could also be used for calculating the root zone concentration in undisturbed soil (orchard crops, some commercial grazing land, all game animal grazing land).

(Page 36, Section 7.2)

It's interesting they do not mention I-129, but let's not raise that red flag.

(Page 37, Figure 7.1)

It's hard to believe that radionuclides below 15 cm of soil could contribute much to the surface dose rate. Are the bottom two zones really necessary?

Except for post operating periods, the soil pathway contributes little to the total dose to man.

PRIMUS

The following are reviewers' comments related to the CRRIS model, PRIMUS, (ORNL-5912):

This program constructs a decay chain data library for use by the other programs in CRRIS. The mathematics involved is "textbook" but complex and nearly impossible to check manually. Hopefully the input/output of PRIMUS has been checked against other similar codes.

Has consideration been given to identifying and/or eliminating daughter products and/or decay chains which contribute little to the final dose estimates? The complexity which these decay chains impose on subsequent calculations is quite significant.

The presentation in chapter 1 of the relationship of PRIMUS to the other modules confused me. On page 1 it is stated that "PRIMUS...is needed...by all...except RETADD-II and DFSOIL." However, the chart on page 2 shows an arrow into RETADD-II from PRIMUS. Page 4 says that ANEMOS can also produce the decay matrices and that if RETADD-II is run rather than ANEMOS then PRIMUS must be run. This implies that ANEMOS, rather than RETADD-II has PRIMUS capabilities.

(Pages 7 and 10)

There are two separate vectors given the symbol S in this write-up (deposition rates, page 7, and source vector, page 10).

(Page 7, Equation 2)

The sign of S in equation 2, page 7 seems to be wrong. If S is a deposition rate (positive if stuff goes from air to ground) then the sign in equation 2 should be a minus.

(Chapter 4)

The question of exactly where are the deposition rates entered and where the coupled differential equations solved is not presented clearly in this write-up. One must interpret internal evidence. There is no provision in the run input (chapter 4) for deposition. The output record seems to have space allocated for it (chapter 5.3, variables DRY and WET) which I guess are filled at the appropriate times by other modules.

(Chapter 2.3)

Chapter 2.3 discusses the decay calculations. The deposition rate (including resuspension or negative deposition) is not included. PRIMUS does not actually do the decay calculations, and in fact cannot because the solution of the differential equations depends upon parameters (deposition) which are the province of other modules and varies among and within them. It should be clearly stated that PRIMUS passes along

only the decay rate and branching fraction coefficients in the differential equations. A description of how they are solved later is however appropriate in the PRIMUS write-up if a common procedure is used by all other modules.

As a final remark on deposition rates, aren't these usually of a functional form more like decay than constant. In other words, $S=kQ$ rather than $S=Constant$. This changes the differential equations significantly.

(Page 7, Equation 1)

Equation 1 is not intuitively obvious. In fact, at first blush, one would think that the first λ_i should be λ_j . The derivation of equation 1 is trivial and can be presented in two lines. Most users would benefit from understanding it. This is first done by expressing the balance equation in terms of N , the number of nuclei, rather than Q and then doing the substitution $Q_i = K N_i \lambda_i$. (K is a unit dependent constant which will cancel out.) In this context, see also CHAIN6, page 16.

(Page 10)

The rationale for the inequalities on page 10 should be explained to the user. Also, how does one arrive at the numbers 17.329 and 2000? The former is second order?

(Page 13, Equation 21)

What does e^{At} mean when A is a matrix? It's matrix elements should be specified for the reader.

The alphanumeric input file should have been included as an appendix.

The transition from equation 15 to equation 16 could use some explanation. Or, Lee (1976) could be included as an appendix (it looks like it is an unobtainable document).

Overall PRIMUS appears to be an efficient and logical method for accomplishing its stated task.

I would suggest that one review panel look further into the subjects listed below:

- (1) Could KTRUNC, EXPSYM, and the removal of very short-lived nuclides have the effect of eliminating significant nuclides from consideration by the assessment modules?
- (2) What is the proper form of the deposition S-vector, kQ or constant?

- (3) Alphanumeric input file and code validation suites should be designed and implemented.
- (4) Instructions, methods, and displays should be provided to the user which minimize the possibility of operation with unintended input data. The absence of an ABEND only means that the program ran, not that it is correct.

(Page 37)

The sample cases are not useful in following the procedure.

Test calculations provided by ORNL were performed using the PRIMUS and DIG programs. PRIMUS sets up the decay data and DIG performs the decay calculation. The calculated results agree with calculations performed using standard decay and daughter ingrowth programs used at PNL. Minor differences appear to be due to slightly different values used for radiological half-lives for some of the radionuclides. The CRRIS appears to handle radioactive decay and daughter ingrowth properly.

(Page 1, Paragraph 2, Line 8)

The reference to 80 km is not consistent with the values of 50 and 100 km mentioned elsewhere.

(Page 10, Items 1 and 2)

The units for λ_d and λ_p are not specified.

I found pages 5 and 6 to be fairly opaque.

(Page 8, Equation 3)

There seems to be a discrepancy between this equation and the expression for a_{ii} given on page 6 of An Introduction to CRRIS (ORNL-TM-8573).

(Page 10, top of page)

The units of λ_d are not defined here.

RETADD-II

The following are reviewers' comments related to the CRRIS model, RETADD-II, (ORNL/CSD-99):

RETADD-II seems to be an adequate model for the stated purpose of "calculating long-term averages (i.e., a month or longer)" (p. 3, paragraph 3). The state-of-the-art in Lagrangian long-range transport, dispersion, and deposition modeling has changed considerably from what is presented here (e.g., the changes include variable mixing layers, diurnal variations in vertical meteorological profiles, and multiple levels or layers to define vertical wind shears). However, the results of incorporating these concepts into the model for the averaging times appropriate to the stated objectives have, as yet, to be studied. This is not to say that a reevaluation of the model is unnecessary; the newer concepts present more realism and an effort should be made to test them with comparative sensitivity studies to determine if the model should be updated.

Although it is not so stated, it would appear that one of the objectives of the RETADD-II report is that it would serve as a user's guide. If this is not the intention, then a separate user's guide should be provided. To facilitate its application as a user's guide, more use should be made of tables and flow diagrams. For example, the instructions for data input (pages 18 and 19) would be easier to use if they were provided in table format. Documentation should be provided for one or more test cases. This should include copies of both input and output files. In addition, the source code should be provided as an appendix. Guidance should be provided on CPU and storage requirements to facilitate comparison with other models.

Since RETADD-II is a trajectory model, a demonstration that the model will reproduce straight-line model results when the necessary steady-state assumptions are made should be provided.

RETADD-II estimates the trajectories of releases using upper air wind data. Except for fossil fuel power plants, most releases of radionuclides are ground-or near-ground-level releases, but no explanation is given for using only upper air data.

This code performs long range atmospheric trajectory calculations on a regional or U.S. continental scale. Since I have never seriously considered this problem there is little I can offer. Their overall approach does appear thorough and feasible but impossible to validate. User options are minimal which is probably a plus. Needless to say, very large averaging times are required to smooth out the variables on a regional basis.

(Pages 21-22)

The concepts employed are state-of-the-art, but if ever there was a case of zero verification data, this is it. It is a good example of where the "computer age" has led us--something bordering on mythology.

(Page 22, Last paragraph)

This paragraph is confusing. If PRIMUS must first be run to obtain daughter product ingrowth and decay matrices to feed RETADD-II, why does the user have to provide the code with the input indicated in the last sentence? Why can't RETADD-II access the PRIMUS output file?

(Page 19, Card No. 7)

Concentration calculations, even long-term averages, could be highly dependent on the depth of the mixing layer. There is nothing wrong with a constant layer, but what constant layer depth should be chosen to be "representative"? The problems involved in answering this question have led researchers to the concept of a variable mixing depth determined by the model from meteorological input (see, for example, Heffter (1980). Even with a variable depth, concentrations can be overcalculated because of restricted mixing to the ground during the night, hence the desirability of incorporating a surface layer in the computations (or multi-layers). My feeling is that these changes might have dramatic effects on some calculated concentrations.

The concept of a yearly constant precipitation rate is worrisome, even when considering long-term averages. Realistically, precipitation should be considered an episodic process along with the episodic Lagrangian trajectory processes. However, in view of the complexities this introduces in the modeling, I suggest that these processes be approximated in a simple manner of temporal and spacial averaging. Thus, average monthly precipitation rates for specific spacial areas (i.e., 5 degree LAT x 5 degree LON) are easily obtained for input and easily modeled. A further subdivision of monthly averages into weekly, or even cyclic averages, might be considered. Here, once again, very dramatic differences in calculated concentration values and patterns will ensue. It remains to be seen if these differences are significant with respect to the overall problem evaluation.

(Page 17, Equation 16)

The value used for u in this equation, given a changing wind speed along a trajectory, is perplexing. The local wind speed along a trajectory may be reasonable, but the equation was derived based on an initial mean wind speed at the source. Modelers have circumvented this problem by using a finite series of expanding puffs to approximate the plume and thus eliminate the need for defining u . The former approach is difficult to defend; the latter is mathematically solid. It would be interesting to determine the significance of the differences between approaches.

AMEMOS is to be used for distances to 100 km and RETADD-II for regional-scale distances. Since these models are used for calculating annual averages and not to simulate short-term or episodic events, explain why it is necessary to calculate the doses past 50 km.

The mathematical models employed are appropriate for the simulation of long-range transport of radionuclides. The only comment I have is that reference should be given to any attempts to validate (even partially) the model against "real" data. Also, information on running time would be useful.

I found the discussion of the calculational limits of RETADD-II particularly confusing. In the CRRIS summary (ORNL/TM-8573) on page 21, RETADD-II is described as a long-term model, suited for time periods over a month, reliant on historical averages of weather data for the appropriate season or month. However, the RETADD-II report appears to me to refer to real-time data, updated on a 6 hour basis, and limited by computer capacity to a trajectory duration of 5 days. This reference to the 5 day computational limit is noted in the description of data inputs to the model; it would have been helpful if this were also addressed in the description of how RETADD-II works.

(Page 14, lines 6 and 9)

Change level depth(s) to layer depth(s) [a level can't have depth]; change midpoint to midpoint between wind levels.

(Page 16, middle and bottom)

Closest (time) and second closest (time) should be changed to second closest and third closest. For example, a 3-hr trajectory segment starting 06Z uses 06Z winds (closest time); if no winds available, then 12Z (second closest time); again, if no winds available, then 00Z (third closest time).

(Page 7, Equation 4)

i in Q_i refers to time as in t_i , not to isotope i ; confusing notation.

(Page 18)

What is typical geographical size of grid cells?

(Page 9, Last paragraph)

The second sentence is not clear.

(Page 10, Figure 3)

Label ordinate "source strength."

(Page 7)

The derivation is not very clear. I would suggest the following rephrasing: For an arbitrary removal rate $\lambda(t)$, the relative change in the effective source strength Q is given by

$$\begin{aligned} dQ/Q &= -\lambda(t) dt \quad \text{so} \\ \ln(Q_i/Q_{i-1}) &= \int_{t_{i-1}}^{t_i} -\lambda(t) dt \end{aligned} \quad (4)$$

Note that there is an error in Equation 4 as presently given in RETADD-II; the upper and lower limits of integration are reversed. This does not affect the subsequent equations.

Assuming $\lambda(t) = a t^{b-1}$, it follows that

$$\ln(Q_i/Q_{i-1}) = \frac{\lambda_i t_i - \lambda_{i-1} t_{i-1}}{b} \quad (5)$$

From the definition of $\lambda(t)$, it follows that

$$b = 1 + \frac{\ln(\lambda_i/\lambda_{i-1})}{\ln(t_i/t_{i-1})} \quad (6)$$

Now define a constant effective dry deposition rate at time step t_i as λ_i^d such that

$$\begin{aligned} \ln(Q_i/Q_{i-1}) &= \int_{t_{i-1}}^{t_i} -\lambda_i^d \\ &= -\lambda_i^d (t_i - t_{i-1}) \end{aligned} \quad (7)$$

Set this equal to Equation 5 to find

$$\lambda_i^d = \frac{\lambda_i t_i - \lambda_{i-1} t_{i-1}}{b (t_i - t_{i-1})} \quad (7)$$

where b is given by Equation 6. From the expression above Equation 2,

$$\lambda(t) = \frac{2}{\pi} \frac{V_d}{\sigma_z} \exp(-\frac{1}{2} h^2 / \sigma_z^2)$$

I see no reason to introduce Equation 7 and the subsequent discussion of the special case $b = 0$. The remainder of RETADD-II seems quite straightforward and satisfactory; it appears to be a useful trajectory model for many purposes.

SUMIT

The following are reviewers' comments related to the CRRIS model, SUMIT, (ORNL-5914):

This is basically a utility code used to combine the output of multiple runs of ANEMOS for different sources and place them on a common grid. My one comment is that in order to achieve flexibility the code is somewhat complex. One wonders if all the user options are really necessary.

(Page 7)

What is the sensitivity of the code to different weighting functions P*? It will be useful for the user to have directives on when each option of P* is applicable.

(Page 12, Table 1)

A figure with a flow-chart at subroutine level would be more useful than this table.

(Pages 7-10)

More details on the interpolation between source grid points to access the methodology are desired.

(Page 19)

SUMIT appears to handle multiple sources evaluated by ANEMOS. The "multiple operating source" provision of ANEMOS are unclear. Can we not settle on a "standard grid" plus specific locations?

We reviewed the documentation for the SUMIT code and had no substantive comments.

TERRA

The following are reviewers' comments related to the CRRIS model, TERRA, (ORNL-5785):

Reiterating previous comments, TERRA should include modeling of doses due to drinking goat's milk and drinking water contaminated by atmospheric deposition. Radioactive feed to milk transfer coefficients, f_x^* for cows and goats should be used. Data are available for milk transfer coefficients for goats (Hoffman and Baes 1979).

The radiological decay and daughter ingrowth are evaluated using a lower triangular matrix method. This method is implemented to solve a system of differential equations for a constant radionuclide source term rate and decay and removal of radionuclides described by constant parameters. The general mathematical equations are described well by equation 1 on page 4 of the TERRA report. Because the decay processor used a constant input rate, additional complexity is introduced into some of the exposure pathway models. For example, the transfer of activity from animal feed to milk requires definition of the standard cow-to-milk transfer factor plus a metabolic turnover rate constant for activity in milk.

(Page 9, Abstract, Paragraph 4, Line 4)

TERRA does not calculate dose due to radioactivity in goat's milk. It is known that the transfer coefficients for certain isotopes, such as iodine, are much higher for goats than for cows. The goat milk-infant thyroid pathway should be included in the CRRIS.

(Page 6, Equation 2.1)

A term appears to be missing. Also, λ_{ri} is not defined.

(Page 7, Section 2.2, Line 3)

The assumption made in TERRA that there is no contribution to total concentration from radionuclides which have leached below the top 1 cm of soil is unfortunate and nonconservative. This assumption could result in the external dose computed from the TERRA ground surface concentrations by ANDROS being underestimated.

(Page 9, Line 1)

Deposition of atmospheric radioactivity onto surface bodies of water is not modeled by CRRIS. In certain cases (for individuals drinking water from small streams or uncovered cisterns), doses due to ingestion of water contaminated by atmospherically deposited radioactivity could be significant.

(Page 21, Line 1)

At various places in the CRRIS documentation, it is stated that ANEMOS is to be used within 50, 80, or 100 km of the source. The exact limit on down-wind distance for which it is appropriate to use ANEMOS should be consistently stated. Conversely, the minimum distance for use of RETADD-II should be consistently specified and it should be demonstrated that the two methods converge at that distance.

(Page 40, Equation 8.2)

B_{en}^a is not defined.

(Page 10, Equation 9)

There appears to be a problem in the description of Equation 9. The parameter f_{tm} is stated to be dimensionless (page 9). However, the equation implies that the parameter has unit of inverse time.

86,400 sec/day

F_m day/kg

m_p kg/day

λ_m per sec

f_{tm} per day

If the equation is based on the assumption that the concentration in the milk is at equilibrium and the activity increase rate is equal to the activity decrease rate (as defined by λ_m), then equation 9 makes sense. A similar problem exists with equation 8 on page 9. It appears that m should be in units of kg/day and f_{tm} is in units of per day (as equation 9 implies).

(Pages 8-9)

It is not clear from this discussion whether processes which remove radionuclides from soil are included.

(Page 10)

Description of F_m as a fraction of daily ingested activity which comes from milk is inconsistent with (d/kg) units

(Page 3, Last Sentence)

If you have site-specific parameters, is TERRA not suitable for an actual site-specific assessment?

(Page 4 Conceptual Models)

TERRA does not consider uptake from undisturbed soil to orchard type crops, pork, poultry or game animal pathways. All of these are important in the Southeastern U.S.

(Page 7, Section 2.2)

The method used for estimating the soil removal constant due to leaching is probably the best approach available for covering the wide variety of elemental forms and environmental conditions to be encountered. But for those cases where the root uptake pathway is a significant contributor to dose, it deserves close scrutiny. ORNL-5786 has a good section on the variability of Kd. Another significant question is the assumption of perfect mixing in the first 15 cm of soil. In short, it is a complex subject deserving considerably more research than it has been given.

(Page 8, Section 2.3, Last Paragraph)

It is not clear what (removal rate constant from plant interiors) is intended to describe. Default value = 0. All that Section 3 contains is a provision for input of some other value by the user. There is no hint as to how the user is to select such a value.

(Pages 8-9, Section 2.4)

They do not appear to have considered the work of Chamberlain and others which relates the interception fraction to plant productivity. However, Baes (Ref. 3, page 28) may have considered it in coming up with the default values. (See last sentence of the second to last paragraph of this section.) This whole topic is discussed in Section 3 of ORNL 5786. The discussion appears academically thorough but requires more time than available to evaluate. It could lead to the design of some valuable research.

(Section 3)

A scan of this section leaves no doubt as to the complexity of the code and the amount of "hands on" training required to use it with any degree of confidence in the meaning of the output.

(Page 1, Last Paragraph)

Food crops and feed crops are considered in considerable detail, and beef from three cattle types are considered. Pork, chicken, and eggs are not, but should be, since they are important in the diet. Radionuclide concentrations in beef may be comparable to those in meat from other livestock provided the animals' diets are similar. The authors might consider this hypothesis. In any case, other animal products should be treated in TERRA.

(Pages 8-9)

Translocation of surface deposited radionuclides from nonedible to edible plant parts should be considered (important for radionuclides such as Cs-137).

(Page 11, Paragraph 2)

The authors note that there is a lack of information on the removal rate for beef λ_b and assign a default value of $5.73 \times 10^{-7} \text{ s}^{-1}$ (equal to a half time of 14 d) for all elements. They then encourage the user to supply his own estimates of λ_b . It seems unfair not to be able to provide guidance and then assign the burden to the user. Furthermore, the authors should discuss the impact of using this default value in TERRA on the resulting estimates of radionuclide concentrations in beef.

(Page 12, Paragraph 2)

It is difficult to grasp this paragraph. One has to go back to page 7 to dig out the definition of t_a and t_b . A comprehensive list of symbols would be useful.

(Pages 13-27)

Lack of time did not permit this reviewer to examine this user's-guide section. However, some general comments may be useful. TERRA like the other codes in CRRIS are rather complex codes, and it seems reasonable that the authors should provide an abundance of guidance to the user or potential user and minimize those situations where he is confronted with a choice. For example, the authors of TERRA and the other codes of the CRRIS should specify those situations where one should select a parameter value to replace the default value and should provide guidance regarding selection.

(Page 5, Figure 2.1)

Why are water pathways (deposition on water supplies; runoff to water supplies; use of these waters for irrigation or animal water supply) not considered?

(Page 12, Equation 14)

Is occurrence or non-occurrence of photosynthesis not considered because of long-term averaging?

(Pages 11-12)

Equation 12 deals with the deposition of resuspended materials. There are two possible problems with this treatment:

- 1) The concentration of m^3 resuspended particulate matter (p_{sus}) is fixed at $15.5 \mu\text{g}/m^3$. Realistically, this must vary at least geographically, depending on soil type, ambient wind speed, and amount of precipitation, even on a long-term average basis. Could a range of be given instead?
 - 2) A single dry deposition velocity of 10 cm/s is used for all particles within the size range 2.5 μm to 15 μm . Actually, the deposition velocity is the result of turbulent transfer (important for small particles) and gravitational settling (important for large particles). Without knowing something about the particle size distribution in any given case, it is impossible to determine the effect of this approximation on the deposition results, but some warning flags should be raised about the uncertainty introduced by this approximation.
-

One general feature I found disturbing was the lack of consideration of poultry, eggs, lamb, pork, etc. in this model. Although it can be modified to handle these foods, I think such common foods should be included in a state-of-the-art risk assessment model. Also, some of the assumptions made about beef, for instance being a weighted average of 3 types of beef, seem unwarranted in many instances--particularly for specific individuals.

Also, I think that the diets of cattle and some other livestock should be time dependent. Although dairy cattle today depend much less on pasture than in the past, family owned milk cows may still be dependent on pasture during the growing season. The change from pasture to stored hay and silage during the non-growing season can have a strong influence on the rate of ingestion of radionuclides.

Also, it appears as though cattle (and presumably any other consumers one wished to simulate) are assumed to ingest no soil. Soil ingestion can be an important pathway in many animals, including cattle, sheep, and poultry.

I could not tell from the documentation whether the model attempted to conserve mass. In particular, I could not determine if the rate of interception of radionuclides by the plant canopy reduced the rate of deposition onto the surface of the soil.

The process of leaf absorption of surface contamination is apparently not considered. Although the rate of absorption of many radionuclides may be low, it does transfer part of the inventory from a compartment exposed to weathering losses to one protected from such losses. The rates of absorption of gaseous forms of contamination could be significant.

(Page 9, Equation 7)

The interception fraction, r^i , in Equation 7 is defined to be crop specific, but not necessarily biomass specific. While the effect of biomass on interception is relatively weak, it can be important to grazers whose forage shows strong seasonal variation. Equation 7 would suggest that, for a given crop, the interception rate is inversely proportional to biomass, i.e., that dense plant canopies would have a lower interception than would sparse canopies. This function therefore seems questionable.

(Page 4, Equation 1)

The subscripts on λ in the second and third terms of the ΔC_k equation appear to be wrong. They should be λ_i , I think.

(Page 8, Equation 6)

The term t_h is time to harvest--relative to what?

The ability to handle decay chains seems most appropriate for natural decay series (uranium, thorium, etc.). How much difference is there for the important reactor radionuclides in using the parent-daughter transport model compared to using the parent only and incorporating the daughter decay energy in the external (shine, ground deposition) and internal dose factors for the parent nuclide (a la Reg. Guide 1.109)?

(Page 11, Equation 11)

It is not clear that the meat transfer model is superior (or even equivalent) to that of Regulatory Guide 1.109 because it has to introduce an arbitrary constant (λ_b) in order to transform the steady state transfer factor F_m into a rate. Allowing the use to override the default value is not really a solution for, as noted, the parameter is not tabulated or otherwise available.

(Page 3, last line)

It would be well to restate here that site-specific parameters can be entered via NAMELIST statements

(Page 4, Equation 1)

The notation $C_i(\lambda_i + \lambda_r)$ is slightly confusing - at first glance it looks like C_i may be a function of the lambdas, like $X(t)$. Suggest if report is ever revised, $(\lambda_i + \lambda_{ri}) C_i$ notation be used.

(Page 12)

Based upon comparison with another tabulation, the f_{wa} values appear reasonable except for grain which seems too low and should be recalculated.

TERRA			Till and Meyer (1983)		
Class	f_{wa}	FW/DW ^(a)	Class	$f_w^{(b)}$	FW/DW
leafy	0.934	14.2	leafy	0.926	12.6
exposed	0.874	6.9	fruits	0.85	5.7
protected	0.778	3.5	root	0.80	4.0
grain	0.112	0.126	grain	0.51	1.1
meat	0.615	1.60	--	--	--
milk	0.870	6.70	--	--	--

(a) FW/DW (fresh-to-dry mass) = $f_{wa}/(1 - f_{wa})$

(b) $f_w = 1/(1 + DW/FW)$

(Page 12)

The carbon content of the atmosphere of 0.18 g C/m^3 is not consistent with the value of 0.16 g C/m^3 used in NRC Regulatory Guide 1.109 and in UNSCEAR 1977, 1982. Examination of one of the original sources of the value indicates that it may have been based on data from a volcanic observatory which might have a higher local CO_2 concentration. For uniformity, the use of the 0.18 versus the 0.16 should be reexamined.

(Page 12, and Table 3.5)

The values for the fraction of carbon in foods appears reasonable except for the values for grains and milk which should be rechecked.

TERRA		Recalculated ^(a)	
	f_c		f_c
leafy vegetables	0.026	leafy	0.026
exposed produce	0.050	other vegetables	0.040
protected produce	0.116	root	0.094
grain	0.293	grain	0.403
beef	0.288	beef	0.228
milk	0.293	milk products ^(b)	0.066

(a) The "recalculated" is based in part upon G. G. Killough and P. S. Rohwer, (1978).

(b) The value for human milk is 6.1 mg Carbon per 100 ml or 61 mg per L (0.061 g/L) is in good agreement with the cow milk value of 0.066. See Table 28 of ICRP 23 (ICRP, 1975).

(Page 19, Section 3.4.2)

The parameters BR(IN) and BV(IN) refer to the soil-to-plant bioaccumulation factors but are not characterized whether they are based upon fresh or dry weight. The soil-to-plant factors which are classified as for crops or for forage are clearly denoted as being wet weight (mass) and dry mass bases respectively, however, it is not clear how these two categories (crop, forage) relate to BR(IN) and BV(IN).

(Pages 23-25, Table 3.4)

The milk transfer factors appear to be comparable (within a factor of 2) to those reported in other compilations.

The value for the meat transfer factor of $3.0 \text{ E-}04 \text{ d/kg}$ for yttrium and the "rare earths" appears low by almost an order of magnitude compared to the tabulations by the IAEA (1982), Mc-Dowell Boyer et al., Ng et al., 1982, and Regulatory Guide 1.109 (NRC 1977a).

It is difficult to compare the BV and BR values to other compilations because they are categorized differently. However, the values for zinc, strontium, and iodine appear to be considerably higher than those presented in other compilations.

The TERRA computer code accesses the SITE data base (ORNL-5786), which contains agricultural, climatological, land use, and demographic parameters for each $1/2$ by $1/2$ degree longitude-latitude cell in the continental United States. This generic data base does not appear adequate for defining the region surrounding any real facility. A detailed comparison of the data base with site-specific data must be done before this modeling approach can be accepted for any real site.

(Page 10, Equation 9)

There appears to be a problem in the description of Equation 9 of the TERRA report. The parameter f_{tm} is stated to be dimensionless (page 9). However, the equation implies f_{tm} that the parameter has units of inverse time. If the equation is based on the assumption that the concentration in the milk is at equilibrium and the activity increase rate is equal to the activity decrease rate (as defined by λ_p), then Equation 9 makes sense. A similar problem exists with equation f_{tm}^m 8 on page 9. It appears that m_p should be in units of kg/d and f_{tm} in units of d^{-1} (as Equation 9 implies).

Because the radiological decay and daughter ingrowth processor used by the computer program uses a constant input rate, additional complexity is introduced into some of the exposure pathway models. For example, the transfer of activity from animal feed to milk requires definition of the standard cow-to-milk transfer factor plus a metabolic turnover rate constant for activity in milk.

PARAMETERS AND DATA BASES

Associated with CRRIS models are two documents containing the parameter values which are used in the models.

AGRICULTURAL PRODUCTION

The following are reviewers' comments related to the CRRIS related document, "Agricultural Production . . .," (ORNL-5768):

The report is a very thorough and well-documented treatment of food pathway parameters. It will be a useful reference for individuals who must perform food pathway dose calculations.

(Page 4, Paragraph 1)

The authors have used the stable (elemental) transfer coefficients f_m rather than the radioactive transfer coefficients f_m^* to model transfer of radioactivity from feed to cow's milk; f_m and f_m^* are related as follows:

$$f_m^* = f_m / \lambda_{mE}$$

where

$$\lambda_{mE} = \lambda^R + \lambda_B$$

effective elimination = radioactive decay + biological turnover.

Where data are available, f_m^* should be used in place of f_m . Ng et al. (1977) states that "Transfer coefficients f_m^* computed for individual radionuclides (Table B-1) will further improve the precision of this model, particularly for those such as Na-24 where the radioactive decay rate is high but the biological turnover rate is relatively low. Use of f_m^* together with the term for turnover rate in milk eliminates the assumption that an instantaneous equilibrium is established between feed and milk and ensures against the overestimation of the recovery of a nuclide in milk."

The same comment applies to TERRA (ORNL-5785).

REVIEW AND ANALYSIS OF PARAMETERS - SITE DATA BASE

The following are reviewers' comments on the document, "A Review and Analysis of Parameters . . .," (ORNL-5786:

This is a very impressive textbook document which covers virtually all of the important parameters required to evaluate transfer of radionuclides through the terrestrial system (agricultural side, at least). It supports TERRA.

Unfortunately it does not have counterparts dealing with atmospheric dispersion and dose calculations. They are needed.

(Pages 1-3)

The SITE data base is a very useful assembly of agricultural, demographic, and climatological data, which can be used for local and regional assessments. However, it must be remembered that the default parameter values adopted for TERRA will generally not be appropriate for such assessments, and the burden of selecting appropriate parameter values is placed upon the user. For site specific assessments, current data pertaining to the actual site rather than data for the location from SITE should be used together with parameter values in TERRA that are appropriate for the site. In this situation an even greater burden is imposed upon the user in selecting appropriate parameter values for TERRA. Unless he is experienced, it will be difficult for him to select appropriate values. Because plant-to-soil concentration ratios are reported as geometric means of geometric means, he may very well have to consult original references. As noted elsewhere, translocation of surface deposited radionuclides from nonedible to edible parts of plants is a process that should be considered.

Reporting of plant-to-soil concentration ratios in terms of B_v and B_r is an interesting departure. It would be useful to compare the statistical parameters of distributions of B_v , B_r , B_{iv1} , and B_{iv2} . It should be noted that some uncertainty due to dry-to-wet weight and wet-to-dry weight conversions is unavoidable because one has to estimate both the concentrations and intakes of foodstuffs.

(Pages 5-9)

The authors state that no a priori biases or protocols were used to produce conservative values. By the same token they should state their intent when selecting parameter values. One has to look elsewhere (in ORNL/TM-8573, "Introduction to CRRIS...") to discover that the default parameters chosen for CRRIS reflect an effort to choose reasonable values. In addition the authors should state the specific applications for which the default parameter values may be used and those for which more appropriate values should be selected, and they should discuss the uncertainty associated with the default values.

(Pages 80-123)

TERRA and SITE consider beef production in considerable detail. This reviewer has previously commented on the attention given to food and feed crops and to animal products from cattle and the lack of attention given to the other animal products that are prominent in the U.S. diet. These other animal products should also be considered. Perhaps concentrations estimated for beef may reasonably approximate those for pork and chicken but would not be comparable to those for eggs.

Uncertainty in parameter value and impact on estimates. Although the milk turnover constants were obtained from a single reference, the authors should discuss the uncertainty associated with their adopted values and their effect on the estimates of concentrations in milk.

(Pages 125-127)

For the turnover constant in beef the authors have adopted a single value for all elements (which is fairly conservative). The authors should discuss the uncertainty associated with this default value and its effect on the estimated concentrations in beef. This reviewer notes that the turnover constant in beef is designated λ_b in ORNL-5785 (TERRA). This apparent inconsistency should be corrected.

(Pages 5-9)

It is not true that past estimates of plant uptake parameters have been based on the assumption of equilibrium. Mostly they have been based on concentrations at harvest.

The authors should discuss the uncertainty associated with the default plant uptake factors. In the case of factors based on experimental data from the literature, the authors have selected the geometric mean of the geometric means of individual investigations as the default plant uptake factor. The geometric means of the individual studies have been plotted in figures, which provide an indication of the variability associated with the data. The authors should discuss the uncertainty associated with the plant uptake factors that are based on correlations with other parameters, elemental systematic, or other collateral information. Identification of those elements for which B_v is (negatively) correlated with the concentration in soil is a significant finding.

(Pages 49-53)

Some of the F_f values are based on experimental values. Some are based on elemental systematics, correlations with B_v or F_m or other collateral information. The authors should discuss the uncertainty associated with these estimates.

(Pages 65-79)

The mathematical formulation of the interception fraction for vegetation, which was derived by Chamberlain (1953), is shown on page 65 as Equation (10). This equation shows, if the exponent is less than 1 and the exponential is expanded, that the ratio of the interception fraction to the biomass should be a constant equal to $2.88 \text{ m}^2/\text{kg}$ for the example chosen. This certainly implies that the interception fraction and the biomass should not be modeled separately, but that only the ratio need be modeled. In fact, others have demonstrated that modeling the two independently can lead to grave errors.

Further, the constant factor of $2.88 \text{ m}^2/\text{kg}$ derived by Chamberlain was only appropriate for gases and vapors, and perhaps some very light spores. Obviously, one must be very careful in the choice of this factor, or risk serious error. I feel the geometric modeling used in this Section is totally unacceptable. There are, in fact, many sources of data that can be used directly for this ratio.

(Pages 128-132)

I feel the resuspension model used is naive at best. One of the greatest problems with the resuspension model is that the mass-loading approach is used exclusively. As shown by others and demonstrated by common sense, the mass-loading approach is only valid for aged sources wherein the deposited radioactivity has already weathering into some reasonable thickness of soil. In fact, on page 129, the authors state that the concentration to be used in the model is the measurement of activity in the first cm of soil.

It is obvious that a fresh, or even a continuing deposition, is not averaged throughout the first cm of soil. Experimental measurements have repeatedly shown that the resuspension process immediately after deposition has occurred is very volatile and decreases rapidly with time. In such situations, the only reasonable modeling approach is to use the time-varying resuspension-factor approach. [See Anspanugh et al., (1975).] Otherwise, the resuspension process will be seriously underestimated.

It is also very disturbing that one of the most significant resuspension pathways, the inhalation of resuspended activity by humans, does not appear to be modeled at all. Rather, the only modeled situation that is apparent from the documentation is the subsequent redeposition of resuspended activity on plants. The inhalation of resuspended radioactivity is generally considered to be the most important pathway for some radionuclides, such as plutonium.

Finally, the choice of only the 2.5 to 15 micrometer fraction of aerosol to be included in the reference "resuspension air concentration" is completely unjustified. Measurements of aerosols in nonurban environments have shown conclusively that most of the mass is associated

(Pages 65-79)

The mathematical formulation of the interception fraction for vegetation, which was derived by Chamberlain (1953), is shown on page 65 as Equation (10). This equation shows, if the exponent is less than 1 and the exponential is expanded, that the ratio of the interception fraction to the biomass should be a constant equal to 2.88 square meters/kg for the example chosen. This certainly implies that the interception fraction and the biomass should not be modeled separately, but that only the ratio need be modeled. In fact, others have demonstrated that modeling the two independently can lead to grave errors.

Further, the constant factor of 2.88 square meters/kg derived by Chamberlain was only appropriate for gases and vapors, and perhaps some very light spores. Obviously, one must be very careful in the choice of this factor, or risk serious error. I feel the geometric modeling used in this Section is totally unacceptable. There are, in fact, many sources of data that can be used directly for this ratio.

(Pages 128-132)

I feel the resuspension model used is naive at best. One of the greatest problems with the resuspension model is that the mass-loading approach is used exclusively. As shown by others and demonstrated by common sense, the mass-loading approach is only valid for aged sources wherein the deposited radioactivity has already weathering into some reasonable thickness of soil. In fact, on page 129, the authors state that the concentration to be used in the model is the measurement of activity in the first cm of soil.

It is obvious that a fresh, or even a continuing deposition, is not averaged throughout the first cm of soil. Experimental measurements have repeatedly shown that the resuspension process immediately after deposition has occurred is very volatile and decreases rapidly with time. In such situations, the only reasonable modeling approach is to use the time-varying resuspension-factor approach. [See Anspanugh et al., (1975).] Otherwise, the resuspension process will be seriously underestimated.

It is also very disturbing that one of the most significant resuspension pathways, the inhalation of resuspended activity by humans, does not appear to be modeled at all. Rather, the only modeled situation that is apparent from the documentation is the subsequent redeposition of resuspended activity on plants. The inhalation of resuspended radioactivity is generally considered to be the most important pathway for some radionuclides, such as plutonium.

Finally, the choice of only the 2.5 to 15 micrometer fraction of aerosol to be included in the reference "resuspension air concentration" is completely unjustified. Measurements of aerosols in nonurban environments have shown conclusively that most of the mass is associated

with particles smaller than 2.5 micrometers. Obviously, the smaller aerosols are also the most important ones in terms of inhalation by humans, and this significant pathway should be included.

The comparison of predicted element concentrations in plants to the range of reported measure levels is somewhat misleading as it assumes a constant "average" soil level. A more informative comparison would be to use the selected bioaccumulation factor with actual soil concentrations to predict concentrations in plants which are then compared to actual measured plant concentrations for that soil level. Admittedly this can usually be done only for the more common elements.

(Page 9, Section 2.12)

The values of $B_r = 0.08$ and $B_r = 0.03$ for cesium appear to be consistent with values (0.07 and 0.03) obtained from an independent compilation of these parameters. It is not clear what the relevancy of the Marshall Island data is as the soil type may be distinctly different than most U.S. soils and as noted the plant concentration included resuspended material.

(Page 13)

The value of 0.075 for sodium B_v appears low in comparison to the value for cesium and in comparison to other compilations. Sodium is not as tightly bound to clay particles as is cesium and is quite mobile in soils hence it would be expected to have a higher B_v value. Other references tend to show a sodium/cesium uptake ratio of B_v around 5 [NRC (1977a) McDowell- Boyer and Baes (1984), Simmons et al. (1982)]. Ng (1982) shows a ratio close to unity. The value suggested of $B_v = 0.075$ would seem comparable to the other references ($B_v = 0.05$) except that the former is on a dry weight basis and the latter on a wet weight basis. Using a fresh to dry weight ratio of 4 (grasses, forage) would give a B_v of 0.2 while a fresh/dry weight ratio of 10 would give 0.5, either one is considerably higher than the suggested 0.075.

(Page 13)

The values for strontium B_v and B_r agreed with published values for an average for legume and non-legume^r forage (2.2) and leafy vegetables (2.2), fruits (0.24) and grain (0.22).

(Page 14, 19)

The selected values for radium seem low. The B_r value of 0.0015 only appears to be low by a factor of two compared to another single reported value. However, the B_v reported value of 0.017 is considerably low than other estimates. As noted (page 19, lines 5-6) it is a factor of 5 lower than McDowell-Boyer et al., and around a factor of 2.5 lower than other reported values for grasses. Other compilations give higher radium plant-to-soil concentration ratios for root crops than for other vegetation, a factor not reflected here.

(Page 24, Section 2.1.4)

The B_r value for polonium looks low compared to other published values of B_v around 0.007 for leafy vegetables and grasses. The B_r value of 4E-04 for polonium appears to be reasonable based upon other published values.

(Page 25, Section 2.1.4)

The B_r value for iodine of 0.05 appears to be reasonable as other published B_r values are between 0.03 and 0.08. However, the value of 0.15 for B_v appears low by at least a factor of two compared to 1.8 for legumes (alfalfa, clover, etc.) and 0.25 for grasses.

(Page 26, Section 2.1.5)

The lanthanide B_r value of 0.004 appears to be reasonable for tubers (sweet potatoes, potatoes) and fruits (tomatoes, cucumbers, etc.) but would be low for legumes (bean, pea, etc.) root vegetables (radish, carrot, etc.) for which 0.03 might be more appropriate. The B_v value of 0.01 appears somewhat low (it is lower than B_r which is unusual). Other values given in the literature are 0.07 for legumes (alfalfa, clover, etc.) and 0.036 for leafy vegetables.

(Page 29)

The manganese B_r value of 0.005 agrees with other published values. The B_v value of 0.25 is lower than the reported geometric mean of 0.41; the latter value appears more appropriate for forage or leafy vegetables based upon other published values.

(Page 29)

The cobalt value of B_v of 0.02 appears low compared to values for grasses (0.04) and leafy vegetables (0.08). No values were available to compare with B_r .

(Page 30)

The reported B_r value for zinc in Till and Meyer (1983) is 0.055 for fruits. This B_r is much smaller than the default of 0.6 derived from the B_v value. The derived B_v for a soil concentration of 50 ppm Zn of 1.35 would be reasonable for the average (1.1) of legumes ($B_v=1.5$) and grasses ($B_v=0.7$).

(Page 39)

The ruthenium B_r of 0.075 and the B_v of 0.02 are in agreement with reported values of B_v 0.056 for leafy B_r vegetables and 0.012, 0.038 for fruits and legumes (beans, peas, etc.).

(Page 45)

The value for plutonium B_v of $4.5 \text{ E-}04$ appears more appropriate for root crops ($3.7\text{E-}04$) and slightly high for forage ($B_v = 2.3\text{E-}04$ for legumes and $9.2\text{E-}05$ for grasses) and leafy vegetables ($1.75\text{E-}04$). The B_r value of $4.5\text{E-}05$ would be close to the geometric mean ($1.1\text{E-}05$) of the value for fruits of $1.0\text{E-}04$ (with some aerial contamination and $8.1\text{E-}06$ for legumes and $1.5\text{E-}06$ for grains.

(Page 45)

Americium CR values appear to be generally higher than those for plutonium by about an order of magnitude. This is reflected in the selected B_r and B_v values.

The values for uranium appear somewhat higher; other reported values indicate a value of around $1\text{E-}03$ for fruits, legumes, and roots and $\text{E-}03$ for grasses rather than a B_r of $4\text{E-}03$ and a B_v of $8.5\text{E-}03$. The B_r and B_v values for neptunium and curium appear reasonable.

(Pages 47 - 49, Figure 2.23)

The differences indicated between this compilation and reference 15 (and consequently NRC Regulatory Guide 1.109) which are of importance in dose assessment are those for strontium, radium, polonium, tellurium, thorium, and neptunium. In the case of strontium and radium, the previous (lower) values appear to be incorrect and the values in this tabulation are more appropriate. The neptunium values in the older tabulations may be based upon an assumed similarity to other actinides. The current tabulation correctly reflects the higher uptake of Np by plants than for other actinides. The remaining values (Te, Th, and Po), for which the present compilation assigns value lower than previously used, may reflect better measurements which more accurately measure "true" soil-to-plant uptake without also measuring the deposition of resuspended soil.

(Page 51, Figure 2.25)

There does not appear to be a rational explanation for why the F_f values for the higher lanthanides differ from those for the lower and better documented values for the lower lanthanides, La, Ce, Pr, and Nd. There are no apparent differences in chemical properties to warrant such differences. The intake for milk, F_m , values do not show these differences.

(Pages 53 - 62)

The discussion of soil-to-water distribution coefficients is comprehensive and well documented. However, the tabulated values in Table 2.13 raise the question of whether parameters which range over several orders of magnitude can be adequately represented by one mean (or geometric mean) value. An alternative would be to attempt to characterize different subgroups of soil types with associated K_d 's.

(Pages 65 - 79)

The theoretical treatment of deposition retention factors seems a bit remote for practical applications. Figures 3.6 and 3.7 are particularly speculative considering the available experimental evidence. If the report were rewritten, this section might be reduced and additional material on comparison of the model predictions with measured values added.

(Pages 80 - 123)

The data collection and analysis effort described here is very well done and provides a significant contribution to performing site-specific assessments.

(Pages 124 - 127, Sections 5.2 and 5.3)

It is not clear that the approach to milk and meat transfer used is markedly superior to that used in Regulatory Guide 1.109 since, for most elements, constant default values are used for T_m and T_f .

(Pages 127 - 132 and Table 2.3, page 8)

The dry-to-wet weight conversion factor for grain is $DW/WW = 0.888$. This implies a WW/DW ratio of $1/0.888 = 1.12$ which appears appropriate compared to other sources. The value given in the TERRA code report is in error as already noted.

Similarly the value for the carbon content of milk of 0.069 in Table 5.2 appears to be appropriate (other data give 0.066). The value of 0.293 in the TERRA report was criticized in my comments but has since been corrected as noted in ORNL-5786.

REFERENCES

- [Anspaugh and Shinn, 1975] Anspaugh, L. R. and N. C. Kennedy. "Resuspension and Redistribution of Pu in Soils," Health Physics, 29, 571-582.
- [Barker, 1982] Barker, D. A. *A Virtual Source Model for Building Wake Dispersion in Nuclear Safety Calculations*. Central Electric Generating Board report TPRD/B/0072/N82. Avail. C.E.G.B., Marchwood Engng. Labs., Marchwood, Southampton, U.K.
- [Breckland and Baes, 1985] Breckland, J. E. and C. F. Baes, III. Report on the Workshop on Food-Chain Modeling for Risk Analysis. ORNL-6051. Oak Ridge National Laboratory, Oak Ridge, TN.
- [Briggs, 1971] Briggs, G. A. *Some Recent Analyses of Plume Rise Observations*, In Proceedings of the Second International Clean Air Congress, Academic Press, Boston, MA.
- [Briggs, 1984] Briggs, G. A. *Plume Rise and Bouancy Effects*, In Atmospheric Science and Power Production. D. Randerson, Ed. DOE/TC-27601. U. S. Department of Energy, Washington, DC.
- [Briggs, 1985] Briggs, G. A. *Analytical Parameterization of Diffusion: The Convective Boundary Layer*. S. Climate and Applied Meterology 24, 1167-1186.
- [Chamberlain, 1953] Chamberlain, A. C. *Aspects of Travel and Deposition of Aerosol and Vapor Clouds*, British Report AERE-HP/R-1261.
- [Draxler, 1976] Draxler, R. R. *Determination of Atmospheric Diffusion Parameters*, Atmospheric Environment, 10, pp. 99-105.
- [ERDA, 1975] ERDA. *Workshop on Environmental Research for the Transuranium Elements*. in Proc. Workshop. Seattle, WA, 12-14 November, 1975. ERDA-76-134. U. S. Energy Administration, Washington, DC.
- [Fackrell, 1984] Fackrell, J. E. *An Examination of Simple Models for Building Influenced Dispersion*. Atmospheric Environment. Vol. 18, No. 1, pp. 89-98.
- [Ferrara and Cagnetti, 1980] Ferrara, V. and P. Cagnetti. *A Simple Formula for Estimating Airborne Concentrations Downwind of Buildings for Discharges Near Ground Level*. In Proc. of Commission of European Communities' Seminar on Radioactive Releases and Their Dispersion in the Atmosphere Following a Hypothetical Reactor Accident. Riso, Denmark, pp. 993-1005.
- [IAEA, 1982] International Atomic Energy Agency. Generic Models and Parameters for Assessing the Environmental Transfer of Radionuclides from Routine Releases. Safety Series No. 57, IAEA, Vienna.

- [Heffter, 1980] Heffter, J. L. *Air Resources Laboratory's Atmospheric Transport and Dispersion Model (ARL-ATAD)*. NOAA Tech. Memo. ERL-ARL-81. NOAA/Air Resources Laboratory, Silver Spring, MD.
- [Heffter et al., 1975] Heffter, J. L., A. D. Taylor, and G. J. Ferber. A Regional-Continental Scale Transport, Diffusion, and Dispersion Model. NOAA Tech. Memo. ERL-ARL-50. NOAA/Air Resources Laboratory, Silver Spring, MD.
- [Hoffman and Baes, 1979] Hoffman, F. O. and C. F. Baes III. *A Statistical Analysis of Selected Parameters for Predicting Food Chain Transport and Internal Dose of Radionuclides*, edited by F. Owen Hoffman and Charles F. Baes III, Section 3.6, pages 64-79, NUREG/CR-1004, October, 1979.
- [Hoult and Weil, 1972] Hoult, D. P., and J.C Weil *Turbulent Plume in a Laminar Air Flow*, Atmospheric Environment, 6, pp. 513-531.
- [Huber, 1979] Huber, A. H. An Evaluation of Obstacle Wake Effects on Plume Dispersion, presented at the Fourth Symposium on Turbulence, Diffusion, and Air Pollution, Reno, NV, Jan 15-18, American Meteorological Society, Boston, MA.
- [Huber and Synder, 1976] Huber, H. A. and W. H. Synder. 1976. *Building Wake Effects on Short Stack Effluents in Third Symposium on Atmospheric Turbulence, Diffusion, and Air Quality*, Raleigh, NC, Oct 19-22, pp. 253-242, American Meteorological Society, Boston, MA.
- [ICRP, 1975] International Commission on Radiological Protection. Report Task Group on Reference Man. ICRP Publication 23, Pergamon Press, Oxford.
- [ICRP, 1977] International Commission on Radiological Protection. Recommendations of the International Commission on Radiological Protection. ICRP Publication 26, Pergamon Press, Oxford.
- [ICRP, 1979] International Commission on Radiological Protection. Limits for Intakes of Radionuclides by Workers. ICRP Publication 30, Part 1, Pergamon Press, Oxford.
- [Jones, 1983] Jones, J. A. *Models to Allow for the Effects of Coastal Sites, Plume Rise and Buildings on Dispersion of Radionuclides and Guidance on the Value of Deposition Velocity and Washout Coefficients*. Fifth report of Working Group on atmospheric dispersion. National Radiological Protection Board report NRPB-R157. Avail. N.R.P.B., Chilton, Didcot, Oxon, U.K.
- [Killough and Rohwer, 1978] Killough, G. G. and P. S. Rohwer. *A New Look at the Dosimetry of ^{14}C Released to the Atmosphere as Carbon Dioxide*, Health Physics 34(2), 141-159, February 1978.

- [Kirchner and Whicker, 1984] Kirchner, T. B. and F. W. Whicker. Validation of Pathway: A Simulation Model of the Transport of Radionuclides Through Agroecosystems. Ecological Modeling. 22: 21-44.
- [Martz et al., 1983] Martz, H. F. A Comparison of the Methods for Uncertainty Analysis of Nuclear Power Plant Safety System Fault Tree Models. NUREG/CR-3263, LA-9729-MS. Los Alamos National Laboratory, Los Alamos, NM.
- [Miller and Little, 1983] Miller, C. W. and C. A. Little Uncertainty Estimates for Predictions of the Impact of Breeder Reactor Radionuclide Releases, pp. 645-654 In Proceedings of the International Topical Meeting on Liquid Metal Fast Breeder Reactor Safety and Related Design and Operational Aspects, Lyon-Ecully, France, July 19-23, 1982.
- [Ng et al., 1977] Ng, Y. C., C. S. Colsher, D. J. Quinn, and S. E. Thompson Transfer Coefficients for the Prediction of the Dose to Man via the Forage-Cow Milk Pathways from Radionuclides Released to the Biosphere, page 25, UCRL-51939, Lawrence Livermore Laboratory.
- [Ng et al., 1982] Ng, Y. C., Colsher, C. S., and S. E. Thompson. Soil-to Plant Concentration Factors for Radiological Assessments. NUREG/CR-2795 (UCID-19463). U. S. Nuclear Regulatory Commission, Washington, DC.
- [Raridon et al., 1984] Raridon, R. J., B. D. Murphy, W. M. Culkowski, and M. R. Patterson. The Atmospheric Transport Model for Toxic Substances (ATM-TOX). ORNL/CSD/TM-94.
- [Romney and Wallace, 1977] Plutonium Contamination of Vegetation in Dusty Field Environments. In Transuranics in Natural Environments, M. G. White and P. B. Dunaway, Eds. NVO-178, pp. 287-302, Energy Research and Development Administration, Los Vegas, NV.
- [Simmons et al., 1982] Simmons, J. R., G. S. Lynsley, and S. M. Haywood. Accidental Releases of Radionuclides; a Preliminary Study of the Consequences of Land Contamination. NRPB-R133-1982. National Radiation Protection Board, U.K.
- [Till and Myer, 1983] Till, J. E. and H. R. Myer, Eds. Radiological Assessment, A Textbook on Environmental Dose Analysis, NUREG/CR-3332, Nuclear Regulatory Commission, Washington, D. C.
- [NRC, 1977a] U. S. Nuclear Regulatory Commission. Calculation of Annual Doses to Man from Routine Releases of Reactor Effluents for the Purpose of Evaluating Compliance with 10 CFR Part 50, Appendix I. Reg. Guide 1.109, Washington, D.C.
- [NRC, 1977b] U. S. Nuclear Regulatory Commission. Methods for Estimating Atmospheric Transport and Dispersion of Gaseous Effluents in Routine Releases from Light-Water-Cooled Reactors. Reg. Guide 1.111, Washington, D.C.
- [Vaurio, 1982] Vaurio, J. K. Statistical Identification of Effective Input Variables. ANL-82-57. Argonne National Laboratory, Argonne, IL.

APPENDIX A

BRIGGS LETTER

The following letter was submitted by Gary Briggs along with his comments. Since it is very informative, it is included here as a further comment on the plume-rise model used in CRRIS.

April 14, 1983

Plume Rise Equations Used in EPA Models

Gary A. Briggs
AMB, MD-ESRL

D. Bruce Turner, Chief
EOB, MD-ESRL

For nearly a decade now, the EPA has been using in many of its models the Briggs (1970) simplified formulation for final plume rise in neutral and unstable conditions:

$$\Delta h = 21.4F^{3/4}/U \quad \text{when } F \leq 55 \text{ m}^4/\text{sec}^3 \quad (1a)$$

$$\Delta h = 38.7 F^{3/5}/U \quad \text{when } F \geq 55 \text{ m}^4/\text{sec}^3. \quad (1b)$$

These equations compromise some substantial differences between neutral (mechanical turbulence) and unstable (convective turbulence) conditions. Virtually all models predict a weaker dependence on U in unstable conditions and a considerably stronger dependence on U in neutral conditions. This follows from the basic physical facts that: (1) unstable conditions is to axially dilute the buoyancy; i.e., it enters only in the form F/U; (2) mechanical turbulence i.e., driven by U and strongly depends on it, with turbulence velocities $\delta \propto U$; plume rise strongly depends on U through the scaling length $F/(Uu^2) \propto F/U^3$, and also depends on source height. Furthermore, Eqn. 1 employ two gross empiricisms, one for eddy energy dissipation rate ($\epsilon \propto U/Z$) and one for the approximate mean height occupied by the plume, for the purpose of calculating ($z \propto F^{3/8}$, but is not allowed to exceed 100m). Equations 1a and 1b were tested against the data in Briggs (1969), Plume Rise, and Bringfelt (1968). These data were inadequate in terms of distance downwind for any real test, since the 2/3 law plume and Eqs. 1 gave only a small improvement in fit over that obtained using the 2/3 law alone.

The assumption for Z did not anticipate high buoyancy, short stack sources such as gas turbines, and is not appropriate for such a source at high wind speeds.

With the publication of my 1975 lecture (Briggs, 1975) and the upcoming publication of Atmospheric Science and Power Production (Briggs, 1984), it seems to me that Eqs. 1 fall rather short of the state-of-the-art. There was much improved understanding of turbulence in neutral and convective boundary layers in the 1970's, and this is reflected in the updated plume rise formulas of 1975 and 1983. Furthermore, these formulas have avoided any gross empiricisms (more physically reasonable estimates for ϵ are used, and factors like Z are calculated, rather than assumed). Truly adequate data are still lacking, but the new formulas have been subjected to some "ground truth" testing via comparisons of observed and predicted maximum ground concentrations, and they seem to do well. In my opinion, it is about time to upgrade this component of EPA models, or at least to assess the degree of error that can be expected using Eqs. The U^{-1} wind speed dependence, for instance, is rather simplistic in light of the new models.

One obstacle to upgrading using the newer models is their relative complexity. However, most of this apparent complexity can be avoided by using some approximations that are broad, but still are far less sweeping than those made in developing Eqs. 1. Some suggestions follow.

In the convective situation, the physical mechanisms that affect plume rise are complex. Yet, the various predictions given by the "breakup" and the "touchdown" models in the 1975 and 1983 references were noted to be similar. In 1984, I recommended a somewhat more conservative (lower rise) form of the "breakup" equation because of its simplicity. There exist no data to support more complicated approaches, anyway. The

Northfleet and Tilbury maximum ground concentration data for low wind speeds are fit fairly well using Eq. 101 of Briggs (1984) with $H = 32 \text{ cm}^2/\text{sec}^3$ (see Fig. 8.11, same ref.):

$$\begin{aligned}\Delta h &= 3(F/U)^{3/5} H^{-2/5} \\ &= 30 (F/U)^{3/5} \quad \text{MKS units} \quad (2)\end{aligned}$$

(this is equivalent to Eq. 86 of Briggs 1975 using $H = 92 \text{ cm}^2/\text{sec}^3$). Note the resemblance of Eq. 2 to Eq. 1b. They are in fact identical at $U = 1.9 \text{ m/sec}$. Otherwise, they differ by a factor proportional to $U^{2/5}$, which is not too serious in the range typical for convective conditions, $U = 1$ to 5 m/sec . The only additional complexity over Eq. 2 I would suggest at this time is a seasonal adjustment - plus 30% in the winter and down 30% in the summer - to allow for the substantial seasonal changes in heat flux and convective turbulence intensity.

For the neutral situation the same formula was recommended in both 1975 and 1983 and was verified using ground concentration data in two periods of lidar-determined plume rise:

$$\Delta h = 1.2 (F/Uu^{*2})^{3/5} (h_s + \Delta h)^{2/5} \quad (3)$$

(Eq. 80 of 1975 and Eq. 97 of 1983). This equation requires reiterative approximation to solve, since Δh cannot be isolated. However, it can be approximated with -4% to 0% error in the computed effective stack height, $h_s = \Delta h$, with

$$\Delta = 1.2(F/Uu^{*2})^{3/5} (h_s + 1.3FUu^{*2})^{2/5} \quad (4)$$

A further simplification is made by selection $U/u^* = 12$, a moderately conservative value. We now have

$$\Delta = 24(F/U^3)^{3/5} (h_s + 200 F/U^3)^{2/5} \quad (5)$$

A further simplification is permissible when $h_s > 7 F^{1/2}$, MKS units.

$$\Delta = 30(F/U^3)^{3/5} h_s^{2/5} \quad (6)$$

This approximation makes no more than 3% error in effective stack height compared to Eq. 3 in the critical range $0 < \Delta h/h_s < 1$, at which highest ground concentrations are obtained, and underestimates rise at higher $\Delta h/h_s$ (lower U). The transition to the convective case is very simply calculated by equating Eqs. 2 and 6: $U = h_s^{1/3}$, which ranges from about 3 to 6 m/sec. If $h_s = 7 F^{1/2}$, Eq. 6 underpredicts effective stack height given by Eq. 3 by 30% at this transition point. For larger h_s and/or larger U , the error is less. Equation 6 also resembles Eq. 16 in the dependence on F . They are equivalent when $U = 1.4 h_s^{1/2}$, $h_s^{1/2}$, which ranges from about 6 to 24 m/sec, but differ by a factor proportional to $U^{-4/5}$; Eq. 1b is bound to be in substantial error somewhere in the neutral range of windspeeds.

For sources like gas turbines (large F , small h_s) Eq. 6 can grossly underpredict rise. For ground sources, such as conflagrations, drop the h term and use

$$\Delta h = 200 F/U^3. \quad (7)$$

this is half the Δh suggested in Briggs (1965), but that value was based on elevated sources (when $\Delta h/h_s = 0.43$, which happens near the "critical windspeed", Eq. 5 gives twice the rise given by Eq. 7). In very high winds, a ground source plume can be quite close to the ground, where turbulence intensity, or U^*/U is somewhat larger - but in such cases, estimates of U^3 and even of F may be the greatest sources of error. Transition to the convective case, Eq. 2, occurs at $U = 2.2 F^{1/6}$, in the range 3 to 10 m/sec. Really high windspeeds cause the greatest ground impact, with $(U\Delta h^2)^{-1} \propto U^5$. Equation 7 is equivalent to the 2/3 law terminated when $\Delta h = x/7$; this suggests that ground impact occurs soon after this point, $x = 1400 F/U^3$.

To illustrate some points made above, two tables are attached. The first compares the neutral Δh approximation with Eq. (3) with $U/u^* = 12$, and includes $20(F/h_s)^{1/3}/U(1+\Delta h_s)^2$ as a weighting factor proportional to maximum ground concentration. One surprising feature shown by this table is that Eq. 7, while grossly underpredicting Δh for elevated sources at high windspeeds, never underpredicts effective source height by more than 18%. Thus, it is a viable candidate for the "simplest tolerable formula." Eq. 6, on the other hand, does much better near the critical windspeed, and grossly underpredicts only when $\Delta+h_s > 4h_s$. Eq. 5 combines the best of both and is really not so complicated, so is the best recommendation for neutral conditions.

The second table compares Eq. 1 with predictions of Eqs. 2 or 3 (with $U/u^* = 12$), whichever is lowest. The recommended equation for neutral conditions, Eq. 5, is essentially identical to Eq. 3. Six very different source types are tested for windspeeds ranging 1 to 16 m/sec. In each case Eq. 1 overestimates rise (compared to the present recommendations) at very low and very high windspeeds, and underestimates at moderate windspeeds (on the average, it does O.K.). Special attention should be given to windspeeds at which the highest ground concentration, or $(U(h_s+\Delta h)^2)^{-1}$, is obtained. For the large F ground source, the conflaguration, the plume "bumps" the top of the mixed layer at ordinary windspeeds, and may even penetrate overlying stable layers. Clearly the case of concern is very high wind. Equation 1 agrees with $200 F/U^3$ at $U = 2.3 F^{1/5}$, 14 m/sec in this case, but differs much from the U^{-3} relationship and will seriously overpredict rise for small ground sources (bonfires) at high windspeeds. (But at 16 m/sec, maybe we worry more about the fire spreading!) For the short-stack sources, the high wind neutral case is again of most concern. Equation 1 tends to seriously overestimate Δh and underestimate ground concentration for these conditions, especially for the large buoyancy sources such as gas turbines. For the high-stack source with small buoyancy, the low windspeed unstable case is worst. Here, Eq. 1 does fine on the average. It overestimates rise at really low U, but who keeps track of windspeeds, which may be the worst case, unless limited mixing occurs (low U, low, impenetrable a^i).

In summary, the predictions of Eq. 1, when compared to the present recommendations, are not very different in any case that counts except for low level sources at high windspeeds, where it may seriously overpredict Δh . I prefer the present approach, Eqs. (2) and (5), because they contain the minimum essential physics, with the maximum allowable simplifications.

References:

- Briggs, G. A. (1965): A Plume Rise Model Compared with Observations, J. Air Pollut. Control Assoc. 15, 433-438.
- _____ (1970): Some Recent analyses of Plume Rise Observations, Second International Air Congress, Wash. D.C., Dec. 1970.
- _____ (1975): Plume Rise Predictions in Lectures on Air Pollution and Environmental Impact Analysis, American Meteorol. Soc., Boston, MA.
- _____ (1984): Plume Rise and buoyancy Effects, Chapter 8 of Atmospheric Science and Power Production (Darryl Randerson, ed.), Technical Information Center, Dept. of Energy, Oak Ridge, TN.
- Turner, D. B. (1985): Preliminary Commentary: Proposed Pragmatic Methods for Estimating Plume Rise and Plume Penetration through Atmospheric Layers, Atmospheric Environment, Vol. 19, No. 7, pp. 1215-1218.

Table 1. Neutral Δh , comparisons with Eq. (3)

Relative x_{\max} $20F^{1/3}h_s^{5/3}$ $U(h_s + \Delta h)^2$	Dimension- less U U $(F/h_s)^{1/3}$	Relative Δh	Ratio of effective stack height predictions ^a		
		$U/u_* = 12$ Eq. (3) $\Delta h/h_s$	$h_s + \text{Eq. (5)}$ $h_s + \text{Eq. (3)}$	$h_s + \text{Eq. (6)}$ $h_s + \text{Eq. (3)}$	$h_s + \text{Eq. (7)}$ $h_s + \text{Eq. (3)}$
.78	21.3	0.1	.998	1.02	.93
.92	14.8	0.2	.994	1.03	.88
.98	10.4	0.4	.984	1.03	.84
.86	8.0	0.7	.974	1.01	.82
.74	6.8	1.0	.968	.98	.82
.60	5.8	1.4	.965	.94	.84
.44	5.0	2	.963	.88	.87
.29	4.3	3	.963	.80	.88
.16	3.5	5	.978	.68	.94
.10	3.1	7	.986	.61	.96
.06	2.75	10	.991	.53	1.06

^aThe calculations for Δh are:

$$\text{Eq. (3): } \Delta h = 23.7 (F/U^3)^{3/5} (h_s + \Delta h)^{2/5};$$

$$\text{Eq. (5): } \Delta h = 24 (F/U^3)^{3/5} (h_s + 200 F/U^3)^{2/5};$$

$$\text{Eq. (6): } \Delta h = 30 (F/U^3)^{3/5} h_s^{2/5};$$

$$\text{Eq. (7): } \Delta h = 200 F/U^3.$$

Table 2. Equation (1) compared to Eq. (2) or (3), various source types^a

Source parameters ^b	U (m/sec)	$\Delta h_{2,3} =$ Eq. (2) or (3)	Relative X _{max}		Eq. (1) (m)	Eq. (1) $\frac{\Delta h_{2,3}}$	$\frac{h_s + \text{Eq. (1)}}{h_s + \Delta h_{2,3}}$
			$\frac{10^4 F}{U(h_s + \Delta h_{2,3})^2}$	$\Delta h_{2,3}$ (m) ³			
$h_s = 0$							
F = 10 ⁴ m ⁴ /sec ³ (conflagration)	1	2, B ^c	1.8	7536	9720	1.29	1.66
	2	2, B	2.0	4972	4860	.98	.96
	4	2, B	2.3	3280	2430	.74	.55
	8	2, B	2.7	2164	1215	.56	.32
	16	3	26	488	608	1.25	1.55
$h_s = 20$ m							
F = 10 m ⁴ /sec ³ U _c = 9 m/sec	1	2	5	119.4	120	1.01	1.01
	2	2	5	78.8	60	.76	.66
	4	3	7	33.7	30	.78	.73
	8	3	15	8.5	15	1.76	1.51
	16	3	13	2.2	7.5	3.41	1.53
F = 100 m ⁴ /sec ³ U _c = 19 m/sec	1	2	4	475	613	1.29	1.64
	2	2	4.5	313	307	.98	.96
	4	2	5	207	153	.74	.58
	8	3	27	48.1	76.7	1.59	2.02
	16	3	69	10.0	38.3	3.83	3.78
F = 1000 m ⁴ /sec ³ U _c = 42 m/sec (gas turbine)	1	2, B	2.7	1893	2440	1.29	1.65
	2	2, B	3.1	1249	1220	.98	.95
	4	2	3.5	824	610	.74	.56
	8	3	8	373	305	.31	.67
	16	3	135	58	152.6	2.63	4.90
$h_s = 200$ m							
F = 100 m ⁴ /sec ³ U _c = 9 m/sec	1	2	2.2	475	613	1.29	1.45
	2	2	1.9	313	307	.98	.98
	4	2	1.5	207	153	.74	.75
	8	3	1.5	85	76.7	.90	.94
	16	3	1.3	22	38.3	1.74	1.15
F = 1000 m ⁴ /sec ³ U _c = 19 m/sec (power plant)	1	2, B	2.3	1893	2440	1.29	1.59
	2	2, B	2.4	1249	1220	.98	.96
	4	2	2.4	824	610	.74	.63
	8	3	2.7	481	305	.63	.55
	16	3	6.9	100	152.6	1.53	1.38

^aThe calculations for Δh are:

Eq. (1): $\Delta h = 21.4 F^{3/4}/U$ when $F < 55 \text{ m}^4/\text{sec}^3$, $\Delta h = 38.7 F^{3/5}/U$ when $F > 55 \text{ m}^4/\text{sec}^3$;

Eq. (2): $\Delta h = 30 (F/U)^{3/5}$ (MKS units);

Eq. (3): $\Delta h = 23.7 (F/U^3)^{3/5} (h_s + \Delta h)^{2/5}$ (any units) } Choose the smaller Δh

^b $U_c = 11.4 (F/h_s)^{1/3}$, the "critical windspeed" for the neutral case.

^c"B" indicates that $\Delta h > 1000$ m. "bumping" is likely, $\Delta h = 0.62 (z_i - h_s)$, or partial penetration.

APPENDIX B

RESPONSES TO COMMENTS BY PEER REVIEW PANEL

B.1. INTRODUCTION

The ORNL staff who have been responsible for the development of the CRRIS would like to thank all of the participants in this peer review process. We realize this was a very time-consuming process, but we feel the comments contained in this report reflect a very high quality effort and they will be very helpful to both us and EPA. We would like to extend a special thanks to the chairman of this panel, D. A. Baker of Battelle Pacific Northwest Laboratories, for his work in seeing this process through from the initial mailing of invitations to the preparation of this document. Without his hard work it would have been very difficult to obtain a coherent view of the individual comments that were provided to him.

The purpose of this appendix is to allow ORNL staff members to reply to the comments provided by the peer review panel members. A response will not be made on each comment. Instead, the individual review comments are summarized into a series of general comments on the CRRIS as a whole and a set of comments on each individual computer program. These are discussed in the following sections. It is hoped that by providing an ORNL perspective on the the review comments the entire peer review process will be of more value to EPA than it might otherwise.

It should be noted that some of the peer review comments, especially those of an editorial nature, have already been incorporated into the CRRIS. The peer reviewers saw unpublished drafts of the documentation for both the ANEMOS and ANDROS codes. These reports were revised to incorporate the review comments wherever feasible before they were published. In addition, errata sheets are being prepared for the other

CRRIS documents where appropriate to correct some of the errors pointed out by the peer reviewers.

B.2. GENERAL COMMENTS

A number of reviewers had general comments about the CRRIS that were independent of any single code or document. These will be addressed in this section.

Comment: CRRIS is too complicated to be of practical use.

Reply: A user's manual for the whole CRRIS should be written. However, it must be recognized that the CRRIS was not designed to be a simple screening tool. As requested by EPA at the time the project was started, CRRIS is designed to be a comprehensive and flexible assessment tool. It is likely that only an experienced assessor will be able to utilize the capabilities of CRRIS to the fullest.

Comment: The operational limits of the CRRIS need to be more clearly specified.

Reply: We agree that further comments on the appropriate operational limits of the various CRRIS codes would be useful.

Comment: The availability and portability of the CRRIS codes need to be specified.

Reply: The CRRIS codes currently run on the IBM machines at ORNL and at the EPA computing facilities in Research Triangle Park, North Carolina. These codes will be made available as soon as possible to anyone who wants them through the Radiation Shielding Information Center at ORNL. The

codes are written in IBM FORTRAN 66. All binary files are currently being converted to character files except for internal scratch files in an attempt to enhance the portability of the system.

Comment: No waterborne pathways are included in the CRRIS.

Reply: Inclusion of waterborne pathways of radionuclide exposure was beyond the scope of this project for sources under consideration for the Clean Air Act at the time when the development of CRRIS was funded by EPA.

Comment: The Clean Air Act regulations to which CRRIS is designed to apply need to be described better.

Reply: The applicable regulations were not promulgated until 1984, too late for inclusion in any detail in many of the CRRIS documents.

Comment: More test runs of the various CRRIS codes need to be provided to the user.

Reply: Many test runs of all of the CRRIS codes have already been made, and more are currently being made as resources allow. A more complete series of test runs should be included in a user's manual to assist persons in running the CRRIS, especially for the first time.

Comment: The CRRIS models do not consider time-dependent processes.

Reply: The Clean Air Act program under which the CRRIS has been developed is concerned with long-term time averages only. As a result, time-dependent models are inappropriate for inclusion in the CRRIS.

Comment: Extensive sensitivity and uncertainty analyses need to be performed for all of the CRRIS codes.

Reply: We agree with this conclusion completely, but such studies are both time-consuming and expensive to carry out, and they are well beyond the current resources of this project at ORNL.

Comment: More discussion is needed of what we already know about the uncertainties in the various models.

Reply: We agree with this comment.

B.3. COMMENTS ON SPECIFIC CODES

In addition to the general comments discussed above, the peer reviewers provided comments specific to each of the computer codes that comprise the CRRIS. These comments were based on reviews of both the Overview document and the specific documents describing each individual code. These code-specific comments are considered in the sections which follow.

B.3.1 Comments on ANDROS

Comment: The method of calculating health effects is not clearly described.

Reply: The method used to calculate health effects is more fully described in the references cited, and it was felt that it was inefficient and wasteful to repeat that explanation in the ANDROS document. Should ANDROS be extensively revised in the future, an attempt will be made to provide a more thorough discussion of the health effects calculational methodology.

Comment: The dose and risk factors in ICRP publications 26 and 30 should be used in ANDROS.

Reply: The dose and risk factors used in ANDROS were developed according to EPA's specifications for use in their technical assessment activities. These factors are based on the dose models used in ICRP 26 and 30 and the risk models from the BEIR-3 report, but they may differ from the ICRP 26/30 factors due to the selection of parameter values for use in the ICRP models.

Comment: Morbidity as well as mortality needs to be considered in risk estimates.

Reply: Mortality has traditionally been the basis upon which radiological health risks have been calculated for developing regulations. Estimating morbidity risks is beyond the specified scope of this project.

B.3.2. Comments on ANEMOS

Comment: The terrain effects model contained in ANEMOS needs to be improved.

Reply: ANEMOS now incorporates the simple terrain effects model given in U. S. Nuclear Regulatory Commission Regulatory Guide 1.111. When work on developing ANEMOS began in 1980, this appeared to be a reasonable and widely-accepted model. Since that time, however, other models for this purpose have gained acceptance, e.g. the methodology incorporated into the ATM-TOX computer code. We will update the terrain effects model in ANEMOS in the future if resources for doing so become available. However, we must also recognize that any method for incorporating terrain effects into a straight-line Gaussian plume model such as ANEMOS is necessarily ad hoc. As a result, ANEMOS will never be wholly appropriate for assessments involving highly complex terrain conditions.

Comment: Do ANEMOS and RETADD-II interface directly at 100 km?

Reply: No, they do not. ANEMOS and RETADD-II were developed independently. They were never designed to interface directly with one another. Furthermore, they were designed to be used for different purposes, and they do not necessarily use complimentary data bases. If an updated version of RETADD is developed in the future, consideration should be given to including a model for estimating air concentrations near the source as an integral part of the regional-scale calculations.

Comment: The power law is inappropriate for representing the vertical wind speed profile.

Reply: Again, when work begin on the ANEMOS code the power law appeared to be an appropriate methodology for estimating the vertical wind speed profile. It is now clear, however, that representation of the wind speed profile are available which give more accurate results. Note also that the model used is inconsistent with the vertical wind speed approximation used in the dry deposition modeling. The power law formulation should be replaced.

Comment: The stability class methodology should be replaced by methods of specifying atmospheric stability as a continuous quantity.

Reply: Specifying atmospheric stability in terms of discreet classes is widely used and accepted in the radiological assessment community. Methods are now available for estimating atmospheric stability on a continuous basis. However, it is questionable if sufficient historical meteorological data are available for many sites to allow these methods to be used for the routine assessments required by EPA. Furthermore, there

have been little data presented to date which demonstrate that these newer methods give more accurate results than the older methods.

Comment: The code contains too many input options which require decisions by the user.

Reply: The input options and data requirements associated with ANEMOS are based on specifications provided by EPA to meet their assessment needs. These specifications were designed to make ANEMOS as flexible as possible. Default values are provided in the code for all of the options and for as much of the basic data as possible. Few of these default values will need to be changed for many assessment problems. However, it is true that the inexperienced user may find it difficult to use ANEMOS in an appropriate manner.

Comment: The plume rise models need to be updated.

Reply: When work began on the development of ANEMOS, a method of estimating plume rise was chosen that was used in a then-recent computer code that was coming into wide use and acceptance by EPA. One result of this peer review process has been the realization that better plume rise models are now available for assessment applications (see Appendix A).

B.3.3 Comments on MLSOIL/DFSOIL

Comment: The data necessary to run MLSOIL are not generally available.

Reply: This model was developed to address EPA's expressed need to reduce the conservatism found in conventional external surface dose estimations when the deposited materials do not penetrate the ground surface. This can lead to gross overestimates of ground surface activity

when long-lived radionuclides have been deposited. While we agree that lack of leaching data is a very serious problem with regard to the model, the model can be used in scoping studies and for those sites where data are available.

Comment: Upward migration of radionuclides in soil should be considered.

Reply: Inclusion of this phenomenon would not be expected to play a major role in dose assessments for most radionuclides. Furthermore, to include this mechanism in MLSOIL will require changes in the basic calculational procedure used by the code.

B.3.4. Comments on PRIMUS

Comment: The discussion of how the decay calculations are implemented in the CRRIS is unclear.

Reply: It is now apparent that each of the individual authors of the CRRIS documents assumed that one of the others was going to provide a detailed discussion of how the decay calculations are implemented in the CRRIS. As a result, no one actually did it. It should have been in the PRIMUS manual.

Comment: The process for truncating decay chains should be improved.

Reply: Currently, decay chains are truncated based on a single maximum chain length that is applied uniformly to all chains being considered at that time. What is needed is a method for truncating each chain individually on the basis of the relative half-lives of the members of the chain and the length of time of the assessment.

B.3.5. Comments on RETADD-II

Comment: The relationship between RETADD-II and PRIMUS is not clearly documented.

Reply: There is no direct interface between RETADD-II and PRIMUS at this time. The user of RETADD-II must directly input by hand the necessary decay data generated by PRIMUS for each individual problem. RETADD-II should be modified to read PRIMUS files directly, as is done by the other CRRIS codes.

Comment: The models used in RETADD-II are outdated; e.g., temporal and spatial variations in precipitation need to be considered, a variable mixing layer depth should be used, and other methods of determining wind speed should be considered.

Reply: We do not agree that the models used in RETADD-II are outdated; rather, they represent a compromise between simplicity and complexity. The use of temporal and spatial variations in precipitation and a variable mixing layer depth are not relevant to outdatedness. They are not new ideas and the RETADD-II authors have been familiar with these ideas for many years. A decision was made not to include these effects in the model and this decision was based on two reasons:

1. Including these effects would require access to significantly more data and in general would add to the complexity of CRRIS. The authors have felt that too much complexity should be avoided and indeed there have been sentiments expressed to the authors that the system is already too complex (see first comment under General Comments).

2. The use of detailed data on precipitation and mixed layer depth has certainly been demonstrated to yield different results than using average values and the argument can be advanced that this will model more realistically an observed (past) meteorological situation. However, the purpose of CRRIS is to make predictions about a typical situation in the future e.g., in order to estimate dose to a population over the next 30 years. But, how does one predict precipitation patterns and mixed layer depths (both spatially and temporally) for future times? The only thing one can do, of course, is to use past patterns together with the implicit assumption that they will repeat. If one adopts this approach then it becomes necessary to use many years of data so as to avoid atypical situations and to give the model an opportunity to sample the predominant patterns in the data. A priority, it seems that this would not differ significantly from an approach which simply uses average values for precipitation and mixed layer depths. In fact, there is much evidence that average values repeat but it is not clear that this is the case for more detailed spatial and temporal patterns. Thus, since the authors were not aware of any experimental evidence to the contrary it seemed scientifically prudent to avoid introducing complexities when it was not clear that there were advantages to be gained.

B.3.6. Comments on TERRA and the SITE Data Base

Comment: Doses from the ingestion of pork, poultry, eggs, and lamb should be considered.

Reply: Although these pathways of radionuclide exposure are not generally considered to be as significant as those pathways presently incorporated into TERRA, it is recognized that the proposed pathways might

be significant under some specific circumstances, e.g. when these are present and the other pathways that are considered are absent.

Comment: Translocation of surface-deposited radionuclides from nonedible to edible parts of the plants should be included.

Reply: This process might be important for terrestrial food chain doses from strontium, cesium, or other soluble radionuclides. However, there is limited data available to quantify this process. It is recognized that this process could be included in the model for completeness, but it would require a significant amount of time to implement.

Comment: Recycling of radionuclides that leave cattle via urine and feces needs to be considered.

Reply: Radionuclide content in plants, beef, and milk are not calculated on a mass-balance basis. That is, air and soil concentrations are not depleted by plant harvesting or removal of cattle for slaughtering. Such an approach is conservative and eliminates the necessity for modifications to achieve mass-balance. Also, this pathway of radionuclide exposure is not generally considered to be significant. This recycling would perhaps result in some "hot spots", but time- and space-dependent considerations of the radionuclide description are beyond the scope of this effort. It is recognized, however, that the proposed pathway might be significant under some specific circumstances, e.g. when no other sources of radionuclides are present.

Comment: Doses from goat's milk and deposition on drinking water should be calculated.

Reply: The goat's milk pathway is important and should be included. The drinking water pathway is not generally considered to be as significant as those pathways presently incorporated into TERRA.

Comment: Specification of plant interception fractions may need to be revised.

Reply: This pathway was developed with little or no empirical data, and represents a theoretical approach. Its merits should be assessed with respect to empirical data on the interception fraction for the edible portions of plants. Interception fraction and their relationship to plant productivity, plant form, and weathering removal could be revisited in the future on the basis of any new data.

Comment: Revision of the resuspension pathway and model needs to be considered.

Reply: TERRA currently uses a mass loading approach to estimate resuspension of previously-deposited radionuclides. It is recognized that other models are available that could potentially give a more accurate representation of the time-dependent resuspension process. However, the resuspension pathway is likely to be significant only when this process is the major source of radionuclide input into the terrestrial system, and all models of resuspension are subject to large uncertainties.

Comment: Soil ingestion by animals should be a pathway considered in TERRA.

Reply: This pathway is important, but soil ingestion is influenced by cattle management practices, e.g., supplemental feeding, forage quality, climate, season, and herd size. Proper consideration of soil

ingestion by cattle and other grazing livestock would require a significant effort. However, some accounting for soil ingestion could be implemented by assuming a fixed percentage soil ingestion by weight.

Comment: Some of the specific parameter values presented need to be updated.

Reply: All of the parameter values used in TERRA and the SITE data base are based on the best information available at the time this work was done. Since that time, new data have likely become available for at least some of the parameters. In addition, the selection of default values for any data set requires the exercise of scientific judgement on the part of the researcher before selecting the particular value to be used. This judgement is always subject to change as new insights and information become available. For these data to remain current, they should be reviewed carefully on a regular basis. The resources to perform this review are not presently available at ORNL.

B.4. CONCLUSIONS

It is clear from the above discussion that this peer review process has provided many comments that should prove very useful to both EPA and ORNL. Some of these recommendations have already been acted upon. Many of the recommendations, however, must wait for implementation until further funding is obtained.

The next step in the process of updating the CRRIS should probably be the prioritization of the recommendations that have not yet been implemented. Emphasis should be placed on making those changes that are most likely to improve the accuracy of the results of the assessments that

will be performed by EPA using the CRRIS. Those items which make the modeling more complete should not be allowed to take priority over those changes that will actually improve the quality of the results of the calculations. It is likely that the CRRIS will need to be used for more actual assessment problems before priorities can be established.

INTERNAL DISTRIBUTION

- | | |
|------------------------------------|-------------------------------------|
| 1. C. F. Baes III, 1505, 6036 | 9. C. R. Richmond, 4500N, MS-6253 |
| 2. B. A. Berven, 7509, MS-6383 | 10-15. A. L. Sjoreen, 7509, MS-6383 |
| 3. K. F. Eckerman, 7509, MS-6383 | 16. ORNL Central Research Library |
| 4. A. R. Hawthorne, 4500S, MS-6126 | 17. Y-12 Technical Library |
| 5. S. V. Kaye, 4500S, MS-6124 | 18. ORNL Patent Section |
| 6. D. C. Kocher, 7509, MS-6383 | 19. Laboratory Records Department |
| 7. B. D. Murphy, 4500N, MS-6236 | 20. Laboratory Records, ORNL-RC |
| 8. F. R. O'Donnell, 4500S, MS-6102 | 21. RSIC Library |

EXTERNAL DISTRIBUTION

22. Office of Assistant Manager, Energy Research and Development, DOE-ORO, Oak Ridge, TN 37831
23. L. R. Anspaugh, Lawrence Livermore Laboratory, P.O. Box 808, Livermore, CA 94550
24. D. A. Baker, Battelle-Pacific Northwest Laboratories, P.O. Box 999, Richland, WA 99352
25. Wayne Bliss, Director, Office of Radiation Programs, Las Vegas Facility, U.S. Environmental Protection Agency, P.O. Box 18416, Las Vegas, NV 89114
26. E. F. Branagan, Office of Nuclear Reactor Regulation, Mail Stop P-712A, U.S. Nuclear Regulatory Commission, Washington, DC 20555
27. Gary Briggs, M.D. 80, National Oceanic and Atmospheric Administration, U.S. Environmental Protection Agency, Research Triangle Park, NC 27711
28. Jon Broadway, Eastern Environmental Radiation Facility, U.S. Environmental Protection Agency, P.O. Box 3009, Montgomery, AL 36193.
29. Byron Bunker, Office of Radiation Programs, ANR461, U.S. Environmental Protection Agency, Washington, DC 20460
30. F. J. Congel, U.S. Nuclear Regulatory Commission, Washington, DC 20555
31. T. V. Crawford, Savannah River Laboratory, Aiken, SC 29801

32. Philip Cuny, Office of Radiation Programs, ANR461, U.S. Environmental Protection Agency, Washington, DC 20460
33. Ray Dickson, National Oceanic and Atmospheric Administration, 550 Second Street, Idaho Falls, ID 93401
34. L. Dupuis, Brookhaven National Laboratory, Upton, NY 11973
35. G. G. Eicholz, School of Nuclear Engineering, Georgia Institute of Technology, Atlanta, GA 20332
36. Samuel L. Finklea III, Brookhaven National Laboratory, Upton, NY 11973
37. V. Fthenakis, Brookhaven National Laboratory, Upton, NY 11973
38. Floyd Galpin, Office of Radiation Programs, ANR460, U.S. Environmental Protection Agency, Washington, DC 20460
39. James Hardin, Office of Radiation Programs, ANR460, U.S. Environmental Protection Agency, Washington, DC 20460
40. J. L. Heffter, National Oceanic and Atmospheric Administration/ARL, 8060 13th Street, Silver Springs, MD 20910
41. Bruce Hicks, Director, Air Resources Atmospheric Turbulence and Diffusion Laboratory, National Oceanic and Atmospheric Administration, Oak Ridge, TN 37831
42. Rayford Hosker, Air Resources Atmospheric Turbulence and Diffusion Laboratory, National Oceanic and Atmospheric Administration, Oak Ridge, TN 37831
43. Chen-Tang Hung, Office of Radiation Programs, ANR460, U.S. Environmental Protection Agency, Washington, DC 20460
44. Bernd Kahn, Environmental Resources Center, 205 Old Civil Engineering Building, Georgia Institute of Technology, Atlanta, GA 30332
45. Tom Kirchner, Natural Resource Ecology Laboratory, Colorado State University, Fort Collins, CO 80521
46. R. W. Kurzeja, E.I. Dupont, Savannah River Plant, Aiken, SC 29808, Attn: R. W. Kurzeja, Bldg. 773-A
47. H. M. Mardis, Office of Radiation Programs, ANR461, U.S. Environmental Protection Agency, Washington, DC 20460
48. L. Marter, Savannah River Plant, Aiken, SC 29801

49. Terrence McLaughlin, Office of Radiation Programs, ANR460, U.S. Environmental Protection Agency, Washington, DC 20460
50. C. Miles, Brookhaven National Laboratory, Upton, NY 11973
51. C. W. Miller, Office of Nuclear Facility Safety, Illinois Department of Nuclear Safety, 1035 Outer Park Drive, Springfield, Illinois 62704
52. Samuel C. Morris, Brookhaven National Laboratory, Upton, NY 11973
53. J. Nagy, Brookhaven National Laboratory, Upton, NY 11973
- 54-57. C. B. Nelson, Office of Radiation Programs, ANR461, U.S. Environmental Protection Agency, Washington, DC 20460
58. Neal Nelson, Office of Radiation Programs, ANR461, U.S. Environmental Protection Agency, Washington, DC 20460
59. John M. Palms, Emory University, 1380 South Oxford Rd., N.E., Atlanta, GA 30322
60. Y. C. Ng, Biomedical and Environmental Research Division, Lawrence Livermore Laboratory, P.O. Box 808, Livermore, CA 94550
61. Barry Parks, Office of Radiation Programs, ANR460, U.S. Environmental Protection Agency, Washington, DC 20460
62. H. T. Peterson, Jr., Health Effects Branch, Office of Nuclear Regulatory Research, U.S. Nuclear Regulatory Commission, Washington, DC 20555
63. Charles Phillips, Eastern Environmental Radiation Facility, U.S. Environmental Protection Agency, P.O. Box 3009, Montgomery, AL 36193
64. Jerome Puskin, Office of Radiation Programs, ANR461, U.S. Environmental Protection Agency, Washington, DC 20460
65. R. P. Reed, Radiological Health, Tennessee Valley Authority, 401 Union Planters Bank Building, Chattanooga, TN 37401
66. Allen Richardson, Office of Radiation Programs, ANR460, U.S. Environmental Protection Agency, Washington, DC 20460
67. M. Rowe, Brookhaven National Laboratory, Upton, NY 11973
68. Madalyn Ronca-Battista, Office of Radiation Programs, U.S. Environmental Protection Agency, Washington, DC 20460
69. J. M. Smith, Eastern Environmental Radiation Facility, U.S. Environmental Protection Agency, P.O. Box 3009, Montgomery, AL 36193.

70. D. L. Strenge, Pacific Northwest Laboratories, Battelle Memorial Institute, Richland, WA 99352
71. H. Thode, Brookhaven National Laboratory, Upton, NY 11973
72. D. Bruce Turner, U.S. Environmental Protection Agency, 401 M Street, S.W., Washington, D.C. 20460
73. W. L. Templeton, Ecosystems Department, Battelle-Pacific Northwest Laboratories, Richland, WA 99352
74. Marilyn Varela, Office of Radiation Programs, ANR461, U.S. Environmental Protection Agency, Washington, DC 20460
75. James R. Watts, Savannah River Plant, Aiken, SC 29808
76. James Walker, Office of Radiation Programs, ANR461, U.S. Environmental Protection Agency, Washington, DC 20460
77. C. G. Welty, Jr., Office of Environmental Guidance (Room 3G-091), U.S. Department of Energy, 1000 Independence Avenue, SW, Washington, DC 20585
78. F. W. Whicker, Department of Radiology and Radiation Biology, Colorado State University, Fort Collins, CO 80521
79. Christopher Whipple, Electric Power Research Institute, P.O. Box 10412, Palo Alto, CA 94303
80. W. H. Wilkie, Carolina Power and Light Company, Harris E&E Center, Route 1, Box 327, New Hill, NC 27562
81. Anthony Wolbarst, Office of Radiation Programs, ANR460, U.S. Environmental Protection Agency, Washington, DC 20460
- 82-91. Office of Scientific and Technical Information, P. O. Box 62, Oak Ridge, TN 37831